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Drug treatment courts and community-level crime

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Abstract

Research has recognized Adult Drug Treatment Courts (DTCs) as benefitting participants, with a wide body of research demonstrating lower levels of recidivism and drug use. A budding community-level body of research, however, has returned mixed results, some studies showing *increases* in arrests and crime relative to DTC initiation. Since DTCs cover over three-fourths of the US population, results showing such unintended consequences must be validated and rectified if held. This study estimated effects for DTCs for community-level crime effects from 1990 to 2018 using a stacked event study identification strategy. Most results indicated no significant effects. However, for population groups between 10,000 and 50,000, DTCs were associated with reductions in some crime categories. Violent index offenses offered the most robust results, and there was a small increase in non-index crimes in communities with populations between 50,000 and 100,000.

Keywords

Drug courts; Econometrics; Fixed-effects analysis; Quasi-experimental criminology; Stacked event study

1. Introduction

Miami-Dade County created the first adult drug treatment court (DTC) in 1989. Since that time, these treatment courts have expanded in focus and jurisdiction (Strong, Rantala, & Kyckelhahn, 2016). Currently, there are over 2100 DTCs operating in the United States, as well as an additional 1800 treatment courts built on the DTC model (National Drug Court Resource Center, 2023). This new model represented a substantial innovation in how US criminal courts handle drug cases. Traditionally, courts operate on an adversarial basis (whether handling drug cases or other crimes), with prosecution and defense teams pitted

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CRediT authorship contribution statement

Patrick F. Hibbard: Conceptualization, Data curation, Formal analysis, Methodology, Software, Supervision, Validation, Visualization, Writing – original draft. **Jason E. Chapman:** Supervision, Validation, Writing – review & editing.

Declaration of competing interest

None.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jcrimjus.2024.102267.

against each other in pursuit of a specific finding (e.g., guilt). DTCs, on the other hand, function collaboratively. Generally speaking, an assumption of guilt is made and then all parties cooperate to achieve the same participant outcome: recovery from substance use and desistence from crime (DeVall, Lanier, & Baker, 2022; Marlowe, Hardin, & Fox, 2016; NADCP, 2018a, 2018b).

While individual-level outcomes analysis overwhelmingly demonstrates improvements (e.g., recidivism, drug use, employment, attitudinal measures; see Downey & Roman, 2010; Rossman, Roman, & Rempel, 2011; Sevigny, Fuleihan, & Ferdik, 2013), the few studies looking at community outcomes indicates more mixed results. Although some studies have indicated improvements in community outcomes (e. g., Zafft, 2014), others point toward *increases* in community-level discretionary arrests (Hibbard, 2024; Lilley, 2017; Lilley, DeVall, & Tucker-Gail, 2019; Lilley, Stewart, & Tucker-Gail, 2020), as well as crime more generally (Lilley, 2013). Thus, the question remains of how DTCs impact the communities in which they operate. This becomes especially relevant currently as nearly two-thirds of all US counties (and county equivalents) are covered by a DTC, representing over three-fourths of the US population (National Drug Court Resource Center, 2021). Further, US criminal courts, and the bulk of criminal jurisprudence, are designed to address public safety in their communities (Wexler, Perlin, Vols, Spencer, & Stobbs, 2016). It is crucial to understand if DTCs fulfill this mandate.

To address these open questions, the current study applies a recently validated econometric method, the stacked event study (Baker, Larcker, & Wang, 2022; Cengiz, Dube, Lindner, & Zipperer, 2019; Deshpande & Li, 2019), adding to this body of literature by evaluating a prevalent policy adaptation, DTCs, at an understudied level, the community. Results increase the knowledge base of quasi-experimental methods toward this type of study, as well as how such programs impact the communities in which they operate.

2. Literature review

Research on DTCs, to date, largely focuses on individual-level outcomes, and of necessity, this work relies on non-equivalent comparison group designs. Such studies, which are quite numerous, consistently find improvements in key outcomes (e.g., Downey & Roman, 2010; Rossman, Roman, & Rempel, 2011; Sevigny et al., 2013). However, due to the real-world settings in which they occur, a challenge is that the studies rarely achieve the rigor of randomized controlled trials (RCTs).

Gottfredson, Najaka, Kearley, and Rocha (2006) used an RCT design, finding reductions in re-arrest in one-, two-, and three-year periods. While contributing important information about DTC effects on individuals, the study was limited to a single DTC (Baltimore City Drug Treatment Court [BCDTC]) and, as such, the results may not be generalizable. Further, extensions of this research found conflicting results regarding long-term recidivism rates and other outcomes. Kearley and Gottfredson (2020) found no effect on drug overdose mortality, and Mackin et al. (2009) found the same regarding long-term recidivism. In contrast, another recent study found 15-year reductions in arrests, charges, and convictions (Kearley & Gottfredson, 2020).

The RAND Corporation's RCT of Maricopa County's (Arizona) DTC randomized participants to either DTC or probation. They found no significant effects regarding new arrests but decreases in technical violations such as failing a drug screen or failing to report to required meetings (e.g., with probation officer; Deschenes, Turner, & Greenwood, 1995). Other researchers performed RCTs on related types of treatment courts and outcomes (see Wilson, Mitchell, & MacKenzie, 2007 for a systematic review). Hassoun Ayoub (2020) examined a Reentry Court, finding a 45 % reduction in recidivism. Marlowe et al. (2003) randomized frequency of judge visits, resulting in no effects for attendence of counseling or abstinence from substance use but increases in detection of infractions by the supervising judge.

This work has two important limitations, namely, it is somewhat dated and individual-level analysis presents empirical limitations and may tell only part of the story. Such an approach estimates how participants fare, but it provides little about broader community impacts. For instance, recent work has found increases in discretionary arrests (e. g., drug possession) associated with DTCs (Hibbard, 2024; Lilley, 2017; Lilley et al., 2020), particularly of minority citizens (Lilley et al., 2019). Though the possibility exists that the increases in arrests resulted from actual increases in crime, these findings more likely point toward law enforcement changing their behavior in the presence of a DTC (Hibbard, 2024; Lilley, 2017; Lilley et al., 2019; Lilley et al., 2020). It should also be noted that the present study does not consider arrests an accurate measure of crime, rather of police activity. Additionally, issues of generalizability arise given variability in the timing and settings of DTC initiation. The BCDTC trial, for example, was "atypical in the type of population it serves (primarily African American, male [individuals with heroin uses disorder])" (Gottfredson & Exum, 2002: 342). Further, as these programs, and criminal courts generally, are funded at the community level and tasked with community-level mandates (e.g., improving public safety), it is important to evaluate the associated community-level outcomes.

Additionally, the few community-level studies of DTCs have shown either no effect or *increases* in crime. Graduate theses by Orrick (2005) and Zafft (2014) found some crime reducing effects; however, the effects were not maintained after accounting for outliers or without relying on imputed data. Lilley (2013) found increases in community-level offenses. Taken with findings of increased discretionary arrests (reviewed above), these results raise the possibility that community characteristics influence individual-level outcomes in the process of aggregating to the community, whether positively or negatively.

