# Local Problems, Local Solutions: Evidence from the Drug-Free Communities Program on Youth Development

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*The Drug-Free Communities (DFC) Support Program aims to curb youth substance use through multi-sector collaboration and locally tailored strategies. This study is among the first to evaluate the program's impact on youth development outcomes. Using the staggered difference-in-differences approach, we find that the DFC program reduces drug-related juvenile crime and improves academic outcomes. Effects are particularly pronounced where coalitions include government agencies. We further explore behavioral mechanisms, documenting reductions in marijuana use and opioid-related inpatient hospitalizations. A welfare analysis shows an infinite marginal value of public funds, suggesting that early-life prevention operates as a self-financing investment in public health.*

(*JEL*: I12, I18, I20, I31, J13, K42)

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Substance use among adolescents remains a persistent public health concern in the United States, with far-reaching implications for individual development and broader societal outcomes. Although policymakers increasingly acknowledge the importance of early intervention, funding remains heavily skewed toward treatment rather than prevention. This imbalance is evident in the 2020 budget of the Substance Abuse and Mental Health Services Administration (SAMHSA), which allocated $3.8 billion to treatment services and only $206 million to prevention initiatives.[1] While prevention programs are generally more cost-effective on a per-capita basis—relying on education, outreach, and early behavioral interventions—they continue to receive less investment because treatment efforts (e.g., addiction services, rehabilitation, crisis response) address acute, visible problems, whereas prevention's benefits are more diffuse, long-term, and difficult to quantify, making them less politically salient (Woolf, 2008).

This study evaluates the Drug-Free Communities Support Program, a federally funded initiative and one of the most ambitious and enduring community-based substance use prevention efforts in the United States. Jointly administered by the Office of National Drug Control Policy (ONDCP) and the Centers for Disease Control and Prevention (CDC), the DFC program supports multi-sector coalitions—including parents, schools, youth, law enforcement, public health agencies, and local businesses—that implement strategies tailored to community-specific needs under the guiding principle of "Local Problems, Local Solutions". By emphasizing cultural relevance, local empowerment, and youth engagement, the DFC program offers a bottom-up alternative to conventional policy tools—such as Prescription Drug Monitoring Programs and Naloxone distribution laws—which primarily operate through top-down regulatory restrictions (Kim, 2021; Doleac and Mukherjee, 2022).

Since its inception in 1998, the DFC program has awarded over 3,300 grants to coalitions operating in more than 2,200 communities across all 50 U.S. states (ONDCP, 2022). Collectively, these coalitions have reached approximately 55% of the national population. In 2022 alone, about 67 million Americans—roughly 20% of the population—resided in areas with an active DFC coalition. This includes approximately 2.6 million middle school students aged 12–14 (20% of all middle school youth) and 3.5 million high school students aged 15–18 (21% of all high school youth). DFC-funded coalitions serve a highly diverse set of communities: nearly two-thirds report targeting specific demographic subgroups, such as Hispanic and African American youth; 54% serve rural and/or frontier communities, and 45% serve suburban areas. This broad and inclusive footprint underscores the DFC program's national scale and longstanding relevance as a leading model of community-based substance use prevention in the United States.

[1]For further details, see https://www.samhsa.gov/sites/default/files/samhsa-fy-2022-bib.pdf.

This study examines two core youth development outcomes that are essential to long-term well-being: criminal delinquency and academic performance. Both are influenced by substance use and reflect broader dimensions of behavioral and cognitive maturation during adolescence. Preventing youth substance use may meaningfully improve these outcomes by supporting physical and mental health, fostering prosocial behavior, and reducing barriers to learning. The DFC program is particularly well-positioned to advance these goals, as it engages both community stakeholders and the youth themselves. Substance misuse is notably prevalent among juvenile offenders—84% of youth in juvenile facilities reported having used drugs, and 60% met diagnostic criteria for substance use disorder between 2008 and 2018 (BJS, 2023). This strong co-occurrence suggests that reducing youth substance use may lower the risk of criminal involvement. Moreover, substance use during adolescence has been linked to cognitive impairments (Ramey and Regier, 2019), which may undermine academic performance and long-term human capital development. While our primary analysis focuses on delinquency and educational outcomes, we also examine substance use behavior to better understand potential mechanisms through which community-based prevention programs like the DFC exert their effects.

Despite its national scale and two-decade history, rigorous evaluations of the DFC program's causal effects on youth outcomes remain surprisingly limited. This lack of empirical evidence is particularly consequential given both the substantial federal investment in prevention and the growing demand for evidence-based policymaking. Prior studies of community-based prevention models—such as Project ALERT (Ellickson et al., 2003), Communities That Care (Feinberg et al., 2007), and campus and community coalitions (Linowski and DiFulvio, 2012; Hallfors et al., 2002; Flewelling et al., 2005)—have offered valuable insights but often faced methodological challenges, including small sample sizes, limited long-term follow-up, or weak identification strategies. For example, Hallfors et al. (2002) evaluated the Fighting Back initiative using observational data and found limited effects on youth drug use and treatment engagement, though the study did not employ a causal identification strategy. Flewelling et al. (2005) reported reductions in marijuana and cigarette use among students exposed to a coalition-based program in Vermont, although the findings were geographically narrow and not readily generalizable. The DFC program presents a rare opportunity to evaluate whether a large-scale, locally tailored, multi-sector initiative grounded in community engagement and youth empowerment can produce measurable improvements in health, behavior, and academic achievement. Our study addresses this critical gap and provides timely evidence to inform the design, scaling, and allocation of resources for effective youth substance use prevention strategies nationwide.

To identify the program's causal impact, we exploit the staggered rollout of initial DFC grants across U.S. counties between 2008 and 2019. Using a difference-in-differences

(DID) framework and data from the Uniform Crime Reporting (UCR) Program, the Youth Risk Behavior Surveillance System (YRBSS), the Healthcare Cost and Utilization Project (HCUP), and the U.S. Department of Education, we document a range of positive effects: a significant 13.12% reduction in juvenile drug crimes (driven primarily by drug possession), a 10.85% reduction in property crimes, a 13.30% drop in marijuana use, and a 17.65% drop in opioid-related inpatient stays among youth. We also observe a significant improvement in math test proficiency rates (+0.063 SD), although reading proficiency rates remain unaffected. The main findings are robust across alternative definitions of treatment and control groups, staggered DID estimators, the inclusion of covariates, and various permutation and placebo tests.

To better understand what drives program effectiveness, we examine heterogeneity in treatment effects by local sector engagement, using manually collected data from public sources. We focus on five key sectors: schools, law enforcement, healthcare providers, local organizations, and governmental agencies. While all DFC coalitions operate under a common baseline framework, we find that coalitions involving local government agencies—such as health departments—consistently generate stronger impacts, particularly in reducing juvenile drug crimes and improving math test proficiency. In contrast, school and local organization involvement is associated with academic gains but not reductions in delinquency. This divergence likely stems from the constrained authority these entities possess in shaping youth behavior outside of educational settings. The consistent effectiveness of governmental agencies highlights their critical role in facilitating cross-sector coordination, mobilizing public resources, and enforcing environmental strategies that reinforce substance-free community norms.

Two key considerations are particularly important in evaluating the welfare impact of the DFC program. First, since DFC coalitions are eligible for federal support for a maximum of ten years, a central question is whether the program's effects persist after funding ends. Our results show no evidence of fading impacts following grant expiration. Second, potential spatial spillovers may bias estimates if neighboring counties experience indirect benefits from nearby DFC coalitions. Robustness checks addressing this concern indicate no evidence of such spillover effects. Together, these findings highlight the sustained and localized impact of the DFC program, offering valuable implications for community stakeholders and future policy design.

Beyond establishing effectiveness, assessing the program's economic efficiency is equally important for policy evaluation. Our welfare analysis shows that the DFC program delivers substantial social benefits. We estimate an infinite marginal value of public funds (MVPF), driven by a positive willingness to pay—capturing averted victimization costs and projected gains in future earnings from improved academic outcomes—and a negative net cost that reflects both the program's mechanical expenditures and the fiscal savings from reduced juvenile incarceration. The resulting

infinite MVPF underscores the program's considerable social value and fiscal efficiency.

In addition to these core findings, our paper contributes to two broad literatures: the evaluation of substance use interventions and the economics of youth human capital accumulation. Recent research on substance use policy has largely emphasized structural reforms, such as PDMP (Kim, 2021), medical/recreational marijuana policies (Cerdá et al., 2017; Hollingsworth, Wing, and Bradford, 2022; Wen, Hockenberry, and Cummings, 2015), naloxone access laws (Doleac and Mukherjee, 2022; Smart et al., 2024), and pharmaceutical market changes like the reformulation of OxyContin (DiNardi, 2021; Alpert, Powell, and Pacula, 2018; Powell and Pacula, 2021) or the relabeling of hydrocodone (Beheshti, 2023). Parallel lines of work have examined youth-specific interventions, including minimum legal drinking ages (Ahammer et al., 2022; Coate and Grossman, 1988; Yörük and Yörük, 2011), tobacco age restrictions (Friedman, 2015; Dave, Feng, and Pesko, 2019; DeSimone, Grossman, and Ziebarth, 2023), and excise taxes (Carpenter and Cook, 2008; Farrelly et al., 2001; Ross and Chaloupka, 2003). Together, these studies have deepened our understanding of how centralized levers shape substance use behaviors and their immediate consequences.

Complementing this body of work, our analysis of the DFC program sheds light on a different paradigm: demand-side, community-driven prevention. In contrast to regulatory approaches, the DFC model fosters intersectoral collaboration among local stakeholders and promotes a decentralized, participatory approach emphasizing capacity building, cultural relevance, and sustained engagement at the community level. Our findings suggest that this bottom-up model can serve as a critical complement to national drug policy—one that addresses behavioral health not only through restriction and remediation but also through proactive investment in youth development and community resilience. By evaluating DFC at scale, we expand the empirical foundation for prevention-based strategies and underscore the importance of community empowerment in shaping durable, localized policy solutions.

Our study also informs a second major area of research: child and adolescent human capital accumulation. We show that early-stage substance use prevention yields multifaceted returns, positioning community-based substance use interventions as powerful complements to traditional education and crime-reduction policies. On the one hand, we provide new evidence that DFC participation significantly lowers juvenile drug-related and property crimes by limiting youth exposure to high-risk environments and disrupting behavioral pathways linked to substance misuse—a connection well established in previous literature (Cuellar, Markowitz, and Libby, 2004; French and Maclean, 2006; Sen et al., 2009). These results offer timely and actionable insights for policymakers and align with existing strategies focused on mental health (Cuellar, McReynolds, and Wasserman, 2006; Deza, Lu, and Maclean, 2022), educational reform (Anderson, 2014; Cook and Kang, 2016; Depew and Eren, 2016), and labor market

opportunity (Fone, Sabia, and Cesur, 2023; Sviatschi, 2022).

On the other hand, we investigate how reductions in adolescent substance use—facilitated through the DFC program—translate into improvements in academic performance. These effects align with earlier findings on the cognitive and educational costs of youth drug use (McCaffrey et al., 2010; Zhao, Konishi, and Glewwe, 2012; Balsa, Giuliano, and French, 2011; Bugbee et al., 2019) and underscore prevention as a valuable mechanism for strengthening cognitive development during a critical window for skill formation. In contrast to educational reforms that emphasize school-based inputs such as curriculum design or teacher quality (Belfield and Levin, 2004; Harris and Sass, 2011), DFC operates through behavioral and environmental transformation, illustrating how non-educational interventions can yield substantial educational benefits. Collectively, our findings underscore the broader developmental spillovers of substance use prevention and position community-based coalitions as vital instruments for fostering long-run human capital accumulation.

The rest of this paper is organized as follows. Section I describes the background of the DFC Support Program. Section II discusses the data and variables of interest. Section III outlines the difference-in-differences estimators used in the primary results and the identification of causal effects. Section IV presents the effects of DFC grants on juvenile drug crime and academic performance. Section V explores the robustness checks and potential mechanisms. Section VI sheds light on the welfare effects of the DFC program. Section VII concludes the paper.

## I. Background

The Drug-Free Communities Support Program was established under the Drug-Free Communities Act of 1997 to reduce substance use among youth aged 18 and under. Funded by the Office of National Drug Control Policy, this program provides up to $125,000 per year to eligible coalitions over a 5-year funding cycle, renewable for a maximum of ten years. To apply, coalitions must meet twelve eligibility requirements, such as having been operational for at least six months, developing a comprehensive 12-month action plan targeting multiple drugs of abuse, and establishing a biannual outcome reporting system for continued funding. Applications are jointly reviewed by the ONDCP and the SAMHSA.

The key goal of the grant program is to reduce youth substance use and, over time, decrease substance use among adults. Participating coalitions selected up to five core substances as the primary focus of their prevention initiatives, with an average of 4.2 substances targeted. In 2022, 73% of DFC-funded coalitions implemented activities to address opioids and/or methamphetamine; among these, most targeted prescription drug misuse (98%) and fentanyl (74%). About 81% of DFC coalitions addressed youth vaping,

of which 96% focused on vaping nicotine/tobacco, and 89% addressed vaping marijuana.

To achieve this goal, a unique "DFC program model" was developed and implemented to establish and strengthen collaboration across 12 sectors, including schools, parents, youth, youth-serving organizations, state/local/tribal government agencies, law enforcement agencies, healthcare professionals, local media, and others. By integrating efforts across sectors, coalitions leverage synergistic change, enhancing both collective and individual sector impact. Specifically, community representatives from 12 sectors work as community-based coalitions to develop and implement practical strategies tailored to youth substance use patterns and contributing factors in the community to meet the prevention needs of youth and families in their community. In these multi-sector collaborations, youth act as stakeholders, rather than passive recipients. In 2022, about two-thirds of DFC-funded coalitions included a dedicated youth sub-coalition, with members actively participating in activities such as attending meetings, volunteering, mentoring peers, and serving as camp counselors.

Guided by the principle of "local solutions to local problems", DFC-funded coalitions are encouraged to implement context-specific, practice-based prevention activities across seven strategic domains (see Table 1). These include three individual-level strategies—(1) providing information, (2) enhancing skills, and (3) offering support—and four environmental strategies—(4) modifying access and barriers, (5) altering consequences, (6) changing the physical design, and (7) educating or informing about policy or legal changes.

Examples of commonly adopted activities include disseminating prevention messaging through brochures, flyers, and social media (providing information); conducting youth education and training sessions on opioid use warning signs, naloxone administration, and stigma reduction (enhancing skills); reducing home and school access to substances through drug take-back events and distribution of lockboxes or disposal bags (modifying access/barriers); enforcing regulations that prohibit sales to minors (altering consequences); removing drug-related waste from public spaces and enhancing surveillance in substance use hotspots (changing physical design); and assisting schools in acquiring tools such as the Halo vaping detection system (educating/informing about policy/legal changes). DFC coalitions frequently adopted a multifaceted approach: in 2022, more than two-thirds implemented at least one activity in five or more of the seven strategy categories. We estimate that approximately 19.1%–31.5% of local youth and 17.5%–37.2% of local adults are directly exposed to the DFC program.[2] These figures

[2]Prior research rarely documents the share of the local population directly engaged in the DFC program. Using Table C.1 of ONDCP (2022), we extract the reported numbers of youth and adults served, together with the number of coalitions involved. For the lower bound estimate, we focus on the activity with the largest single exposure—social networking—and calculate the average number of individuals served per coalition (4,501 youth; 13,642 adults). For the upper bound, we aggregate across all activities, yielding 7,428 youth and 28,969 adults. We then standardize these averages by the corresponding county-level population cohorts (23,575 youth; 77,800 adults).

indicate both substantial program reach and engagement extending beyond the program's primary youth focus.

According to the ONDCP (2022) report, DFC coalitions have made measurable progress toward their goal of preventing and reducing youth substance use within their communities. Comparisons of core measure data collected before and after coalition establishment indicate significant reductions in high school students' self-reported past 30-day use of alcohol, tobacco, marijuana, and prescription drugs. Additionally, youth reported increased perceived risk associated with prescription drug misuse, as well as greater perceived peer and parental disapproval of substance use. However, these findings are purely descriptive and do not account for underlying confounders or broader secular trends. To address these limitations, our study provides a rigorous causal evaluation of the DFC program's impact, leveraging longitudinal data and difference-in-differences methods across multiple youth outcomes. While our primary focus is on developmental outcomes such as delinquency and academic performance, we also examine substance use behavior to shed light on the mechanisms through which community-based prevention efforts may produce broader youth benefits.

## II. Data and Sample

We construct novel panel datasets by linking information on DFC-funded coalitions from the SAMHSA and CDC (2008–2019) to a range of administrative data sources. These include agency-level juvenile crime data from the Uniform Crime Reporting Program (2008–2019) and school-level standardized test proficiency rates from the U.S. Department of Education (2009–2018).

*The List of Drug-Free Communities Grant Funded Coalition*—We construct a comprehensive DFC-funded coalition dataset (2008-2019) by searching the Grant Award Archive from the SAMHSA and supplemental records from the CDC.[3] This dataset consists of coalitions that have ever received DFC grant funding, with details on coalition name, location (city and state), and the start and end years of each grant cycle.[4] We match coalition cities to counties using FIPS codes, assigning the county with the largest population when cities span multiple counties.[5] Coalitions missing from SAMHSA records—particularly in 2019—are supplemented using CDC data. Each county may host

[3]Source: Dataset compiled from the SAMHSA Grant Award Archive (https://www.samhsa.gov/grants/archive) and CDC coalition directory (https://www.cdc.gov/drugoverdose/drug-free-communities/coalitions.html). The combined dataset includes 732 DFC coalitions across 512 counties in all 50 states, excluding Washington, DC.

[4]Each community coalition can receive funding for a maximum of two cycles (10 years). For example, a coalition may receive a DFC grant from 2009 to 2019 and receive no further funding thereafter.

[5]According to the raw data, approximately 6.48% of DFC grants covered more than one county. Among this subgroup, 75% of the counties with the largest populations had a population coverage ratio exceeding 86.5%.

multiple DFC-funded coalitions, with individual coalitions initiating funding at different points in time. We define a county's DFC implementation year—also referred to as its DFC year—as the earliest year in which any coalition within the county first received DFC funding.

*Crime*—The crime data are sourced from the agency-year-level UCR dataset compiled by Kaplan (2021), which includes the number of crime arrests reported to police agencies across the United States, disaggregated by age, gender, and race.[6] This dataset encompasses most of the approximately 18,000 police agencies nationwide and applies standardized definitions of crime categories across agencies and over time Kaplan (2021). However, because participation in the UCR program is voluntary, the completeness and consistency of reporting vary across agencies and years. To address this limitation, we restrict our analysis to agencies that have reported consistently throughout the year (see, e.g., Bondurant, Lindo, and Swensen, 2018). Our final sample includes 10,952 agencies with at least one year of data between 2008 and 2019. This agency-year-level dataset is linked to the DFC data using county identifiers.

Juvenile offenses accounts for 20.41% of all recorded crimes, underscoring their significance within overall criminal activity.[7] We focus on three major categories of juvenile crime, defined as offenses committed by individuals under the age of 18: (1) drug crimes (sales and possession), (2) property crimes (burglary, theft, and motor vehicle theft), and (3) violent crimes (murder, rape, robbery, and aggravated assault). We compute crime rates by standardizing agency-level counts relative to the population served by each agency.

*Academic Performance*—We obtain academic performance data from the U.S. Department of Education, covering school years 2009-2010 through 2018-2019.[8] The dataset provides math and reading proficiency rates for middle and high school students at the school level. The proficiency rate is defined as the percentage of students scoring at or above the "proficient" level on state assessments, with higher values indicating stronger academic performance in the respective subject. To construct a composite measure of school performance, we average the test proficiency rate across all grades within a school for each subject.

Due to variations in assessment content and difficulty across states and years, these proficiency rates are not directly comparable nationwide. To address this, we follow the methodology used by Duque and Gilraine (2022) and Jacqz (2022), standardizing the proficiency rate within each state-year-subject cell. The resulting outcome variable has a mean of zero and a standard deviation of one, allowing effect sizes to be interpreted

[6]The UCR arrests data (version 15) are sourced from the Inter-University Consortium for Political and Social Research (ICPSR) (see https://www.openicpsr.org/openicpsr/project/102263/version/V15/view). The compiled data are at the agency-year level.

[7]This percentage is calculated using the UCR data from 2008–2019.

[8]The starting year of each academic year is used for merging with other datasets.

as a change in the standard deviation of the test proficiency. To reduce the influence of outliers, we exclude schools with exceptionally large testing cohorts—specifically those in the top one percent[9]—and those with very limited test participation, defined as fewer than 100 students tested.

*Sample and Descriptive Statistics*—Figure 1 illustrates the staggered rollout of 457 DFC-funded counties in the constructed list of DFC grants between 2008 and 2019: 66 counties initiated DFC funding in 2008, followed by 49 counties in 2009 and 52 counties in 2010. We exclude always-treated counties—those treated in the first year of the sample period (66 for the crime analysis and 115 for the education analysis)—from the analytical sample. We also drop treated counties that do not appear in the outcome data (53 for crimes and 57 for education). Our final sample for the crime analysis consists of 338 counties that hosted at least one DFC-funded coalition between 2008 and 2019, and 1,813 counties that received no DFC funding during the same period. For the education analysis, the corresponding figures are 252 treated counties and 1,558 control counties between 2009 and 2018.

Figure 2 maps the geographic variation in the timing of DFC implementation. Counties are shaded from pale to dark green based on their first year of funding. Those that received funding before 2009 (counties in the darkest green) are categorized as always treated and excluded from our staggered DID analysis. In contrast, counties without any DFC-funded coalition or with implementation after 2019 are considered never treated. The northeastern U.S., particularly coastal counties, exhibits a dense pattern of early DFC adoption, while the Midwest and West display more dispersed patterns, with a mix of early, late, and untreated counties.

Table 2 presents summary statistics. The average juvenile drug crime rate is 37.56 incidences per 100,000 population, with the majority attributed to drug possession (32.66 per 100,000; 25.12% of all juvenile crime) and the remainder to drug sales (4.19 per 100,000; 3.22%). Property crimes are more prevalent (77.99 per 100,000; 59.97%), while violent crimes occur less frequently (14.49 per 100,000; 11.14%). For academic outcomes, the average school-level proficiency rate is 56.79% in math and 62.57% in reading prior to normalization.

