Drug Treatment Courts and Community-Level Crime: A Stacked Event Study Analysis

Abstract

Objectives: Explore the extent to which adult drug treatment courts (ADCs) affect community-level crime, demonstrate the application of stacked event study estimation in quasi-experimental criminology research, and introduce a crime data optimization strategy.

Methods: Static and dynamic fixed effects estimation were performed on a longitudinal dataset constructed to address potential issues of bias, which included crime data linked to a unique list of ADCs and relevant control variables, resulting in over 52,000 county-year observations between 1990 and 2018.

Results: Crime reduction estimates range from 2% property index to a violent index drop of 6%. Counterfactual analysis indicates over 417,000 fewer crimes within this sample between 1995 and 2013 (which accounts for the stack design).

Conclusions: Considering their limited footprint, ADCs have an outsized effect on crime alluding to complex interactions between participants and communities. Though further investigation is needed, expanding these programs would likely continue to improve public safety.

Introduction

Since Miami-Dade County created the first adult drug treatment court (ADC) in 1989, these specialty courts have expanded in focus and jurisdiction (Strong et al. 2016). Currently, over 3,800 treatment courts built on this model operate in the United States

(National Drug Court Resource Center 2021). The bulk are ADCs, numbering over 1,700[1] by 2018 (NDCRC, 2018).

Little research into the effectiveness of these courts evaluates community-level impact, though scholars have called for more research examining broader, macro-social crime outcomes (Lipsey et al. 2006). ADC research almost universally examines individual participant outcomes (e.g., recidivism, drug use, employment, attitudinal measures), largely finding improvements (GAO 2005; Rossman et al. 2011; Sevigny et al. 2013; Shaffer 2011). Research evaluating community-level outcomes, on the other hand, has found no effect or increases in relevant arrest and crime rates (Lilley 2013; 2017; Lilley et al. 2019, 2020; Orrick 2005; Zafft 2014). Disparate findings between these two units of analysis allude to individual-level outcomes possibly not translating to benefits for the community. The first task, then, is reconciling these incongruent results.

The present study fills this need using state-of-the-art econometric techniques, stacked event study design (described in Appendix B), on rigorously verified data, with the most up-to-date information on ADCs (all data sources and cleaning processes described in Appendix C). The current analysis also extends this line of inquiry by including a longer time period (1995-2013) and a larger set of courts than previously considered. Data are available for 1990 through 2018 but the stacked event study construction (see Appendix B) requires a five-year cushion before the first and after the final implementation year. Prior to 1995, ADCs numbered in the dozens, so were not widely adopted. After a peak of new ADC implementations in 2007, this process decelerated, with fewer new courts each year (see Figure 1 below). This period also covers a wide swath of crime-rate vicissitudes. Thus, 1995 to 2013 represents overall trends in the US.

This study examines the extent to which ADCs affect community-level crime. Since this program has diffused throughout the US, covering nearly every jurisdiction, large amounts of public resources have been devoted to their operation. Communities develop and implement such programs in an attempt to improve public safety. A robust demonstration of their community effects is thus called for.

Given disparate findings between individual- and community-level research, ADCs might be associated with either increases or decreases in crime. David Lilley (2013) found an increase, explaining the number of non-graduates committing more crimes may outnumber graduates committing fewer. It might be the case that ADC participants who do not graduate are worse off than if they had not participated at all. Some work has also found ADCs may pull people into the criminal legal system with low levels of substance use, creating an entanglement detrimental to individuals' lives and communities (DeMatteo et al. 2009; Lilley et al. 2019, 2020). It stands to reason this entanglement may increase the likelihood of future commission of crimes, increasing community-level crime rates (Motz et al. 2020).

On the other hand, the bulk of individual-level ADC research shows reductions in recidivism and drug use. National and local efforts at improving operations have increased graduation rates, equitable access, and screening, meaning previous findings of increases in crime and arrests may not paint the current picture (Marlowe et al. 2016; NADCP 2018a, b). Given the known connection between substance use problems and crime (Bean 2008), if ADCs successfully address these issues, then the expectation is that crime rates decrease. Altogether, the nascence of this line of inquiry and variation in previous findings makes the relationship between ADCs and crime *a priori* indeterminant.

Background
Drugs, Crime, and Courts

The literature has demonstrated a link between drug use and crime (e.g., Bean, 2008). Those with substance use issues not only harm themselves and their familial/social networks, but often fall into delinquent behavior – both as a way to support a drug habit and as a consequence of the drug use lifestyle (Bennett and Holloway 2010; Pernanen et al. 2002). These crimes also impact communities, exacting costs both monetary and social. Those committing them as a result of substance dependency, victims, and communities suffer not only the immediate consequences of each crime, but long-term, indirect impacts as well (Hunt et al. 2017; Pettit and Gutierrez 2018).

