Adult Drug Treatment Courts and Community-Level Drug Possession Arrests

Appendixes

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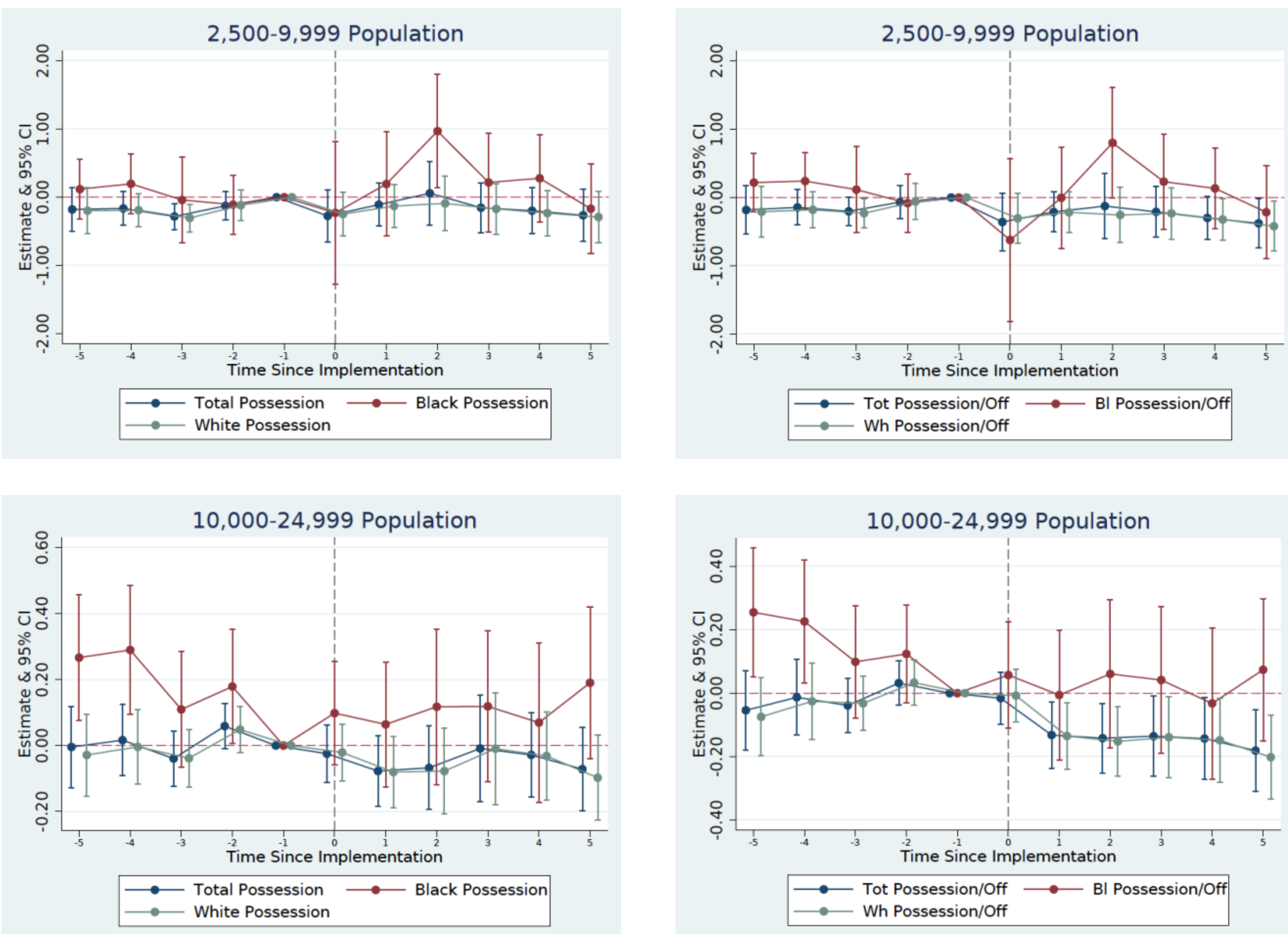
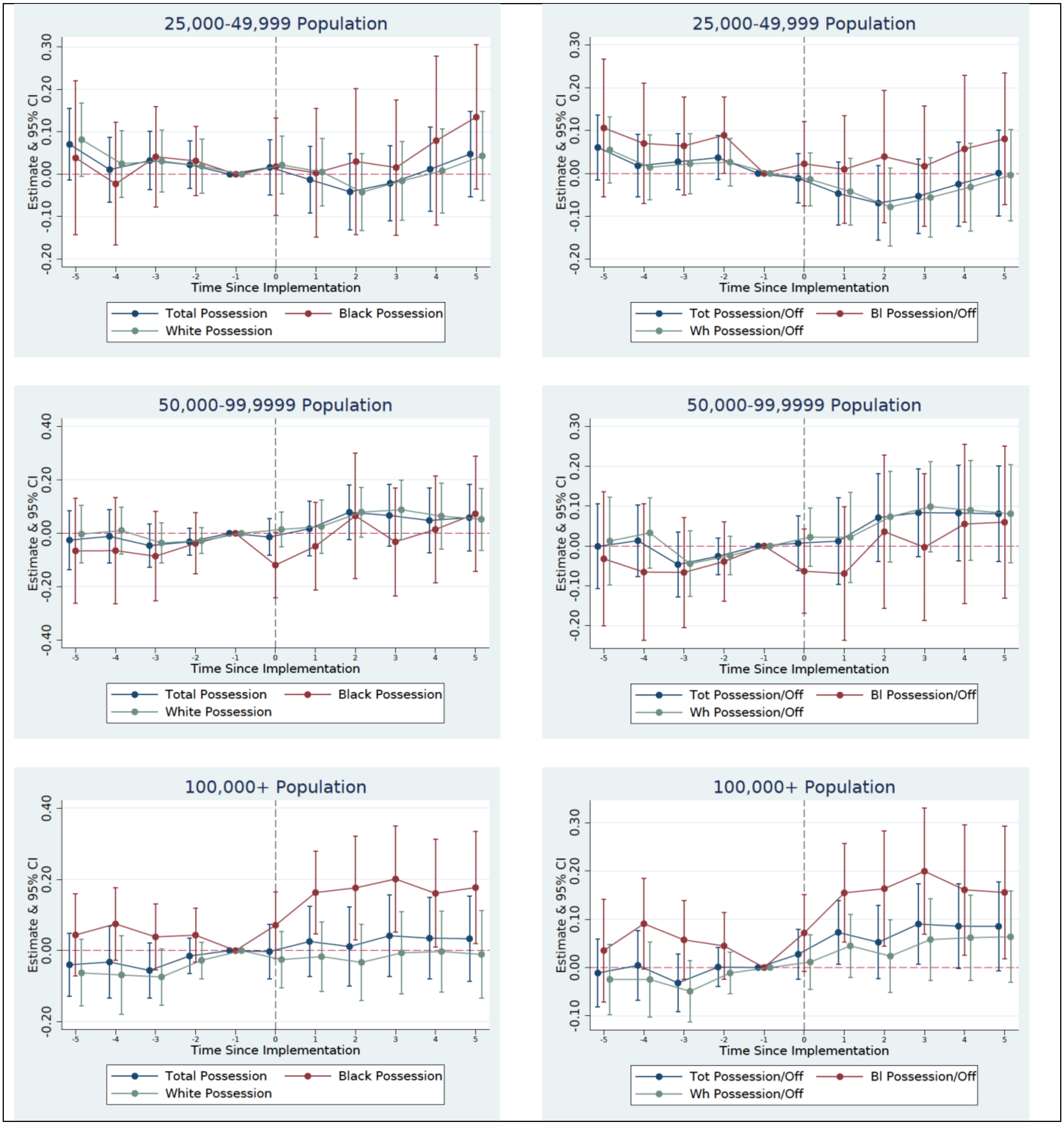
A. Additional Tables and Graphs