## III. Identification Strategy

We employ a difference-in-differences model to estimate the effects of DFC-funded coalitions by exploiting the staggered establishment of these coalitions across counties. We choose the CSDID estimator proposed by Callaway and Sant'Anna (2021) instead of the two-way fixed effect (TWFE) estimator for our benchmark results. Recent research

[9]The number of test-takers per school is right-skewed: the 99th percentile is 1,305 for math (maximum = 5,251) and 1,382 for reading (maximum = 5,437).

indicates that heterogeneous treatment effects over time, especially when treatments are rolled out in different periods, can bias DID estimates (Borusyak, Jaravel, and Spiess, 2024; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021), as well as event study estimates using TWFE estimation (Sun and Abraham, 2021). TWFE-DID estimators quantify an average treatment effect by weighting all pairwise comparisons, leading to potential issues due to the forbidden comparison between later-treated and early-treated groups. This inappropriate comparison might even result in an incorrect sign of treatment effects due to negative weighting issues (de Chaisemartin and d'Haultfoeuille, 2020). In contrast, the CSDID estimator avoids these complications by explicitly excluding forbidden comparisons and ensuring positive weighting.

To construct the CSDID estimator, we first define $g$ as the treatment cohort, representing the time when an area unit first had a DFC-funded coalition, and $G_g$ as an indicator for being treated in time $g$.[10] Additionally, we define $C$ as an indicator for a never-treated group, consisting of area units that never had a DFC-funded coalition during the sample period. The treatment effect of DFC-funded coalitions on the treated in cohort $g$ in calendar year $t$, denoted as $ATT(g,t)$, can be written as

$$ATT(g,t) = E[Y_t - Y_{g-1} \mid G_g = 1] - E[Y_t - Y_{g-1} \mid C = 1], \tag{1}$$

where $Y_t$, in our main analysis, represents the juvenile crime rate or academic performance in calendar year $t$.

Given the nonparametrically identified $ATT(g,t)$s, the parameter of interest, the overall treatment effect on the treated, ATT or $\theta_{DID}$, can be obtained by aggregating all the $ATT(g,t)$s across treatment cohorts and over time:

$$\theta_{DID} = \frac{1}{\sum_g \sum_t 1\{t \geq g\} P(G = g \mid C \neq 1)} \sum_g \sum_t 1\{t \geq g\} ATT(g,t) P(G = g \mid C \neq 1), \tag{2}$$

where $1\{t \geq g\}$ is an indicator for observations since the start of DFC funding; $P(G = g \mid C \neq 1)$ represents the share of counties granted in year $g$ out of all granted counties. Intuitively, this aggregation scheme takes a weighted average of $ATT(g,t)$s, where the weights are assigned by the relative number of observations.

The validity of our empirical design hinges on the parallel trends assumption. This assumption requires that, in the absence of funding, the outcome of interest in the treatment group would have evolved over time in parallel with that in the control group. Although this assumption cannot be directly tested, we use an event-study-type analysis, as shown in Equation 3, to provide some suggestive evidence:

$$\theta_{es}(e) = \sum_g 1\{g + e \leq T\} ATT(g, g+e) P(G = g \mid G + e \leq T, C \neq 1), \tag{3}$$

[10] In the crime analysis, the analysis unit is agency-year level, thus an area refers to an agency. In the academic performance analysis, the unit is school-year level, so an area refers to a school.

where $e$ denotes the number of years relative to the initiation of DFC grants. For instance, $e = -3$ indicates years that are three years prior to the grant initiation, and $e = 3$ indicates years that are three years post-initiation. Equation 3 is another aggregation scheme that aggregates the $ATT(g,t)$s of all the treatment cohorts observed $e$ years after or before the start of the DFC grants, using the relative frequency of observations as weights. We employ the long difference in estimating the pre-treatment parameters, selecting the year immediately preceding the grant initiation, $e = -1$, as the reference year. That being said, $\theta_{es}(e)$s reflect the effect size relative to this reference year. A necessary condition for the parallel-trends assumption is that $\theta_{es}(e)$s should all be close to zero and statistically insignificant for $e < 0$. Moreover, $\theta_{es}(e)$s for $e \geq 0$ illustrate the dynamic effects of the DFC program over time. We cluster the standard errors at the county level to accommodate correlations within counties over time.

*Potential Endogeneity*—One potential threat to our study's external validity is the endogeneity of receiving DFC grants. However, we argue that the approval of the DFC Support Program is plausibly exogenous due to both the program's design and our empirical evidence. Although the grant eligibility criteria are grounded in coalition readiness and structural capacity, not in local demographic or socioeconomic characteristics (ONDCP, 2022), meeting these criteria requires sufficient local resources. Specifically, as mentioned in Section I, applicant coalitions must: (1) have been active for at least six months; (2) include representatives from twelve required community sectors; (3) focus on youth substance use prevention; (4) address at least two substances; (5) provide a 100% non-federal match; and (6) have not received more than ten years of prior DFC funding. Therefore, counties with a high capital stock are more capable of unifying local sectors and acquiring the matching funds. This self-selection may pose a threat to the causality of the identification strategy. Aside from this, the selection of the target group is not taken into account in the application review process. Although many coalitions report targeting specific populations—such as Hispanic or African American youth—this likely reflects local needs assessments and implementation strategies rather than predetermined criteria for grant selection.

To empirically validate this, we examine the relationship between DFC grant receipt and a set of predetermined county-level demographics using a panel of 3,107 counties from 2008 to 2019. As shown in Table A1, we regress one-year lagged, current-year, and one-year forward indicators for DFC grant receipt on a set of predetermined demographic variables, drug policy indicators, and opioid supply measures. Across all specifications, we find no statistically significant relationships between DFC grant receipt and demographic variables such as the percent female, racial/ethnic composition, poverty rate, or age distribution. Additionally, none of the opioid supply variables (oxycodone, hydrocodone, fentanyl) are significantly associated with DFC funding. The only exception is a marginally significant positive association with medical marijuana laws

in two specifications—columns (2) and (3)—which is weak in magnitude and inconsistent across time periods. These results provide no evidence that DFC funding systematically favors counties with specific demographic or drug-use profiles.

Taken together, both institutional design and empirical patterns support the plausibility of exogenous grant allocation. Nevertheless, to adopt a conservative stance, we interpret our empirical results as the average treatment effect on the treated.[11]

# IV. Main Results

## A. *Descriptive Evidence*

Figure 3 depicts the evolution of juvenile drug crime rates and normalized math and reading proficiency rates before and after the introduction of DFC-funded coalitions. To analyze these trends, we define each county's timeline relative to its DFC implementation year, restricting the event window to a range of $-6$ to $+6$ years. We then calculate the average outcome for each relative year.

For counties that never received DFC funding, there is no explicit treatment year. To construct a comparable timeline for these control counties, we follow the approach described in Chu (2014). As an illustrative example, to calculate the average outcome one year after treatment (e.g., event time $+1$) for the control group, we first identify the calendar years that correspond to year $+1$ among treated counties. Since different counties received DFC funding in different years, the post-treatment year $+1$ spans a distribution of calendar years. We then use the relative frequency of these calendar years as weights to aggregate the corresponding outcomes for the control counties, so that the aggregated outcomes serve as a valid control for event time $+1$.[12]

For juvenile drug crime rates, Figure 3 reveals parallel pre-treatment trends between the treatment and control groups, followed by a clear divergence in outcomes immediately following grant receipt. For math and reading proficiency, the patterns are less

[11] We focus on the average treatment effect on the treated (ATT) because DFC grants are not randomly assigned across all counties. Instead, coalitions self-select into the application process and must meet programmatic and structural criteria to qualify. Accordingly, our estimates reflect the causal impact of receiving DFC funding among counties that obtained it, rather than the average effect for all counties (ATE). Given the selective nature of treatment and the absence of full ignorability or common support, ATT offers the most credible and policy-relevant interpretation of the estimated effects.

[12] To estimate the average outcome one year after a "pseudo-DFC-funded coalition" in control counties, we first identify the distribution of DFC adoption years among treated counties that contribute to event time $+1$. For instance, if 30% of treated counties adopted DFC-funded coalitions in 2010, 50% in 2012, and 20% in 2014, then their corresponding post-treatment year $+1$ occurs in calendar years 2011, 2013, and 2015, respectively. We use this distribution to assign weights—30% for 2011, 50% for 2013, and 20% for 2015—and then calculate the average outcomes for control counties in those same calendar years. If the average juvenile drug crime rates in control counties for 2011, 2013, and 2015 are 4.5, 5.2, and 4.8, respectively, we compute the weighted average using the assigned weights: 0.3, 0.5, and 0.2. This method ensures that control counties reflect the same distribution of calendar years as treated counties, preserving the event-time structure needed for a valid counterfactual.

pronounced, although there is a modest improvement in math performance following treatment. These visual patterns are suggestive but require formal validation through our empirical models.

## B. *Effect of DFC Program on Juvenile Drug Crime*

This subsection examines whether the DFC program has contributed to measurable reductions in juvenile drug offenses, a key indicator of youth behavioral outcomes. We begin by outlining the mechanisms through which DFC-funded strategies may influence juvenile crime, followed by empirical estimates using the CSDID framework.

As discussed in Section I, DFC coalitions implement both individual and environmental strategies that can influence youth criminal behavior through distinct pathways. Individual-level strategies focus on providing information, skills training, and supplemental activities designed to engage participants. These interventions aim to increase awareness of the risks associated with drug use and its potential consequences for both individuals and their communities, thereby reducing the likelihood of drug-related criminal offenses.

Environmental strategies, by contrast, seek to influence broader social norms and structures. These include enhancing community-level disapproval of drug misuse and criminal behavior, promoting community standards, and fostering collaboration across local institutions. These strategies may exert a deeper and more lasting influence on adolescent development by shaping peer dynamics, encouraging parental engagement, and reinforcing local drug-deterrence efforts.[13]

*Juvenile Drug Crime*—Using the CSDID estimator, Figure 4 presents both the overall ATT and the dynamic effects from the event study. The overall ATT—reported in the subtitle of Figure 4—is estimated at -6.81, with a standard error of 2.87, indicating a statistically significant reduction in juvenile drug crime following DFC implementation. Compared to the baseline average juvenile drug crime rate of 51.93 per 100,000 population in the treated group prior to treatment, this effect size represents a 13.12% reduction—a substantial decrease that highlights the potential of community-based interventions in reducing drug-related delinquency among youth.

A common concern is the potential violation of the parallel trends assumption in our design. First, concurrent policy changes targeting substance use and criminal justice may have varied across jurisdictions and coincided with DFC grant implementation. For example, the late 2000s and 2010s saw growing adoption of Good Samaritan laws and prescription drug monitoring programs (PDMPs), which could differentially affect drug crime trends in ways unrelated to the DFC program. If these overlapping reforms

[13]Peer effects and parental involvement have been shown to be key determinants of juvenile crime outcomes. See Díaz and Patacchini (2023) for empirical evidence highlighting the importance of these mechanisms.

were more likely to be adopted in areas receiving DFC grants, observed post-treatment changes could be erroneously attributed to the program. Second, pre-existing coalition activity presents another challenge. DFC grants are awarded to coalitions that already demonstrate baseline levels of organizational capacity, including a history of community mobilization and coordination around substance use prevention. It is therefore possible that observed reductions in juvenile drug crime could stem from the effectiveness of these already-active coalitions rather than the impact of DFC funding. If this were the case, one would expect to see declining crime trends even before the grant year, as coalitions began influencing behavior independent of DFC resources.

Figure 4 directly addresses these threats by estimating dynamic impact of the DFC program on juvenile drug crime rates using an event study. It reports the ATT estimate for each year relative to the last year prior to the DFC grant implementation. Prior to DFC grant implementation, drug crime trends remain relatively stable, with no discernible pre-treatment differences between treated and untreated areas. All estimates are statistically indistinguishable from zero, and the absence of any downward trend suggests that neither concurrent policy activity nor pre-existing coalition effects were driving crime reductions prior to funding. This lack of differential pre-trends provides strong empirical support for the parallel trends assumption, reinforcing the causal interpretation of the DFC effects.[14]

Moreover, the introduction of the DFC program marks the beginning of a notable decline in juvenile drug crime rates, with a consistent reduction observed from the second year post-implementation onward. The enduring nature of the reductions in juvenile drug crime rates reinforces the premise that concerted and localized strategies focused on curbing substance abuse can have a significant and lasting impact. The DFC program's comprehensive approach, which includes educational initiatives, collaborative ventures, and regulatory enforcement, has evidently made substantial strides in mitigating the negative effects of drug use among youth.

*Juvenile Drug Possession/Sales Crime*—We further disaggregate our analysis to examine juvenile drug possession and sales crimes separately—two components of juvenile drug offenses—to assess which category primarily drives the overall results.

Figure 5 shows that for juvenile drug possession crimes, the ATT post-DFC implementation stands at -6.05, with a standard error of 2.43. This represents a significant decline in drug possession crimes among youth, amounting to a 13.51% reduction relative to the pre-treatment mean for the treated, and aligns with the overall downward trend in drug crime rates depicted in Figure 4. Regarding juvenile drug sales offenses, the ATT is -0.83 with a standard error of 0.53, suggesting no statistically significant effect of DFC

[14] Although pre-trend stability mitigates the concern regarding coalition effects, data on coalition establishment dates and pre-funding activity are unavailable, preventing us from empirically disentangling coalition effects from the impact of DFC grant funding. Our estimates should therefore be interpreted as intent-to-treat effects of DFC participation.

programs on juvenile drug sales. The null effect is reasonable because, as shown in Table 2, the prevalence rate of sales offenses is much lower than that of possession offenses for juveniles.

### C. *Effect of DFC Program on Student Academic Performance*

Through its influence on youth behavior, the DFC program may also affect human capital accumulation—a closely intertwined aspect of adolescent development. Drug-related criminal activities are often linked to school absenteeism and reduced study time, both of which can undermine academic performance. Conversely, academic success may deter delinquent behavior. Higher test scores are associated with greater school completion rates, and existing research has documented the negative relationship between educational attainment and criminal activity (Anderson, 2014; Martins et al., 2022). Lochner and Moretti (2004) further documented that education influences the financial and psychological incentives for engaging in crime. Given the DFC program's observed effects on reducing juvenile drug crime, we next examine whether it also leads to improvements in students' math and reading achievement, potentially through enhanced school engagement, structured support systems, and reduced risky behaviors.

Figure 6 displays the event study results for student academic performance in math and reading tests. The pre-treatment coefficients for both outcomes are close to zero, indicating no significant pre-trends. For standardized math proficiency rates, we observe significant positive effects from the first through fifth year post-treatment. The aggregate ATT of 0.063 indicates that DFC grants significantly improve math proficiency by 0.063 standard deviations. In contrast, while there is a short-term improvement in standardized reading proficiency, the aggregate ATT is not statistically significant. As a robustness check, Figure A1 presents the effects on the percentile rank of math and reading proficiency, yielding similar results: the DFC program significantly increases math test proficiency by 0.013 percentile ranks but has minimal effects on reading proficiency.[15] The dynamic effects follow the same pattern as those of the standardized proficiency.

To understand the magnitude of these effects, we compare our estimated gains in standardized math proficiency with findings from prior studies linking substance use and educational outcomes. The effect size in our study (≈0.06 SD) is comparable to those reported by Carrell, Hoekstra, and West (2011) and Lindo, Swensen, and Waddell (2013), who found that legal access to alcohol significantly lowered grades by 0.06–0.10 and 0.033 standard deviations, respectively, and by Marie and Zölitz (2017), who documented that partial cannabis prohibition improved grades by 0.109 standard deviations.[16]

[15]The percentile rank of proficiency rates is defined as a school's rank in proficiency relative to the total number of schools within the same state-year cell. The unit of change in the outcome is one percentile rank.

[16]It is worth noting that our estimated effects are likely driven by the program's direct coverage and

The heterogeneous effects on academic performance are also consistent with Marie and Zölitz (2017), who found that reduced access to cannabis yielded larger improvements in courses requiring stronger mathematical skills. The differential impacts on math and reading proficiency may stem from the distinct effects of drug use on human brain structure. Specifically, drug use impacts white matter—commonly associated with reading skills (Meisler and Gabrieli, 2022)—and grey matter—linked to math abilities (Viesel-Nordmeyer and Prado, 2023)—in different ways. Hampton, Hanik, and Olson (2019) observed that substance use and addiction are often associated with atrophy and pathology in grey matter, whereas the evidence for white matter disruption is less consistent. These neurobiological patterns suggest that the DFC program may have a greater influence on math skills, helping to explain the absence of a significant effect on reading test proficiency.

## D. *Which Local Sectors Matter?*

Our findings thus far indicate that the DFC program significantly reduces juvenile drug-related offenses and enhances student academic performance in math. Given its multifaceted approach to addressing local problems, it is important to examine which types of local sector involvement are most closely associated with these impacts. To do so, we manually compile data from public sources on each DFC coalition included in the analytical sample.

As shown in Figure A2, the most frequently adopted strategies include *Providing Information*, *Educating about Policies or Laws*, and *Treatment*.[17] The most commonly engaged sectors are *Media*, *Schools*, and *Healthcare Professionals*. Due to challenges in accurately identifying strategy implementation across all coalitions—primarily arising from inconsistent reporting on public websites—we refrain from stratifying the sample by DFC strategy. Instead, we focus on local sector involvement, for which we have more reliable and consistently coded data.

Our analysis emphasizes five key sectors with sufficient data quality: schools, law enforcement, healthcare professionals, local organizations (including youth-serving, religious, and volunteer groups), and governmental agencies.[18] Figure A3 presents the

indirect peer effects from exposed to non-exposed youth. As noted in Section I, 19.09%–31.51% of youth are directly exposed to the DFC program. To incorporate peer effects, we adopt a simple homogenous-mixing model in which exposure propagates through social networks with a marginal peer effect of 0.578, as documented by Powell, Tauras, and Ross (2005). Under infinite diffusion, total exposure is given by $R = \frac{r}{[1-\beta(1-r)]}$, where $r$ denotes the direct exposure rate and $\beta$ the marginal peer effect. Substituting $\beta = 0.578$ yields $R = 0.359$ when $r = 0.1909$ and $R = 0.522$ when $r = 0.3151$. These values imply that DFC exposure ultimately reaches 35.9%–52.2% of the youth population. Dividing the aggregate effect of 0.063 SD by these total exposure rates gives an implied treatment effect of 0.176–0.121 SD among youth.

[17]The "Changing Access/Barriers" strategy was split into "Reducing Substance Access" and "Treatment" during data collection. The sector bar chart excludes the following category: "Other organization involved in reducing substance abuse".

[18]Media is excluded because it is not an exclusive attribute of the DFC programs. Business, parents,

correlation matrix between sector engagement and DFC strategies, revealing strong associations. For example, the engagement of healthcare professionals is highly correlated with the implementation of the treatment-based strategies. Similarly, schools and law enforcement show strong correlations with education about policies or laws and with consequence-based strategies, respectively. These patterns suggest that local sector involvement is systematically associated with specific strategic approaches. This motivates our analysis of heterogeneous treatment effects by sector engagement, enabling us to identify which types of local institutional participation contribute most to the program's overall effectiveness.

To estimate the effects by sector, we stratify treated counties based on the type of local sector engaged, while retaining a common control group of counties without DFC coalitions. We apply the CSDID estimator to each stratified sample.[19] The top panel of Figure 7 presents the effects of the DFC program on juvenile drug crimes. We find that only DFC coalitions that engaged local governmental agencies show a statistically significant reduction in juvenile drug crime, with an ATT of -10.19—slightly larger than the baseline. The pattern is similar for juvenile drug possession crimes.

In contrast, the bottom panel of Figure 7 shows a different pattern for standardized math proficiency. All five sector-specific ATTs are positive and statistically significant, with magnitudes exceeding the overall average. The largest effects are observed for DFC coalitions involving schools (0.115 SD), local organizations (0.121 SD), and governmental agencies (0.116 SD). Although the confidence intervals across sectors overlap—indicating that sectoral differences are not precisely estimated—the results consistently point to the effectiveness of partnerships with governmental agencies, such as local health departments. These collaborations appear to enhance both academic and behavioral outcomes, underscoring the pivotal role of government agency participation in promoting youth development. While we cannot fully isolate the impact of individual sectors due to small sample sizes and overlapping sector involvement, the results suggest that participation by governmental agencies may be a necessary—though not sufficient—condition for the program's success. Importantly, our findings do not diminish the contributions of other sectors; rather, they highlight the critical role that governmental agencies can play in enhancing coalition effectiveness.

We also find that school and local organization involvement is associated with improvements in math proficiency but not with reductions in drug-related crime. This likely reflects their primary focus on education and their role in providing structured

and youth are also omitted due to substantial measurement error in the data.

[19]One potential concern in interpreting these heterogeneous effects is that DFC coalitions may selectively engage certain sectors based on local characteristics. To address this, we examine the correlation between sector selection and county demographics. As shown in Figure A4, the correlation coefficients are uniformly weak across all pairs, suggesting that sector involvement is not strongly determined by underlying demographic patterns and can thus be considered relatively exogenous.

learning environments, which directly support academic performance but offer limited authority or capacity to address delinquent behavior.[20]

Our findings offer important insights into the effectiveness of the DFC program. Notably, authoritative institutions continue to play a central role, even within a framework built on the principle of "local problems, local solutions". Local agencies may be uniquely positioned to convene multiple sectors, especially in counties with fragmented service systems. Vulnerable groups, particularly those with substance use problems, are more likely to respond to policies endorsed by trusted public institutions. Additionally, local agencies possess distinct advantages in coordinating services, allocating DFC resources strategically, and tailoring interventions to community-specific needs. Their ability to maintain local data on youth substance use and disseminate information about emerging patterns strengthens their capacity for responsive programming. Finally, with sufficient regulatory authority, local agencies can directly influence the environment by reducing access to substances and expanding access to treatment services through coordination with healthcare providers.

## V. Robustness Checks and Behavioral Mechanisms

In this section, we assess the robustness of our main findings and explore potential mechanisms linking the DFC program to youth development outcomes. We begin by conducting a series of robustness checks to demonstrate that our results are qualitatively stable across alternative samples, model specifications, inference methods, and falsification tests. We then examine potential mechanisms, focusing on behavioral changes in youth substance use. While the DFC program may influence youth development through strengthened social cohesion, improved school climate, and life skill development, we lack direct measures of these pathways. Thus, we focus on youth substance use outcomes, which are directly observable in the data and may reflect the broader influence of the DFC program on youth behavior. As mentioned in Section I, ONDCP (2022) presented descriptive evidence suggesting that the DFC program helps prevent and reduce youth substance use in participating communities. We build on that foundation by providing a more rigorous causal evaluation of the program's impact on youth substance use.