Crime increased dramatically during the 1980s and 90s in the United States. This increase occurred on the heels of research seemingly indicating little effect from most rehabilitation efforts, the so-called "nothing works" doctrine (Martinson 1974; see also Cook 1975 for work disparaging therapeutic corrections efforts). Concurrently, political rhetoric and public sentiment bent toward tough-on-crime policies, culminating in the War on Drugs and exponential increases in law enforcement and criminal punishment (Alexander 2012; Jensen et al. 2004). Drastic increases in activity choked the criminal legal system, including courts, which saw lower levels of funding increases relative to other institutions like law enforcement (Eisenhardt 2019).

Criminal courts exceeded capacity and, thus, experienced diseconomies of scale; marginal costs began increasing (Davis et al. 1994; Lurigio 2008). The courts first responded to capacity issues by specializing – creating special docket courts, such as drug courts (Nolan 2003). These dockets featured judge, prosecutor, and defense

attorneys specializing in the topic (in this case, drug crimes; Marlowe 2021). Even these efforts, though, failed to improve efficiency enough to handle the flood of cases. As marginal costs continued to increase, incentives to adapt new processes increased (substitution effect). In 1989 the Miami-Dade County drug court transitioned from contemporary thought, solely addressing probability of conviction and punishments for crime, to attempting to address antecedents to crime, such as substance use disorder (Goldkarnp and Weiland 1993), creating the first drug treatment court.

Drug Treatment Courts

Drug treatment courts operate on the premise of service delivery, rather than punishment. Internal processes transformed from the traditional adversarial environment to cooperative, in which all parties were tasked with considering the success of participants (Marlowe et al. 2016; Rossman et al. 2011). This new model also featured an expanded set of actors, including service providers. Further, courts began connecting themselves to non-traditional external actors like service providers and employers (Bureau of Justice Assistance 2004; NADCP 2018a). Proximate jurisdictions replicated the ADC model over the next few years, but the innovation gained national prominence after being included in federal policy (1994 Crime Bill). ADCs diffused across the US rapidly, with over 1,700 ADCs currently in operation (National Drug Court Resource Center 2021).

The Model

The drug treatment court model is based upon *Ten Key Components* early ADC professionals set down as the standard (Bureau of Justice Assistance 2004; NADCP 2018a). These include (1) integrating treatment services into court case processing

systems, (2) non-adversarial internal operations, (3) early criminal career intervention, (4) access to a continuum of services for participants, (5) monitored abstinence from drugs and alcohol, (6) coordinated strategies between court actors governing responses to participant compliance or non-compliance (i.e., all actors provide input), (7) ongoing judicial interactions (usually frequent meetings with presiding judges in their courtroom), (8) ongoing program evaluation, (9) ongoing professional development of court actors, and (10) building relationships with community stakeholders (BJA, 2004).

Though the model represents innovative practice, some elements align with other criminal case outcomes. Cases that terminate with probation or incarceration also experience drug and alcohol use monitoring, as well as some connection with services like SUD counseling (see e.g., Kelly et al. 2005). The call for a continuum of service for those involved in the criminal legal system also includes non-ADC options, though it does not appear other areas have achieved the levels of program adherence seen in PSCs (*Behavioral Sciences & the Law* printed a double issue on the subject; Pinals & Felthous 2017). Similar calls for cross-boundary collaboration within and without the criminal legal system (including other types of courts) have seen spotty implementation (e.g., Morrissey et al. 2009).

The Process

Following arrest and charges alleging criminal offense, individuals participate in traditional criminal case processes, including plea negotiations. ADCs come into play as one option within the set of alternatives prosecutors might offer in exchange for a guilty plea (Public Defender 2018). Though some ADCs operate prior to plea and sentencing

phases, the vast majority continue to require participants to plead guilty (Rossman et al. 2011).

Most ADCs focus on people who have committed first-time, non-violent offenses, though recent developments have opened this up a bit (Marlowe et al. 2016; Saum et al. 2001). Often, plea agreements include sentence deferral, in which a guilty plea is thrown out upon successful completion (Noia et al. 2018; Rossman et al. 2011). Deferral itself, though, is also not uncommon for probation supervision sentences (Mueller-Smith and Schnepel 2017). Often, plea agreements include sentence deferral, in which a guilty plea is thrown out upon successful completion of some set of conditions (Noia et al. 2018; Rossman et al. 2011).

Previous Research

The majority of ADC research evaluates individual-level outcomes (e.g., recidivism, drug use, employment, attitudinal measures), and uses non-equivalent comparison group designs, consistently finding improvements. Though quite numerous, these studies generally offer low-quality analyses (GAO 2005).

