Drug Money, Police Incentives, and Crime

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August 8, 2026

Abstract

Do drug-money seizures influence police behavior? In this paper, I exploit variation from the 2015 suspension of payments under the Equitable Sharing program, a federal asset forfeiture initiative that sent monetary kickbacks to local law enforcement for policing drug crimes. Using a difference-in-differences design, I find that participating local agencies cut drug-related arrests in response to the suspension of adoptions payments by close to 5%. The decline scales with pre-suspension participation and is strongest for possession offenses and municipal police departments. Despite the decline in drug arrests, I do not find evidence that violent or property crime increased in participating jurisdictions after the suspension. Taken together, my results suggest that civil forfeiture revenue meaningfully expands drug enforcement effort, but with little identifiable impact on overall crime rates.

Keywords: civil asset forfeiture, policing, crime, drug trafficking, rent-seeking, revenue generation, labor economics, public economics

JEL Codes: H76; K42; H26; H11; D73; I18; D72; H70

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†Acknowledgments: I would like to thank Amanda Agan, Douglas Miller, and Elio Nemier-David for their insightful feedback on this paper. This work also greatly benefited from discussions with Max Kapustin and Isaac Swensen. I would also like to thank Evan Riehl and the other participants at Cornell's Labor Works in Progress Seminar (LWIPS) for providing valuable feedback while this work was still in the early stages. Finally, I thank Levon Barseghyan, Francesca Molinari, and Lars Vilhuber for their work running the Third Year Research Seminar. This work is an adaptation of my masters' thesis.

1 Introduction

In the United States, law enforcement has the legal authority to seize and retain property deemed to be involved in illegal activity without a criminal conviction. This practice is called civil asset forfeiture, and it is employed by law enforcement agencies at every level of government – local, state, and federal. Forfeited assets, which often take the form of cash, cars, boats, and houses, usually become the legal property of the seizing agency. Opponents have long argued that the practice of civil asset forfeiture creates a revenue incentive that pushes law enforcement toward corruption and over-policing high-yield crimes (e.g. Sheth 2016; Knepper et al. 2020; Alban and James 2023), which conflicts with the normative standard of police as impartial enforcers of the law acting solely in the interest of the public.[1] Law enforcement has historically denied the existence of a revenue incentive tied to forfeiture, instead arguing that the funds acquired are integral to public safety and reduce black market capital that criminals rely upon (Worrall 2001). Despite these conflicting claims, little research to date has documented the extent to which civil asset forfeiture revenue shapes police incentives and behavior.

This paper studies the incentive effects of civil asset forfeiture by examining the 2015 suspension of federal adoptions through the Department of Justice's Equitable Sharing program. Under Equitable Sharing, federal agencies may choose to "adopt" assets that have been seized by local law enforcement when the asset is allegedly employed in the violation of federal law. The process is relatively straightforward: after the property is seized by local law enforcement, it is relinquished to an adopting federal agency within the Department of Justice. Police use the adoptions route most commonly for assets implicated in a suspected violation of federal drug law. Consequently, the Drug Enforcement Administration (DEA) operates as the adopting agency in nearly every case of federal forfeiture adoptions (see Figure 2). After the value of the asset is appraised, the local police agency receives 80% of any forfeiture revenue resulting from the seizure, while the federal agency receives a 20% cut.

In January of 2015, then Attorney General Eric Holder suspended the practice of adoptions due to mounting political pressure, which abruptly reduced the ability of local agencies to retain forfeiture proceeds through Equitable Sharing (Office of the Attorney General 2015). Why might we expect local agencies to respond to this suspension? First, prior literature has found that Equitable Sharing may provide a loophole that allows police to skirt state-level forfeiture statutes (Holcomb et al. 2011, 2018). Second, the legal burden of proof required to seize assets that are eventually adopted by federal agencies is lower than it is in many states (Knepper et al. 2020). Finally, the Department of Justice takes on fixed costs of appraising and liquidating the value of the adopted assets, which can be especially helpful for smaller agencies with limited resources.