### A. *Robustness*

*Spatial Spillover Effects*—Our baseline analysis classifies DFC-funded counties as treated and all others as controls. However, residents in neighboring non-DFC counties may

[20]Some local organizations offer afterschool programs, which may help to explain the strong effects observed on math test proficiency.

be indirectly exposed to the program's influence—for example, by attending school, participating in activities, or accessing services in adjacent DFC counties. Such spillovers violate the Stable Unit Treatment Value Assumption (SUTVA), which assumes no interference between units (Rubin, 1980). This violation can bias estimates—typically downward—if indirectly exposed counties are misclassified as untreated, thereby attenuating the observed treatment effects.

To assess this possibility, we refine the treated group by adding counties within a 20-mile radius of any DFC county, assigning them the earliest grant year among neighboring DFC counties as their treatment year. As shown in Figure A5, this adjustment leads to slightly smaller estimated effects across all outcomes. For instance, the ATT for juvenile drug crime changes from -6.811 to -6.584, for drug possession crime from -6.051 to -5.753, and for math proficiency from 0.0627 to 0.0518. The modest reductions in effect sizes suggest that spatial spillovers to nearby counties are limited and do not materially bias the baseline estimates.

As a complementary check, we also remove all non-DFC counties within a 20-mile radius of any treated county to construct a cleaner control group. The resulting estimates (Figure A6) closely match the baseline, reinforcing the conclusion that spatial spillovers do not meaningfully bias our main results.

*Alternative Estimators*—While our main analysis relies on the CSDID estimator, we assess the robustness of our findings by applying four additional DID estimators that differ in their handling of treatment effect heterogeneity and staggered adoption. These include the traditional TWFE estimator, as well as more recent methods developed by Sun and Abraham (2021), de Chaisemartin and d'Haultfoeuille (2020), and Borusyak, Jaravel, and Spiess (2024). Each of these newly developed approaches addresses the widely known limitations of TWFE under staggered treatment adoption and provides alternative identification strategies for causal inference.

The event study results from these estimators are presented in Figure A7. Across most methods, we observe broadly consistent dynamic patterns in the effects of DFC grants, supporting the temporal validity of our findings. The exception is the estimator by de Chaisemartin and d'Haultfoeuille (2020), which appears empirically less efficient in estimating juvenile drug crime outcomes, likely due to its stricter weighting scheme or reduced sample usage under some configurations.

The corresponding ATTs are reported in Tables A2 and A3 for juvenile crime and academic performance, respectively. Notably, the TWFE estimator yields effect sizes that are approximately half the magnitude of those produced by the other estimators. This attenuation is likely driven by the well-documented issue of negative weighting in TWFE when treatment effects vary over time—a recognized source of bias in staggered adoption contexts. Taken together, the consistency of results across the more recent estimators reinforces the credibility of our main findings and indicates that the observed effects are

not sensitive to the choice of estimation method.

*Stacked Sample*—In the baseline results, the timing of DFC adoption is highly staggered across counties, resulting in an unbalanced number of observations across years relative to the treatment. This sample imbalance may introduce compositional changes that confound the estimated treatment effects and lead to spurious conclusions regarding the treatment effect heterogeneity over time. We mitigate this concern by constructing a stacked DID sample for each outcome.[21] Figure A8 shows the corresponding CSDID estimates using the stacked sample. While the magnitudes of the aggregate ATTs are slightly larger across all outcomes, the overall interpretation of the results remains consistent.

*Inclusion of Covariates*—In our primary analysis, we find that the parallel trends assumption likely holds even without controlling for covariates, as indicated by the absence of pre-treatment effects shown in Figures 4–6. To further assess the robustness of this assumption, we test the conditional parallel trends assumption, which allows for differential trends across units with varying characteristics. This assumption posits that, conditional on observed characteristics, treated and control counties would have followed similar trends in the absence of DFC funding. To operationalize this, we add a set of time-invariant county characteristics, including the shares of female, White, Hispanic, individuals under age 25, and individuals aged 25 to 54,[22] to account for covariate-specific trends. Figure A9 shows the results using the doubly-robust CSDID estimator. The estimated effects remain broadly consistent across the major outcomes, with only slight reductions in magnitude, offering additional support for the robustness of our findings under alternative identifying assumptions.

However, a key limitation of the doubly robust CSDID estimator is that it incorporates only stationary covariates, preventing adjustment for contemporaneous policy changes that may also influence outcomes. For example, the effects of drug control policies—such as prescription drug monitoring programs (PDMPs), medical marijuana laws, or pill mill laws—may be partially captured in our baseline estimates. To evaluate the sensitivity of our results to these policies, we use the Borusyak, Jaravel, and Spiess (2024) estimator, which allows for the inclusion of time-varying county-level characteristics. As shown in Figure A10, the estimated ATTs closely resemble those in Figure A9, which adjusts only for static county characteristics. This suggests that the inability to control for contemporaneous drug policies does not substantially bias our results.

[21]For the drug crime outcomes, we define a time window of six years before and after treatment, using UCR data available from 2004 onward, and match control units that remain untreated within the same window. For the test outcome, we use a similar approach with a narrower window—four years before and after the treatment—due to the shorter data availability starting in 2009.

[22]We do not add drug policies here, as the CSDID estimator only allows for stationary covariates. Adding the time-varying covariates may introduce a "bad control" problem and induce bias (Roth et al., 2023).

*Permutation Test*—We conduct the permutation test for our main results (Heß, 2017), an alternative to classic inference that assesses whether the observed treatment effects in a given sample are likely to arise by chance even if the true effect is zero. To implement this test, we randomly reassign treatment years to counties—effectively breaking the original link between each county and its actual treatment year—while preserving the overall distribution of treatment timing. We repeat this process 1,000 times and re-estimate the CSDID ATT in each iteration. Figure A11 exhibits the distribution of these 1,000 simulated ATTs, with the vertical dashed line marking the baseline estimate. The corresponding randomization p-values (0.018, 0.021, and 0.036)[23] allow us to reject the null of no effect, lending further credibility to our findings.

*Falsification Test*—As a robustness check to assess whether our empirical model captures the true effects of the DFC program, we conduct a falsification test using an outcome that should not plausibly be affected by the intervention. Since the DFC program is explicitly designed to target youth through school-based prevention, sector collaboration, and community engagement, we examine adult drug-related crime rates—an outcome unlikely to be directly influenced by program activities. Figure A12 presents dynamic estimates close to zero, with an ATT of 0.20 (SE = 11.11), indicating no statistically significant impact. While some indirect community-wide spillovers on adults cannot be entirely ruled out—for example, through norm shifts or environmental strategies—the lack of any meaningful association in this test strengthens confidence in our identification strategy by confirming that our main findings are not driven by broader trends or spurious correlations.

## B. *Youth Substance Use*

Because the primary goal of the DFC program is to prevent substance use among adolescents, examining changes in youth substance use helps illuminate the behavioral pathways through which the program may influence downstream outcomes such as juvenile crime and academic performance. Previous studies have established links between adolescent substance use and various negative outcomes. For instance, Sen et al. (2009) found that substance use, especially among females, may causally contribute to delinquent behaviors such as property destruction, theft, and running away from home. Drug misuse has also been linked to higher risks of school dropout and lower educational attainment (Chatterji, 2006; McCaffrey et al., 2010). Motivated by this literature, we assess whether the DFC program influences youth substance use behaviors using nationally representative survey data.

We use data from the Youth Risk Behavior Surveillance System, administered

[23]We calculate the proportion of simulated ATTs whose absolute values exceed that of the baseline estimate to obtain p-values under randomization inference.

biennially by the Centers for Disease Control and Prevention from 2009 to 2019, to construct measures of youth substance use. This survey tracks health-related behaviors among students in grades 9 through 12. We define binary indicators for current alcohol, cigarette, and marijuana use based on responses to questions asking whether students consumed each substance on at least one day in the past 30 days. Table A4 summarizes the outcomes used in the mechanism analysis. On average, 30% of students reported alcohol use, 12.8% reported cigarette use, and 18.7% reported marijuana use. Since only state FIPS codes are available in the YRBSS dataset, treatment is defined at the state level. For each state-year cell, we compute the proportion of individuals aged 12–18 residing in counties with DFC grants, which proxies for state-level exposure to the DFC program. We then use the first quartile of the exposure distribution as the cutoff to define treatment status. States with exposure above this threshold at any point in the sample are classified as treated, with the first year of exceedance designated as the treatment year. States whose exposure remains below the threshold throughout the sample serve as the control group.

Figure 8 presents the estimated impact of the DFC program on the three substance use outcomes. The effects on alcohol and cigarette use are statistically indistinguishable from zero, suggesting that the program had no measurable impact on the use of these substances. In contrast, a significant reduction in marijuana use is observed: the ATT shows a 2.67 percentage point decline, equivalent to a 13.30% reduction relative to the pre-treatment mean. Moreover, the dynamic estimates show that the magnitude of the effects on marijuana use increases over time, suggesting that the DFC interventions may become more effective as youth internalize the harms and social consequences of marijuana use and encounter greater barriers to access.

These differential effects support the hypothesis that the observed decline in juvenile drug crimes may be partially attributed to reduced marijuana use. At the same time, the null findings for alcohol and cigarette use highlight the complexity of achieving broader substance abstinence among adolescents. One explanation is that alcohol and cigarettes, being more socially accepted and legally accessible, are less responsive to DFC prevention efforts. Another possibility is behavioral substitution: some adolescents who abstain from marijuana may shift to using alcohol or cigarettes—substances they perceive as less risky (Anderson, Hansen, and Rees, 2013; Dave et al., 2023). Therefore, while the DFC program shows effectiveness in reducing marijuana use, its broader influence on youth substance use remains limited in our analysis.

We benchmark the estimated effects of the DFC Support Program against findings from prior studies on community-level interventions targeting youth substance use—such as public health media campaigns and the Communities That Care program (see Table A5). The impacts of these interventions are mixed: while some studies report null effects, the majority find reductions in substance use. Among those with positive effects,

estimated odds ratios range from 0.26 to 0.50 for marijuana use, 0.40 to 0.69 for alcohol use, and around 0.49 for cigarette use. By comparison, our study yields an odds ratio of approximately 0.82 for any marijuana use, indicating a more modest reduction in odds.[24]

In addition to odds ratios, some studies report percentage reductions ranging from 9.15% to 24% for marijuana use and from 7.67% to 30% for tobacco use. Our estimated 13.30% decline in marijuana use falls within these ranges, though closer to the lower bound. Taken together, these comparisons suggest that the DFC program's impact on youth substance use is relatively modest when viewed alongside targeted interventions. Nonetheless, this more conservative estimate should be interpreted in context. Unlike many prior studies that focus on smaller-scale or highly targeted programs, our analysis uses a national sample and employs the CSDID, which captures average treatment effects across a diverse range of settings. As such, the observed effects—although smaller in magnitude—may reflect broader generalizability and sustained cost-effectiveness at scale.

## C. *Opioid-Related Hospitalization*

Marijuana is not the sole substance associated with drug-related crime. We also investigate the impact of the DFC program on opioid misuse. To capture this, we use opioid-related emergency department (ED) visits and inpatient hospitalizations as proxies for opioid-related outcomes among youth.

Using publicly available data from the Healthcare Cost and Utilization Project Fast Stats covering 2008 to 2019, we construct two state-level outcomes: opioid-related ED visits and inpatient stays per 100,000 population. These outcomes include cases of opioid-related disorders (ICD-10 code F11) and poisoning by narcotics (ICD-10 code T40). The dataset encompasses 46 states and provides breakdowns by age group, allowing us to focus specifically on a young cohort aged 1 to 24.[25]

Figure 9 presents the estimated effects of DFC grants on opioid-related hospital utilization.[26] We find no statistically significant change in ED visits, suggesting limited impact on acute overdose incidents, which are often difficult to prevent. In contrast, we observe a significant reduction in opioid-related inpatient stays. The overall ATT suggests a decrease of 10.71 inpatient stays per 100,000 population—a 17.65% reduction relative to the pre-treatment mean.

Event-study results shown in Figure 9 further support these findings. While the

[24]The probability of any marijuana use is around 0.1696 for the control group, and the corresponding ATT is -0.0267. Thus, the predicted probability of any marijuana use for the treated group is $0.1696 - 0.0267 = 0.1429$. Finally, the odds ratio $= \left(\frac{0.1429}{1-0.1429}\right) / \left(\frac{0.1696}{1-0.1696}\right) \approx 0.82$, which indicates an 18% reduction in odds of any marijuana use.

[25]In the publicly available HCUP dataset, age is reported in bins, preventing further separation of juveniles from the broader 1–24 age cohort.

[26]Because the HCUP dataset is at the state level, state-level exposure is generated analogously to the approach used in the YRBSS dataset, except that it focuses on individuals aged 1–24.

estimates for ED visits exhibit a downward trend, they are imprecisely estimated. For inpatient stays, significant treatment effects emerge three to six years after the initiation of DFC funding. Taken together, these results suggest that the DFC program may help reduce serious and chronic forms of opioid misuse among youth, complementing its impact on marijuana use.

## VI. Assessing the Broader Welfare Impacts of DFC Funding

This section evaluates the broader welfare implications of the DFC program. We begin by examining the sustainability of its effects following the discontinuation of funding. We then assess whether the program's benefits extend beyond drug offenses to other juvenile crimes. Finally, we estimate the marginal value of public funds to quantify the program's overall social value.

### A. *Do Treatment Effects Persist Beyond Active Funding?*

The sustainability of the DFC program's effects is directly relevant to social welfare. Since each DFC coalition can receive funding for a maximum of ten years, treatment may eventually be discontinued—a feature not explicitly accounted for by the CSDID estimator, which assumes continued exposure. We formally examine whether program effects persist after funding ends by treating discontinuation as a separate treatment event. If outcomes remain stable, our baseline estimates reflect enduring impacts. However, if outcomes revert to pre-treatment levels, treated counties may resemble untreated ones, introducing downward bias in our estimates.

To investigate this empirically, we conduct a local event study, treating grant discontinuation as the event of interest. The treatment year is defined as the first year in which a county ceases to receive the grant. We focus on an eight-year window with no missing observations for both treated and control counties. The treated group consists of counties that received funding during the four years preceding the treatment year and no funding during the four years following it. The control group consists of counties that continued to receive uninterrupted funding over the same eight-year period. The results, presented in Figure 10, show no significant increase in drug crime rates and no significant decrease in math test proficiency in the years following discontinuation. These findings suggest that the effects of DFC funding are durable, persisting even in the absence of ongoing financial support. This strengthens confidence in the robustness of our main estimates and provides encouraging evidence that time-limited funding can yield long-lasting social returns—thereby reducing fiscal costs without compromising welfare outcomes.

## B. *Beyond Drug Offenses: Impacts on Juvenile Crimes*

Drug offenses frequently coincide with other forms of criminal activity, serving as both a precursor to and a consequence of violent and property crimes. For instance, drug use may lead to violent confrontations or, conversely, the need to fund drug habits may result in property crimes (Bennett, Holloway, and Farrington, 2008; Brownstein, 2015). By addressing the cycle of drug abuse, the DFC program curtails drug offenses, as demonstrated in our main results, and may also help prevent escalation into more severe criminal behaviors among youth—an issue explored in this subsection. This is critical for understanding the full scope of benefits delivered by DFC interventions.

The top panel of Figure 11 illustrates the DFC program's impact on juvenile violent crime. The aggregate ATT is -0.60 (SE = 0.87), statistically indistinguishable from zero. Post-implementation ATTs remain close to zero and imprecise, suggesting no meaningful reduction in violent crime among youth. The bottom panel examines juvenile property crime. We find a significant ATT of -11.05 (SE = 4.26), indicating a 10.85% decline from the pre-treatment mean of 101.88. Juvenile property crime rates are relatively stable prior to program initiation, and the effect does not emerge immediately, as shown by the insignificant coefficients in the first two years post-implementation. However, the subsequent years display gradual and consistent declines, reflected in a downward trend. This trend mirrors that of juvenile drug crime and provides suggestive evidence of the co-occurrence between these two types of offenses.

## C. *Marginal Value of Public Funds*

While our results demonstrate that DFC grants reduce juvenile drug and property crimes and improve academic performance, the broader question remains: do these grants yield net social benefits given their substantial funding levels? To address this, we estimate the marginal value of public funds associated with awarding one DFC coalition over a ten-year funding period. The MVPF is defined as the ratio of beneficiaries' willingness to pay (WTP) to the government's net cost.[27] It measures the extent to which government expenditures translate into benefits for policy beneficiaries. Compared with a traditional cost-benefit framework, the MVPF provides a more appropriate welfare measure for this context (Hendren and Sprung-Keyser, 2020).

*WTP*—The willingness to pay comprises averted victimization costs and projected

[27] Formally,

$$MVPF = \frac{WTP}{MC + FE},$$

where $WTP$ denotes beneficiaries' willingness to pay for crime reduction and improvements in academic performance; $MC$ is the mechanical cost of the DFC program; and $FE$ represents fiscal externalities. The denominator $MC + FE$ defines the net cost. The MVPF, which measures the shadow price of delivering government resources to beneficiaries, ranges from zero to infinity.

gains in future earnings resulting from improved academic performance. First, following Aslim, Mungan, and Yu (2024) and Miller et al. (2020), we assign a value of $5,256 (2019 USD) per drug crime and $3,131 per property crime to the averted victimization costs. Multiplying these unit costs by the average reductions in juvenile drug and property crimes observed in our baseline estimates yields average annual avoided victimization costs of $16,829 and $18,297 per county, respectively.[28]

Second, we translate improved math performance into long-term economic gains. Chetty, Friedman, and Rockoff (2014) documented that a one-standard-deviation increase in math scores is associated with a $2,998 increase in annual earnings at age 28. Applying this to our observed 0.063 standard deviation increase and multiplying by the average number of students per county, we estimate an annual gain of $1,231,514.[29] Summing these two components, we estimate a total WTP of approximately $1,266,639 per county per year.[30]

*Net Cost*—The net cost to the government is the sum of the mechanical cost and the fiscal externalities. The mechanical cost of the DFC program includes the direct federal allocation of $125,000 per year and required non-federal matching contributions (e.g., donations, volunteer time), which range from 100% to 150% depending on grant duration.[31] To be conservative, we apply the 150% rate, resulting in a mechanical cost estimate of $312,500 per county per year.

Moreover, the beneficiaries' behavioral responses to the DFC program induce fiscal externalities. Incarceration, particularly of youth, is costly for the government. Our main findings regarding reduced juvenile drug and property crimes suggest positive fiscal externalities, which lower the cost of implementing the DFC program. Using the estimate from the Justice Policy Institute (2020), which places the average annual cost of youth confinement at $35,837, we calculate the averted incarceration costs. Applying this figure to the crime reductions, we estimate savings of $114,742 for drug crimes and $209,424 for

[28]Our results for juvenile drug crime indicate a reduction of 6.811 per 100,000 population, and the average population in 2019 of our CSDID sample is 58,761. The total averted victimization cost for drug crimes per county is $5,256 \times 58,761 \times 6.811 \times 0.8/100,000 = 16,829$ in 2019 USD, where the factor of 0.8 adjusts for the eight significant post-treatment years within the ten-year study period. Similarly, the total averted victimization cost for property crimes is $3,131 \times 5,8761 \times 11.050 \times 0.9/100,000 = 18,297$, where 0.9 reflects the nine significant post-treatment years out of the ten-year period.

[29]We first convert the increment in future earnings from Chetty, Friedman, and Rockoff (2014) into $3,574 in 2019 USD. The total future income gain from improved academic performance is then calculated as: $3,574 \times 0.063 \times 34,381,000 \times 0.5/3,143 = 1,231,514$, where 0.063 is the ATT from the baseline result; 34,381,000 is the total number of secondary school students in 2021, taken from https://www.census.gov/content/dam/Census/library/publications/2023/acs/acs-55.pdf; 3,143 is the number of counties; 0.5 reflects the five significant post-treatment years, under the assumption of no effects in years nine and ten.

[30]Notably, the WTP is likely understated, as our analysis does not account for potential effects on labor market outcomes, long-term health improvements, or intertemporal gains.

[31]For more details, see https://www.samhsa.gov/sites/default/files/grants/dfc-standard-award-terms-091719.pdf.

property crimes, totaling $324,165 per county per year as the value of fiscal externalities.[32] By subtracting these positive fiscal externalities from the mechanical cost, we obtain a net cost of -$11,665.

Finally, the negative net cost corresponds to a special case in Hendren and Sprung-Keyser (2020). The combination of a positive WTP ($1,266,639) and a negative net cost (-$11,665) implies an infinite MVPF, indicating that the DFC program effectively pays for itself and results in a Pareto improvement. An infinite MVPF highlights the economic value of the existing funding structure: community coalition efforts not only recoup the costs but also generate substantial social welfare gains. This result also underscores the effectiveness of the "Local Problems, Local Solutions" model in addressing youth substance use. These findings carry important policy implications, suggesting that ONDCP should continue supporting new DFC coalitions in the coming years. Consistent with the pattern summarized in Hendren and Sprung-Keyser (2020) that policies targeting children generally yield higher MVPFs than those targeting adults, our estimated MVPF supports the welfare-based rationale for early-life substance use prevention.

## VII. Conclusion

This study provides the first causal evidence on the impact of the Drug-Free Communities Support Program on youth development outcomes. We find that implementation of the DFC program leads to a 13.12% decrease in juvenile drug offenses and a 0.063 standard deviation increase in math test proficiency. As supporting evidence, we also observe reductions in marijuana use and opioid-related hospitalizations, confirming the program's effectiveness in curbing substance use among youth. While substance use prevention is the program's primary goal, our findings suggest that its benefits extend further—regulating social behaviors and enhancing human capital formation. Our findings highlight the critical role of addressing youth substance use, which is intricately linked to multiple dimensions of youth development. As such, the potential gains from effective intervention are substantial. Our welfare analysis confirms that the program's MVPF is infinite, underscoring the value of continuing—and potentially expanding—DFC funding as a self-financing strategy for promoting the well-being of future generations and strengthening community health.

Our results also highlight the promise of community-based collective action in addressing the broader drug crisis. The age-adjusted drug overdose death rate increased from 8.9 deaths per 100,000 population in 2003 to 32.6 in 2022 (Garnett and Miniño,

[32]The total averted incarceration cost for drug crimes is $35,837 \times 58,761 \times 6.811 \times 0.8/100,000 = 114,742$ in 2019 USD. For property crimes, the corresponding averted incarceration cost is $35,837 \times 58,761 \times 11.050 \times 0.9/100,000 = 209,424$.