A.1 Event Studies

Figure A1 provides event study graphs reporting point estimates and 95% confidence intervals of the impact adult drug treatment courts have on drug possession arrests. These were reported in the main text as linear combinations of relative time bins 1 through 5. The only pre-implementation trends come from black arrests in the 10,000 to 24,999-population group.

Figure A1: Dynamic Specification Event Studies




25,000-49,999 Population
Estimate & 95% CI
Time Since Implementation
Total Possession
Black Possession
White Possession
25,000-49,999 Population
Estimate & 95% CI
Time Since Implementation
Tot Possession/Off
Bl Possession/Off
Wh Possession/Off
50,000-99,9999 Population
Estimate & 95% CI
Time Since Implementation
Total Possession
Black Possession
White Possession
50,000-99,9999 Population
Estimate & 95% CI
Time Since Implementation
Tot Possession/Off
Bl Possession/Off
Wh Possession/Off
100,000+ Population
Estimate & 95% CI
Time Since Implementation
Total Possession
Black Possession
White Possession
100,000+ Population
Estimate & 95% CI
Time Since Implementation
Tot Possession/Off
Bl Possession/Off
Wh Possession/Off

A.2 Model Predictions vs. Actual Rates

Figures A2 through A4 show quantile-quantile plots comparing predicted and observed arrest values. Though a few outliers exist, these values align well, though not so closely as to present over-specification.