My research answers the following question: how did the 2015 suspension of adoptions under the Equitable Sharing program impact the drug- and non-drug-related arrest rates of participating law

[1] This sentiment likely traces its roots back to Sir Robert Peel's *Policing Principles*, penned in 1829 (LEAP 2025). Peel is deemed by many as the "Father of Modern Policing," and a central theme across his 9 principles is that police should have no aims or goals that are contrary to the general welfare.

enforcement agencies? Because adoptions are so often used in the policing of drug crime, I model this suspension as a shock to the profitability of conducting drug-related arrests. Intuition provided by Benson et al. (1995) and Mast et al. (2000) predicts that a negative shock to the profitability of the revenue-generating task (such as policing drug crime) will *decrease* resources allocated to the revenue-generating task, but the effect on non-revenue-generating tasks is *ambiguous*, and is a function of the elasticity of substitution across tasks. As such, we should expect a strong, negative effect of the suspension on drug arrest rates and an ambiguous effect on non-drug arrest rates. In the Supplemental Appendix, I provide a simple model that formalizes these intuitions.

To estimate causal effects, I use administrative data from the Department of Justice (DoJ) and FBI to compare the drug and non-drug arrest bundles of agencies which participated frequently in the adoptions program during the pre-suspension period to agencies which did not participate frequently. Exploiting variation in pre-suspension participation in adoptions, I use a difference-in-differences framework to show that agencies with higher reliance on adoption funds in the pre-period substantially reduced drug-related arrest rates immediately following the suspension relative to non-participating agencies. Consistent with my theoretical predictions, pooled difference-in-differences results indicate that the participating agencies experienced a 5% negative deviation in drug-related arrest rates relative to baseline average. I observe qualitatively similar results using event studies and a synthetic DiD design. I do not observe any impact on nondrug arrests. Effects on drug arrests are mostly driven by a decline in possession arrests, and they are much stronger for local police departments rather than sheriff offices. I do not observe robust evidence of a concurrent increases in crime within participating police jurisdictions. Taken together, these results suggest that policing practice is sensitive to changes in the incentives posed by civil asset forfeiture, but but these changes in policing practice may not impact public safety.

Literature evaluating the strength of the revenue incentive posed by forfeiture has been limited, often focusing on the tradeoffs between various means of engaging in revenue-generating activity (Holcomb et al. 2011; Holcomb et al. 2018; Billy 2020). Data limitations and the lack of universal reporting requirements on the extent to which police engage in civil asset forfeiture also make studying these effects difficult. Kelly and Kole (2016) finds a very muted effect of forfeiture receipts on drug arrests and clearances using panel data of very large agencies and lagged forfeiture receipts. This aligns with the conclusion in Gius (2018), which finds a near-null correlation between the value of state-level forfeiture receipts and drug arrests using a random effects design. However, these papers lack plausibly exogenous variation in asset forfeiture receipts, which likely biases their results. More relevant for my study, Kantor et al. (2021) studies the roll-out of the Comprehensive Criminal Control (CCC) Act of 1984, which instituted the Equitable Sharing program. The authors show that police in states with more restrictive forfeiture laws saw crime fall and drug arrests rise relative to police in states with less strict forfeiture laws. I build on this result by providing a panel with much more power (around 12 times the number of observations in Kantor et al.), and by generating reverse-causal evidence that police are responsive to *negative* shocks to forfeiture receipts. I am also able to exploit intensive-margin variation to show that more participation in

Equitable Sharing leads to more sensitivity to shocks.