2024), and although it slightly decreased to 31.3 in 2023, the long-term trend remains concerning. The DFC model offers key insights into potential solutions. First, our study suggests that targeting youth is both effective and worthwhile. Effectively reducing and preventing substance use among young people may, over time, contribute to lower overdose mortality as this cohort ages. Our findings suggest that public health budgets could be more efficiently allocated by investing earlier in life. Second, our study reveals that effective drug prevention requires more than merely restricting access to substances. Policy approaches that focus on changing youth attitudes and shaping the school and community climate are more likely to generate sustained reductions in substance use by fostering internalized norms and community support.

The success of the DFC model also raises the possibility of adapting community coalition approaches to other public health challenges, such as youth mental health. However, scaling such models poses challenges. Prior research (Zakocs and Edwards, 2006) noted that effective coalitions share several traits: formal governance procedures, strong leadership, active member participation, diversity, inter-agency collaboration, and group cohesion. The DFC program embodies these characteristics, and our findings show stronger effects on youth development outcomes where local governmental agencies are involved. By aligning individual and community interests, the DFC approach influences both individual behaviors and broader community health, suggesting a promising model for future public health initiatives.

While our study provides a comprehensive assessment of the initial effectiveness of the DFC program, several limitations warrant discussion. First, the use of aggregate data limits our ability to examine how the DFC program shapes neighborhood-level drug-free environments, even though we find no evidence of spatial spillovers across counties. Second, the lack of data prevents us from examining potential mechanisms such as changes in parental attitudes toward substance abuse or shifts in peer-group risk perceptions—both of which may play important roles in shaping youth behavior. Third, although our findings to date do not reveal any adverse effects, this does not rule out the possibility of unintended consequences. For instance, the program could potentially lead to substance use substitution, increased stigmatization or surveillance of at-risk youth, or crowding out of other support services.

Future research should investigate these possibilities and explore longer-term outcomes—such as labor market performance in adulthood—for individuals exposed to the DFC program during adolescence. Such extensions would provide a more complete understanding of the program's societal impact and support a fuller evaluation of its value as a policy tool for addressing youth substance use.

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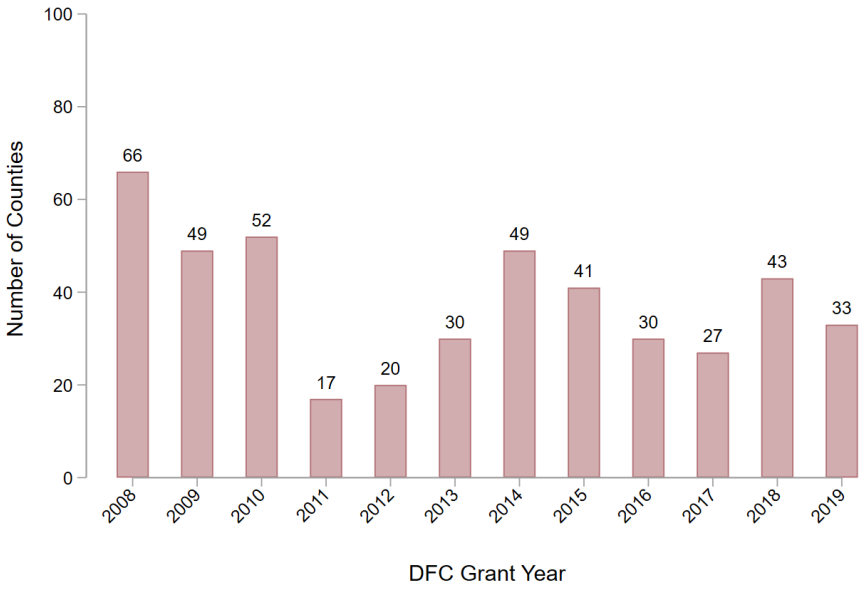
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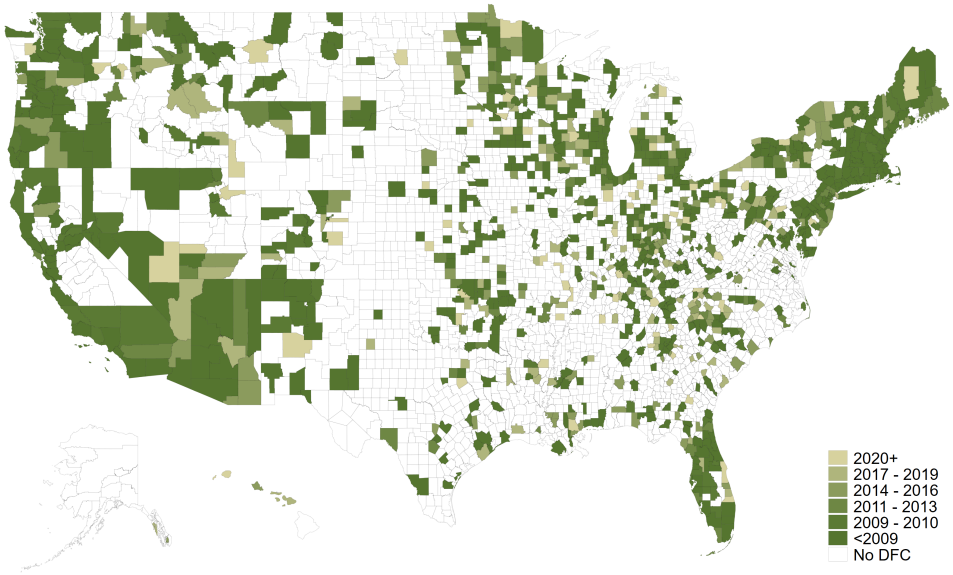
# Figures and Tables

**Figure 1. Number of Counties First Receiving DFC Grants by Year**



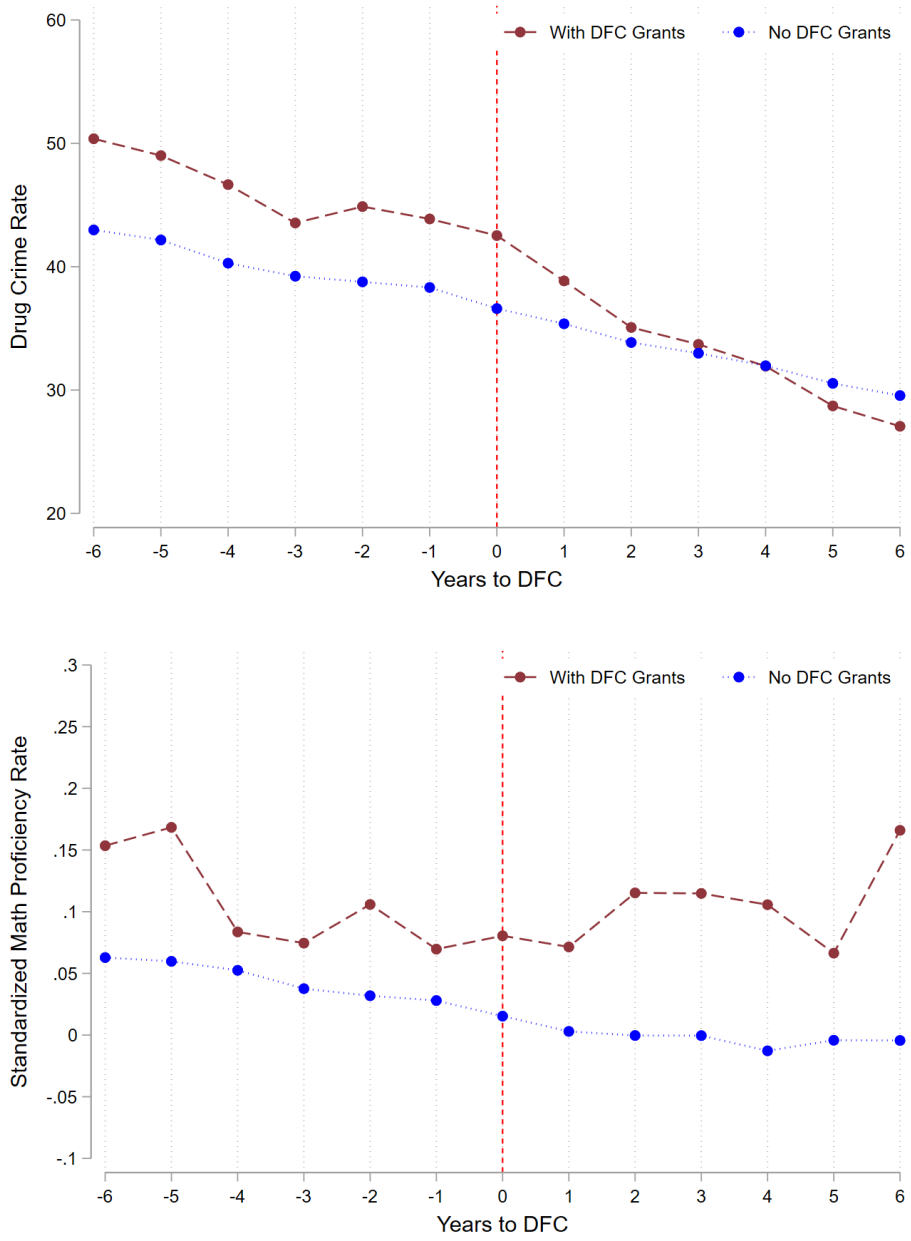

*Notes*: The DFC grant data are collected from SAMHSA and the CDC. This histogram presents the number of counties that received DFC funding for the first time during 2008–2019.

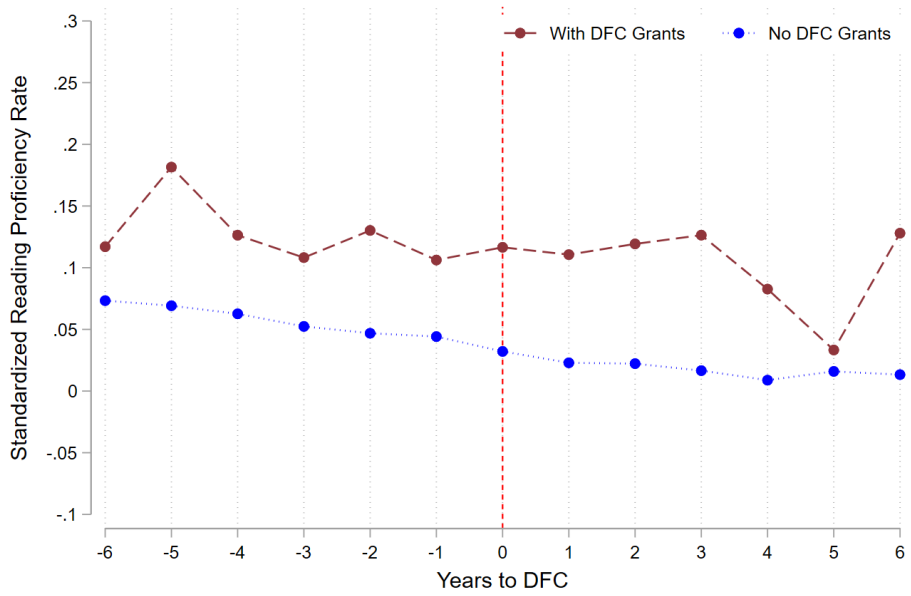
**Figure 2. Geographic Distribution of Counties with DFC Grants by Year**




*Notes*: The DFC grant data are collected from SAMHSA and the CDC. This map shows the geographic distribution of DFC grant recipients by county, with the legend indicating the first year of DFC funding.

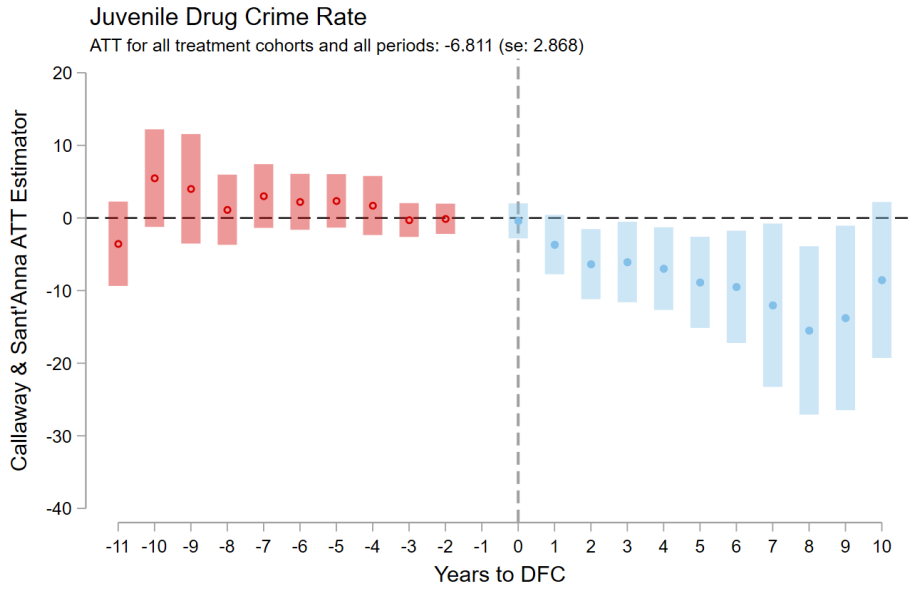
**Figure 3. Time Trend of Juvenile Drug Crime and Academic Performance**





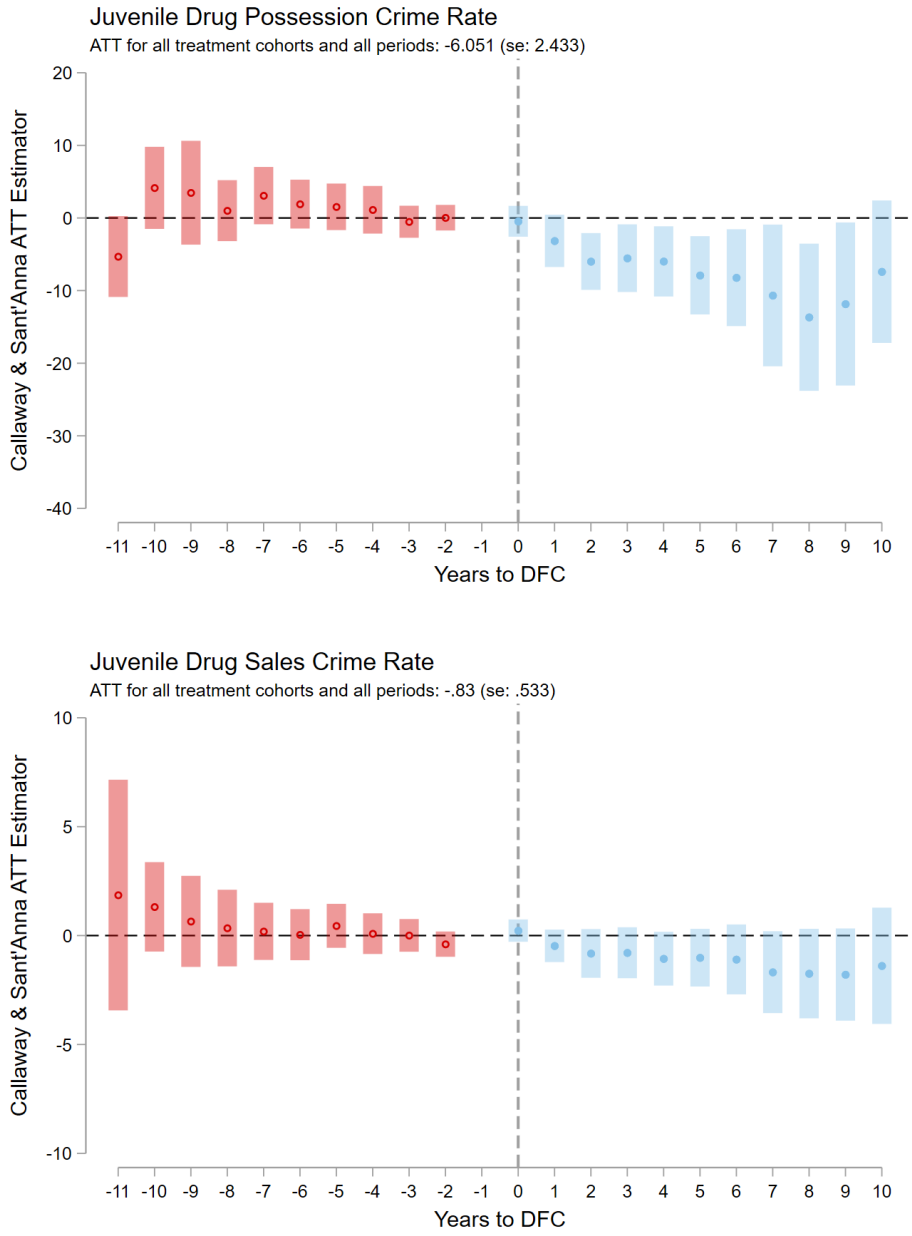

*Notes*: Data are from the UCR Program (2008–2019) and the Department of Education (2009–2018). Following the method in Chu (2014), these figures show trends in outcomes by years relative to initial DFC grant receipt for both DFC and no-DFC counties. Each dot represents the average outcome prevalence in a given relative year within our analytical sample.

**Figure 4. Effects of DFC Grants on Juvenile Drug Crime Rate**



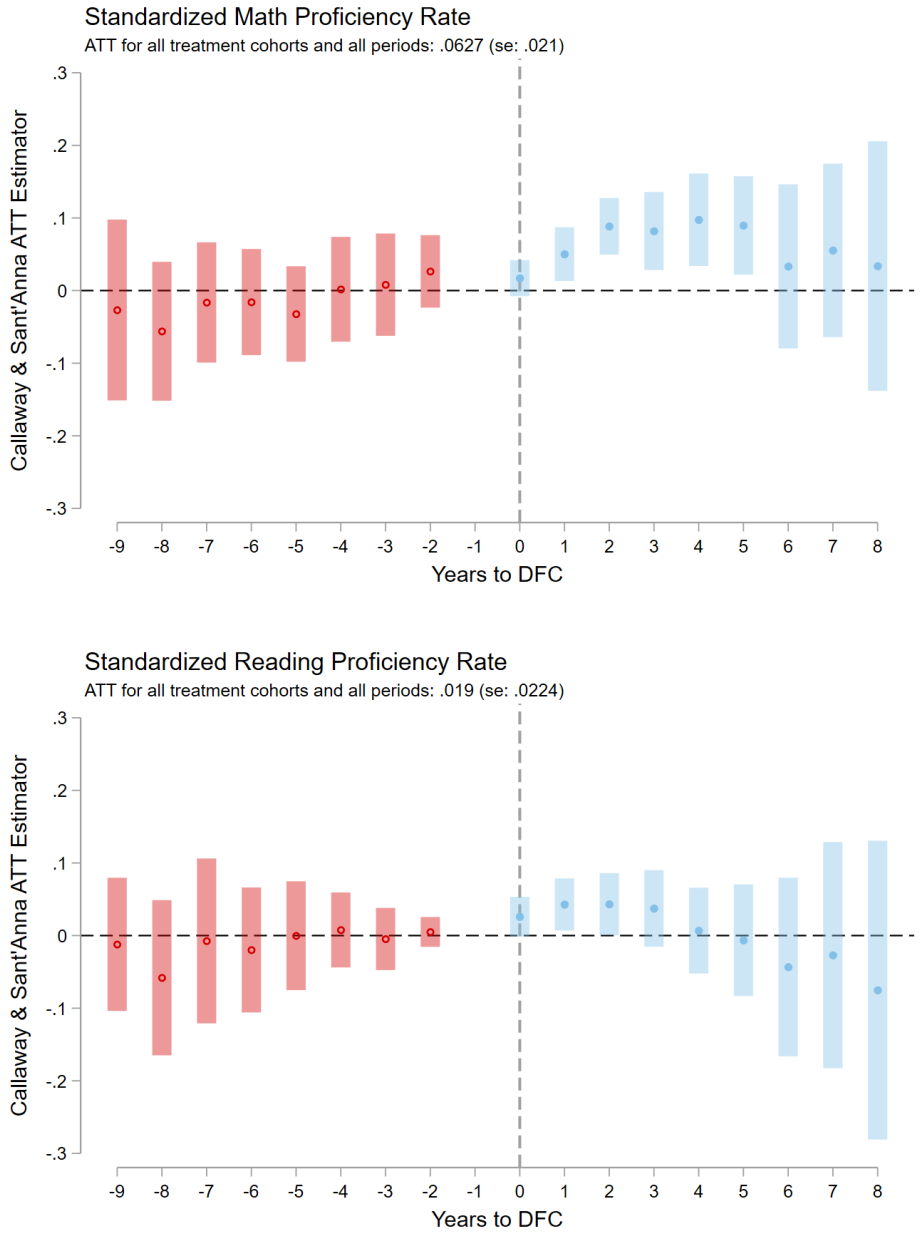

*Notes*: The working sample consists of 6,470 police agencies from 2008–2019. This figure shows the dynamic effects of DFC grants on juvenile drug crime (per 100,000 population), estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