Figure A2: 2,500-9,999 Population Group

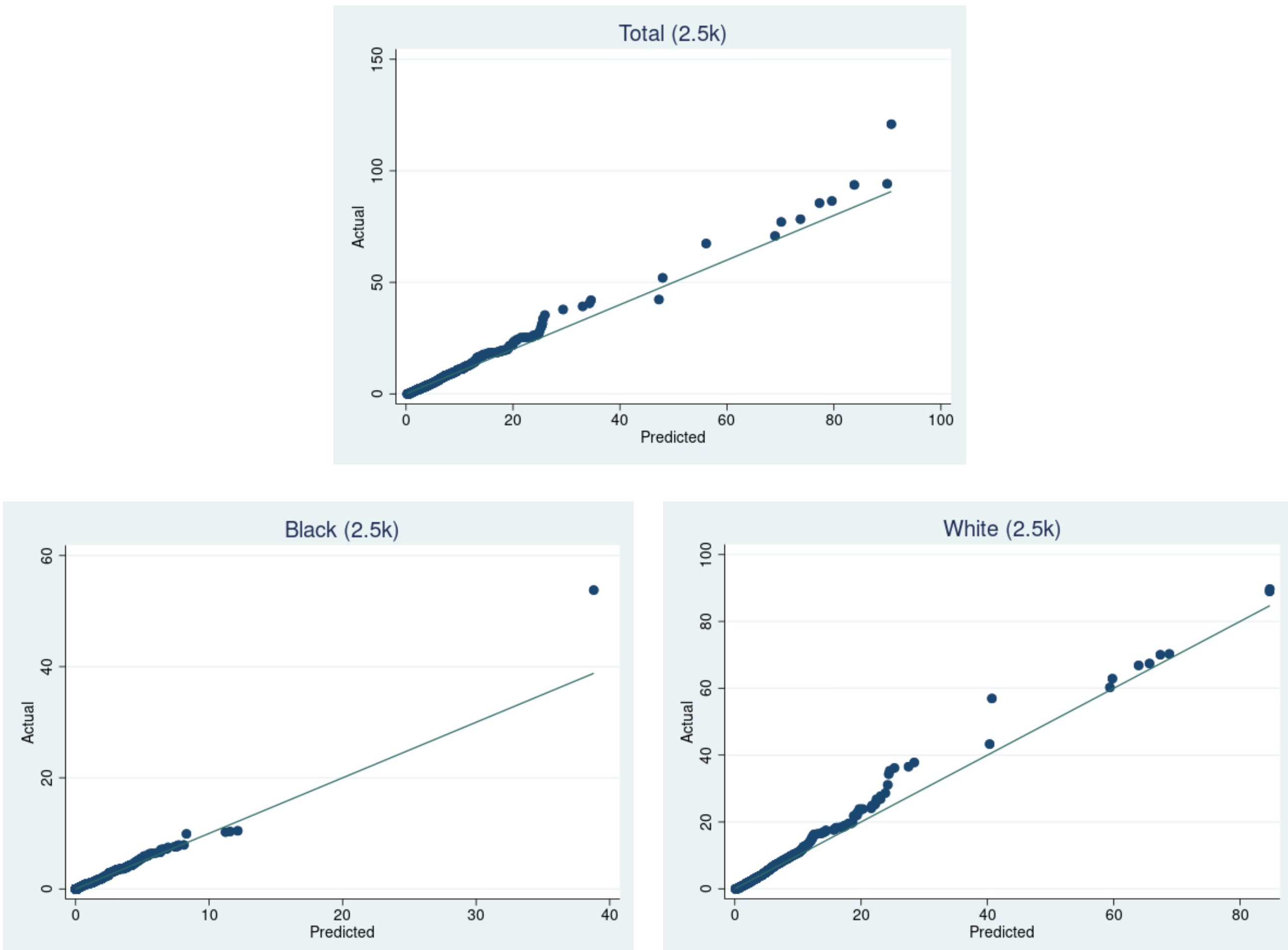


Figure A3: 10,000-24,999 