Most similar to my design is Billy (2020), which uses a large hand-collected dataset on county-level forfeiture receipts and variation in state forfeiture laws to study whether the 2015 suspension of adoption revenue caused substitution into other revenue-generating activity (measured by local forfeiture and traffic tickets). The focus on state forfeiture law is qualitatively similar to the design in Kantor et al. (2021). However, Billy finds null results, and attributes it to a combination of the poor quality of the local asset forfeiture data and the vague language of state laws restricting the practice of forfeiture. My study sheds new light on the effects of the adoption suspension by instead exploiting pre-suspension participation in adoptions as "treatment." I argue that this provides a better proxy for overall reliance on asset forfeiture revenue, and thus responsiveness to negative shocks. My agency-year data panel also allows me to account for fixed unobserved heterogeneity across different agencies within the same county. Finally, instead of focusing on alternative revenue-generating behavior (which is more second-order), I study whether the suspension directly impacted police agencies' observed policing of the revenue-generating task by focusing my analysis on drug-related arrests.

Section 2 covers institutional details necessary for understanding both civil asset forfeiture and the Equitable Sharing program. Section 3 describes the datasets and particular sample used to conduct the statistical analysis described in Section 4. Section 5 presents baseline results, checks to robustness, and subsample analysis. Concluding remarks are given in Section 6.

2 Background and Identification Strategy

2.1 Civil Asset Forfeiture

Asset forfeiture is a practice whereby law enforcement confiscates property (e.g., cash, vehicles, weapons, houses, boats) that is deemed to be involved with illegal activity. Asset forfeiture can be conducted by local, state, and federal law enforcement agencies. In most jurisdictions, if the property owner cannot demonstrate in court that the property was not used in illegal activity, law enforcement gets to keep the asset. Non-liquid assets are usually auctioned off to the public, or repurposed for law enforcement use. In this sense, asset forfeiture may pose a financial incentive for the seizing agency.

Legally, asset forfeiture in the US is delineated between civil asset forfeiture and criminal asset forfeiture. Civil forfeiture is controversial because it is considered *in rem* ("against a thing") proceedings, meaning that the forfeiture action is considered not against a person, but the seized object. The consequence of this designation is that the property owner need not be implicated in the illegal act with which the property is associated. As such, no arrest, charge, or conviction need be made in order to engage in civil asset forfeiture. This substantially lowers the legal burden of proof required in most courts for law enforcement to engage in the practice. Because it requires no judge-approved proof of owner involvement, law enforcement entities generally need only "reasonable suspicion" of the seized property's connection to a crime—a much lower legal burden than the “beyond a reasonable doubt” that is required in criminal forfeiture, which requires a criminal conviction. Criminal forfeiture is rarer and is not the focus of this analysis, so unless otherwise noted, "asset forfeiture" refers to the practice of civil asset forfeiture in this paper.

Some states have passed laws regulating the practice of civil asset forfeiture. State laws regulate the practice of asset forfeiture in three primary ways: restrictions on *de jure* (“by right”) financial incentives, innocent owner burdens, and burdens of proof in criminal courts. The last two are judicial and governed primarily by case law, which varies by state. Legislators can directly control the incentive systems faced by law enforcement by restricting the use of monetary returns that the enforcement agency gets to keep by right. Most either have no *de jure* incentive regulations, or if they do, local agencies are often allowed to retain most of the value of the seized assets.[2] Work by Billy (2020) indicates that these laws may be either unenforced or too vague to be effective.

Several empirical studies have documented negative effects of asset forfeiture. For example, Operti (2018) uses Italian province data to show that more criminal asset confiscation led to a significant decrease in the number of legally legitimate entrepreneurial entities within a region. The author posits that police confiscation creates capital-inhibiting spillovers which reduce the number of new businesses that are founded. Additionally, Makowsky et al. (2019) finds that in US states with low forfeiture regulation, higher budget deficits are linked to more intense policing of minority communities. The authors argue that this is possible evidence of systemic racism in policing institutions. When law enforcement agencies are strapped for cash, there may be increased likelihood that Black and Latino communities will be over-policed to compensate.