Gottfredson, et al. (2006) used a randomized controlled trial (RCT) design, finding reductions in re-arrest in one-, two-, and three-year periods. The study, however, only included one adult drug treatment court (Baltimore City Court) and may not be generalizable. Further, extensions of this research found conflicting results regarding long-term recidivism rates and other outcomes (Kearley et al. 2019 found no effect on drug overdose mortality; Mackin et al. 2009 found no effect on long-term recidivism), though recent extensions have shown 15-year reductions in arrests, charges, and convictions (Kearley & Gottfredson 2020).

The RAND Corporation's RCT of Maricopa County's (Arizona) ADC randomized participants to either ADC or probation, finding no significant effects regarding new arrests but lower technical violations (low-level offenses such as failing a drug screen or failing to report; Deschenes et al. 1995). Others have performed similar randomized controlled trials on other types of treatment courts and other issues within ADCs, as well, with results as ambiguous (e.g., Hassoun Ayoub 2020 examines a Reentry Court; Marlowe et al. 2003 randomizes frequency of judge visits; see Wilson et al. 2007 for a systematic review).

Individual-level analysis, however, presents empirical and theoretical issues. Beyond the obvious considerations of empirical rigor, such an approach estimates how participants fare and little about broader community impacts. Even the RCT "gold standard" only tells us about those individuals in that community at that time. The high degree of variation in time and place between ADCs makes generalization of the few experimental studies difficult. The Baltimore Drug Court trial, for example, was "atypical in the type of population it serves (primarily African American, male heroin addicts)" (Gottfredson & Exum 2002: 342). Further, as these programs, and criminal justice courts generally, are funded at the community level and are tasked with community-level mandates (e.g., improving public safety), it makes sense to measure performance at the more macro level.

Of greater concern, the few community-level studies of ADCs have shown either no effect or *increases* in crime. Orrick (2005) and Zafft (2014) found some crime reducing effects that did not stand up to rigor. Lilley (2013) found increases in known offenses, at the community-level, while other studies found increases in low-level arrests (Lilley 2017; Lilley et al. 2020), especially of minorities (Lilley et al. 2019). These results

highlight the possibility of contextual factors either dampening or magnifying aggregation of individual-level results to communities.

As ADCs operation is wide-spread – demanding high levels of public resources, affecting hundreds-of-thousands of individuals and nearly every community in the US – rigorously estimating their impact on community crime represents an urgent need. If previous work holds, that ADCs are associated with an increase in community crime, such programs should be reviewed for sources of these unintended consequences and ways to rectify them. On the other hand, a finding of crime reductions associated with ADCs would call for further investigation into causal channels toward replication.
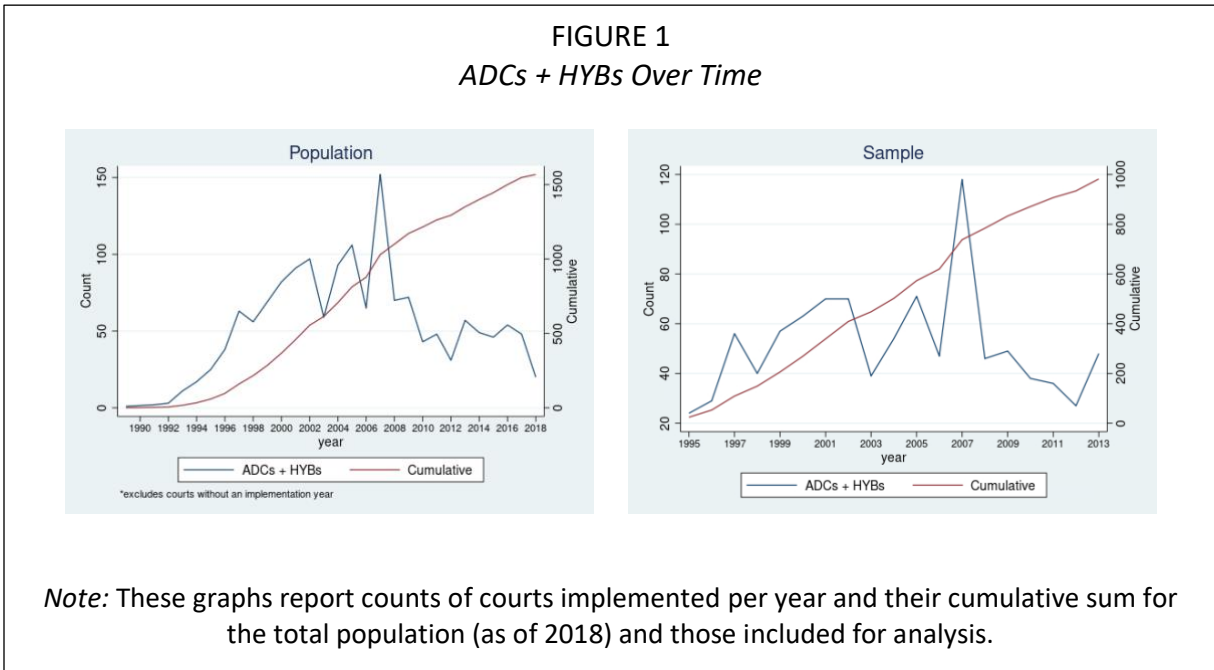
The Current Study
Data
Adult Drug Treatment Courts

Two types of Treatment Courts are included in this study: Adult Drug Treatment Courts and Hybrid DWI/Drug Treatment Courts (HYB). Most of the latter evolved from ADCs and perform nearly identical functions, whether begun as an ADC or not. For brevity, this study uses the term ADC to include both types of courts. Table 1 indicates counts for each of these and their sum. The existence of any court serves as the primary independent variable throughout this study.