First, research thus far must be validated with more rigorous methods. Despite the rapid increase in communities covered by DTCs over the past 30 years, they continue to represent approximately 10 % of all criminal drug cases (Belenko, Fabrikant, & Wolff, 2011; Bhati & Roman, 2010), limiting the possible impact they might have on community crime. If previous work holds, that DTCs are associated with increases in community crime, then DTCs might be costing communities more than saving, whether resources or public safety. In such a case, it is necessary both to identify and rectify the sources of these unintended outcomes. On the other hand, a finding that associates DTCs with reductions in community crime calls for further investigation of how this occurs, hopefully leading to improved operations and, thus, public safety. The present study addresses this need in three ways.

First, by using a state-of-the-art econometric technique, a stacked event study design. Next, up-to-date information on DTCs are used. Finally, the current analysis extends previous work by including a longer time period (1990–2018), a larger set of DTCs than previously considered, and examines differential effects within population groups.

3. Present study

Given disparate findings between individual-level (lower rates of recidivism and drug use) and community-level (higher rates of crime and arrests) outcomes, DTCs might be associated with either increases or decreases in crime. The nascence of this line of inquiry and variation in previous findings makes the relationship between DTCs and crime a priori indeterminant. With some researchers citing a maximum of 10 % of potential criminal cases covered by DTCs (Belenko et al., 2011; Bhati & Roman, 2010), and the doubt this small fraction presents for impacting community crime, the first task is to validate previous work. This proportion issue becomes especially important considering the rationale used in previous work citing an increase in community crime associated with DTCs. Lilley (2013, 8) found increases in Offenses Known related to DTC initiation, The proposed explanation was poor graduation rates, citing "data from a two-year follow-up study of discharged participants in Southeast County drug court from 2002 to 2004," indicating non-graduates were arrested at a much higher rate than graduates, possibly even more than non-participants. This suggests that people who participate in DTCs but do not graduate might be worse off than if they had never participated. This issue of graduation rates, though, has been a priority of professional groups like the National Association of Drug Court Professionals over the past several decades (NADCP, 2018a, 2018b). With these efforts at professional development and improving DTC operations, graduation rates have improved, as well as many other DTC elements (e.g., participant selection and referral to services, punishment/incentive policies (DeVall et al., 2022; Marlowe et al., 2016). Therefore, the current study cannot make a prediction toward the same association of increased crime associated with DTC initiation, especially not based upon poor graduation rates.

The purpose of this effort, then, is to begin by exploring whether *any* impact on crime might be associated with DTC initiation at the community level. Given the previously described differences in community types by population, and the time trend of DTC initiation (more populous areas early on, rural areas later; see Fig. A.1 in the Appendix), the basic question of whether or not DTCs impact community crime was expanded into several sub-analyses for different communities regarding population. Additionally, given the small proportion of cases DTCs cover in each community, the most logical prediction would be no effect.

3.1. Data

3.1.1. Outcomes—This study used publicly available summary data from the FBI's Uniform Crime Reporting (UCR) program, concatenated by Kaplan (2021b, 2021c, 2021d). The data source included 60 years of data across more than 18,000 law enforcement agencies reporting crime known to them (i.e., the Offenses Known dataset). With the present focus on community-level DTC outcomes, which feature county-level jurisdictions, data were aggregated to county level[1], spanning 1990 to 2018. The analyses (detailed in

Empirical Strategy) focused on specific periods of time, and agencies were retained only if they reported all 12 months of UCR data for each year of the respective period.

The data included three categories of crime outcomes: all-crimes, crime indexes (property, violent, total), and non-index crimes. Each outcome was expressed as the number of crimes per 100,000 population. The outcomes followed a count distribution, and as such, each was transformed via Poisson distributions during estimation using Stata's Poisson pseudo-maximum likelihood high-dimensional fixed effects (ppmlhdfe) package (Correia, Guimarães, & Zylkin, 2019; O'Hara & Kotze, 2010).

A critical feature of the data is that the timing of DTC initiation was associated with community size, specifically, with urban areas initiating DTCs earlier and more rural areas initiating later (Noia, Youngers, Parmelee, McGranahan, & Post, 2018; Fig. A.1 in the Appendix). Given the high degree of variation in communities (e.g., urban vs. rural access to services, social construction of issues like crime and drug use), especially over time (e.g., changes in interventions available or funding options over time), the current study treated different types of communities based on population size as separate sub-studies. With DTCs specifically, more populous areas saw them initiate earlier (e.g., Miami, Los Angeles, New York City), with more rural areas covered later (see Fig. A.1 in the Appendix). Add to this the changes in drug and crime behaviors (e.g., the Opioid Epidemic and its various phases), as well as policies and public sentiment, over this period, and the potential for confounding variable bias becomes too large to account for in a single model. Thus, analyses were performed independently for each of five population groups: 2500–9999 (2.5 k), 10,000–24,999 (10*k*), 25,000–49,999 (25 k), 50,000–99,999 (50 k), and over 100,000 (100*k*). These are the population groups designated by the FBI, though groups over 100,000 were folded together here since those at and above this level represented the smallest number of communities (possibly creating issues with power; Kaplan, 2021e).

3.1.2. Independent variable—

The initial data source, based on information from state-level court administrators, was retrieved from a publicly available database previously maintained by the National Drug Court Resource Center (2018; no longer publicly available). This provided a snapshot of DTCs operating as of 2018. Building from this, each DTC entry was individually verified and expanded to include all counties within each DTC's jurisdiction (i.e., rural courts may cover multiple counties) and the year in which the DTC was initiated. Of note, although the data source provides an accurate view of DTCs as of 2018, one limitation is that a court could have opened and closed prior to data collection and, as a result, would not be represented in the data. For each county—and to differentiate years before and after DTC initiation—a binary variable indicating the existence of any DTC serves as the primary independent variable throughout this study. The variable "turns on" (changes from 0 to 1) in the year a county opens any DTC and does not "turn off" (i.e., it is taken as "absorbing;" Sun & Abraham, 2021). Unfortunately, records for when DTCs initiated are not exact, usually indicating only the year of initiation and not month or date. This means that a court that opened December in a given year would be coded

[1]This makes the unit of analysis super-agencies – larger than a single agency but smaller than a county – but the term "county" is used throughout for brevity.

the same as one that opened January. Including multiple post-initiation periods in analysis, though, mitigates this concern. Fig. 1 shows the number of courts initiated over time and their cumulative sum for both the total population of courts and those included in analysis.

DTC initiations saw general increases over time, with some decreases, up to a peak in 2007. This pattern may be attributable to federal funding vicissitudes. From the first funding in 1990, the US Congress increased funding until the early 2000s, then reduced it (Franco, 2010). A protracted effort by industry groups like the National Association of Drug Court Professionals countered this trend and the funding again saw an increase the mid-2000s (Franco, 2010). As most US communities became covered by DTCs, though, these funding efforts shifted to other types of treatment courts (e.g., Veterans Courts, Mental Health Treatment Courts; DeVall et al., 2022; Marlowe et al., 2016).