2.2 Equitable Sharing

This paper focuses on revenue from local forfeitures done under a program called Equitable Sharing. Federally instituted in the Comprehensive Crime Control Act of 1983, the Equitable Sharing program establishes collaboration procedures for forfeiture revenue to be shared between federal agencies and local agencies. After a forfeiture occurs that qualifies under Equitable Sharing, a local agency may request their shared proportion of forfeiture funds by submitting a DAG-71 form to the Department of Justice.

Various federal agencies participate in the DOJ’s Asset Forfeiture Program. The primary agencies that engage in enforcement activities that could result in forfeited funds are the Drug Enforcement Administration (DEA), the Federal Bureau of Investigation (FBI), and the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF). The types of assets these agencies often seize can be easily predicted from their official names. The ATF is tasked with enforcing laws against the illegal manufacturing and trafficking of arms, explosives, and tobacco products. As such, the ATF often recovers caches of illegal contraband made up of guns, bombs, makeshift explosives, and other firearms. The DEA, on the other hand, enforces US laws against the trafficking of illegal drugs,

[2] Recently, a few states (such as Maine and Indiana) have disallowed law enforcement from retaining any of the monetary return from asset forfeiture. Agencies are required to deposit money into educational funds or relinquish it to the state.

which means they often recover drug-manufacturing equipment, large sums of cash, and other drug-related contraband. The FBI enforces a wider range of laws, and their seizures may result in cars, houses, stocks, bank accounts, and other types of property that have been acquired illegally. Other agencies are involved with the prosecution of asset forfeiture cases and the establishment of asset forfeiture proceedings. These include the Money Laundering and Asset Recovery Section (MLARS), the Asset Forfeiture Management Staff (AFMS), and the US Marshals Service (USMS). The US Marshals Service is exclusively tasked with the evaluation of market value and selling of forfeited, non-cash capital assets.

Two procedures for revenue sharing exist under the Equitable Sharing program: adoptions and joint investigations. Adoption occurs when a local or state agency completes a property seizure in a case where federal law is suspected to have been violated. The local agency then has the option to request that the forfeiture be adopted by federal law enforcement, turning over the seized assets. In all cases of adoption, the federal agency retains 20% of the forfeiture proceeds, while the local agency that initiated the forfeiture keeps 80%. Local agencies can also participate in Equitable Sharing through joint operations, which establish task forces that both local and federal agencies contribute resources to. Joint task forces usually engage in "drug busts," or concerted efforts to take down large cartels. Local agencies receive funds commensurate with the proportion of resources expended to assist with the investigation, often measured in work-hours contributed. However, in all cases of joint operations, the federal government keeps no less than 20% of the total funds recovered. Importantly, the legal burden of administrative forfeiture applies whenever the federal government is involved with a forfeiture case. This means that state and local law enforcement agencies cooperating with the federal government often abide by less stringent standards when participating in Equitable Sharing.

There are several reasons why local agencies opt for the adoptions route rather than seize property under state law. First, Equitable Sharing provides a loophole that allows police to skirt state-level forfeiture statutes. States that regulate asset forfeiture by restricting the *de jure* monetary incentives for assets seized under state law, for example, usually ignore the Equitable Sharing option entirely. Prior literature has found that law enforcement in high-regulation states are more likely to use the Equitable Sharing option (Holcomb et al. 2011, 2018). Second, the vast majority of forfeitures conducted by federal agencies are considered administrative (Knepper et al., 2020).[3] Even fewer legal protections for property owners exist under administrative forfeiture than under civil forfeiture in most states. In administrative forfeiture, a federal agency (or a local agency acting as a proxy) does not need to file a complaint with a court giving reasons for the forfeiture in question. The agency merely sends a notice to the property owner(s) informing them of the government's intent to forfeit the property, as well as the timeframe within which they can file a petition in court. This window can be as short as 20 days, depending on the circumstances. However, even if a petition is filed by the property owner, government attorneys retain the final say

[3]Despite the different name, administrative forfeiture is analogous to civil forfeiture because the property owner need not be convicted of a crime in order for federal agencies to seize assets.

in whether to accept or reject the proposal. The Institute for Justice points out that between 1997 and 2015, at least one-third of claims by property owners claiming innocence were rejected, most for technical reasons (Knepper et al., 2020). Finally, agencies that seize assets that are later adopted by federal agencies are not responsible for the costs of appraising and selling the seized asset, which could be substantial, especially for smaller agencies that lack access to national markets.