Table 1. Court Counts

	ADC	HYB	Both
Population	1290	413	1703
Sample	725	302	1027

Figure 1 shows the number of courts implemented over time and their cumulative sum

for both the total population of courts (as of 2018) and those included in analysis (i.e.,

those that fit within the 1995-2013 stack periods and survived data pre-processing).

FIGURE 1
ADCs + HYBs Over Time

Note: These graphs report counts of courts implemented per year and their cumulative sum for the total population (as of 2018) and those included for analysis.

The Uniform Crime Reporting Program

I use publicly available data from the FBI's Uniform Crime Reporting program

concatenated by Jacob Kaplan (2021b, c, d). Since this study focuses on the community

impacts of ADCs, I use county-level crime (operationalized as offenses known) as core

outcome data, including total crimes and crime indexes (property, violent, and total).

See Appendix C for a full explanation of these data and the preparation process.

Following convention, I express all crime data as the number of crimes per

100,000 population. As these data feature skewed distributions, a log transformation

would be preferred. The dataset includes zero values sufficient to make this inadvisable

(see O'Hara & Kotze 2010), though, so I transform outcomes via Poisson distributions

during estimation, using Stata's Poisson pseudo-maximum likelihood high-dimensional fixed effects (ppmlhdfe) package (Correia et al. 2019). Table 2 presents descriptive statistics for all UCR variables used in analysis, including means of the total sample, treatment, and control sub-samples. Quantities represent the entire study period (1990-2018).

Table 2. UCR Descriptive Statistics

Variable	Total Mean (N = 52,275)	Treat Mean (N = 14,474)	Control Mean (N = 37,801)
Known Offenses (per 100k)			
All	4,635.75	5,145.72	4,438.59
Total Index	3,550.77	3,977.16	3,385.93
Prop Index	3,191.22	3,564.02	3,047.10
Violent Index	361.61	415.47	340.78
Law Enforcement (per 1k)			
Tot LE/1k	2.86	2.93	2.83
Officers/1k	2.01	2.12	1.97
Civ/1k	0.86	0.83	0.87
Clearance by Arrest (%)			
All	33.39	31.79	34.01
Total Index	24.86	23.87	25.25
Prop Index	21.18	20.17	21.57
Violent Index	58.55	56.62	59.30

Control Data

I include covariates the literature has shown to influence crime and service provision. These include reporting agencies' covered population,[2] population density as a proxy for access to services (Allard 2004), demographic characteristics associated with crime rates (percentages age 15-24, male, and white; NIH | SEER 2021), unemployment rates (Bureau of Labor Statistics 2021), and proportion of presidential party votes (proximity weighted to proximate year for non-election years) as a measure of local construction of issues like crime and substance use disorder (Stavick & Ross 2020). All

control variables were log transformed for pre-processing (nearest neighbor matching) and analyses. Table 3 provides descriptive statistics for these, along with a t-test and standardized difference in means. Though population, white percentage, and Republican vote proportion categories show the highest standard difference, these amount to nominal dissimilarities (0.224, -0.144, and -0.159 standard deviations respectively). Using these as control variables in the model, as well as fixed effects for county, year, and state-by-year, accounts for remaining heterogeneity between groups. Additionally, I include the number of law enforcement officers and civilians per 1,000 (see Table 2) as covariates to account for police influence on crime.

Table 3. Control Variables Descriptive Statistics

Variable	Total Mean (N = 52,275)	Treat Mean (N = 14,474)	Control Mean (N = 37,801)	T-Test of difference	Standardized difference
Population	112,609	170,395	90,269	0.000	0.224
Population Density	343.809	254.344	378.396	0.073	-0.021
Age 14-25 (%)	13.645	14.175	13.439	0.000	0.218
Male (%)	49.589	49.475	49.633	0.000	-0.093
Female (%)	50.411	50.525	50.367	0.000	0.093
White (%)	88.154	86.785	88.684	0.000	-0.144
Black (%)	8.682	9.202	8.480	0.000	0.058
Unemployment Rate (%)	6.149	6.044	6.190	0.000	-0.055
Republican Vote Proportion (%)	55.375	53.842	55.968	0.000	-0.159

Empirical Strategy

As described in Appendix B, this study utilizes static and dynamic fixed-effects specifications – both performed on a dataset shaped to address issues of treatment effect heterogeneity and omitted variable bias. This process involves creating separate sub-experiments ("stacks") for each year under review that includes counties implementing ADCs that year (treatment group), as well as those that do not for that year, five years prior, and five years after (control group). As the study period includes

1990 to 2018, 19 stacks were created (1995-2013) that account for pre- and post-implementation periods. Not only does this process minimize heterogeneity between treatment and control groups, but also balances the dataset in both calendar and relative time for each stack (Baltagi 2021).