Population Group

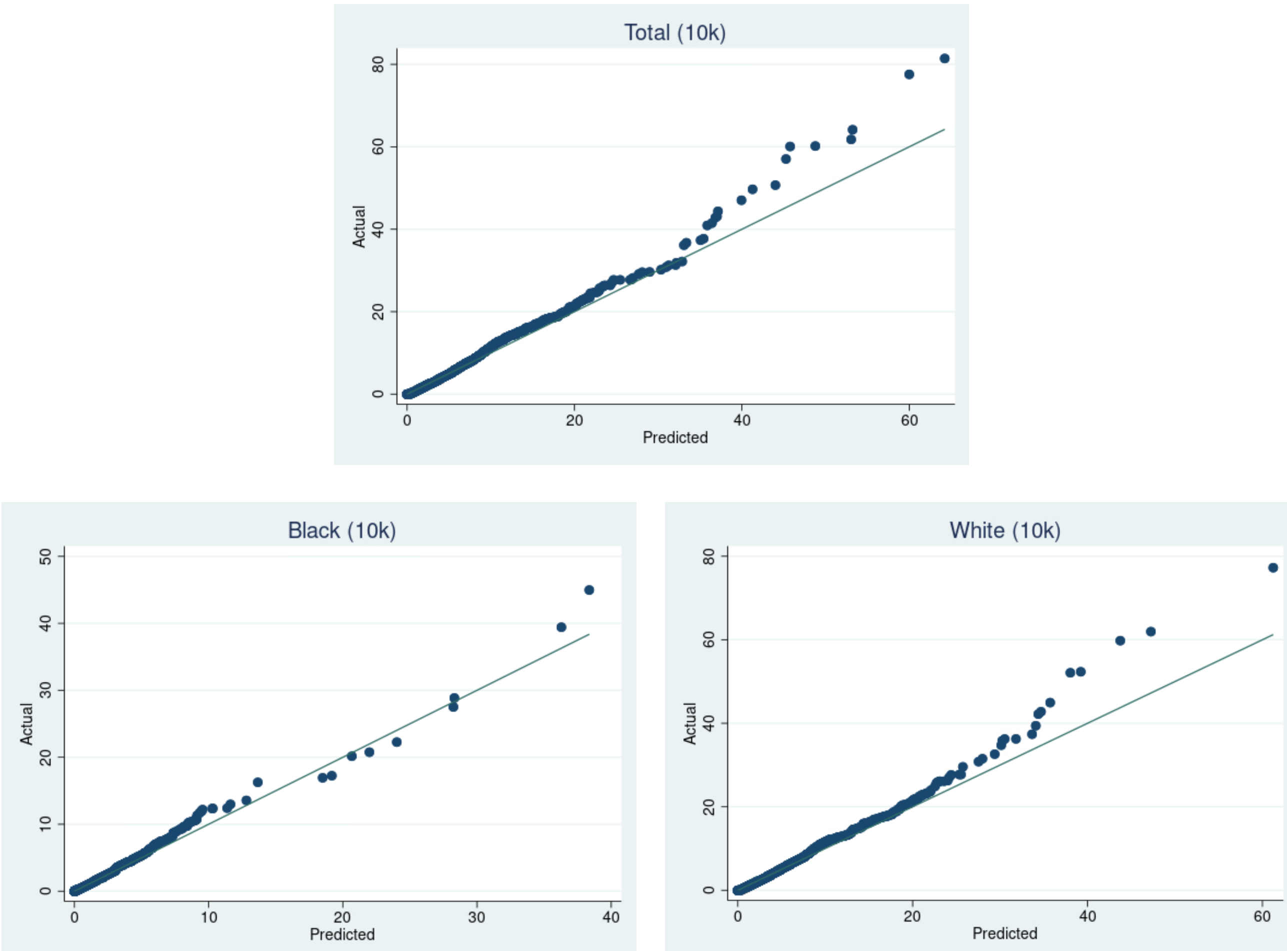
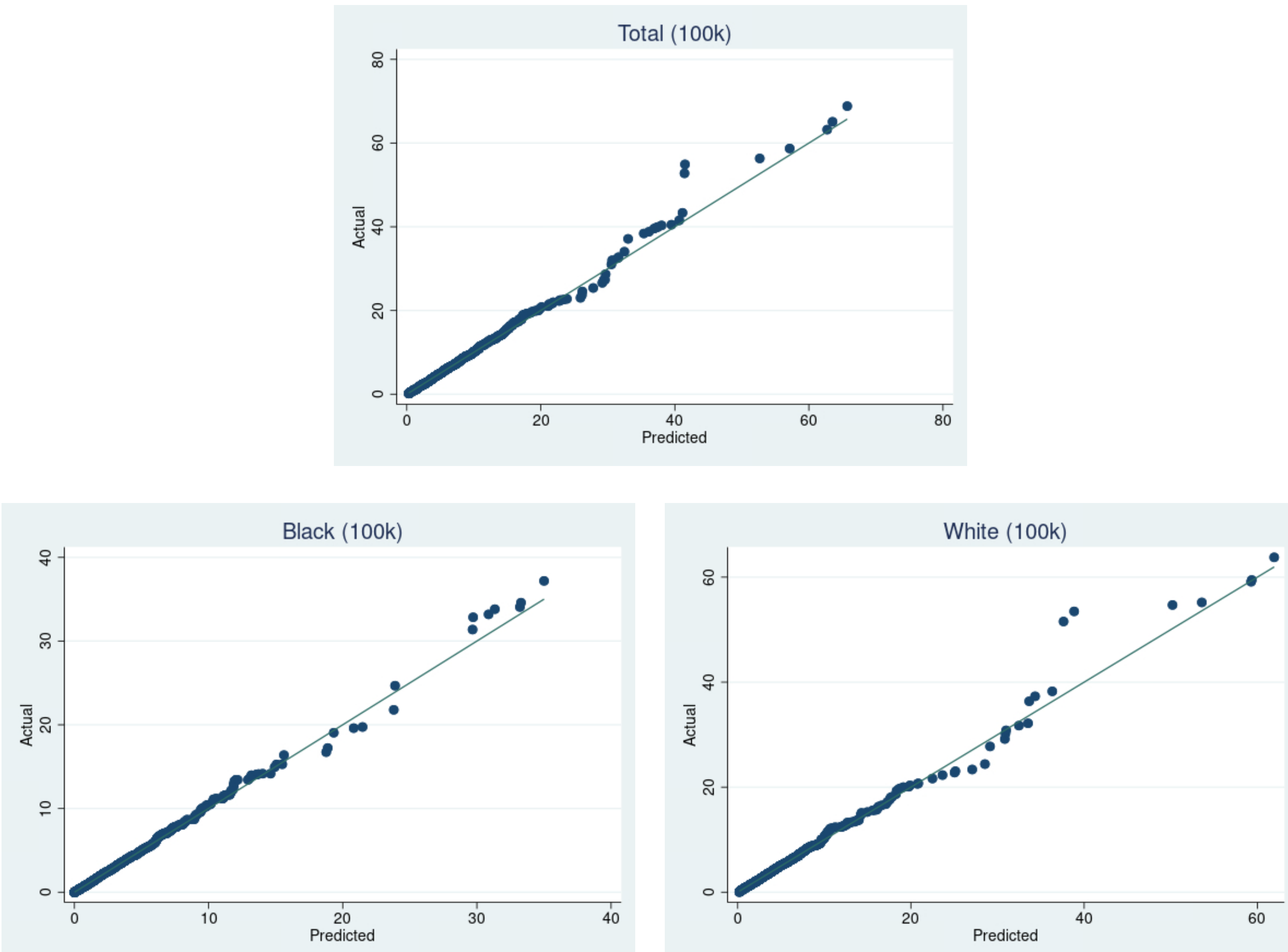