2.2.1 Suspension of Adoptions in 2015

On January 16, 2015, Attorney General Eric H. Holder, Jr. issued the following order to the Department of Justice substantially limiting the practice of federal adoptions under Equitable Sharing:

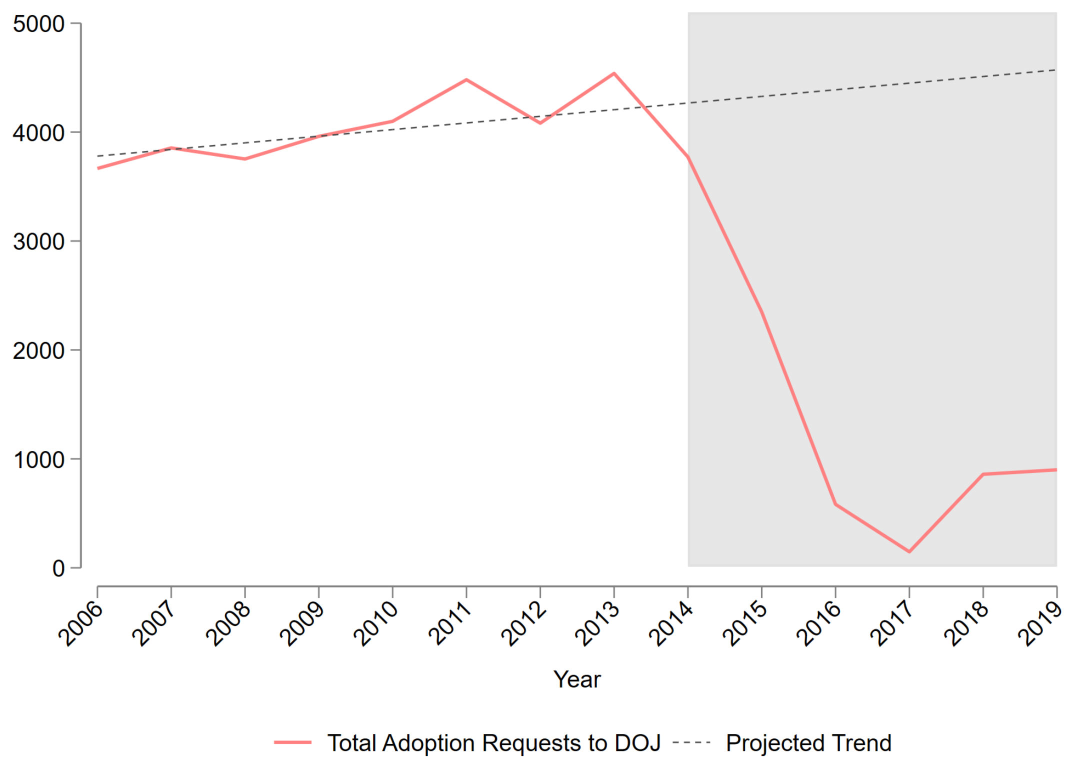
> "Federal adoption of property seized by state or local law enforcement under state law is prohibited, except for property that directly relates to public safety concerns, including firearms, ammunition, explosives, and property associated with child pornography. To the extent that seizures of property other than these four specified categories of property are being considered for federal adoption under this public safety exception, such seizures may not be adopted without the approval of the Assistant Attorney General for the Criminal Division. The prohibition on federal adoption includes, but is not limited to, seizures by state or local law enforcement of vehicles, valuables, and cash, which is defined as currency and currency equivalents, such as postal money orders, personal and cashier's checks, stored value cards, certificates of deposit, travelers checks, and U.S. savings bonds." (Office of the Attorney General 2015)