While creating each of these stacks, I also matched each county from the treatment group with up to its four nearest neighbors from the control group relative to all control variables included in analysis. Gary King and Richard Nielsen (2019: 1) recently demonstrated propensity score matching "often accomplishes the opposite of its intended goal—thus increasing imbalance, inefficiency, model dependence, and bias." Therefore, the current study avoids this technique, instead using a more statistically sound data preprocessing method: nearest neighbor matching using a the Mahalanobis distance function (Abadie et al. 2004; Ho et al. 2007).

Static fixed-effects analysis (Equation (1) from Appendix B) begins the core analysis section below, followed by a dynamic model (Equation (4)). The former provides a tidy estimate of treatment effects but comes at the cost of temporal nuance, missing dynamic effects dependent on length of exposure and potentially biasing pre-implementation trends. Ergo, inclusion of a dynamic model. This method reports estimates for time bins relative to year of implementation (i.e., an event study).

Results
Core Analysis
Static Model

Table 4 reports estimates from a static model (β_1 from Equation (1) in Appendix B) of ADCs on all crimes, property and violent indexes, and their total, as well as percentage change ($\exp(\beta_1) - 1$). In all four general categories, ADCs are associated

with significant reductions, with all crimes showing a reduction of roughly 2.5%, the total and property indexes nearly 3%, and 3.6% for the violent index.

Table 4. Static Model Results

	All	Total Index	Property Index	Violent Index
ADC_{t-1}	-0.025***	-0.030***	-0.029***	-0.037**
	(0.007)	(0.006)	(0.007)	(0.012)
% Change	-2.46	-2.96	-2.86	-3.63
N	68730	68730	68730	68730
R2	0.925	0.924	0.918	0.848

Standard errors in parentheses
*p<0.05 **p<0.01 ***p<0.001

Dynamic Model

Figure 2 reports dynamic model results in the event study format for all crimes and crime index categories. The parallel trends assumption holds, providing confidence for estimates after implementation. Similar to static estimation, all four categories indicate an association between ADCs and crime reductions, with the largest proportion in the violent index. One advantage of this dynamic model comes from the ability to assess short- and long-run treatment effects. Event studies for all and total index crimes show not only continuing crime reductions but an improvement over time.

FIGURE 3
Dynamic impact of ADCs on All and Indexe Crimes
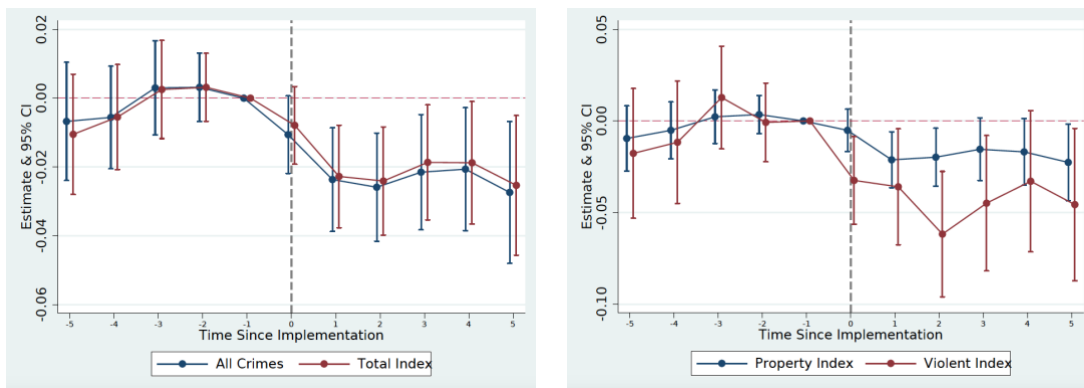
Notes: These graphs show point estimates, and 95% confidence intervals of the effect ADCs have on All Crimes, Total Index, Property Index, and Violent Index categories reporting the sum of main and interaction effects (coefficients $\delta_\tau + \alpha_\tau$ from Equation (4) in Appendix B).

Robustness

UCR data present measurement error issues which require exploration (Kaplan 2021e). Despite best efforts to optimize data quality and generalizability (see Appendix C), we cannot ignore the possibility of bias. As such, I ran sensitivity tests, on the all crimes category and all three indexes, by limiting crime data inclusion to increasing population cutoffs (Table A.1 in Appendix A). Though these results show a reduction in both effect size and statistical significance, estimates continue to be negative. Any measurement error associated with smaller reporting agencies may have some influence but not such that negates results.