Two types of treatment courts are included in this study: DTCs and hybrid DTC/DWI courts (note that analysis did not include courts that only covered DWIs). Most hybrid courts evolved from DTCs (i.e., some DTCs added coverage of DWIs, especially in rural areas) and perform nearly identical functions, whether begun as a DTC or not. In fact, researchers and practitioners consider hybrid courts to be a “subset of [DTCs]” (DeVall et al., 2022, 15; Marlowe et al., 2016, 35). For brevity, this study uses the term DTC to denote both types of courts. Of the 1568 represented in the full dataset (1170 DTC, 398 hybrid), 1108 were included in analysis (796 DTC, 312 hybrid). These figures are the result of the stack design (described below) that requires accounting for pre- and post-periods, as well as a trimming process that excludes law enforcement agencies reporting fewer than 12 months per year in each stack.

3.1.3. Covariates and other variables—Each model included a number of control variables known to be associated with service availability in communities as well as crime outcomes. In keeping with customary control variables, all models included population and population density (Allard, 2004; Boivin, 2018), demographic characteristics (percentage age 15–24, male, white; NIH | SEER, 2021), unemployment rate (Bureau of Labor Statistics, 2021), as well as the rate of law enforcement officers (with arresting power) civilian personnel (without arresting power) per 1000 population (Kaplan, 2021b). Since DTCs must operate within their local policy contexts, and such context would likely impact DTC operations, local treatment services, and criminality around drugs and drug use, models also included a proxy variable for local perception of crime and substance use in the form of percentage Republican Party presidential votes (Stavick & Ross, 2020). Denham (2019) demostrated party affiliation as associated with attitudes toward marijuana legalization. Prior to analysis, the distribution of each variable was evaluated, and based on those results, all were log transformed for analysis (descriptive statistics in Table 1 below indicate raw percentages, rather than logged values).

Data from the Annual Survey of Public Employment and Payroll (ASPEP) dataset were used in robustness checks, evaluating whether efforts within the criminal legal system, or other community efforts might account for any observed changes in crime relative to DTC initiation (Kaplan, 2021a). These data, though, come from a voluntary survey with response rates below 100 %, meaning they do not cover the entire analysis sample (US Census, 2020).

Due to missing data, these analyses were exploratory in nature. The number of full-time equivalent employees was converted to per-1000 population rates.

3.2. Empirical strategy

Two data features had implications for the empirical strategy: (1) each county had multiple years of crime data and (2) the timing of DTC initiation varied from county to county. To address these features data analyses were performed using a stacked event study specification (i.e., dynamic treatment effects; e.g., Goh, 2021; Perez-Vincent, Schargrodsky, & García Mejía, 2021), an extension of difference-in-difference methods, to test the effect of DTCs on community crime. The first step compared community-level crime rates before and after a specific year of DTC initiation. The resulting difference, in the second step, was compared for counties that did and did not initiate a DTC during the respective period. With the difference-in-difference specification, a critical assumption is that of parallel trends (Angrist & Pischke, 2009). Prior to DTC initiation, if rates had been improving, and then following initiation continue this trend, post-initiation improvements may just be a continuation of the trend. In other words, a finding of parallel trends prior to DTC initiation indicates the same parallel pattern would likely continue beyond initiation in the absence of a new DTC (Roth, Sant'Anna, Bilinski, & Poe, 2023). The advantage of an event study stems from the inclusion of several time periods before and after DTC initiation, in addition the year of initiation. This model provides estimates both *before* initiation and *after*. If there is evidence that earlier changes in crime rates are associated with later DTC initiation, then the parallel trends assumption is violated, likely biasing results. Such a finding would also point toward reverse causality, since communities may have initiated a DTC *in response* to changes in crime rates. This feature also allows assessment of conditions leading up to DTC initiation, if preinitiation periods return significant coefficients. If this assumption is met, though, the model provides dynamic estimation of post-initiation effects. Additionally, programs like DTCs likely take time to have an impact on community crime – estimates may depend on length of exposure (Callaway and Sant'Anna, 2020). An event study can detect such an occurrence. Additionally, the event study specification provides a way to evaluate many communities over many time periods, providing a high degree of econometric rigor. It should also be noted that the inclusion of pre-initiation periods does not "control for" crime trends leading up to DTC initiation. Including any of the outcome information other than the focal period would constitute a "bad control" and introduce a new source of bias, altering effect sizes in the direction of their sign – lower crime rates prior to an intervention period would draw them down and higher rates vice-versa (Angrist & Pischke, 2009; see also Cinelli, Forney, & Pearl, 2022 for a recent review). Rather, an event study evaluates the counterfactual: By assessing whether pre-initiation trends run parallel to each other, the analyst can then determine if these would have continued in parallel *save for the intervention*.

For the present data, a central feature is that DTCs initiated at different times. To address variation in timing, it is necessary to align *calendar* years into *relative* years (Callaway and Sant'Anna, 2020). For example, in a standard event study, two counties opening DTCs in different years (e.g., 1995 vs. 2013) would be considered together when describing the counties' first year of DTC operation. The conversion from calendar to relative time highlights another assumption of the standard event study model: homogenous treatment effects (Sun & Abraham, 2021), which means that the effect of a DTC is consistent regardless of the relative time period in which it initiates. However, this is a strong assumption. DTCs were initiated at disparate time periods by disparate types of communities. A program opened in 1990 in New York City would like exhibit much different treatment effect than one in rural Indiana in 2018, meaning heterogenous effects likely exist (Callaway and Sant’Anna, 2020; Sun & Abraham, 2021l see also Baker et al., 2022). To address it, the event study model is extended to a stacked event study for a more robust identification strategy (see Hibbard, 2024, Appendix B.1 for a more detailed description). Specifically, the stacked event study creates a separate “stack” for each year being evaluated. Each stack includes counties initiating DTCs that year (treatment group), which are compared to counties that did not open a DTC that year or in the five preceding/ following years (control group; Cengiz et al., 2019; Deshpande & Li, 2019). The interpretive consequence is that DTC effects—rather than ignoring the year of initiation—are adjusted for the year of initiation, addressing the concern of heterogenous treatment effects. Prior to analysis, all stacks are appended to each other (stacked), creating the primary analysis dataset. This model can be defined as:

$$Pois(Y_{cth}) = \sum_{\tau = -5, \tau \neq -1}^{5} \sigma_\tau D_{cth}^\tau + \sum_{\tau = -K, \tau \neq -1}^{5} \pi_\tau (DTC_{ch} \times D_{cth}^\tau) + \omega_{ch} + \varsigma_t + \kappa_{st} + \varepsilon_{ct} \tag{1}$$

Y_{ct} is the outcome for county c in calendar year t. Relative time periods are indexed $\tau = -5, \ldots, -2, 0, \ldots 5$, with $\tau = 0$ representing the year of initiation. Following convention, $\tau = -1$ is excluded as a referent (Callaway & Sant’Anna, 2018; Sun & Abraham, 2021). D_{ct}^τ is an indicator variable for each relative year, with σ_τ estimating changes in crime rates per relative time period. An interaction term is also included for each time bin and whether county c ever initiates a DTC ($DTC_{ch} \times D_{cth}^\tau$). This model includes county, year, and state-by-year fixed effects (ω_{ch}, ς_t, and κ_{St}). The difference between this model and a standard event study is embodied in the subscript h, which indexes sub-experiment stacks $h = 1995, 1996, \ldots, 2013$ (accounting for a five-year cushion before and after implementation in each stack within the 1990–2018 study period). The main and interaction coefficients, $\sigma_\tau + \pi_\tau$, together estimate the average effect DTCs have on crime rates across all stacks for each relative year. To account for variation between stacks, the fixed-effects term for counties has been expanded to reflect county-by-stack fixed effects. Also of note, to manage heterogeneity within each stack between treatment and control counties and ensure analysis of equivalent groups, each treatment county was matched to its four nearest neighbors from the control group. This was accomplished via Mahalanobis distance function using all the control variables listed above (Abadie, Drukker, Herr, & Imbens, 2004; Ho, Imai, King, & Stuart, 2007). Because there were multiple years of data for each county, model standard errors were clustered at the county level to account for serial correlation (Abadie, Athey, Imbens, & Wooldridge, 2017; Moody & Marvell, 2020).