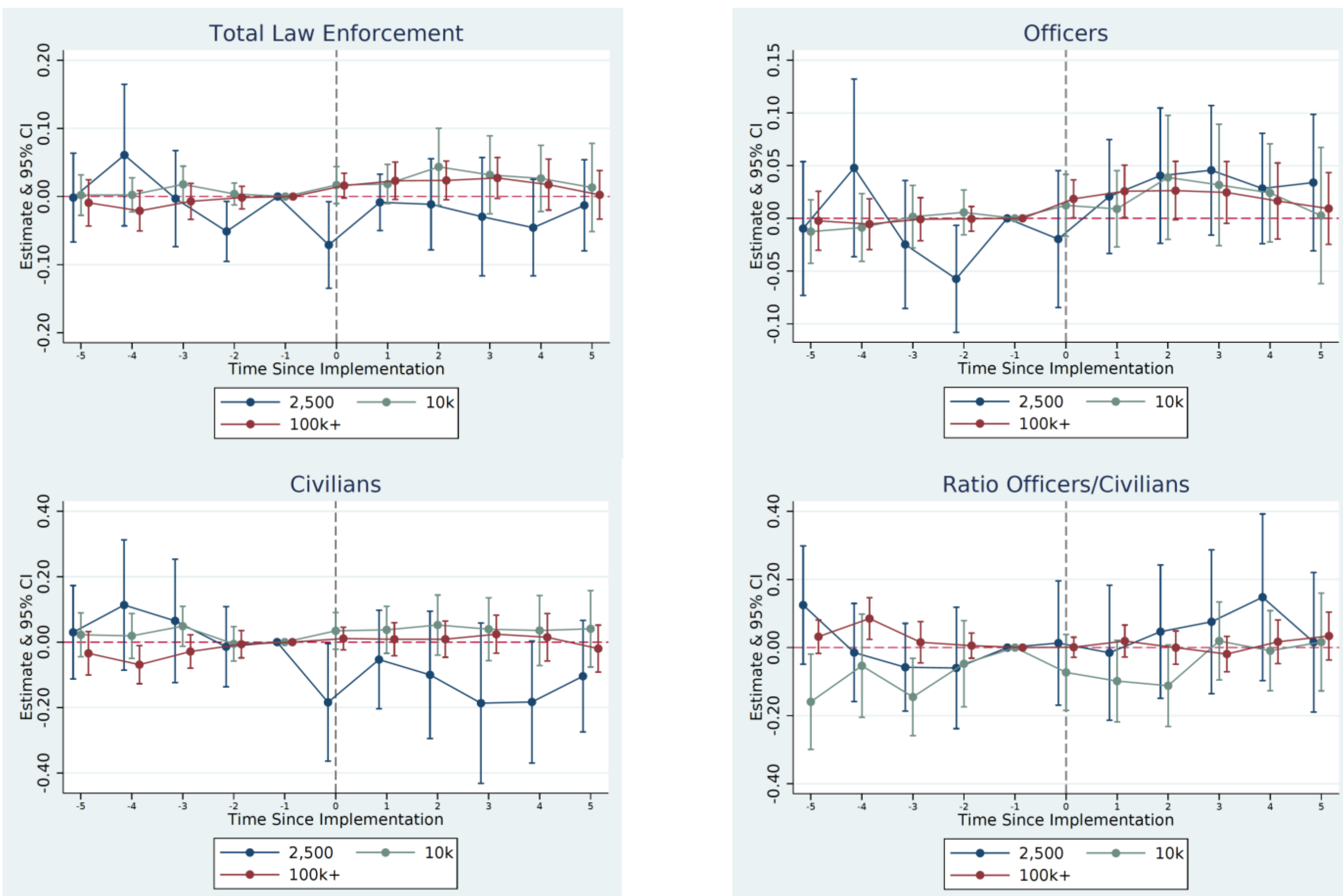
Figure A4: 100,000 and Above Population Group