**Figure 5. Effects of DFC Grants on Juvenile Drug Possession and Sales Crimes**



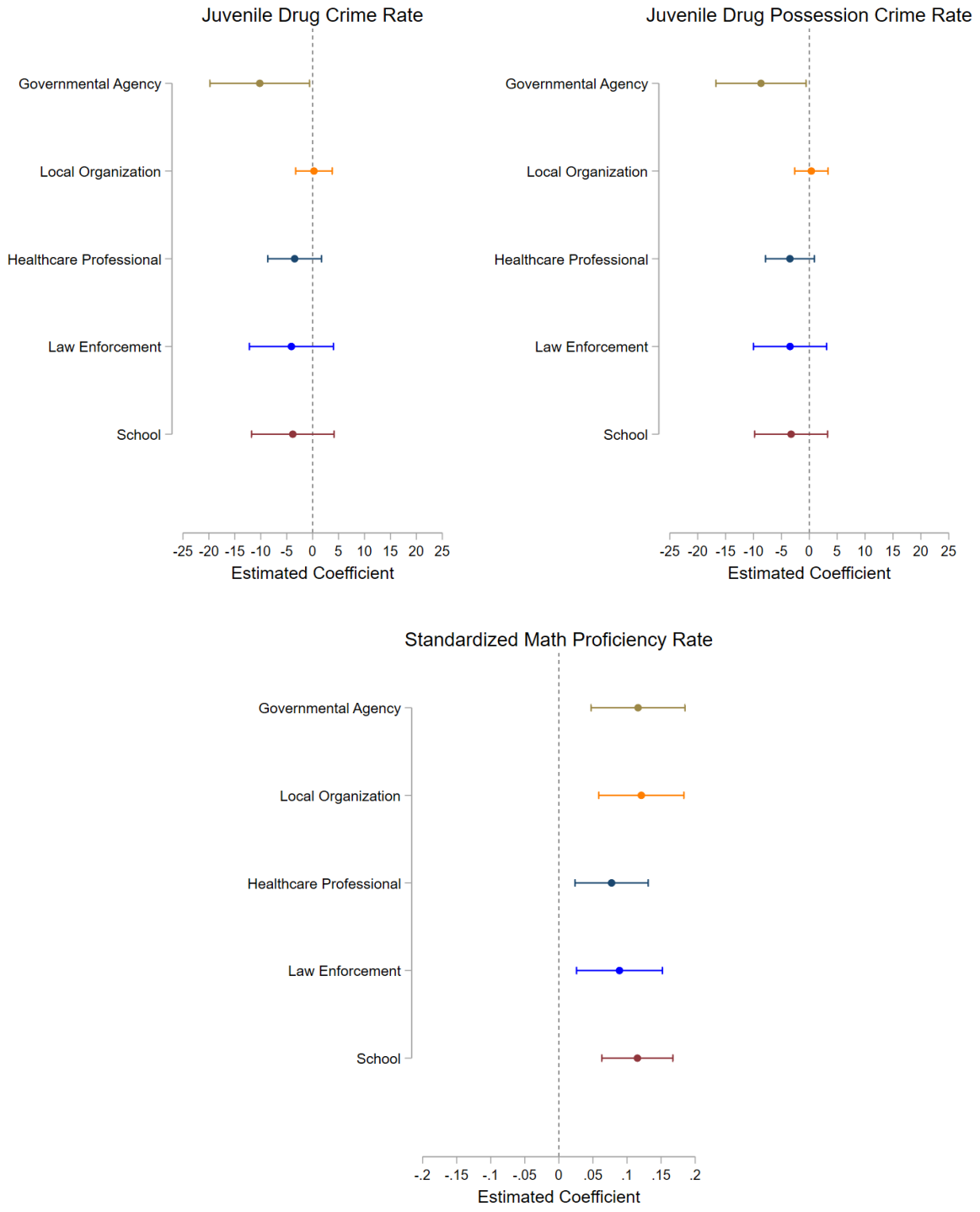

*Notes*: The working sample consists of 6,467 police agencies for juvenile drug possession crimes and 6,482 agencies for drug sales crimes from 2008–2019. These figures show the dynamic effects of DFC grants on juvenile drug possession and sales crimes (per 100,000 population), estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

**Figure 6. Effects of DFC Grants on Standardized Test Proficiency**



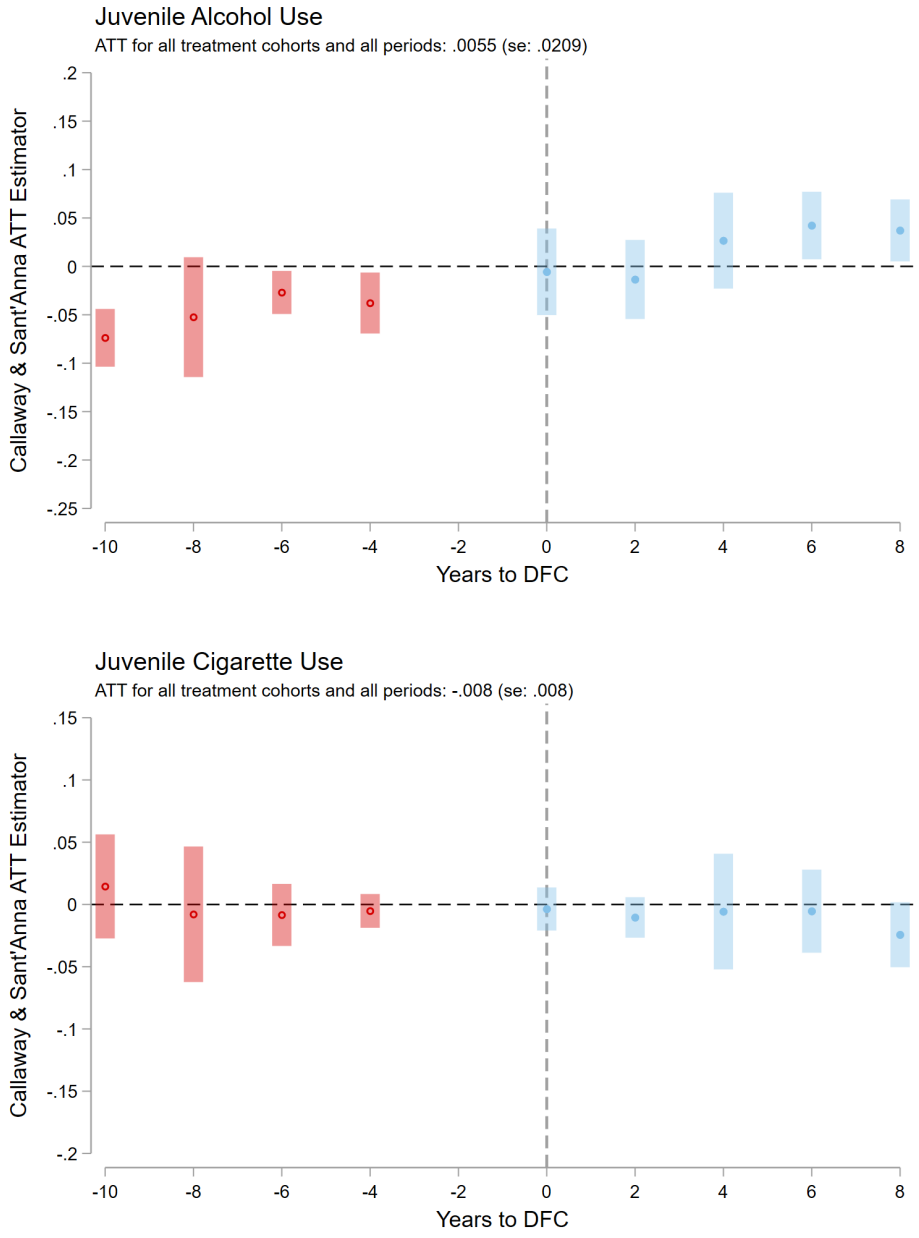

*Notes*: The working sample is sourced from the school-level Department of Education data (2009–2018) and contains 11,285 and 11,451 schools for math and reading proficiency, respectively. These figures show the dynamic effects of DFC grants on the standardized math and reading test proficiency, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

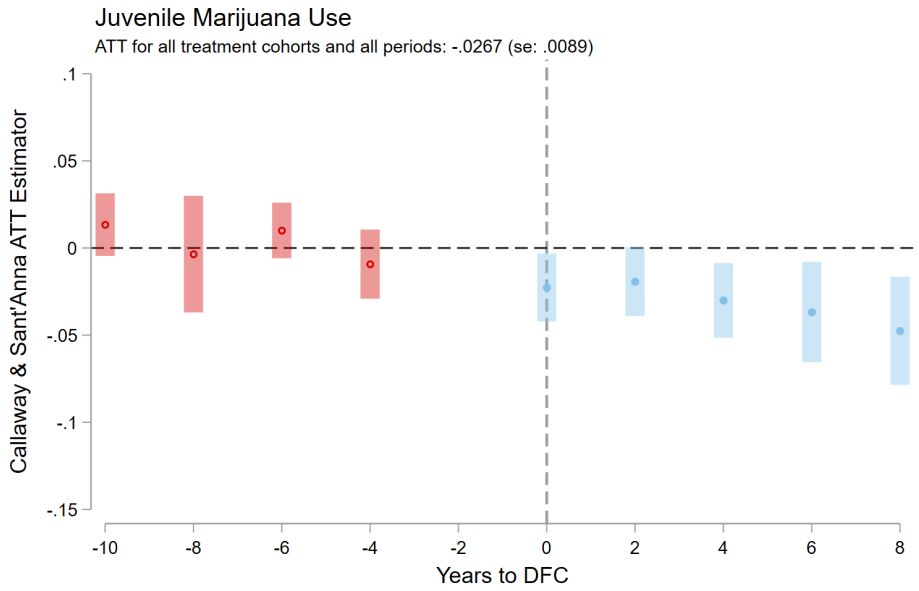
**Figure 7. Effects of DFC Grants by Sector**




*Notes*: The working sample is sourced from the UCR (2008–2019) and the Department of Education (2009–2018). The treated group is stratified by sector engagement, while the control group remains the same across all subsamples. These figures present the CSDID estimates for each subsample. Each dot represents an ATT, and the corresponding band indicates the 95% confidence interval.

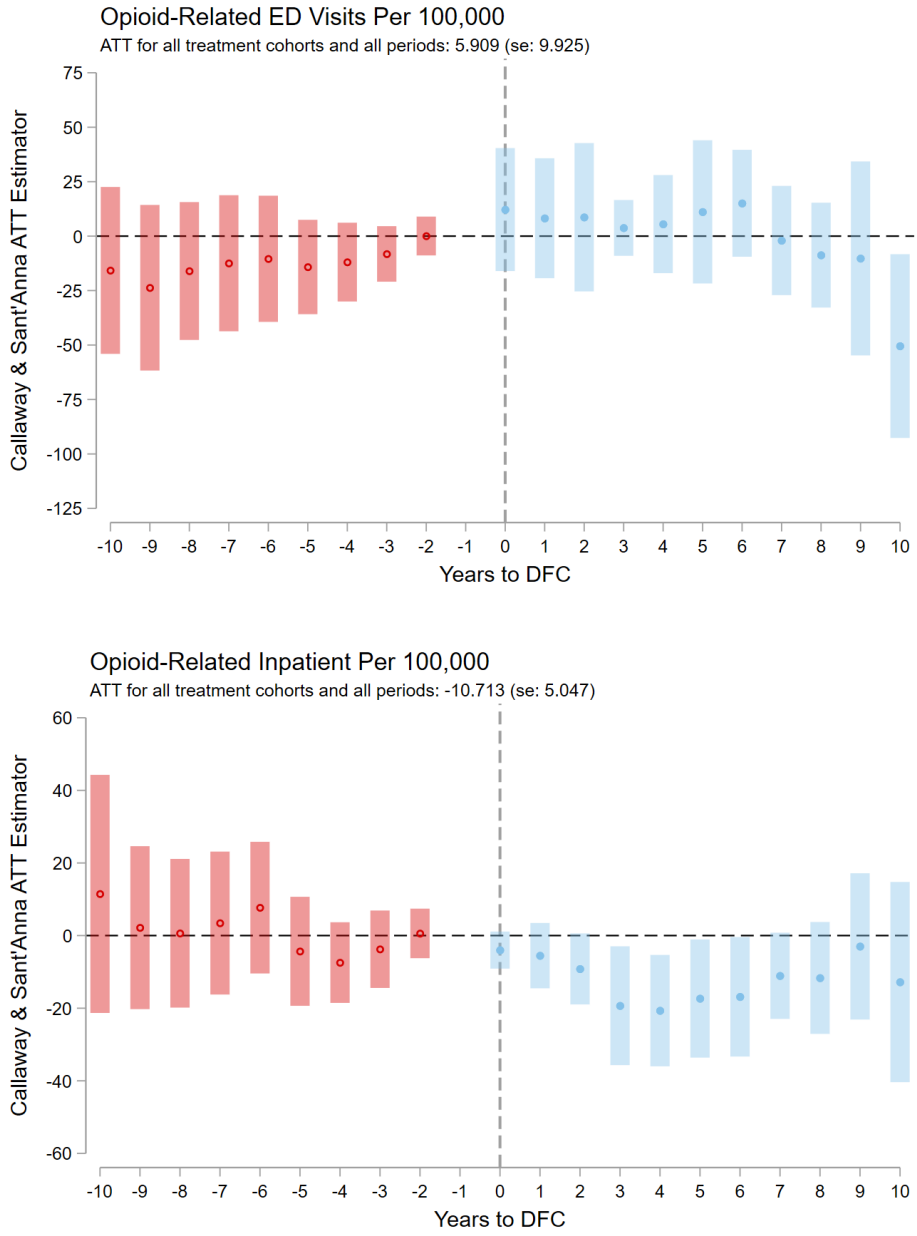
**Figure 8. Dynamic Effects of DFC Grants on Youth Substance Use**





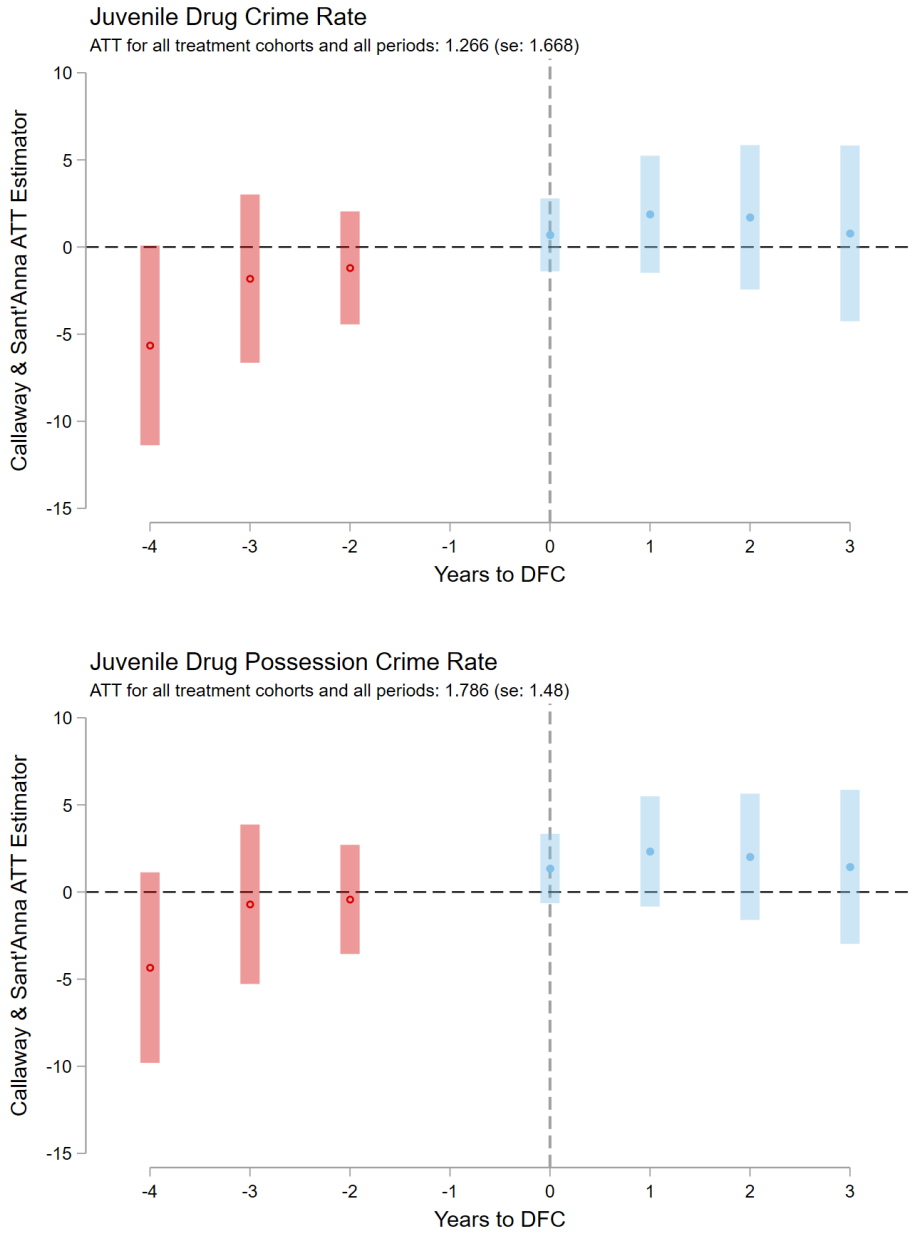

*Notes*: The working sample is sourced from the YRBSS (2009–2019). The dependent variables include alcohol use, cigarette use, and marijuana use in the past month. These figures show the dynamic effects of DFC grants, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

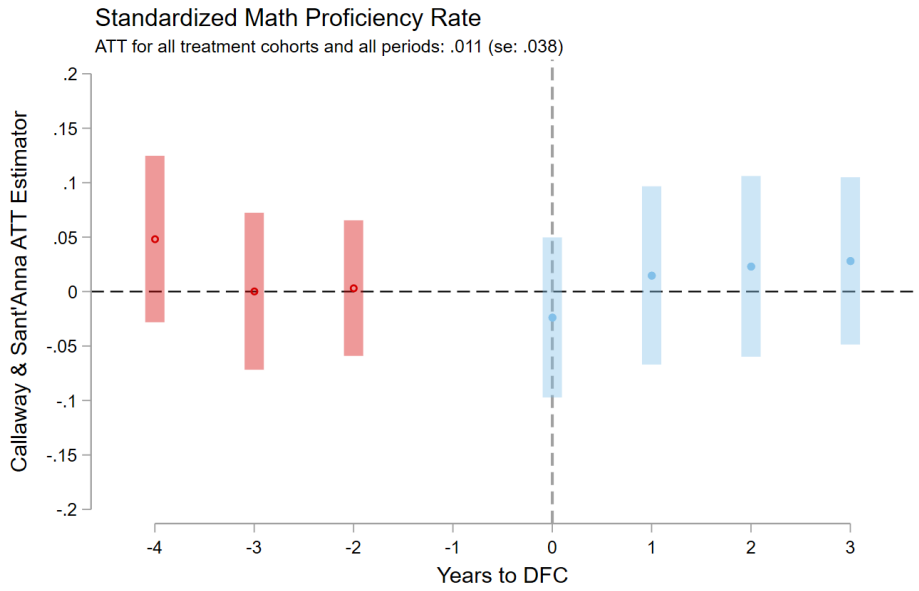
**Figure 9. Dynamic Effects of DFC Grants on Youth Opioid-Related Hospitalization**




*Notes*: The working sample is sourced from the state-level HCUP data (2008–2019). The dependent variables include alcohol use in the past month, cigarette use in the past month, and marijuana use in the past year. These figures show the dynamic effects of DFC grants, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

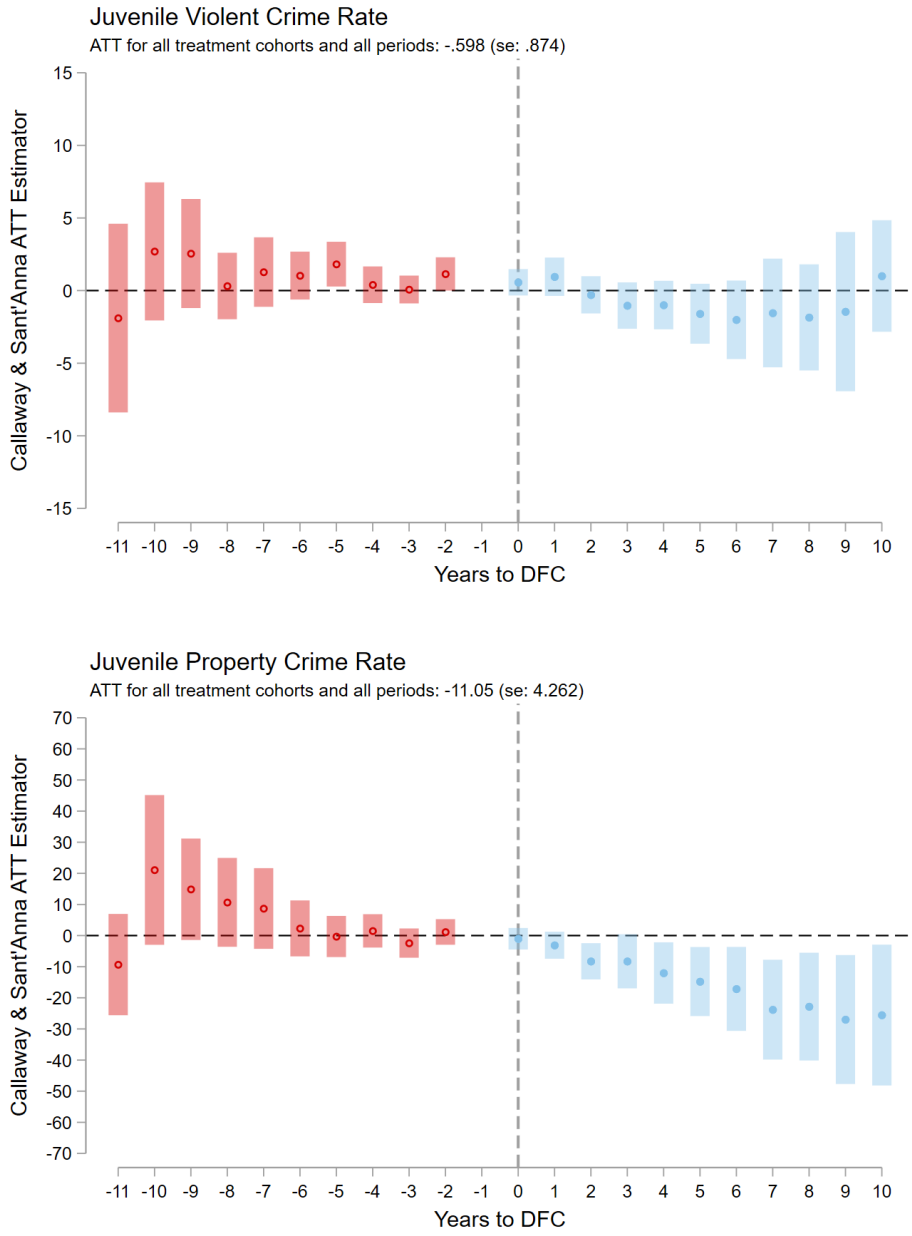
**Figure 10. Effects of DFC Grant Discontinuation**






*Notes*: The working sample is sourced from the UCR (2008–2019) and the Department of Education (2009–2018). These figures show the dynamic effects of DFC grant discontinuation on juvenile drug crime and math test proficiency, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

**Figure 11. Effects of DFC Grants on Juvenile Violent and Property Crimes**




*Notes*: The working sample consists of 6,481 and 6,476 police agencies from 2008–2019, respectively. The figures show the dynamic effects of DFC grants on juvenile drug violent and property crimes, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

**Table 1. Strategies Implemented by DFC-Funded Coalitions**

| DFC Strategies | Definitions | Examples |
|---|---|---|
| *Panel A: Individual Strategies* | | |
| Providing Information | Activities provide community members with information related to youth substance use, including prevention strategies and the consequences of use. | Informational Materials Disseminated: Brochures, flyers, posters, etc.; Social Networking: (e.g., Facebook, Twitter, etc.); Direct Face-to-Face Information Sessions. |
| Enhancing Skills | Activities designed to increase the skills of participants. | Community Member Education and Training Programs; Parent Education and Training Programs on drug awareness, prevention strategies, parenting skills, etc. |
| Providing Support | Activities to support community members participating in activities that reduce risk or enhance protection. | Alternative/Drug-Free Social Events: Drug-free parties, and other alternative events supported by the coalition. |
| *Panel B: Environmental Strategies* | | |
| Changing Access/Barriers | Activities designed to improve systems and processes to increase the ease, ability, and opportunity to utilize those systems and services or designed to create systemic barriers to accessing substances. | Reducing Home and Social Access to Alcohol and Other Substances (e.g., prescription drug disposal); Increased Access to Substance Use Services (e.g., court mandated services, assessment and referral). |
| Changing Consequences | Activities designed to increase or decrease the probability of a specific behavior that reduces risk or enhances protection by altering the consequences/incentives for performing that behavior. | Strengthening Enforcement (e.g., supporting DUI checkpoints, shoulder tap, open container laws); Strengthening Surveillance (e.g., monitoring "hot spots," party patrols). |

*(Continued)*

| DFC Strategies | Definitions | Examples |
|---|---|---|
| Changing Physical Design | Activities to change the physical design or structure of the environment to reduce risk or enhance protection. | Identifying Physical Design Problems (e.g., environmental scans, neighborhood meetings, windshield surveys); Cleanup and Beautification (e.g., Improving parks and other physical landscapes, neighborhood cleanups). |
| Educating/Informing about Modifying/Changing Policies or Laws | Activities to educate and inform with the goal of creating formal change in policies or laws. | School: Policies promoting drug-free schools; Underage Use: Laws/public policies targeting use, possession, or behavior under the influence for minors. |

*Notes*: The definitions of DFC strategies are extracted from the DFC Program National Evaluation Report (ONDCP, 2022).