3.2.1. Robustness checks—The stacked event study model was also used to measure changes in potentially confounding variables, including law enforcement personnel, public court employees, and other relevant public employee categories, as well as clearances by arrest. These were substituted as the dependent variable. The aim was to evaluate competing explanations for the observed effects and, in turn, the extent to which such effects are attributable to DTC initiation versus alternative community specific programs or policies. Those most likely estimates to confound results would be positive coefficients (representing increases in activity), as this would indicate a parallel increase in these other efforts. Strong, significant negative coefficients, however, may also indicate changes in policy priorities – that communities may be shifting resources away from these areas toward DTCs.

External validity was evaluated using two strategies. First, covariates were tested for structural differences between included and excluded agencies using a difference-in-means test. If this returns significant and substantial differences between these groups, current results may only apply to those communities included in analysis – they may not be generalizable. This test provides an indirect estimate of structural differences, though, if the primary concern is whether these differences would influence the relationship between DTCs and community crime. Thus, supplementary analyses were performed on a dataset expanded to include previously excluded agencies which were imputed according to the number of months reported down to a minimum of six months (described fully in Appendix C).

All analyses were performed using the Stata software package (version 17).

4. Results

4.1. Descriptive statistics

Table 1 provides means and standard deviations for crime outcome variables, as well as covariates, per population group. Though all crime figures were converted into rates per 100,000 population, more densely populated groups exhibit higher rates.

Descriptive statistics for supplementary analyses (clearances by arrest, public employee data) are reported in the Appendix (Tables B.1 and B.2).

4.2. Analysis

4.2.1. Stacked event study—Table A.1 in the Appendix provides results for all primary stacked event study analyses, including estimated coefficient and *p*-value for each crime category, population group, and relative year. Of 25 models, seven did not return any significant results, 10 violated parallel trends, and eight significant results after initiation that met the parallel trends assumption (in bold). Of the latter eight, seven indicated an association between DTCs and lower crime rates and one higher. The following sections provide more detailed findings for each population group, including trend graphs for those showing significant results.

4.2.1.1. 2500–9999 population group.: The model did not return significant post-initiation estimates for the 2.5 k group. It did, however, show a statistically significant negative association between DTC initiation and the property index three years prior and a positive association four years prior for the violent index, violating the parallel trends assumption. Appendix Fig. A.2 provides detailed graphs.

4.2.1.2. 10,000–24,999 population group.: Fig. 2 displays event study graphs for the 10 k group. The x-axis indicates years relative to DTC initiation (five years before and after, with year 0 [the year of initiation] accentuated by a grey, dashed vertical line), and the y-axis representing effect sizes. Ninety-five percent confidence interval bars are provided as well. In each crime category, the parallel trends assumption was met. DTCs are associated with a statistically significant initial decrease (−3.44 %, $p = 0.026$, in year 0) in the all-crimes category, which continues for the first year (−4.75 %, $p = 0.003$) and second year (−4.02 %, $p = 0.023$) after initiation.[2] The same pattern appears for total and property indexes, while smaller in effect sizes and the only significant period being one year after DTC initiation (−3.54 %, $p = 0.024$, for the total index and − 3.44 %, $p = 0.041$, for property). Non-index crimes offer similar results, though with larger effect sizes (−7.13 %, $p = 0.003$, in year 0; −7.41 %, $p = 0{,}008$, in +1; and − 6.67 %, $p = 0.041$, in +2). The violent index shows DTCs associated with substantial and significant reductions in the +2 (−10.42 %, $p = 0.016$) and + 5 (−11.26 %, $p = 0.015$) years. Fig. A.3 in the Appendix provides individual event study graphs for each crime category.

4.2.1.3. 25,000–49,999 population group.: Results for the 25 k population group are displayed in Fig. 3. Though the parallel trends assumption is met for the all-crimes, violent index, and non-index crimes categories (i.e., no significant coefficients to the left of year 0), this is not the case for the total and property indexes. The more reliable estimates (i.e., that do not violate the parallel trends assumption) show a 9.38 % ($p = 0.017$) drop in the violent index two years after initiation but no significant post-initiation effects for the non-index category. Though the all-crimes category met the parallel trend assumption, the model returns not significant estimates post-initiation. Individual event study graphs for each crime category can be found in the Appendix (Fig. A.4).

4.2.1.4. 50,000–99,999 population group.: The model shows DTCs associated with lower rates of total and property index crimes prior to initiation, and higher violent index rates, within this population group. This violates the parallel trends assumption, so post-initiation estimates cannot be relied upon. The all-crimes category showed no effects, but DTCs are associated with a 9.53 % ($p = 0.033$) increase in non-index crime rates five years after initiation. Fig. 4 provides a graph for the non-index category, detailed event study graphs for the remaining categories can be found in the Appendix (Fig. A.5).

4.2.1.5. 100,000+ population group.: Analysis within this group showed DTCs associated with lower rates in the all-crimes category prior to initiation, as well as the total and property indexes. The results indicated that DTC initiation was not significantly associated with later change in crime rates. Detailed event study graphs can be found in Fig. A.6 in the Appendix.

[2] As analysis was performed using a dichotomous independent variable on a Poisson-transformed dependent variable, percentage change is calculated $\% \Delta = (exp[\beta]) - 1$.

4.3. Robustness

As described above, several robustness checks were performed to assess whether other community efforts might also be responsible for the changes in community crime associated with DTCs. This process involved substituting these outcomes as the dependent variable in the same stacked event study specification. Additionally, external validity was tested by evaluating difference-in-means of all control variables used in analysis between communities included and excluded from analysis, as well as a supplementary model that included more data from law enforcement agencies reporting between six and 12 months within each stack (see Appendix C for a detailed description).

4.3.1. Law enforcement—The effects DTCs have on law enforcement personnel variables were estimated for the 10 k and 25 k population groups, which are presented in Appendix B. As can be seen in Fig. B.1, DTCs are associated with significant increases in civilian personnel (i.e., those without arresting powers) for both groups. For the 10 k group, a positive coefficient indicating 5.92 % increase ($p = 0.033$) appears in the year of DTC initiation. Positive coefficients show up in the 25 k group for years +1 through +5 (max 16.13 % increase 1 year after initiation, $p = 0.031$). Using these coefficients to estimate the number of additional employees indicates and average 0.21 new civilian law enforcement employees the year of DTC initiation in the 10 k group, and 0.60 each year after initiation in 25 k.

Clearances by arrest are shown in Appendix Fig. B.2. In the 25 k subset of data, there was not a significant association between DTCs and clearances by arrest. In the 10 k group, however, both total index (5.68 % increase, $p = 0.035$) and property index (7.82 %, $p = 0.011$) show an increase the year of DTC initiation. The all-crimes category exhibits a similar pattern, though the coefficient was not statistical significance (4.31 %, $p = 0.056$).