Threats to Validity

Measuring outcomes at this unit of analysis requires considering threats to validity, including violation of parallel trends and statistical independence via

concurrent, confounding factors. First, and perhaps most importantly, the parallel trend assumption holds, as demonstrated in Figure 3. Next, a community capable of implementing an ADC might exhibit other changes with similar impacts on crime or that might distort the relationship between ADCs and crime. These community influences can be categorized as those within the criminal legal system and other local efforts (e.g., service provision). I examine law enforcement and court factors to assess the former, as well as public employee categories to operationalize the latter.

Law Enforcement

It seems plausible that communities with the wherewithal to create an ADC may also invest in other crime efforts, such as increasing law enforcement or improving efficiency. If this were the case, then we would expect to see increases in law enforcement personnel or clearances by arrest relative to ADC implementation. Toward this end, I estimated effects ADCs have on these variables, presented in Appendix A. As can be seen in Figure A.1, ADCs are associated with a small increase in civilian personnel (i.e., those without arresting powers) one year after implementation. The pattern of positive coefficients continues post-implementation without statistical significance. An even smaller negative association shows up four and five years before implementation for officers (those with arresting powers). While not definitive, these results do not provide evidence law enforcement personnel influence crime reductions associated with ADCs. Civilian personnel increase by approximately 6.2%. These employees average 0.86 per 1,000 population in this sample (Table 2), meaning they increase by about 0.05 per thousand. Officers average 2.01 per 1,000 and show just over 2% fewer prior to ADC implementation.

Clearances by arrest are shown in Figure A.2, with a small increase in property crime clearances the year of implementation and lower violent crime clearances two years prior. Both estimates appear as anomalies, though, not as part of a pattern. Thus, it does not appear law enforcement improved efficiency concurrently with ADC implementation. The lower clearance rate for violent crimes, in fact, would bias estimates toward zero, creating a countervailing force against ADCs reducing crime.[3]

Court Employees

To test whether other court efforts led to crime reductions, rather than ADCs, I estimated the impact ADCs have on court employees, using the Annual Survey of Public Employment and Payroll dataset's Justice and Legal category for full-time equivalent employees (Kaplan 2021a). The category covers all court actors paid through public funds – judges, administrators, prosecutors, etc. (US Census 2020). If this factor confounds the relationship between ADCs and crime, results will indicate increases in employees relative to ADC implementation. Figure A.3 indicates no effects – other court efforts do not likely confound current results. These data, though, come from a voluntary survey with imperfect response rates. So, analysis was performed on a subset of the data used for other analyses. The complete lack of any significant coefficients or pattern, though, make a reasonable case against any confounding influence.

Other Community Efforts

Though law enforcement and court factors do not seem to have a confounding influence, communities may be implementing other services that led to crime reduction. To estimate such a relationship, I analyzed other public employee categories. As with the Judicial and Legal category above, I included all full-time equivalent employees. If

communities that include ADCs coincide with higher levels of employees in these categories, then results might indicate communities with better resources for addressing crime see crime reductions, rather than ADCs alone being responsible. As Figure A.4 shows, some patterns emerge but nothing statistically significant. The Social Welfare and Housing and Community Development categories indicate more employees post-ADC implementation, and the Health category (in which out-patient and other SUD treatment services are included) is associated with lower levels. The Hospital category (including in-patient and more intensive treatment services) shows some positive coefficients prior to starting an ADC. While ambiguous, these results show no definitive evidence of other community efforts confounding results above. As stated, though, the ASPEP dataset does not perfectly align with the current analysis sample.

Counterfactual Analysis

I also performed counterfactual analysis by predicting outcomes from the dynamic model (Equation (4) from Appendix B) and subtracting the effects predicted from this model using Equation (5). This process allows assessing how well the model performed by comparing actual and predicted values. Further, subtracting counterfactual from predicted values provides both an approximation of change in crimes and another validity check.

Figure A.5 displays quantile-quantile plots of actual and predicted values for total, property, and violent indexes. The model appears to have performed well, as these values align appropriately – similar but not suspiciously exact.

Crimes prevented per year can be seen in Figure A.6. ADCs are associated with substantial decreases in number of crimes for all three indexes. Though they had a

larger percentage impact on violent crimes (Table 4 and Figure 3), property crimes number far more per annum. Thus, ADCs are associated with three times fewer property than violent crimes.