**Table 2. Summary Statistics for Outcomes in Main Results**

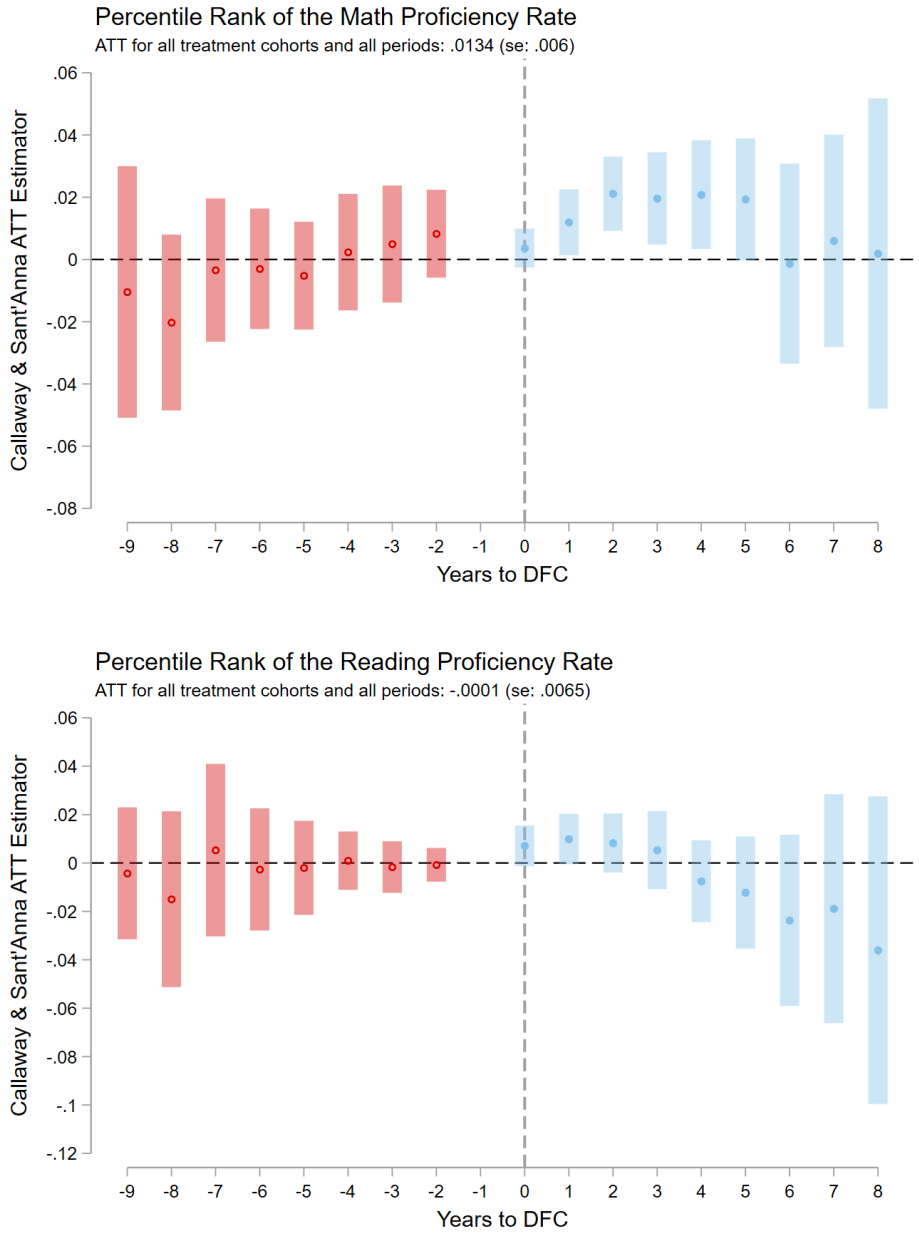
| | Mean | SD | N |
|---|---|---|---|
| **UCR** | | | |
| Drug Crime—Juvenile | 37.56 | 42.75 | 58,162 |
| Drug Possession Crime—Juvenile | 32.66 | 38.59 | 58,126 |
| Drug Sales Crime—Juvenile | 4.19 | 8.68 | 58,184 |
| Violent Crime—Juvenile | 14.49 | 18.42 | 58,300 |
| Property Crime—Juvenile | 77.99 | 97.39 | 58,297 |
| Drug Crime—Adult | 418.90 | 363.34 | 58,108 |
| **Department of Education** | | | |
| Raw Math Proficiency Rate | 56.79 | 23.61 | 104,247 |
| Raw Reading Proficiency Rate | 62.57 | 21.10 | 105,483 |

*Notes*: This table reports the mean and standard deviation of outcomes in the baseline analysis. The crime rates are relative to 100,000 agency-covered population, while the proficiency rates are relative to 100 students.

# Appendix: For Online Publication

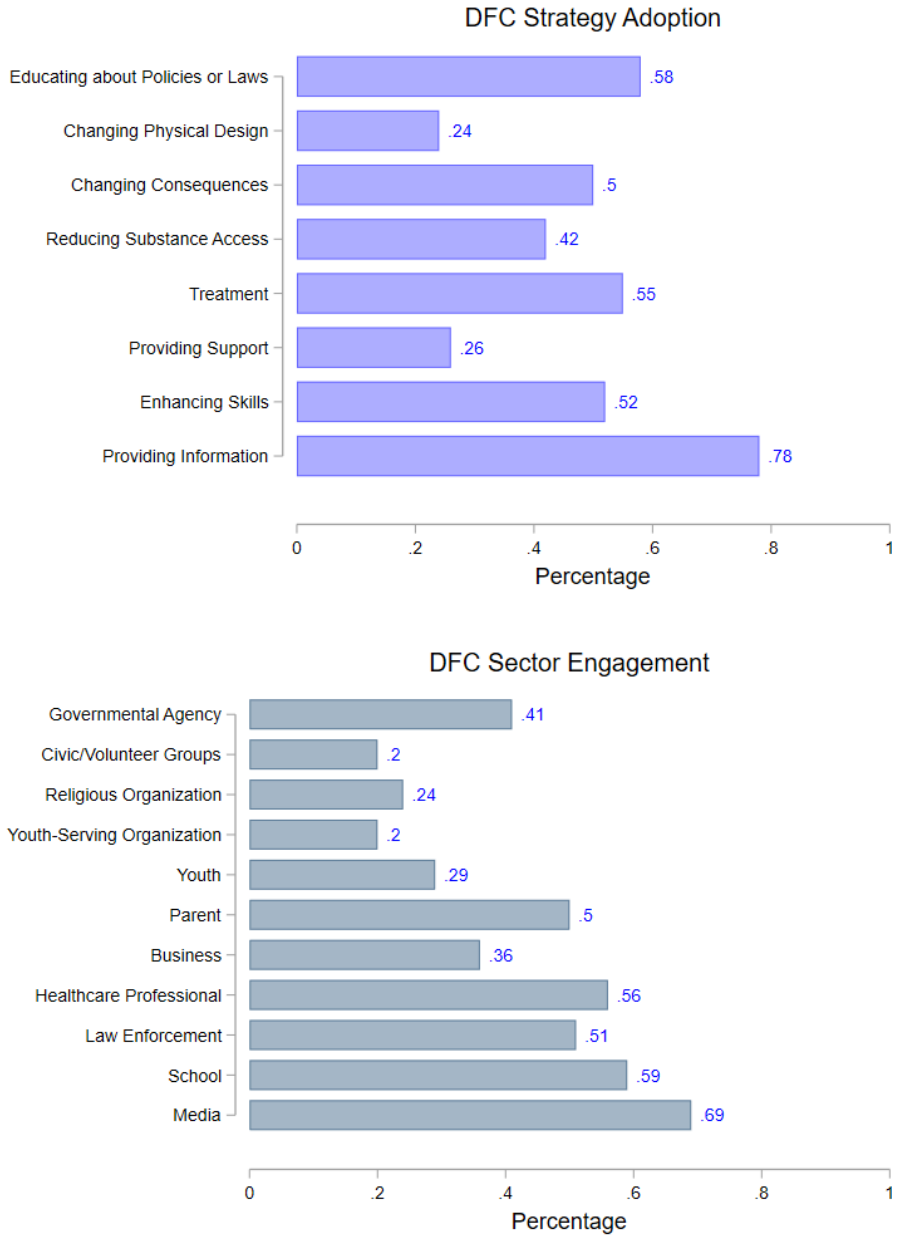
## A. *Additional Figures and Tables*

**Figure A1. Effects of DFC Grants on Percentile Rank of Test Proficiency**



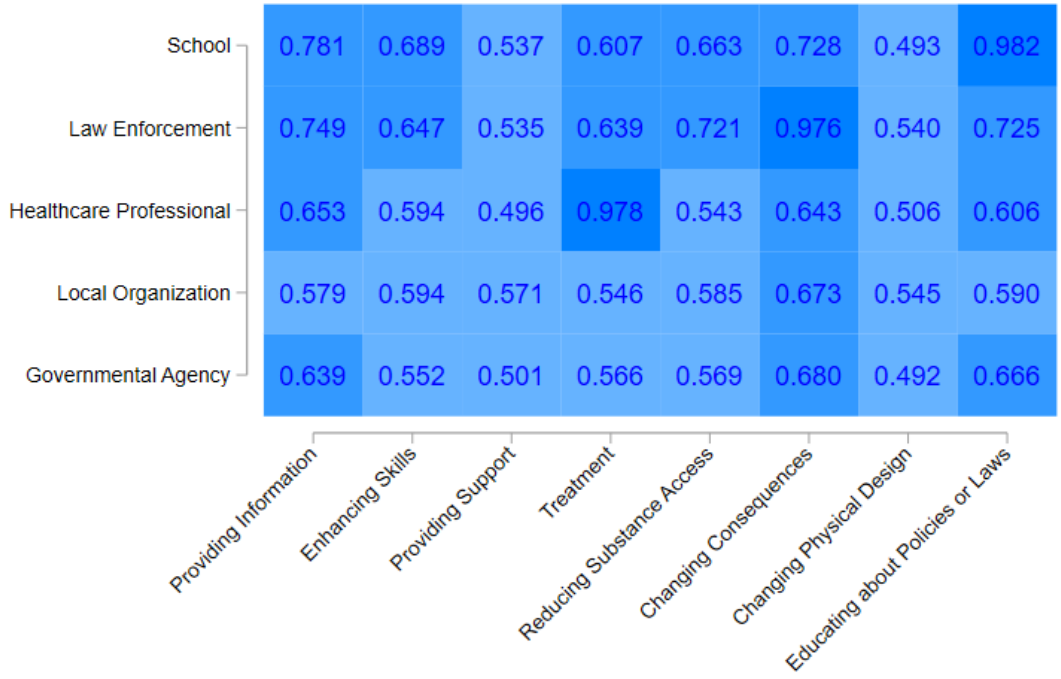

*Notes*: The working sample is sourced from the school-level Department of Education data (2009–2018). These figures show the dynamic effects of DFC grants on the percentile rank of math and reading proficiency, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

**Figure A2. Strategies and Sector Involvement in DFC Program**




*Notes*: The bar charts show the percentage of DFC programs that implement a strategic approach or engage a specific local sector for DFC coalitions included in the analytical samples.

**Figure A3. Correlation Coefficients between Sector Engagement and Strategies**



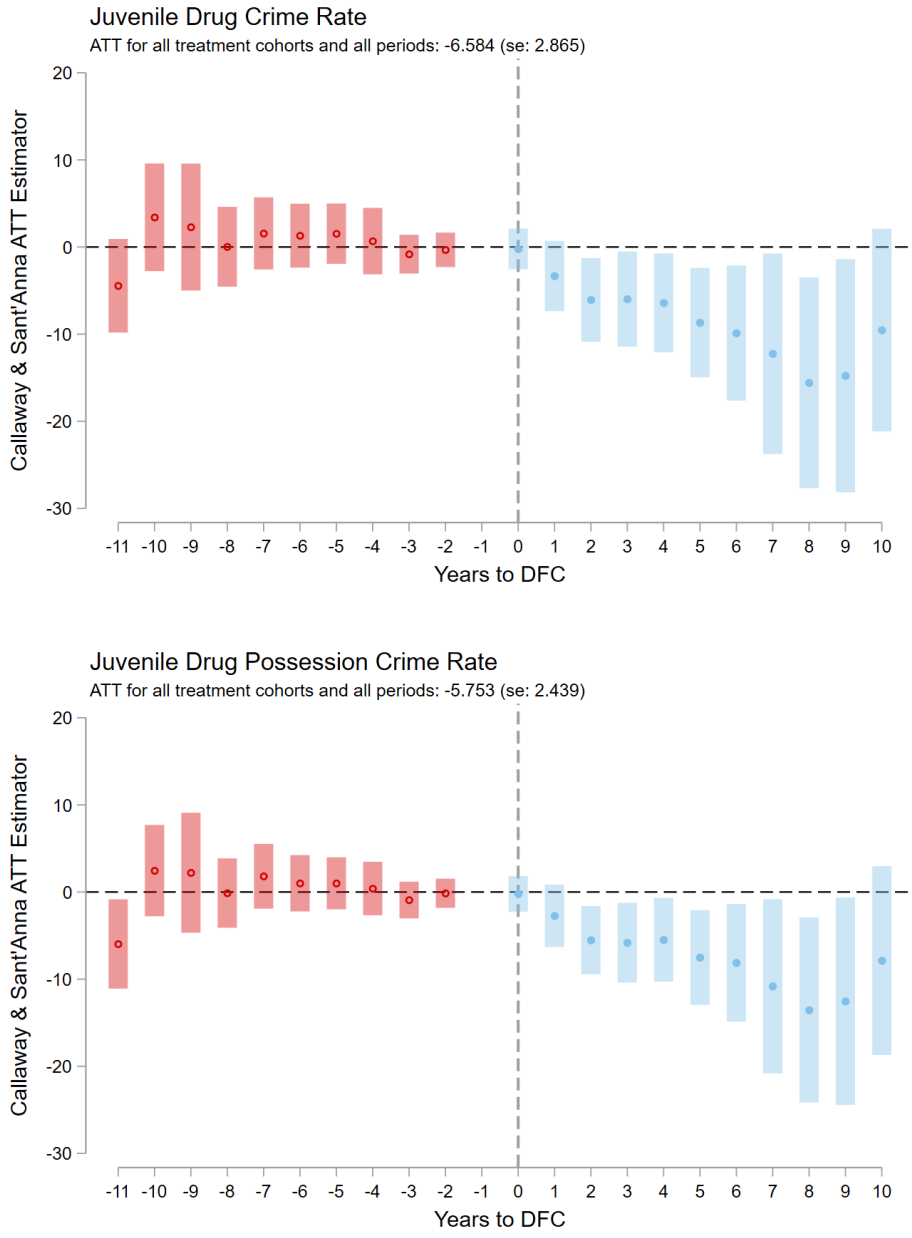

*Notes*: This figure shows the correlation coefficients between sector selection and strategy implementation from 2008–2019. The vertical axis includes a set of dummy variables for sector engagement, and the horizontal axis includes dummies for strategies implemented. A darker color represents a stronger correlation.

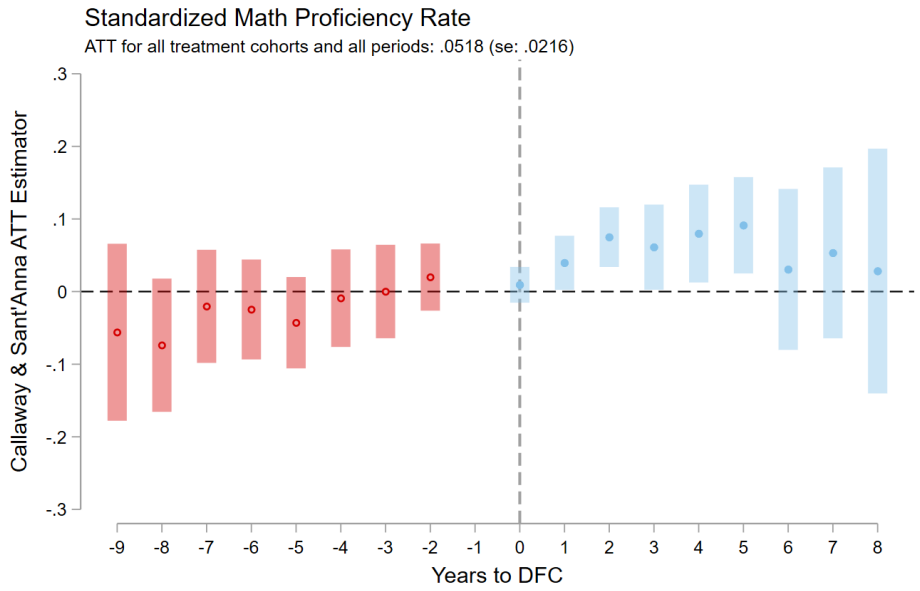
**Figure A4. Correlation Coefficients between Sector Engagement and County Demographics**




*Notes*: This figure shows the correlation coefficients between sector selection and county demographics from 2008–2019. The vertical axis includes a set of dummy variables for sector engagement, while the horizontal axis includes county characteristics. A darker color represents a stronger correlation.

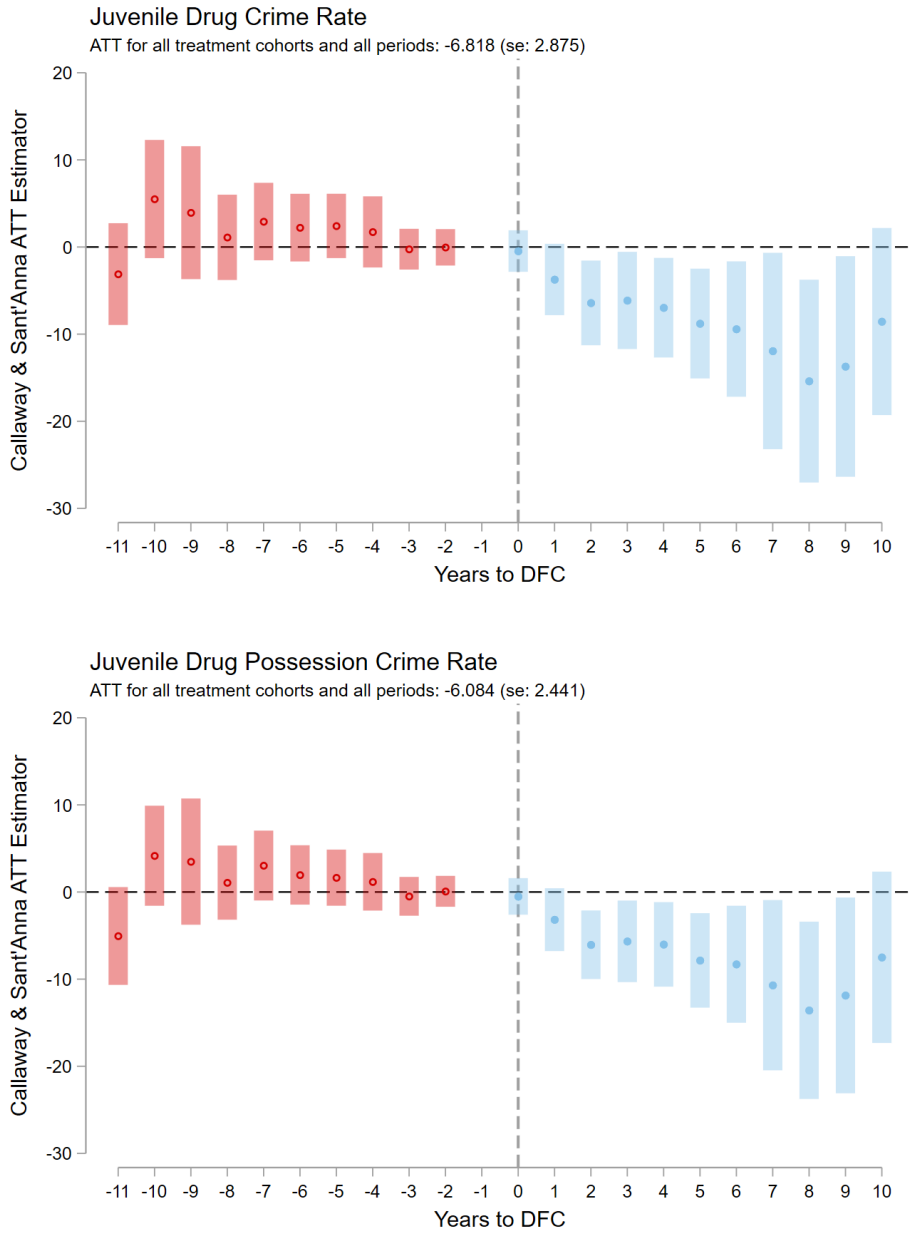
**Figure A5. Robustness Check: Alternative Treated Counties**

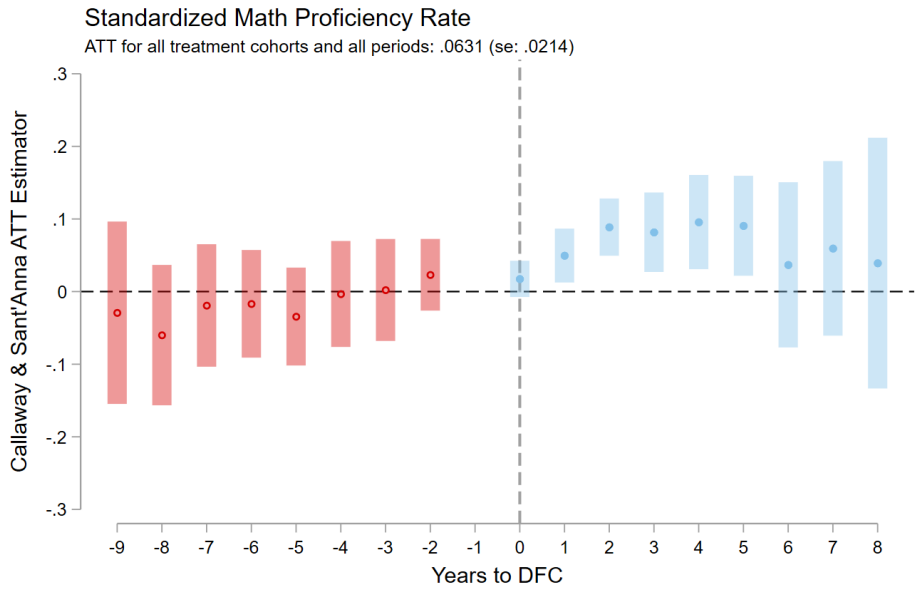





*Notes*: The working sample is sourced from the UCR (2008–2019) and the Department of Education (2009–2018). These figures show the dynamic effects of DFC grants on juvenile drug crime and math test proficiency, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period. The treated counties include counties with DFCs and non-DFC counties within a 20-mile radius, while the control group refers to the remaining non-DFC counties.