4.3.2. Public employees

4.3.2.1. Justice/legal.: To test whether other court efforts, rather than DTCs, led to crime reductions, this study also estimated the impact of DTCs on court employee rates (per 1000 population). The category covers all court actors paid through public funds – judges, administrators, prosecutors, etc. (US Census, 2020). Fig. B.3 indicates no effects for the 25 k group and a substantial significant negative coefficient (−47.51 %, p = 0.008) five years prior to DTC initiation (which amounts to approximately 1.1 fewer employees that year, across the analysis sample).

4.3.2.2. Other public employee categories.: Other public employee categories included in the ASPEP were also analyzed, including total, welfare, housing and community development, hospital (includes in-patient substance use treatment), and health (includes out-patient substance use treatment; US Census, 2020). All analyses indicate no significant effects (Fig. B.4).

4.3.3. External validity—As can be seen in Table C.1. Appendix C, there are substantial and significant structural differences between agencies that were included in analysis and those that were excluded. All covariates, within each population group, indicated statistically significant differences. Supplementary analyses, though, that included communities reporting between six and 12 months per year for each year in each stack, showed generally similar results. However, an important exception exists for the 25 k population group. Within this group, previously significant results were reduced to null effects.

5. Discussion

This study examined the relationship between DTCs and community-level crime for five different population groups between 1990 and 2018 using a stacked event study identification strategy. The model indicated no significant post-initiation effects between DTCs and crime rates for the least (2500–9999) and most populous (100,000+) counties. Results, however, did show DTCs significantly associated with reductions in crime within counties in which law enforcement covers between 10,000 and 50,000 population. This was true for each crime category within the 10 k group. All but the non-index crimes category for the 25 k group showed a significant association. The total and property indexes, however, did not meet the parallel trends assumption. Results for the 50 k group, on the other hand, indicated an *increase* in non-index crimes five years after initiation. Further, DTCs appear to have a relationship with higher levels of civilian law enforcement personnel in the 10 k and 25 k population groups, as well as with an increase in clearances by arrest for the 10 k group within the all-crimes, and total and property crime index categories. Additionally, the balance between internal and external validity was not achieved. To ensure internal validity, the most accurate crime data were used, excluding agencies that reported fewer than 12 months for each year in each stack. In supplemental analyses, included and excluded agencies showed structural differences regarding all covariates included in analysis, and analysis of expanded data (including some of the excluded agencies) reduced results in the 25 k group to null effects throughout. Thus, most results described here cannot be generalized – they can only be considered robust for communities included in analysis (formally, the average treatment effect on the treated).

To respond to the first task of this study, previous research evaluating DTC impacts on community crime rates was not validated, especially those indicating increases in crime. With many models returning null effects, it appears that individual-level improvements in recidivism do not translate to lower community crime rates. This might be simply due to the small proportion of criminal cases DTCs represent. Current results showing a relationship between DTCs and lower community crime rates can also be put into this context. Assuming previous work estimating up to 10 % of potential cases covered by DTCs nationally (Belenko et al., 2011; Bhati & Roman, 2010), model estimates listed above seem plausible, particularly since these range between 2 and 11 % crime reductions. Consider, as well, that the larger negative coefficients came from the 10 k population group regarding the Violent Index. With a low incidence of these crimes generally, a small number of fewer crimes would represent a relatively large percentage change.

The most robust results, surprisingly, came from the violent index, for which the 10 k group exhibited 10.4 % decrease two years after DTC initiation and 11.26 % five years after, and the 25 k group exhibited a 9.4 % decrease two years post-initiation. These results remain robust regarding external validity in the 10 k group, with expanded dataset analysis indicating larger coefficients and more post-initiation periods significant. Previous work criticized DTCs as treating participants as specialists, rather than generalists, when it comes to crime (Pratt & Turanovic, 2019). According to these authors, criminological research has demonstrated that those who commit crimes do so generally, perpetrating offenses that fit a variety of circumstances (like SUD issues or opportunity), rather than focusing on one specific crime category. Yet, DTCs have historically limited participation to those accused of committing non-violent offenses (Marlowe et al., 2016; Rossman et al., 2011), treating people who commit crimes as "specialists" (Pratt & Turanovic, 2019: 3). Their logic holds in the current analysis, though from a different perspective. Considering results do not appear until two years after DTCs begin operations, and the most substantial results after five years within the 10 k group, it appears they catch individuals who will commit violent crimes in the future (in communities with law enforcement agencies covering between 10,000 and 50,000 population), but who are currently accused of non-violent offenses, and addressing underlying conditions (e.g., SUD) before they do so, pointing toward a preventative impact on violent crime. More work is needed, however, to causally infer such a connection.

The variation in results between community types also requires discussion. The most robust results coming from the 10 k and 25 k groups might make sense when put into the context of trends in DTC initiation over time between the different types of communities. As can be seen in Fig. A.1 (Appendix), more populous areas opened DTCs earlier in the study period, with rural areas catching up later. DTCs were a drastic departure from traditional, adversarial court structures, and incorporated connections to community services in novel, and substantive ways. Not only did this innovation shift to a cooperative environment, with all parties working toward a common goal, DTC teams include community service provider representatives. These teams make decisions about punishments or incentives for participants and have a voice in policy changes over time. Additionally, most policies authorizing the formation of DTCs, and all of the federal funding available, included a mandate for evaluating performance. Add to this the growth of the National Association of Drug Court Professional (and several similar state- and local-level organizations) directly aimed at supporting and improving DTC operations, and it follows that DTCs improved over time. While more populous communities included in this study also likely improved operations over time, the study period included these earlier learning phases. Further study is needed that looks at discrete time periods to refine this line of inquiry.

Results indicate DTCs associated with a reduction in non-index crimes within the 10 k group. These crimes, relative to index crimes (Part I offenses), are often considered less severe (Stogner, 2015) and, as such, typically receive less attention in the literature (Kaplan, 2021e). However, non-index crimes better align with DTCs, particularly given their focus on drug offenses (i.e., non-Part I offenses). With the reduction in non-index crimes only observed in the 10 k population group, this finding identifies a direction for further investigation. For the 50 k group, there was evidence of crime rates changing significantly in the years preceding DTC initiation, but following initiation there was limited evidence of change. An exception, however, was non-index crimes, which showed a marked *increase* five years following DTC initiation (Fig. 4). Additionally, results for both the 2.5 k and 50 k groups indicated DTC initiation was predicted by both lower property index crime and higher violent crime (Figs. A.2 and A.5). Though such results bias post-initiation estimates, they provide information about conditions leading up to these types of communities starting a DTC. While DTCs were originally designed to include participants accused of lower-level offenses (those that would fit into the property index or non-index categories), the issue of crime appears to have been more salient in these communities due to higher levels of violent crime.