A.3 ADCs and Law Enforcement Personnel

Figure A5 provides point estimates and 95% confidence intervals for the impact ADCs have on law enforcement personnel. In the 2.5k population group, ADCs are associated with fewer personnel in all three outcomes at or just before implementation. The 10k group shows no effects for personnel numbers but a tendency toward lower officer-to-civilian ratios prior to implementation. 100k estimates indicate a small association with fewer civilians four years prior to implementation, and a higher ratio in the same relative time bin. All significant coefficients come in less than 10%, save that for lower civilians in the 2.5k category. Given their small effect sizes and the inclusion of officers and civilians in the main specification, law enforcement personnel do not likely confound results.

Figure A5: Law Enforcement Personnel Event Studies



B. Methods

B.1 Core Analysis

To estimate the impact Adult Drug Treatment Courts (ADCs) have on drug possession arrests I applied static and dynamic fixed-effects models (event studies) on a dataset set up to address issues of heterogenous treatment effects (see below). A static fixed-effects model may be formally defined:

$$Y_{ct} = \beta_0 + \beta_1 ADC_{ct-1} + \beta_2 \boldsymbol{X}_{ct} + \omega_c + \varsigma_t + \kappa_{st} + \varepsilon_{ct} \tag{1}$$

Here, counties are indexed $c = 1 \dots C$, and calendar years as $t = 1990 \dots T$. Y_{ct} represents the outcome of interest for county c at time t, generally drug arrest categories in the current study. ADC_{ct-1} indicates if county c had an ADC at time $t-1$, following previous studies' design (e.g., Lilley, 2017), with β_1 the coefficient of interest. $\boldsymbol{X}_{ct}$ is a vector of control variables, and fixed-effects include counties (ω_c), calendar year (ς_t), and state-by-year (κ_{st}). These last implicitly control for county variant but time invariant, time variant but county invariant, and state-wide changes per calendar year.

This specification, though, provides limited information. Though static fixed-effects specifications address calendar time effects (see Dettmann et al., 2020), dynamic treatment effect likely occur in this context, in that it may depend on the length of exposure (Callaway & Sant'Anna, 2020). Contemporary research generally looks at a more dynamic model, such as an event study, that provides a nuanced view of estimates across relative time periods both before and after a program or policy is implemented (see Goodman-Bacon, 2019 for a discussion of how dynamic event studies may prove more robust). Thus, one can detect pre-implementation trends, which will bias estimates (Wing et al., 2018), and estimate effects post-implementation in a single elegant identification. Commonly, this process involves aligning observed time periods (year in the present study) into time bins relative to implementation. These bins differ from calendar time. Considering the current study, counties are arranged into annual time bins relative to ADC implementation. I denote the year of implementation as “0,” years prior as -5 to -2 (the -1 year is usually excluded as a referent), and years after 1 to 5. In this setup, and given the study period, an ADC implemented in California in 1995 will enter the data in the -5 bin at year 1990, -4 1991, and so on. One started in Indiana in 2013 will enter -5 at 2008, -4 at 2009, etc. We can formally define this identification strategy as

$$Y_{ct} = \sum_{\tau=-K,\tau\neq-1}^{T} \sigma_\tau D_{ct}^{\tau} + \sum_{\tau=-K,\tau\neq-1}^{T} \pi_\tau (ADC_c \times D_{ct}^{\tau}) + \omega_c + \varsigma_t + \kappa_{st} + \varepsilon_{ct} \tag{2}$$