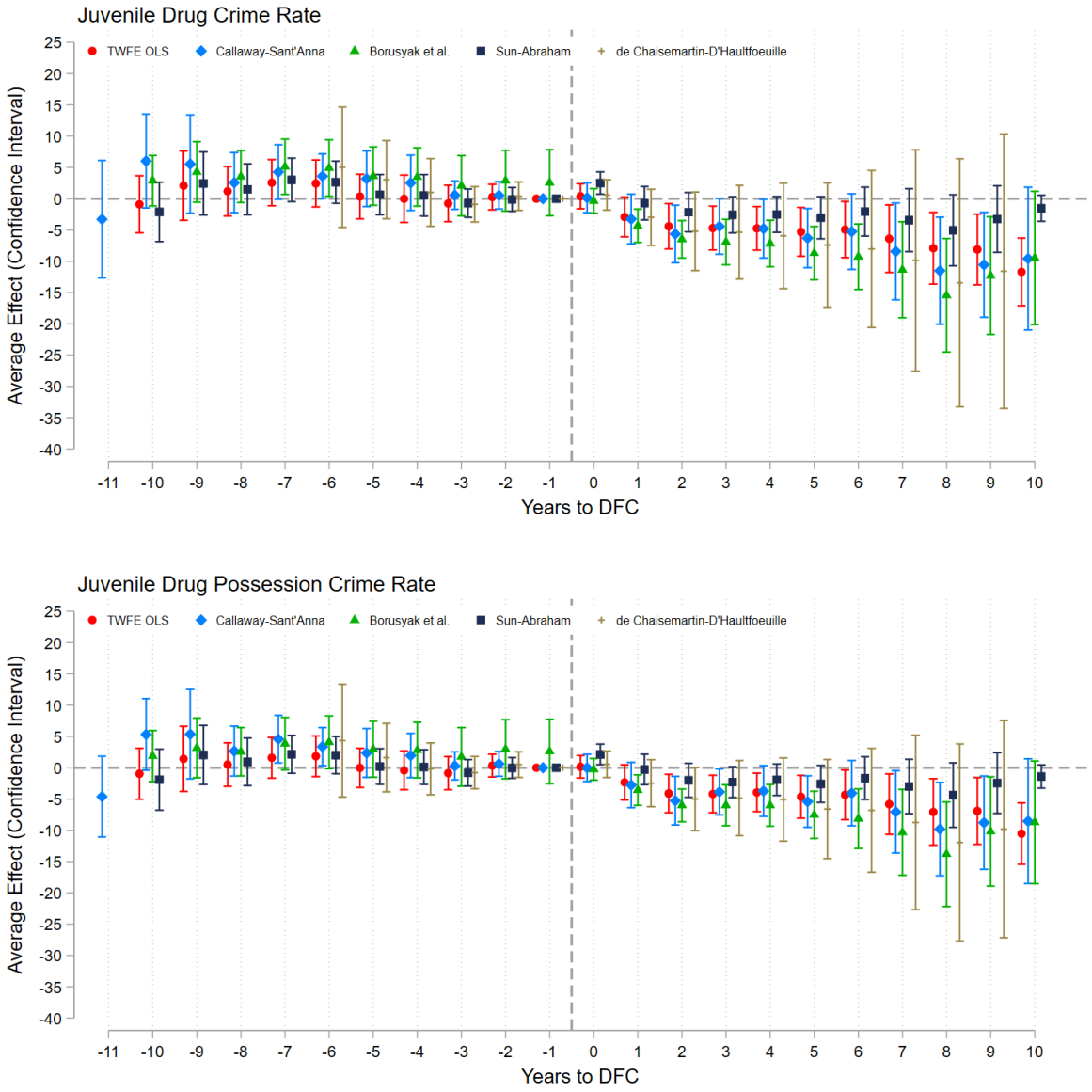
**Figure A6. Robustness Check: Exclusion of Adjacent Counties**

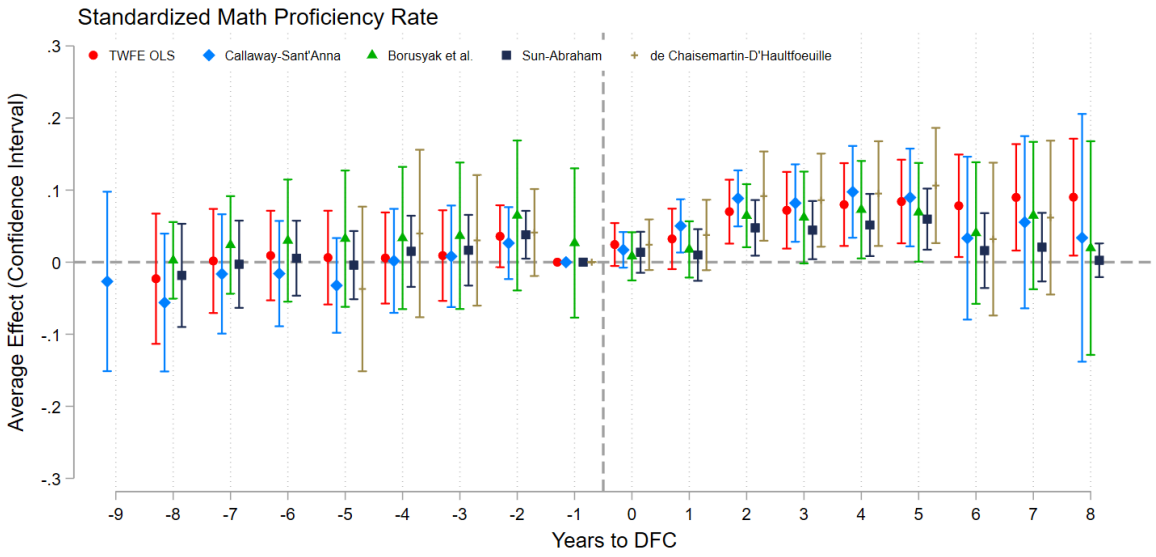





*Notes*: The working sample is sourced from the UCR (2008– 2019) and the Department of Education (2009–2018). These figures show the dynamic effects of DFC grants on juvenile drug crime and math test proficiency, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period. Non-DFC counties within a 20-mile radius of the DFC counties are excluded from the analytical sample. The treated counties include counties with DFCs, while the control group refers to the remaining non-DFC counties.

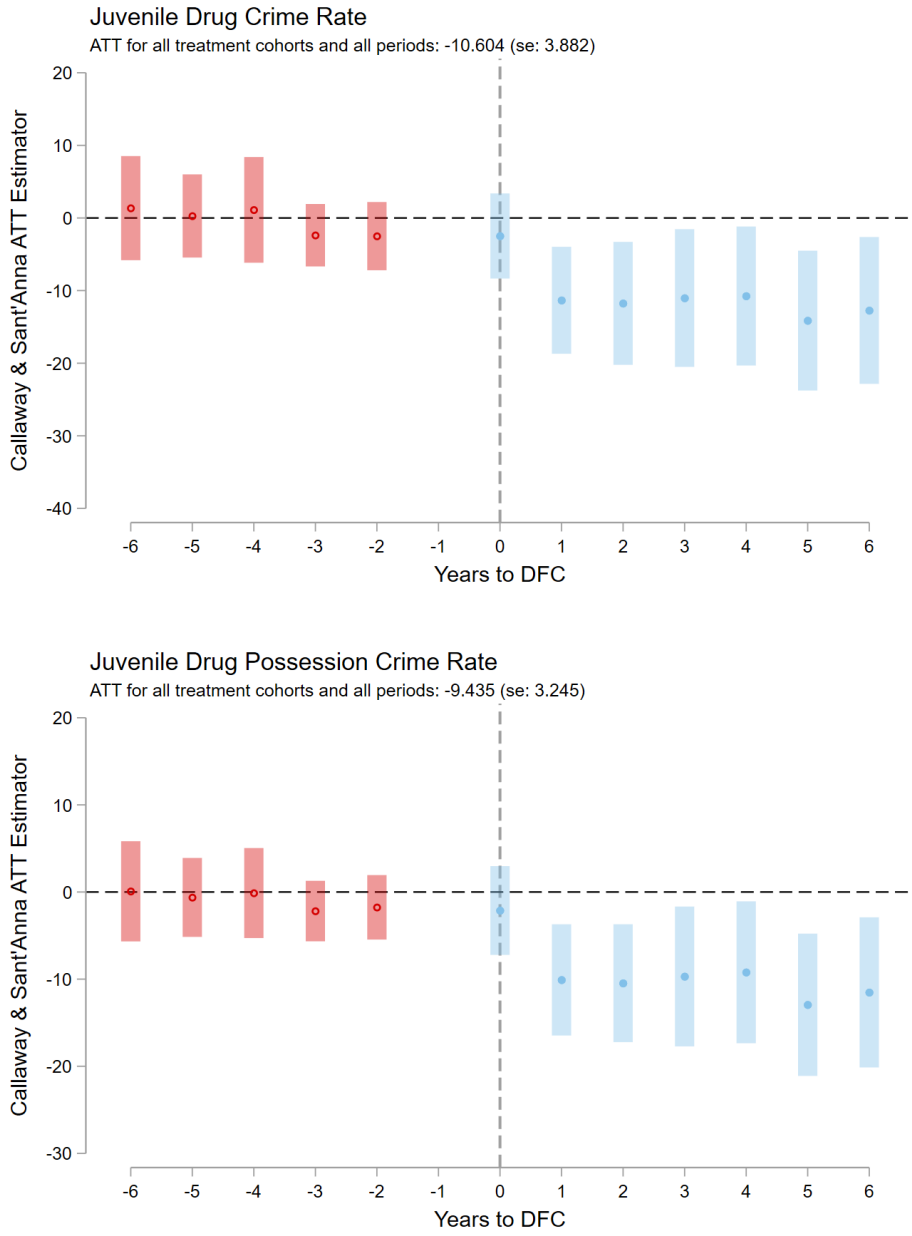
## Figure A7. Robustness Check: Alternative Estimators

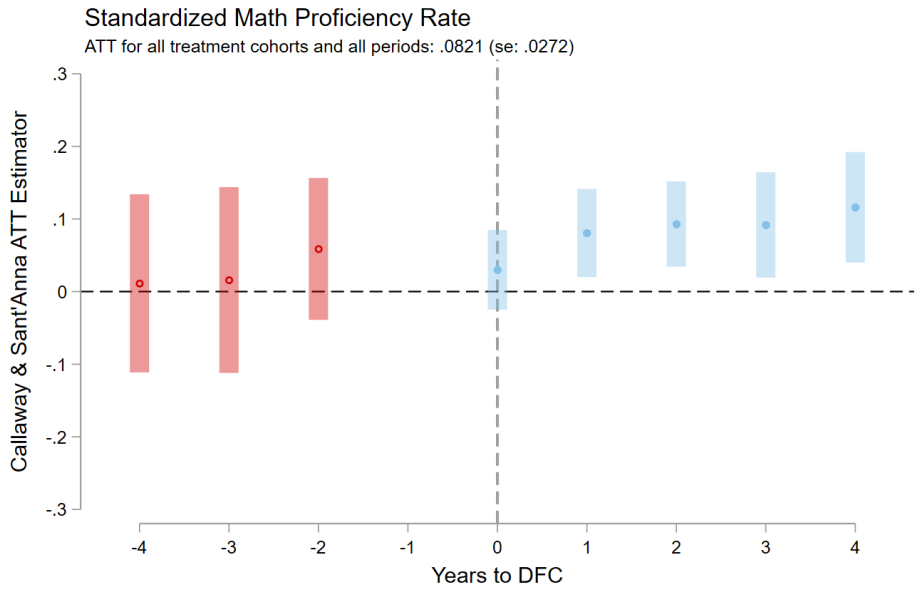





*Notes*: These figures show the dynamic effects of DFC grants on juvenile crime and math test proficiency, estimated using various DID estimators.

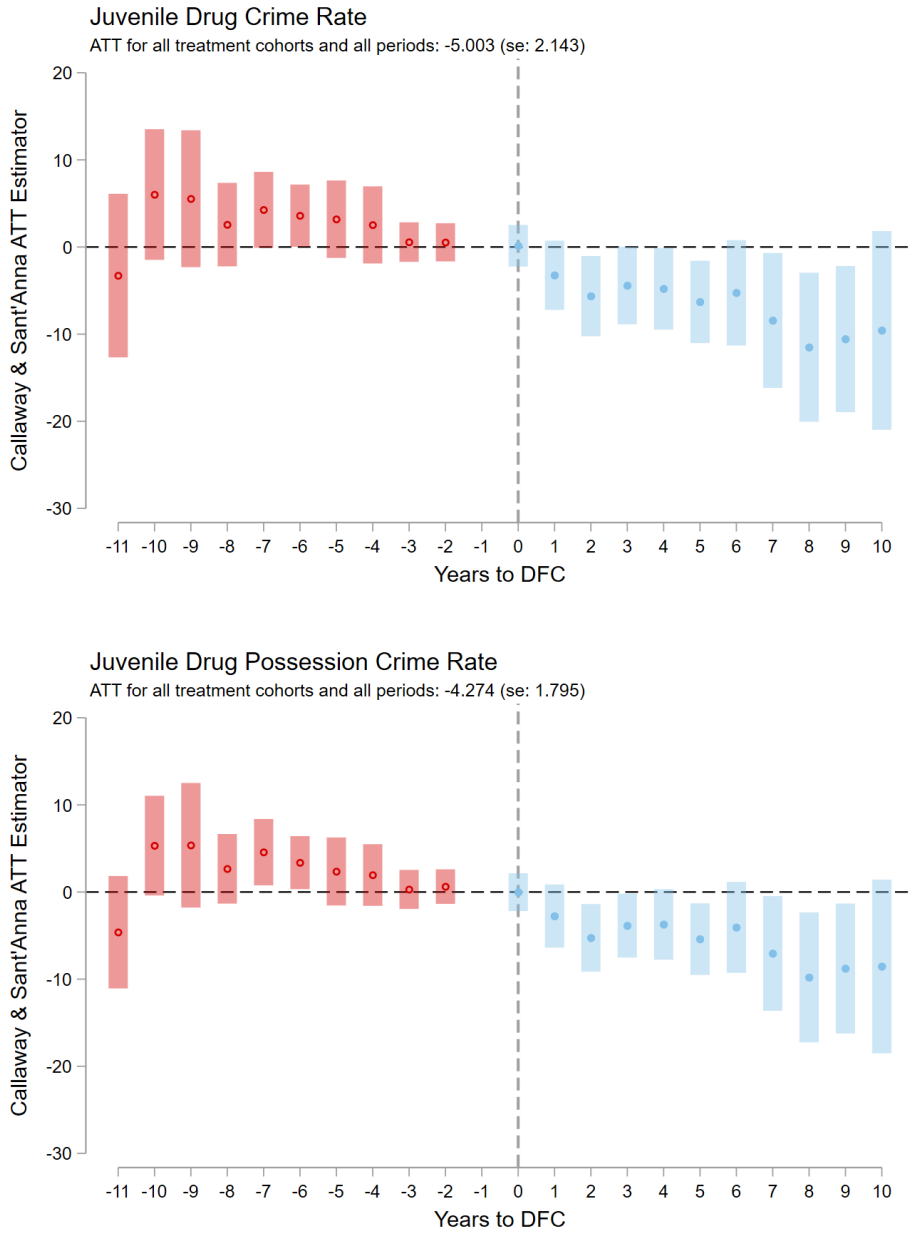
**Figure A8. Robustness Check: Stacked Sample**

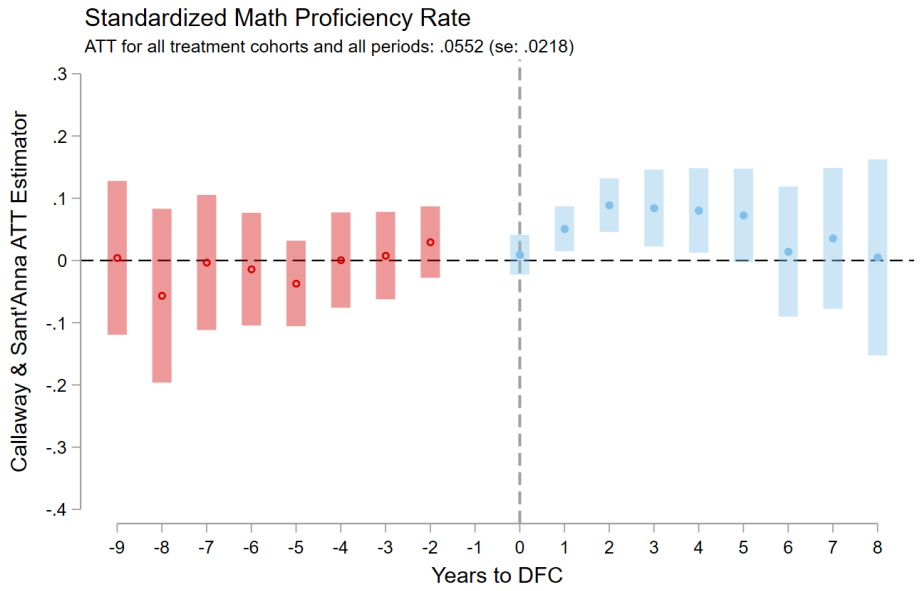





*Notes*: These figures show the dynamic effects of DFC grants on juvenile crime and math test proficiency using the stacked DID sample and the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

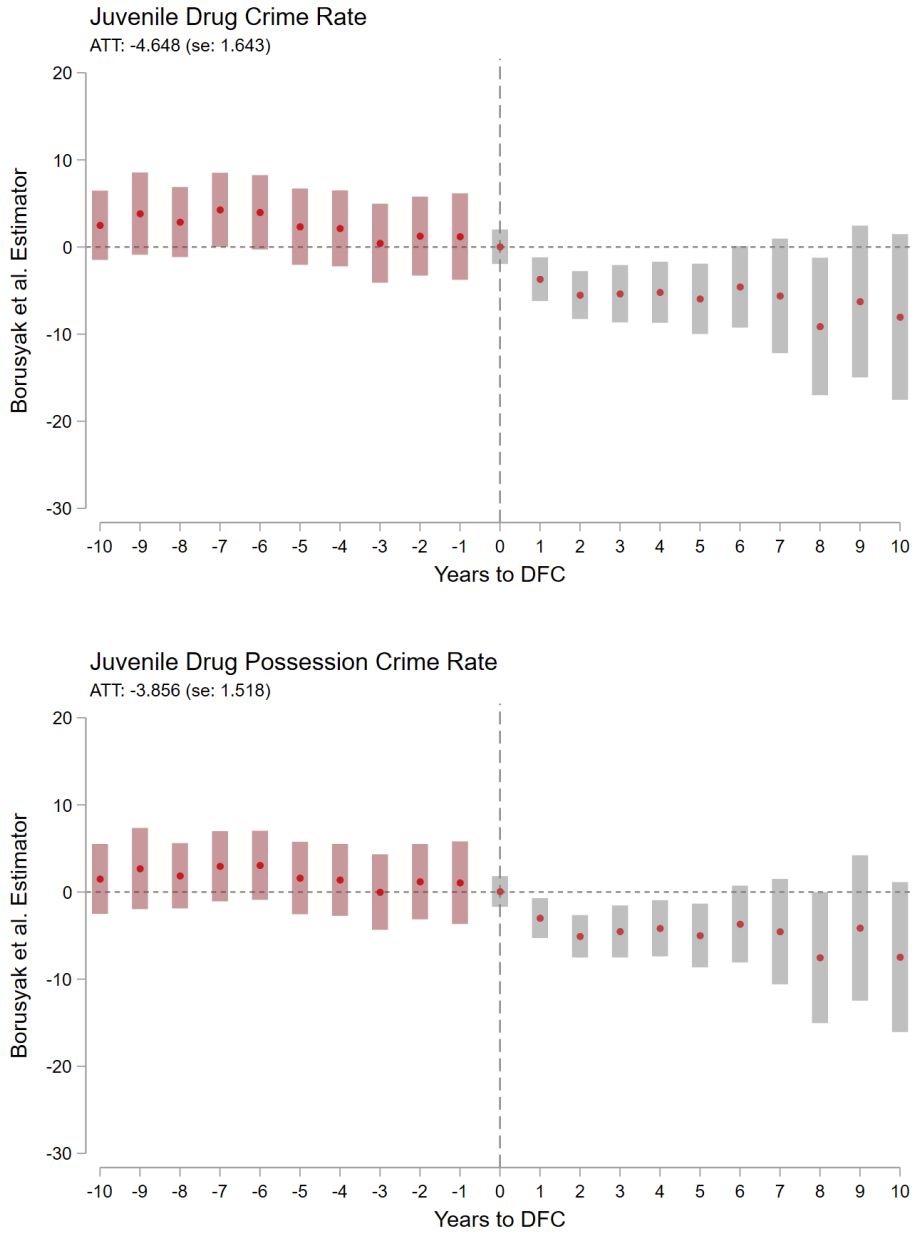
**Figure A9. Robustness Check: Inclusion of County Characteristics**

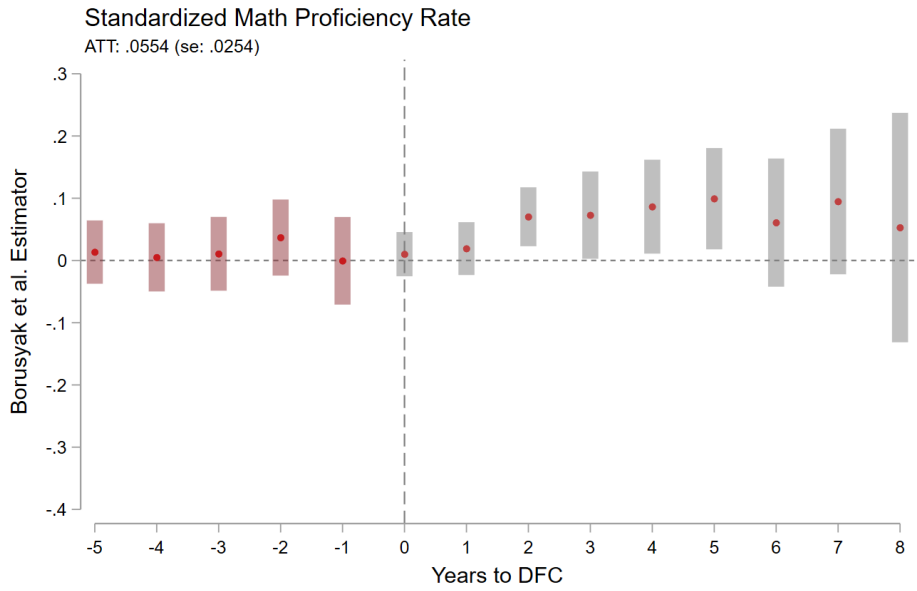





*Notes*: The working sample is sourced from the UCR (2008–2019) and the Department of Education (2009–2018). These figures show the dynamic effects of DFC grants on juvenile drug crime and math test proficiency, estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period. The covariates include the shares of female, White, Hispanic, individuals under age 25, and individuals aged 25 to 54.