Reductions in the all-crimes category and indexes also warrant discussion. For the 25 k group, there was evidence of pre-trends in the total and property indexes. Results from the 10 k group do not indicate the same pre-initiation bias, though these may be countervailed by increases in clearances by arrest within the same categories (Fig. A.8). In the 10 k and 25 k population groups, DTCs were associated with an increase in civilian law enforcement personnel. While the current study cannot interpret results causally, this finding indicates communities shifted policy priorities concurrent to DTC initiation (or, maybe DTC initiation was a part of this shift in priorities). That is, the initiation of DTCs and additional civilian personnel may be a reflection of the community's overall efforts to combat crime. Civilian personnel do not have arresting powers and have traditionally included dispatch and clerical duties. More recently, especially as technology and public sentiment toward public safety have changed, the position has expanded to include planning, public information, and administrative; as well as roles associated less with enforcement like victim advocates and mental health consultants (Elkins, 2021). Thus, communities opening DTCs may also be shifting priorities in their approach to crime overall, expanding the role of non-traditional enforcement (e.g., crisis intervention). The possibility also exists, though, that these additional civilian personnel served in technological enforcement roles (e.g., forensics), leading to higher levels of law enforcement efficiency (as represented by the higher levels of clearances by arrest). Elkins (2021), in fact, called civilian personnel a "force multiplier." These variables were included in all models to account for both officers and civilian personnel, though, meaning these other community efforts represent *an additional* element to account for changes in crime rate.

Now to attempt reconciliation of the disparity between the current study and the only other peer reviewed work along these lines, which found *increases* in community crime following the initiation of DTCs (Lilley, 2013). Lilley (2013: 4) evaluated communities with a minimum of 10,000 residents for years 1995–2002. The study utilized a standard fixed-effects regression, in which the model addressed endogeneity by including covariates for other grants specifically distributed to areas with high crime (Lilley, 2013: 5, 7–8). However, there was limited information for causal attribution of effects, including causal direction. Specifically, it may be that observed effects reflected increases in crimes *before* DTC initiation during a period when these courts were associated with weaker crime reductions – the time period used in this previous study covered an early period of DTC operations, which have improved since, leading to the current crime-reduction results.

With evidence that DTC initiation leads to downstream crime reductions, it is important to consider the potential mechanisms at play. The first, and most obvious, mechanism comes from the primary DTC function: connecting participants with applicable services.

Selecting the right participants (those who would benefit from the program) and providing each with needed services, in fact, has been one of the primary focuses of DTC operational development (NADCP, 2018a, 2018b; Rossman, Roman, Zweig, et al., 2011). This line of inquiry becomes especially important as DTCs provide few services themselves, doing so through a network of collaborating service providers. It may be the case that DTCs achieve this intended purpose, connecting participants with services that address underlying issues (e.g., SUD).

It may also be the case that opening a DTC increases community service capacity and/or quality, providing needed services to the wider community, even those who do not participate in DTCs. Starting a DTC creates new demand for services, incentivizing new entrants into the market or expansion of capacity by existing providers. DTCs also generally choose the service providers they refer participants to, creating an incentive for effective operations (Lurigio, 2008; Monchick, Scheyett, & Pfeifer, 2006; Wenzel, Turner, & Ridgely, 2004). Further, a substantial portion of DTCs formally contract with providers, including quality provisions (Office of Justice Programs, 2001). Both processes, picking which providers to use and formal contracts, may have the effect of improving the quality of community services, whether for DTC participants or the general public.

One other potential causal channel is the opposite of social disorganization. This phenomenon occurs when a community has difficulty achieving common goals like public safety (Kubrin & Weitzer, 2017). Research has demonstrated removing individuals from the community via incarceration serves as one underlying element of this disorganization (Clear, 2007). Since DTCs are often an alternative to incarceration individuals remain in the community, possibly avoiding some measure of social disorganization. DTCs might be reducing crime by simply keeping people out of prison.

The current study adds to a nascent line of inquiry, pointing toward several directions for future research. First, more work is needed to establish a causal connection between DTCs and community crime. This is a significant challenge; however, replication studies using a variety of methods and data sources should provide further evidence of such an impact. Variation in results between types of communities also bears further investigation. While the difference in results between less and more densely populated areas may have to do with DTCs inhabiting a larger proportion of court operations in more rural areas – more densely populated regions include more service efforts (Allard, 2004), DTCs generally scale capacity relative to the local context (Bouffard & Smith, 2005; Collaborative Justice Court Roster, 2020). That is, courts opening in urban areas are more numerous and have larger capacity. The current study cannot rule out this relative capacity potential, though, so more work is needed to evaluate the role of relative capacity (e.g., DTC participants per capita).

Another interesting line of inquiry pertains to the role of shifting policy priorities within communities initiating DTCs, including the effects of policy priorities on downstream outcomes like crime. The current study encountered potential evidence of this with the finding of increases in law enforcement civilian personnel. This apparent relationship calls for a study focused on the question. Efforts such as DTCs do not happen in a vacuum – they are impacted by, and impact, several other elements withing their communities.

If a causal connection between DTCs and changes in crime is established, work looking at causal channels makes sense. A few potential mechanisms were presented above, which should provide a starting point for future work.

5.1. Limitations

Though the current study takes several steps toward robust analysis of DTC impacts on community-level crime, causal interference remains elusive. Applying state-of-the-art econometrics gets us closer to inferring DTCs reduce community crime, albeit only for specific population groups (a large portion of results indicated no effects). More work is necessary to establish this connection definitively. The current study considered whether DTCs were present or absent in communities; however, the study did not consider the relative court capacity of DTCs. This is a factor that could influence results. For instance, for two communities with DTCs, if one represents a higher proportion of the overall court operations, its effect likely would be more potent. As mentioned above, further study is needed using relative court capacity as an independent variable to validate results. Additionally, robustness checks toward external validity indicate structural differences between included and excluded agencies, meaning these results cannot be generalized.

Another limitation arising from an evaluation using just the presence of a DTC comes from the possible ebb and flow of DTCs. The variable “turns on” in the year a county opens a court and does not “turn off” from that point forward (i.e., is taken as “absorbing;” Sun & Abraham, 2021). Some counties initiate, but later close, DTCs. Since data were attained in 2018 (National Drug Court Resource Center, 2018) and verified in 2019–2020, counties that operated a DTC as of this time period were represented accurately. However, if a county closed its DTC prior to 2018, it was coded as never having one, which could influence results.

Crime and other data sources may also include measurement error. Crime data, whether from the UCR or other sources, present known measurement error issues. As the more reliable National Incident Based Reporting System begins to provide longer periods of data for more jurisdictions, replication studies will be possible (Strom & Smith, 2017). Other crime data sources should be used as well (e.g., the National Crime Victimization Survey). The ASPEP data also come with issues, being a survey sample of communities. There is the possibility that some characteristics of responding communities correlate with changes in crime or confound the relationship between DTCs and crime.

One other issue comes from the milieu of activity attempting to address the same issues DTCs work on. For instance, Eaglin (2016: 596) credits the “drug court paradigm” with broader sentencing reform, which would have non-random influence on estimates of DTCs’ impact on community outcomes, especially crime. More work is necessary to ensure the connection between DTCs and crime reduction was not made spuriously.

6. Conclusion

This study adds to the sparse research evaluating the impact of adult drug treatment courts on community-level outcomes. DTCs were associated with a robust reduction in violent crimes within communities between 10,000 and 50,000 populations (i.e., the 10 k and 25 k groups) and less serious, non-index crimes in those between 10,000 and 25,000. Results also showed these same communities had a relationship with reductions in the all-crimes and index categories, though less robust. Additionally, validity checks identified an association between DTC initiation and increases in civilian law enforcement personnel.