Discussion
Interpretation

These results show a significant, substantial, and lasting relationship between ADCs and crime reductions. Despite rigorous methods and a bevy of robustness checks, beneficial estimates shine through – somewhat surprisingly, given the relatively small footprint ADCs have within local legal systems. Using figures attained through the counterfactual analysis discussed above, I estimate 417,667 total index crimes prevented between 1995 and 2013. The study sample included a total of 5,492 ADC-years (sum of total ADCs each year). If we take the average number of participants provided in Marlowe et al. (2016) as 45, then a bit more than 247,000 people participated in ADCs over the study period. This makes each participant responsible for 1.7 fewer crimes. This, of course, assumes all participants graduate and would have committed more crimes (recidivated) – neither likely. Considering 50 to 75 percent graduation rates and 75 percent recidivism to stay on the higher, conservative side (Marlowe et al., 2016), we can see ADCs are associated with more crimes prevented than participants who graduate, and would have recidivated, would likely be responsible for (Table 5). Setting aside for now the idea that these results might be incorrect (either through faulty analysis or spurious correlation, both discussed below), the question of how ADCs might reduce crime at these levels bears discussion.

Table 5. Crimes Prevented per Participant

	Participants	Crimes/Participant
Crimes Prevented: *417,667*		
Participants	247,140	1.70
75% Graduation	247,140	2.25
50% Graduation	164,760	3.38
75% Grad/75% Rec	123,570	4.51
50% Grad/75% Rec	82,380	6.76

First, let us consider the treatment and control conditions. What does this study measure? Those facing criminal charges generally travel four paths, whether through plea negotiations or a trial: (1) no punishment (due to acquittal or dismissal), (2) incarceration, (3) community supervision, or (4) an alternative program like ADCs.

Incarceration incapacitates individuals from committing crimes (in the communities of interest) for the duration of imprisonment, so can be set aside. It stands to reason that acquittals, and other non-punishment outcomes, are evenly distributed (i.e., as good as random), so we can also set these aside. Considering the additional points that most ADCs deal with people who have committed first-time, low-level offenses,[4] and that RCT studies of ADCs generally randomize participants into the ADC or probation (treatment-as-usual) condition (e.g., Deschenes et al., 1995; Gottfredson & Exum, 2002), the primary counterfactual condition to ADC participation is some form of community supervision.

Community supervision ranges from unsupervised probation to house arrest. As mentioned in the background section above, many features of community supervision overlap with ADCs. Those on probation and ADC participants both submit to frequent

drug tests, meet with an authority regularly (probation officer or ADC judge), adhere to specific requirements like stable employment, must pay fines and fees (though ADC rates generally run higher), and are referred to services. ADCs, however, not only monitor service and general program adherence, but do so in a cooperative, collaborative environment (NADCP 2018a, b). Service providers serve on ADC teams with influence on policies and decisions (Drug Court Administrator A 2019; Rossman et al. 2011). Probation and other community supervision types feature no such arrangement.

In 2018 approximately 1,700 ADCs operated in the US, adding up to 76,500 participants (Marlowe et al., 2016; National Drug Court Resource Center, 2018). That same year, reports the Bureau of Justice Statistics, 3.5 million people were on probation (Kaeble 2020). Even assuming authorities double count ADC participants as on probation as well, general supervision far outnumbers ADC participation. Given that every jurisdiction includes probation as an outcome for criminal justice cases, the two conditions investigated in this study amount to counties with probation but without an ADC, and those featuring both probation and an ADC.

I propose, then, that the effects seen above represent crime reducing impacts of services within the criminal legal system espousing internal cooperation and external collaboration with cross-sector organizations. Over the past decade, practitioners and researchers have positioned public health and public safety as a crucial intersection for addressing either set of issues (see, e.g., Milgram et al. 2018). Connecting these two systems, even on the most fundamental level, has proven difficult (Pepler and Barber 2021). ADCs, however, provide more than a simple bridge, they represent integrated functionality. Service providers hold influential positions on ADC teams, influencing

decisions and policy, and ADCs influence service provision through increased demand and contractual requirements.

We can find guidance for such efforts in the Public Administration literature. Since the 1980s, most western governments have fundamentally changed how they operate, especially concerning service provision. Where traditional bureaucracies functioned from silos, with distinct boundaries between types of services and levels of government (see, generally, Kettl 2006), contemporary government service provision commonly occurs through a maze of formally and informally connected organizations; variously termed service networks, Collaborative Governance, Collaborative Governance Regimes, or Joined-Up Government (6 2004; Ansell and Gash 2007; Bingham and O'Leary 2008; Emerson and Nabatchi 2015; Frederickson 1999). As public needs have become more demanding – "wicked" problems deemed "complex, unpredictable, open ended, or intractable" (Head and Alford 2015: 712) – and public sentiment veered toward less direct government provision – decentralization, privatization, disarticulated government (Frederickson 1999; Marini 1971; Osborne and Gaebler 1992) – public service implementation evolved to meet public demand and sentiment.

ADCs fit squarely within this literature, making them the ideal case for studying Collaborative Governance. Thus, the current analysis presents evidence for positive performance of such arrangements within the criminal legal system. Though more research is necessary, Collaborative Governance elements found in ADCs (and possibly the communities that operate them) likely reduce crime effectively.