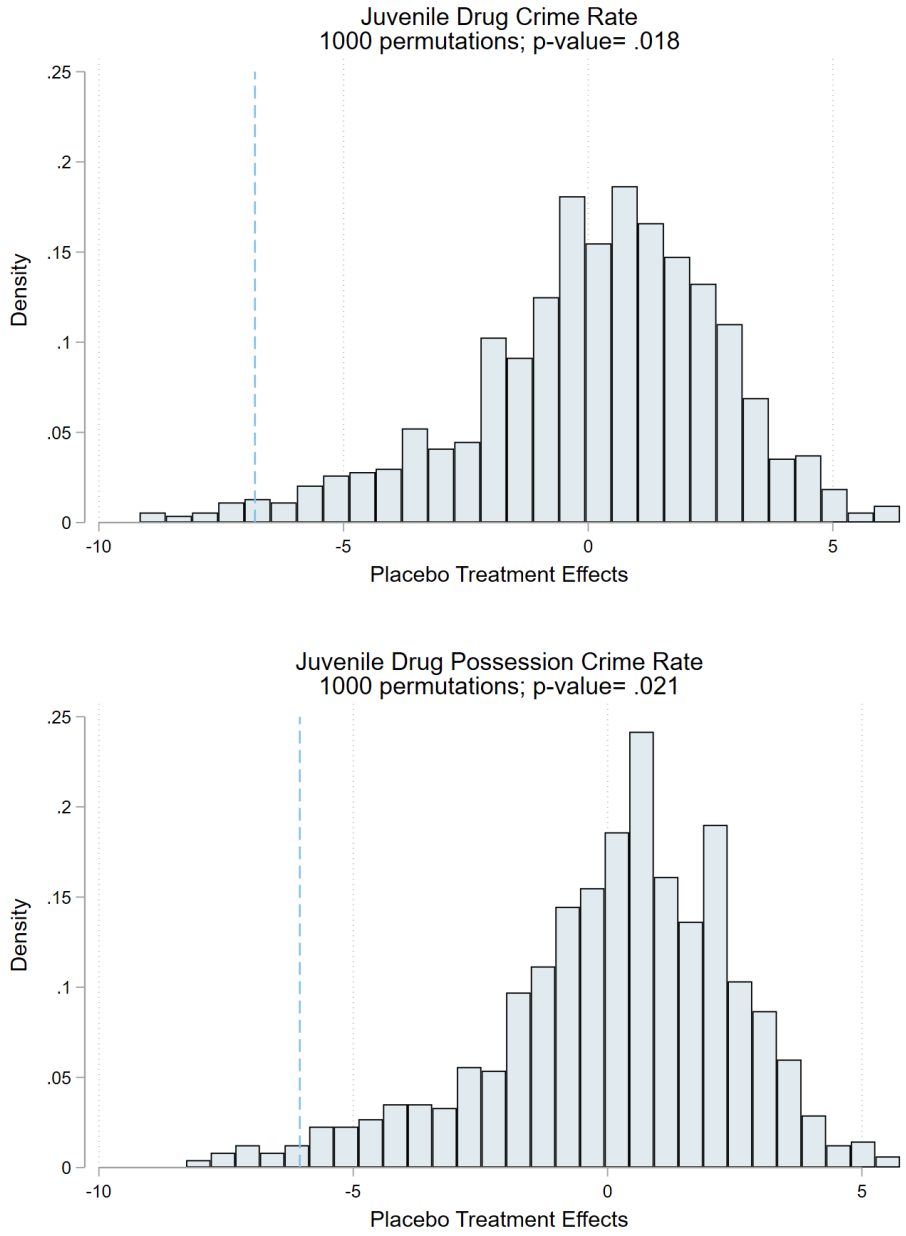
**Figure A10. Robustness Check: Inclusion of Time-Varying Covariates**

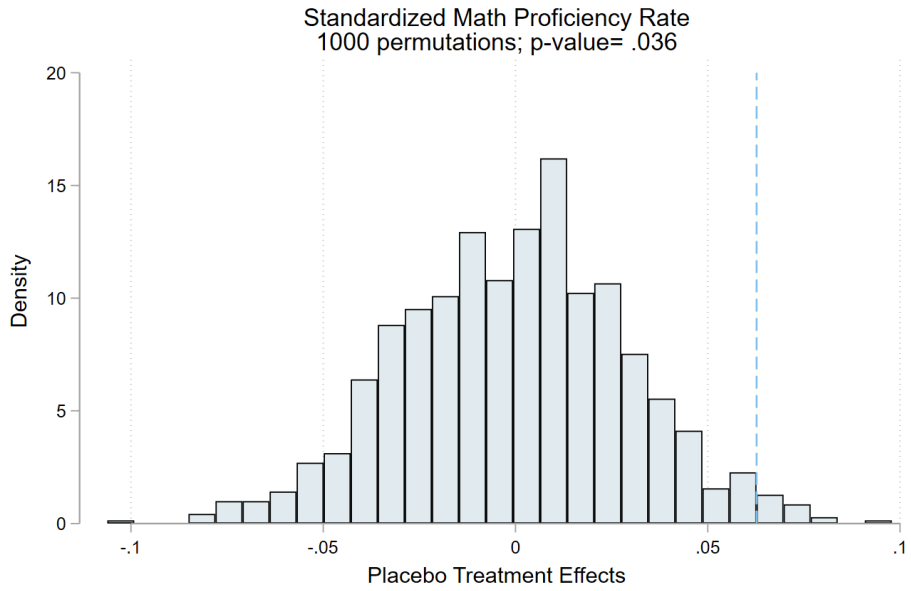





*Notes*: The working sample is sourced from the UCR (2008– 2019) and the Department of Education (2009–2018). These figures show the dynamic effects of DFC grants on juvenile drug crime and math test proficiency, using the Borusyak, Jaravel, and Spiess (2024) estimator. The covariates include the shares of female, White, Hispanic, individuals under age 25, individuals aged 25 to 54, PDMP mandates, Medical Marijuana Laws, and Pill Mill Laws.

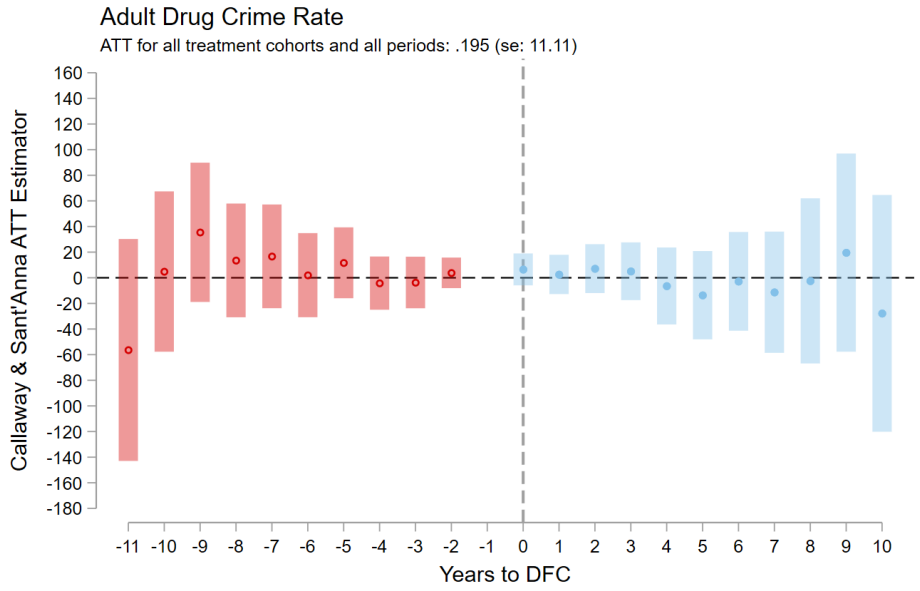
**Figure A11. Robustness Check: Permutation Test**






*Notes*: The working sample is sourced from the UCR (2008– 2019) and the Department of Education (2009–2018). These figures show the distribution of 1,000 CSDID estimates from the permutation test, with the p-value reported in the subtitle.

**Figure A12. Effects of DFC Grants on Adult Drug Crimes**




*Notes*: The working sample consists of 6,463 police agencies from 2008–2019. This figure shows the dynamic effects of DFC grants on adult drug crime (per 100,000 population), estimated using the CSDID method. Shaded areas represent 95% confidence intervals. Year -1 is omitted and serves as the reference period.

**Table A1. Assessing the Exogeneity of the DFC Grants**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | $DFC_{t+1}$ | | | $DFC_t$ | | | $DFC_{t-1}$ | |
| % Female | -0.3319 | -0.3995 | -0.3996 | 1.3098 | 1.3270 | 1.3313 | 2.4351 | 2.4939 | 2.5067 |
| | (2.1225) | (2.1368) | (2.1232) | (2.2022) | (2.2274) | (2.2167) | (2.2576) | (2.2847) | (2.2751) |
| % White | -0.2751 | -0.5146 | -0.5396 | 0.0490 | 0.0599 | 0.0211 | 0.0450 | 0.1847 | 0.1331 |
| | (1.5966) | (1.6730) | (1.6696) | (1.3943) | (1.4615) | (1.4548) | (1.3104) | (1.3522) | (1.3471) |
| % Black | 0.3622 | 0.0220 | 0.0355 | 0.3979 | 0.4030 | 0.4224 | -0.0587 | 0.1301 | 0.1437 |
| | (1.9867) | (2.0849) | (2.0604) | (1.8496) | (1.9174) | (1.8899) | (1.7800) | (1.8069) | (1.7834) |
| % Hispanic | -0.5002 | -0.4487 | -0.3912 | 0.5201 | 0.5277 | 0.6178 | 1.6030 | 1.5803 | 1.7031 |
| | (0.9192) | (0.9323) | (0.9602) | (1.1491) | (1.1478) | (1.1834) | (1.3353) | (1.3212) | (1.3651) |
| % Age 0-24 | 0.1756 | -0.0178 | -0.0445 | 0.2005 | 0.1717 | 0.1311 | 0.0591 | 0.1315 | 0.0752 |
| | (1.1410) | (1.1409) | (1.1429) | (1.3074) | (1.2923) | (1.2945) | (1.4832) | (1.4507) | (1.4559) |
| % Age 25-54 | -1.1384 | -1.1440 | -1.1467 | -0.9652 | -0.9496 | -0.9529 | -0.9828 | -0.9669 | -0.9717 |
| | (1.1083) | (1.1102) | (1.0976) | (1.2001) | (1.2032) | (1.1842) | (1.2550) | (1.2573) | (1.2380) |
| Poverty Rate | 0.0027 | 0.0024 | 0.0024 | 0.0038 | 0.0037 | 0.0037 | 0.0036 | 0.0036 | 0.0037 |
| | (0.0028) | (0.0028) | (0.0028) | (0.0029) | (0.0030) | (0.0030) | (0.0029) | (0.0030) | (0.0030) |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| PDMP Mandates | | -0.0026 | -0.0030 | | 0.0032 | 0.0024 | | 0.0054 | 0.0043 |
| | | (0.0136) | (0.0133) | | (0.0152) | (0.0148) | | (0.0168) | (0.0162) |
| Medical Marijuana Laws | | 0.0270* | 0.0266* | | 0.0073 | 0.0067 | | -0.0074 | -0.0080 |
| | | (0.0148) | (0.0148) | | (0.0144) | (0.0143) | | (0.0160) | (0.0159) |
| Pill Mill Laws | | -0.0030 | 0.0011 | | -0.0085 | -0.0022 | | -0.0059 | 0.0023 |
| | | (0.0207) | (0.0218) | | (0.0213) | (0.0218) | | (0.0239) | (0.0237) |
| Oxycodone | | | 0.0340 | | | 0.0529 | | | 0.0695 |
| | | | (0.0591) | | | (0.0658) | | | (0.0727) |
| Hydrocodone | | | 0.0008 | | | 0.0026 | | | 0.0046 |
| | | | (0.0826) | | | (0.1000) | | | (0.1100) |
| Fentanyl | | | 1.6938 | | | 2.3467 | | | 1.3571 |
| | | | (3.0645) | | | (3.2822) | | | (3.1095) |
| N | 37,243 | 37,243 | 37,231 | 37,243 | 37,243 | 37,231 | 37,243 | 37,243 | 37,231 |
| Drug Policies | | Y | Y | | Y | Y | | Y | Y |
| Opioids Supply | | | Y | | | Y | | | Y |

*Notes*: The working sample consists of 3,107 counties from 2008–2019. The dependent variables are indicators for DFC grants at the one-year lagged, current-year, and one-year leading levels. The drug policies include PDMP mandates, Medical Marijuana Laws, and Pill Mill Laws. The opioid supply variables include county-level oxycodone, hydrocodone, and fentanyl, collected from the ARCOS dataset. All the coefficients are estimated by the OLS and weighted by county population. Standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

## Table A2. The Effect of DFC Grants on Juvenile Crime

| | (1) Callaway & Sant'Anna | (2) Borusyak et al. | (3) de Chaisemartin & D'Haultfoeuille | (4) TWFE |
|---|---|---|---|---|
| **Outcomes** | | | | |
| **Juvenile Drug Crime** | -6.811** | -6.983*** | -5.803 | -3.502** |
| | (2.868) | (2.057) | (4.398) | (1.470) |
| Mean | 37.556 | 37.523 | 37.647 | 37.647 |
| N | 58,162 | 60,011 | 64,424 | 64,424 |
| **Juvenile Drug Possession Crime** | -6.051** | -6.097*** | -5.281 | -2.973** |
| | (2.433) | (1.848) | (3.647) | (1.321) |
| Mean | 32.661 | 32.650 | 32.738 | 32.738 |
| N | 58,126 | 60,002 | 64,413 | 64,413 |
| **Juvenile Drug Sales Crime** | -0.830 | -0.653 | -.671 | -0.042 |
| | (0.533) | (0.406) | (.774) | (0.366) |
| Mean | 4.187 | 4.195 | 4.248 | 4.248 |
| N | 58,184 | 59,990 | 64,402 | 64,402 |
| **Juvenile Violent Crime** | -0.598 | -1.883** | -.522 | -1.098 |
| | (0.874) | (0.946) | (1.326) | (0.704) |
| Mean | 14.486 | 14.464 | 14.481 | 14.481 |
| N | 58,300 | 60,098 | 64,480 | 64,480 |
| **Juvenile Property Crime** | -11.050*** | -13.968*** | -8.454 | -5.497* |
| | (4.262) | (3.903) | (8.588) | (2.945) |
| Mean | 77.990 | 77.832 | 77.533 | 77.533 |
| N | 58,297 | 60,138 | 64,570 | 64,570 |

*Notes*: This table shows the effects of DFC grants on juvenile crime using various DID estimators. Estimates are weighted by agency-covered population, and standard errors are clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

## Table A3. The Effect of DFC Grants on Academic Performance

| | (1) Callaway & Sant'Anna | (2) Borusyak et al. | (3) de Chaisemartin & D'Haultfoeuille | (4) TWFE |
|---|---|---|---|---|
| **Outcomes** | | | | |
| **Standardized Math Proficiency** | 0.063*** | 0.045* | 0.066** | 0.033 |
| | (0.021) | (0.024) | (0.026) | (0.024) |
| Mean | 0.063 | 0.063 | 0.067 | 0.067 |
| N | 104,836 | 106,373 | 109,818 | 109,818 |
| **Standardized Reading Proficiency** | 0.019 | -0.008 | 0.020 | 0.007 |
| | (0.022) | (0.025) | (0.023) | (0.022) |
| Mean | 0.078 | 0.078 | 0.082 | 0.082 |
| N | 106,047 | 107,341 | 110,825 | 110,825 |

*Notes*: This table shows the effects of DFC grants on math and reading proficiency rates using various DID estimators. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

**Table A4. Summary Statistics for Outcomes as Mechanisms**

| | Mean | SD | N |
|---|---|---|---|
| **YRBSS** | | | |
| Alcohol Use | 0.300 | 0.458 | 155,736 |
| Cigarette Smoking | 0.128 | 0.334 | 155,736 |
| Marijuana Use | 0.187 | 0.390 | 155,736 |
| **HCUP** | | | |
| Opioid-Related ED Visits | 41.255 | 40.620 | 180 |
| Opioid-Related Inpatient Stays | 57.111 | 36.176 | 180 |

*Notes*: This table reports the mean and standard deviation of outcomes in the mechanism analysis.

**Table A5. Summary of Related Literature**

| Study | Population | Community-Level Interventions | Results |
|---|---|---|---|
| *Panel A: Media Messages* | | | |
| Flynn et al. (2006) | Grade 7-8 students in the 16 Media and Comparison Area schools in Vermont. | A 4-year media campaign (1997–2001). | The media interventions did not significantly affect alcohol use or its mediators. |
| Slater et al. (2006) | Eight media treatment and eight control communities throughout the US were randomly assigned. | 2-year In-school and community-based media efforts (1999–2003). | Fewer users at final post-test for marijuana ($OR = 0.50$), alcohol ($OR = 0.40$), and cigarettes ($OR = 0.49$). |
| Hornik et al. (2008) | Three nationally representative cohorts of US youths aged 9 to 18 years. | National Youth Anti-Drug Media Campaign (1999–2004). | No effects on marijuana use, and even weaker antidrug social norms. |
| Slater et al. (2011) | Grade 7-8 students from 20 US communities. | "Be Under Your Own Influence" (Intervention) and "Above the Influence" (ONDCP) campaign (2005–2009). | Exposure to the "Above the Influence" campaign predicted reduced marijuana uptake ($OR = 0.26$). |
| *Panel B: Communities That Care System* | | | |
| Feinberg et al. (2007) | Students in Pennsylvania schools. | Communities That Care program (2001 and 2003). | Less alcohol for grades 6 ($coef = -0.106$) and 12 (-0.105) and cigarette use for grade 6 (-0.10), and less drug involvement for grade 12 (-0.058). |
| Hawkins et al. (2014) | Grade 12 students from 24 small towns in 7 states. | Communities That Care Prevention System. | Sustained abstinence from: any drug use ($RR = 1.32$), drinking alcohol ($RR = 1.31$), and smoking cigarettes ($RR = 1.13$). No significant differences in the past-month or past-year substance use. |

*(Continued)*

| Study | Population | Community-Level Interventions | Results |
|---|---|---|---|
| Oesterle et al. (2018) | Grade 5 through age 21 years in Colorado, Illinois, Kansas, Maine, Oregon, Utah, and Washington State. | Communities That Care Prevention System (2004–2014). | Increased the likelihood of male sustained abstinence from tobacco use by 30% and marijuana use by 24%. |
| *Panel C: Campus and Community Coalition* | | | |
| Flewelling et al. (2005) | Vermont students in grades 8 through 12. | A nonrandomized community trial in Vermont in which 23 community coalitions were funded for 3 years (1997, 1999, and 2001) | Significant reductions in the percentage of past-30-day marijuana use by 2.7 percentage points (9.15%) and past-30-day cigarette smoking by 2.4 percentage points (7.67%), and no effects on alcohol use. |
| Linowski and DiFulvio (2012) | Undergraduate students at the University of Massachusetts. | Campus and Community Coalition to Reduce High-risk Drinking (2005–2009). | Significant reductions in student drinking ($OR = 0.69$) and consequences. |
| *Panel D: Project ALERT Program* | | | |
| Ellickson et al. (2003) | Grade 8 students from fifty-five South Dakota middle schools. | Project ALERT program. | This program curbed cigarette and marijuana use initiation, current and regular cigarette use, and alcohol misuse. Reductions ranged from 19% to 39%. But it had no effects on current and regular marijuana use. |
| St. Pierre et al. (2005) | 7th-grade students from eight Pennsylvania middle schools. | Project ALERT program. | No effects on past month alcohol, cigarette, or marijuana use. |

*Notes*: This table summarizes the literature regarding the community-level actions that aim to curb youth substance use.