Despite not supporting causal inference, these results indicate communities in the 10 k and 25 k population groups feature some characteristics leading to a relationship between DTCs and crime reductions. This holds particularly true for violent crimes, which returned the most substantial and reliable results. This may be due to DTCs achieving their intended purpose (providing needed services to participants to prevent future crime), stimulating increases in community service capacity and quality (whether utilized by DTC participants or the larger community), or providing an alternative to incarceration (keeping individuals within their communities, lowering the potential for social disorganization).

While this line of inquiry is in early stages, some distinct policy recommendations arise. First, though research on DTCs' community effects varies quite a bit, none of this work has shown individual-level outcomes directly benefit communities. While individual participants in DTCs tend to fare better than non-participants, some element(s) in communities appears to alter these results as they translate to the community – individual-level benefits do not simply aggregate to the community. Communities should explore the relationships between DTCs and other local actors (e.g., law enforcement, substance use services). Understanding how interactions between DTCs and other organizations influence the extent to which individual outcomes translate to community benefits will provide information to improve performance and, thus, public safety.

Another policy implication comes from the strong association found between DTCs and violent crime. Since most of these programs limit participation to those accused of non-violent offenses, it appears DTC programming (connecting justice-involved people with services) may have a preventative effect. Finding out more about how DTCs (and similar efforts) might be interrupting criminal careers will lead to not only higher levels of public safety but also lower long-term costs throughout the criminal legal system (e.g., via lower incarceration rates).

Such cost savings, though, occur across institutions with generally siloed streams of funding, such as courts and prisons. This points toward another policy implication. A cost-benefit evaluation of DTCs would be incomplete without inclusion of other institutions like jails and prisons. A more holistic view of the criminal legal system, and how operation of specific elements like DTCs impact other elements' operations, would better inform funding and policy decisions toward efficiency. For instance, violent crimes incur harsher penalties than property offenses, often incarceration. If programs like DTCs lower long-term violent crime rates, then court operations likely impact downstream corrections budgets.

Finally, results above point toward many effects taking time, often more time than the typical election cycle, making accurate evaluation difficult. Though the recommendation to make policy decisions based on a realistic timeline (i.e., long-term) is not new (see, e.g., Lab, 2004), the current study continues this call. Communities would do well to plan and evaluate programs like DTCs not only across multiple organizations but also a longer timeline.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Data availability

Except for the unique list of adult drug treatment courts in the US, all data used in this study are public domain. Data regarding DTCs will be provided upon reasonable request for replication.

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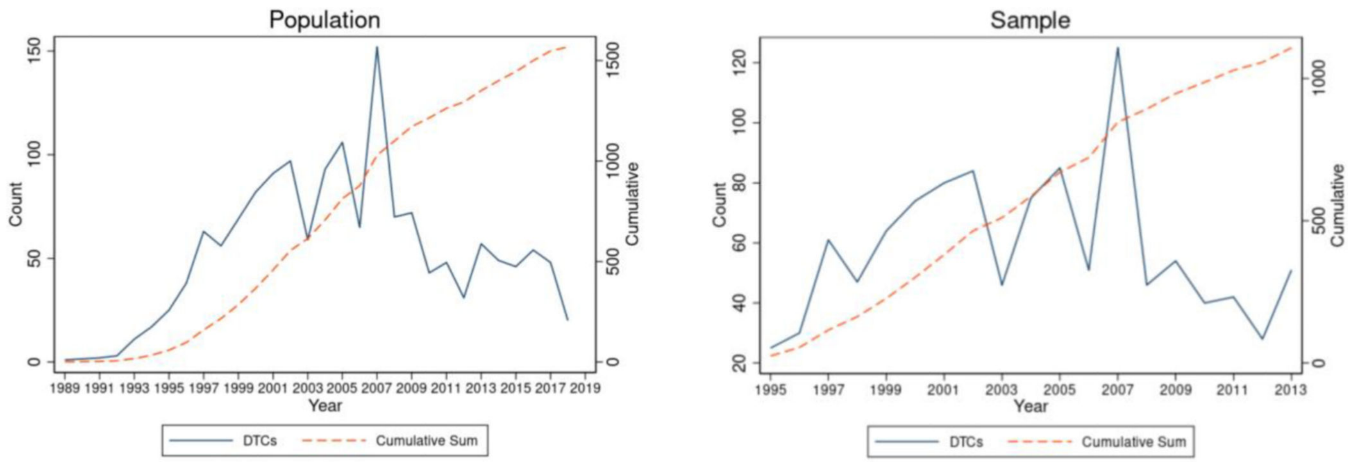

Fig. 1.

Adult drug treatment courts over time.

Note: These graphs report counts of adult drug treatment courts initiated per year (solid blue line; left y-axis) and their cumulative sum (dashed red line; right y-axis) for the total population (as of 2018) and those included for analysis. Courts included in analysis were limited to 1995–2013 to account for the stacked event study specification.

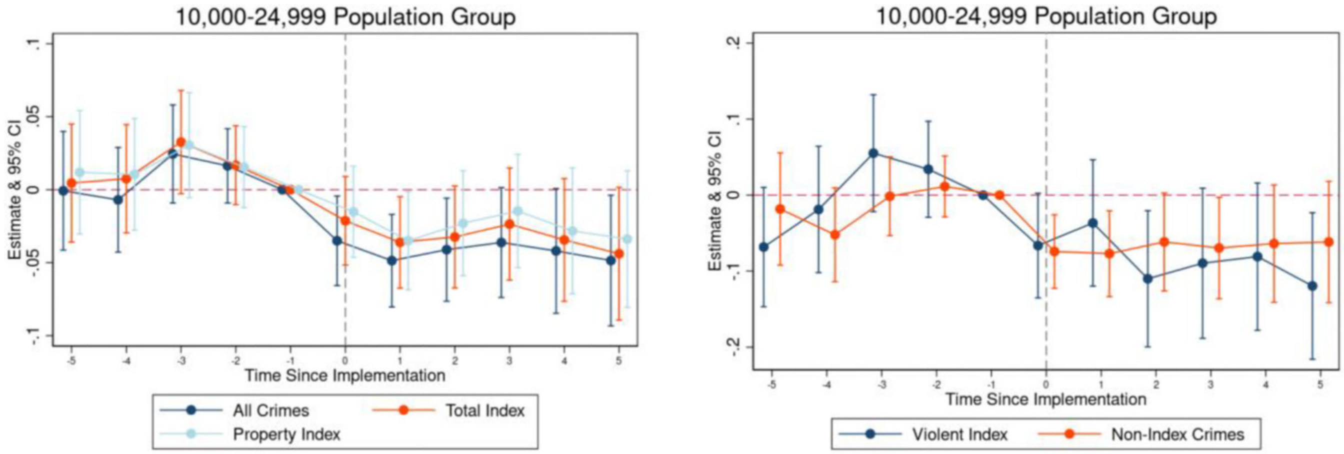

Fig. 2.

Event study graphs: 10 k population group.