The effects these courts have on violent crime also provide interesting results. One important critique posits ADCs treat those who commit crimes as specialists when the bulk of criminological research indicates, rather, generalization and opportunism

(Pratt and Turanovic 2019). Results above show such criticism may be correct, though from a different perspective. Treating antecedents to committing crime prevents more than just drug or property crime, these efforts also reduce violent crime.

Policy Implications

As ADCs are associated with reductions in crime at the community level, to an extent that some knock-on effect seems to be at play, communities should consider the elements of this program likely driving such improvements (e.g., service orientation, bridging criminal legal and substance use services fields, cross-boundary collaborative networks) and expand them, though more work is needed to demonstrate precisely which causal channels these results flow through. More broadly, the cross-boundary connections ADCs exhibit can be exploited to not only improve outcomes, but to share risk and costs (Ulibarri 2019).

Limitations

Though the current study takes several steps toward robust analysis of ADC impacts on community-level crime, causal interference remains elusive. Quasi-experimental methodologies have improved substantially over the past decade, gaining Nobel Laureate fame recently (see The Committee for the Prize in Economic Sciences in Memory of Alfred Nobel 2021). Applying state-of-the-art econometrics gets us closer to inferring ADCs reduce community crime, to be sure, but more work is necessary to establish this connection definitively.

Crime data, whether from the UCR or other sources, present measurement error issues. There can be no guarantee that some error in this information has not biased results, especially those correlated with community capacities like ADC implementation.

As the more reliable National Incident Based Reporting System begins to provide longer periods of data for more jurisdictions, replication studies will be possible.

One other issue comes from the milieu of activity attempting to address the same issues ADCs work on. For instance, Jessica M. Eaglin (2016: 596) credits the "drug court paradigm" with broader sentencing reform, which would have non-random influence on estimates of ADCs' impact on community outcomes, especially crime. Though the models above explicitly and implicitly control for many such factors, and several potentially confounding elements were explored, more work is necessary to ensure the connection between ADCs and crime reduction was not made spuriously.

Conclusion and Future Work

This paper explored the extent to which adult drug treatment courts affect community-level crime, finding a negative correlation. Not only are these programs associated with reductions in the expected property crimes but show substantial impact on violent crimes as well. These results prove robust to several checks. Given a counterfactual condition of community supervision, the mechanisms likely to facilitate this impact come from the cooperative, collaborative elements found in ADCs but not other forms of supervision.

The service-oriented, collaborative arrangement ADCs represent will likely continue to offer benefits. Thus, policy recommendations include shifting priorities in criminal legal systems toward not only including service provision, but to do so in a way that integrates public health and service system actors as fully integrated, decisionmaking partners.

These results indicate a few paths for future research. Further investigation is necessary to define causal connection between ADCs and community crime. Another obvious question is cost effectiveness: Are these efforts a good investment? Since ADCs provide few services directly, doing so primarily through a collaborative network of service providers, it is also important to consider causal mechanisms linking ADCs to improvements in public safety. Do the services provided have their intended impact? Or does simply incarcerating fewer people reduce social disorganization (Kubrin and Weitzer 2003), thereby reducing crime?

The question of mechanisms becomes even more pertinent in light of the apparent knock-on effects discussed above – ADCs are associated with more crime reduction than participants are likely responsible for. Studying relative changes in service provision, economic activity, and other overlapping factors may provide insight into how ADC operations interact with other elements to have this magnified effect.

Finally, these programs seek to address underlying public health conditions, so it makes sense to investigate public health outcomes. Though a few studies have begun such a line of inquiry (e.g., Kearley et al. 2019), we know little about outcomes like overdose deaths and emergency department visits. Further, criminogenic factors provide a limited view of conditions, for individuals and communities. Contemporary theorists on public safety and criminal desistance recommend including more nuanced outcomes, like public health measures, toward more accurate and, importantly, useful research (Rocque 2021).

End Notes

[1] For purposes of this paper, hybrid drug/DWI courts are included as many evolved from traditional ADCs and continue to operate on similar premises, save the inclusion of DWI cases.

[2] Since I also scale outcome variables by population (rate per 100,000), I explored whether inclusion of population as a covariate introduces bias into the model, finding none.

[3] It should be noted that clearances by arrest present measurement difficulties, both in recording these events and by construction. In addition to the usual worries with UCR data, the number of offenses known to law enforcement does not indicate how many people are suspected of committing each crime. Thus, officers might arrest multiple people for a single incident, creating the possibility of clearance rates of 100%. As this potion of analysis only serves as a validity check, I simplify the issue by creating a ceiling of 100% clearance rate.

[4] Though these eligibility requirements have loosened up a bit over the past few years, allowing people with a longer history of crimes and/or more violent crimes, the vast majority of ADCs continue traditional practices (Marlowe et al., 2016).

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