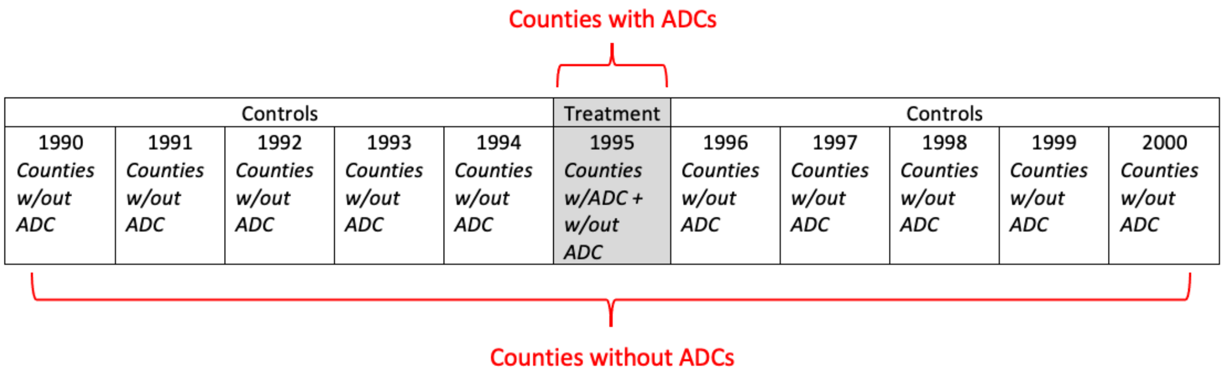
As described above, relative years include five to two years before implementation, the year of implementation, and the five proceeding years: $\tau = -K, \tau \neq -1$. Y_{ct} represents a drug arrest outcome for county *c* in year *t*. D_{ct}^{τ} is an indicator variable for each relative year bin, ADC_c indicates if a given county ever implemented an ADC, and the presence of an ADC in a relative time bin indicated by the interaction of these two, $(ADC_c \times D_{ct}^{\tau})$. ω_c, ς_t, and κ_{st} represent county, year, and state-by-year fixed effects, respectively.

This model, though, requires certain assumption are met. The most pertinent to this study is homogeneous treatment effects. If heterogenous effects exist, they bias estimates and cause relative time bins to influence each other (Sun & Abraham, 2020). Taking the same two courts as an example above – one from 1995 California and another 2013 Indiana – it is not likely that the variety of courts, locations, and times exhibit homogenous treatment effects.

To address this issue, I employ a stacked event study design, similar to those used in Cengiz et al. (2019) and Deshpande & Li (2019). This design culminates in an event study similar to that presented above but sets up “sub-experiments” (here termed “stacks”) for each year in a study period. Each stack includes counties which implemented that year, and those without a TC that year, five years prior, and five years after, as illustrated in Figure 1.

FIGURE B.1

Individual "Stack"



To create the full dataset, I stacked all these together, similar to the representation in Figure 2, which includes relative time bins below. The present study period, from 1990 to 2018, allowed for 19 stacks, considering the necessity of five years before and after each stack's implementation bin.

FIGURE B.2

Appended Stacks

Controls					Treatment	Controls				
1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001
1992	1993	1994	1995	1996	1997	1998	1999	2001	2001	2002
…	…	…	…	…	…	…	…	…	…	…
2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018
-5	-4	-3	-2	-1	0	1	2	3	4	5

Relative years – time to/from implementation

The formal definition for a full stacked event study is

$$Y_{cth} = \sum_{\tau=-5,\tau\neq-1}^{5} \delta_\tau I_{cth}^{\tau} + \sum_{\tau=-5,\tau\neq-1}^{5} \alpha_\tau (ADC_{ch} \times I_{cth}^{\tau}) + \mu_{ch} + \rho_t + u_{st}. \qquad (3)$$

Like Equation (2), relative time periods are included from -5 to 5, excluding -1 as referent. Here, I_{cth}^{τ} represents relative time bin τ for county c in calendar year t within stack h. The presence of an ADC continues to be indicated using the same binary variable. Fixed effects are similar but now indicate county-by-stack μ_{ch} (to account for specific within-stack county variation), calendar year ρ_t, and state-by-year u_{st}. The coefficients of interest are δ_τ and α_τ, which respectively represent average relative bin main and average interaction effects – making the sum of these two $\delta_\tau + \alpha_\tau$ the term of interest.

To make Equation (3) more tractable, I take a linear combination sum of bins 1-5 to present in the main paper. Full event study graphs, however, are presented in Appendix A.