Note: These graphs show point estimates, and 95 % confidence intervals of the effect DTCs have on the all-crimes, total index, property index, violent index, and non-index crime categories within the 10 k population group, reporting the sum of main and interaction effects (coefficients $\delta_\tau + \alpha_\tau$ from Eq. (1) on the y-axis. Years relative to DTC initiation are indicated on the x-axis with the year of initiation (year 0) indicated by a vertical, dashed grey line.

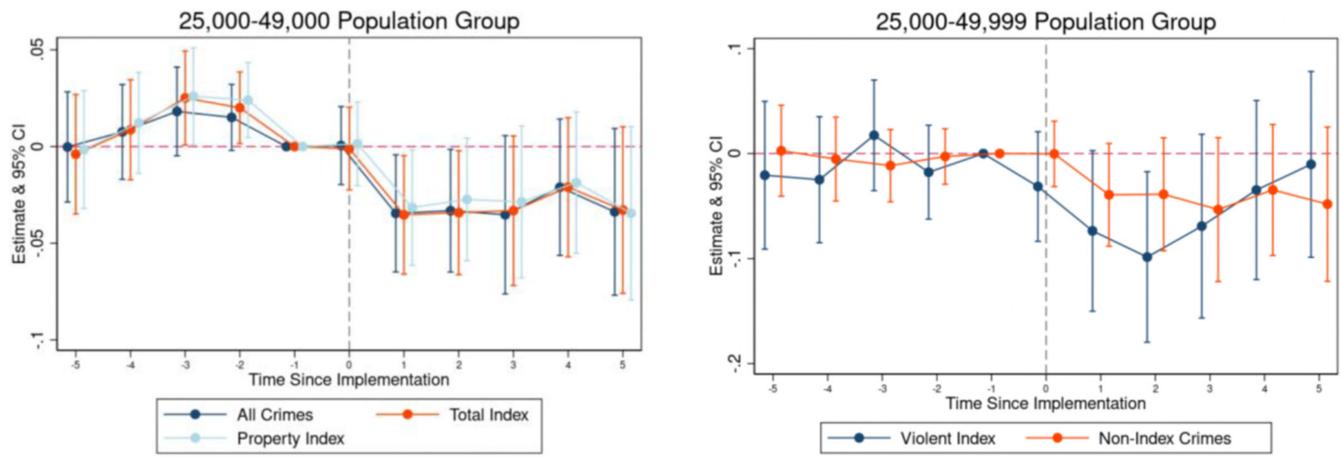

Fig. 3.

Event study graphs: 25 k population group.

Note: These graphs show point estimates, and 95 % confidence intervals of the effect DTCs have on crime within the 25 k population group on the y-axis. Years relative to DTC initiation are indicated on the x-axis with the year of initiation (year 0) indicated by a vertical, dashed grey line.

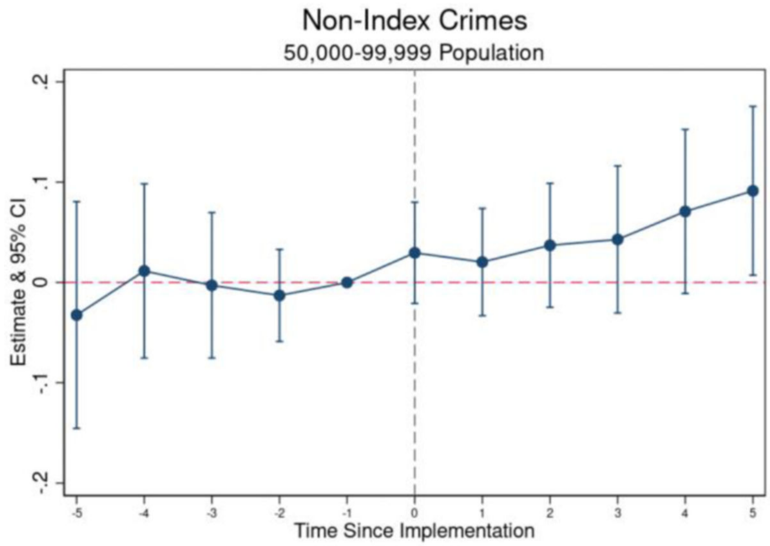

Fig. 4.

Event study graph of 50 k group and non-index crimes.

Note: This graph displays point estimates and 95 % confidence intervals of the effect DTCs have on non-index crimes within the 50 k population group on the y-axis. Years relative to DTC initiation are indicated on the x-axis with the year of initiation (year 0) indicated by a vertical, dashed grey line.

Table 1

Variable means and standard deviations (in parentheses).

Variable	2.5 k	10 k	25 k	50 k	100 k
Known Offenses (per 100 k)					
All	3148.58	3945.65	5070.98	5045.96	6170.96
	(1699.59)	(2464.40)	(3721.32)	(2359.95)	(2800.10)
Total Index	2341.22	2959.45	3865.78	3945.58	4916.91
	(1303.81)	(1849.84)	(2913.55)	(1866.19)	(2369.23)
Prop Index	2100.19	2661.92	3502.85	3585.44	4338.33
	(1194.01)	(1666.15)	(2671.06)	(1684.35)	(2040.13)
Violent Index	241.97	299.46	365.44	364.38	579.80
	(222.90)	(288.22)	(349.98)	(275.34)	(412.23)
Non-Index	808.17	989.45	1210.58	1106.49	1257.52
	(564.13)	(809.81)	(1036.50)	(725.49)	(686.97)
Control Variables					
Total Law Enf. Personnel/1000	3.27	2.83	2.68	2.77	3.00
	(1.68)	(1.49)	(1.54)	(1.83)	(1.23)
Officers/1000	2.07	1.98	1.97	2.01	2.14
	(1.20)	(1.21)	(1.21)	(1.46)	(0.87)
Civilian LE/1000	1.21	0.88	0.72	0.77	0.88
	(0.95)	(0.69)	(0.62)	(0.58)	(0.59)
County Population Covered*	8880	20,232	53,794	150,305	498,706
	(4262)	(11,410)	(52,233)	(123,315)	(800,696)
Population Density	21.35	48.24	111.15	282.60	1752.67
	(91.20)	(216.04)	(288.34)	(398.36)	(15,153.04)
Age 14–25 (%)	12.91	13.12	13.76	15.64	14.16
	(2.22)	(2.97)	(3.65)	(5.11)	(2.72)
Female (%)	49.68	50.19	50.51	50.84	51.05
	(2.41)	(1.86)	(1.38)	(1.07)	(0.98)
Male (%)	50.32	49.81	49.49	49.16	48.95
	(2.41)	(1.86)	(1.38)	(1.07)	(0.98)

Variable	2.5 k	10 k	25 k	50 k	100 k
Black (%)	4.16	7.97	8.38	8.41	14.54
	(9.13)	(13.59)	(11.35)	(9.10)	(13.50)
White (%)	91.94	89.74	88.90	87.70	80.41
	(11.96)	(13.69)	(12.21)	(8.86)	(13.80)
Unemployment Rate (%)	5.68	6.55	6.28	5.51	5.69
	(2.50)	(2.83)	(2.59)	(2.36)	(2.50)
Republican Vote Proportion (%)	61.07	57.22	55.98	51.57	47.44
	(14.40)	(12.92)	(11.73)	(12.10)	(13.22)

* This number represents the total population covered by law enforcement agencies included in analysis aggregated to the county level.