B.2 Counterfactual Analysis

To further explore this question of ADC effects on community-level possession arrests, I performed counterfactual analysis on all arrest categories. This process subtracts the policy effect from predicted values to construct the hypothetical case in which ADCs never existed. This can be formally defined as:

$$\hat{y}^{rate}_{counter} = \hat{y}^{rate}_{predicted} - \sum_{\tau=-K,\tau\neq-1}^{T} \hat{\alpha}_{\tau}(PSC_{ch} \times I^{\tau}_{cth}) \tag{4}$$

Since the core analysis used Poisson transformed outcome values, I took the exponent of predicted values, then reverted them back to counts (from rates per 1,000) by reversing the process used to convert them for analysis: multiplying by population and dividing by 1,000. The construction of this analysis sample necessitates limiting such assessment to 1995 through 2013.

This supplementary procedure provides a few benefits. First, it allows a useful approximation of the number of either additional or fewer arrests. Second, it provides something of a validity check. If predicted values used in this process do not resemble the underlying data structure, or if returned counterfactual values appear too far off from predicted, then the model may be missing something.

C. Data

To evaluate the impact adult drug treatment courts (ADCs) have on drug arrests, I use a unique dataset expanding on information previously available publicly through the National Drug Court Resource Center, arrest data from the Uniform Crime Reporting Program, and relevant control variables.

C.1 Outcomes

I use arrest data from the FBI's Uniform Crime Reporting (UCR) summary reporting program, which Jacob Kaplan concatenated into annual files (Kaplan, 2021b). UCR data come with worries about measurement error (see Boylan, 2018 and Loftin & McDowall, 2010). Law enforcement agencies report voluntarily, limiting incentives for accurate and consistent reporting. To deal with these issues, I limited data to those agencies reporting all 12 months per year for the entire study period and cover at least the population group listed in analyses at any point. I also keep only agencies that enter the dataset for all years (1990-2018), creating a fully balanced sample in calendar years.

UCR data come from individual agencies, necessitating some type of aggregation strategy to match these with ADC data, which represents counties (see below). Since the sample selection process returns a balanced dataset, this process becomes straightforward: collapsing to the county-year level. Technically, this makes the unit-of-analysis a "super-agency" – larger than an agency but smaller than a county – but I use the term county for simplicity. In keeping with this strategy, any agency covering jurisdictions that cross or are outside of a county (e.g., campus police, state police) are excluded.

I scale possession arrests in two ways: per 1,000 population and per 1,000 law enforcement officers (see Appendix B).

C.2 Independent Variable

I built upon data previously offered publicly by the National Drug Court Resource Center (National Drug Court Resource Center, 2018). Their data presented information gained from a survey of court administrators throughout the US, which included name and type of court, primary county of jurisdiction, and some implementation dates. I expanded this to include all counties each court covers and added more implementation dates. This process included gleaning information from public websites, personal contact with court administrators, and communications with the National Association of Drug Court Professionals.

Hybrid DWI/Drug Treatment Courts evolved from ADCs, and perform nearly identical functions, so are collapsed into the independent variable. Throughout this study I refer to these only as ADCs. Since the dataset does not include all implementation dates, I exclude any county indicating such a court but no implementation date. To conform to the empirical strategy, the primary independent variable used for analysis indicates if any TC operates in a given county-year or not. This misses some information, such as whether additional courts offer more or less impact, or if some courts shut down, but fits the most robust analytical techniques with the given information.

A note is also necessary regarding jurisdiction. Some urban counties contain several ADCs (and other types of treatment courts – Los Angeles County operates nearly 50 in total), while some rural courts cover several counties. The model presented below includes a binary covariate indicating whether a court covers multiple counties to address this.

C.3 Covariates and Other Variables

Though my empirical strategy (See Appendix B) implicitly controls for many temporal and geographical factors that might influence the relationship between ADCs and arrests, I also include control variables along these lines. For instance, though analysis includes arrest rates per population, I include log-transformed population as a covariate in all specifications. I use population density[1] as a proxy measure of access to services (Allard, 2004). Further, I include covariates commonly associated with crime rates and likely to influence the effect ADCs have: percentages of those aged 15-24, male, and white (NIH | SEER, 2021); and unemployment rates (Bureau of Labor Statistics, 2021). As a measure of local construction of issues like crime and substance use, I include a temporal-proximity weighted measure of the percentage of Republican presidential votes (Stavick & Ross, 2020). Analyses also include data from the Law Enforcement Officers Assaulted or Killed dataset (Kaplan, 2021a), which are converted to an officers per 1,000 population rate.

C.4 Data Statement

The outcome and control variable data used in this study came from publicly available sources, so can be accessed easily. Data used for analysis (with county FIPS codes masked) and Stata code are available at *[masked for review]* for replication.

[1] $Population_{ct}/CountySquareMiles_{ct}^2$