While the Attorney General's directive substantially limited the practice of federal adoption under Equitable Sharing, it did not completely abolish it. The language of the directive clearly allows for certain exceptions in the name of public safety. Further, the Attorney General may, by his own discretion, allow certain instances of adoption even under the new directive. The order further clarifies that sharing rules apply regarding property seized as a result of a joint task force between local and federal agencies, as well as those that are seized pursuant to federal warrants to obtain assets seized under state law. Thus, only the practice of federal adoptions, whereby federal agencies assume control of an asset seized by local law enforcement, would be affected. The same day that Holder issued this order, the Department of Justice circulated the order via their Equitable Sharing Wire, a regular report issued to local law enforcement agencies in the US detailing updates and changes to the Equitable Sharing program.

To shed light on the effect of this order, Figure 1 plots total adoptions requests from 2006 through 2019 in real dollars.[4] There is a precipitous drop in aggregate adoptions requests received by the DOJ immediately following the 2015 directive. The decline reaches a low in 2017, then rises slightly by the end of 2019, likely as a result of a slight clarification to the order made by AG Jeff Sessions in July of 2017 (DOJ Press Release 2017). Nevertheless, there seems to be a clear break in the trend from the relatively constant aggregate number of requests from before 2014. Notice

[4] Data used to produce this plot come from the Consolidated Assets Tracking System, which is further explained in Section 3.

Figure 1: Local Equitable Sharing Requests, 2006–2019



This time series graph displays adoptions requests within the Equitable Sharing program from 2006 through 2019. Revenue amounts are in real 2014 dollars. The vertical dashed line at 2015 marks the year that the practice of adoptions through Equitable Sharing was suspended by then AG Eric Holder.

that total adoptions requests never quite reach zero, likely due to the several exceptions outlined in AG Holder's order.

3 Data

There are 3 main data sources in this analysis: local Equitable Sharing receipts provided by the DOJ's Consolidated Assets Tracking System (CATS), various measures from the FBI's Uniform Crime Reporting (UCR) program, and yearly county-level demographics from ACS estimates.

3.1 Consolidated Assets Tracking System (CATS)

The Department of Justice (DOJ) manages a consolidated federal database that tracks assets forfeited by federal agencies. This database, called the Consolidated Assets Tracking System (CATS), was created in 1990 for the purpose of recording the entire life cycle of assets seized by various agencies within the Department of Justice, Department of Defense (DOD), Food and Drug Administration (FDA), US Department of Agriculture (USDA), US State Department, and US Postal Inspection Service (USPIS). In 2020, the CATS database became publicly accessible due to a Freedom of Information Act (FOIA) request initiated by the Institute for Justice (Knepper et al. 2020).

Before the implementation of CATS, all federal agencies maintained their own systems for recording information on asset forfeitures. One of the primary goals in creating a consolidated database was to maintain a universal, systematized catalog for all of the aforementioned agencies' seized property. Each item in the CATS database is assigned a unique identifier and has over 1,100 possible characteristics (descriptors) assigned to those assets. To my knowledge, CATS records this information for assets seized since 1993.

The CATS database also records all assets seized by and with local police under Equitable Sharing, which is the data I use in this analysis. For each incident of adoptions or joint investigations, I observe identifying information on the local law enforcement agency, violation codes that justified the forfeiture, administrative processing dates, asset valuation, and the total revenue that is returned to the local agency. I aggregate asset forfeiture receipts by local agency and year, focusing on the years 2010 through 2019.[5] I do not observe asset forfeitures conducted by law enforcement under state and local law. Most states do not require police to record information on local asset forfeitures, and the few states with reporting requirements suffer from severe misreporting issues (Billy 2020). Since I cannot document the extent of possible substitution between Equitable Sharing and local forfeiture, my estimates may be conservative (biased toward zero).

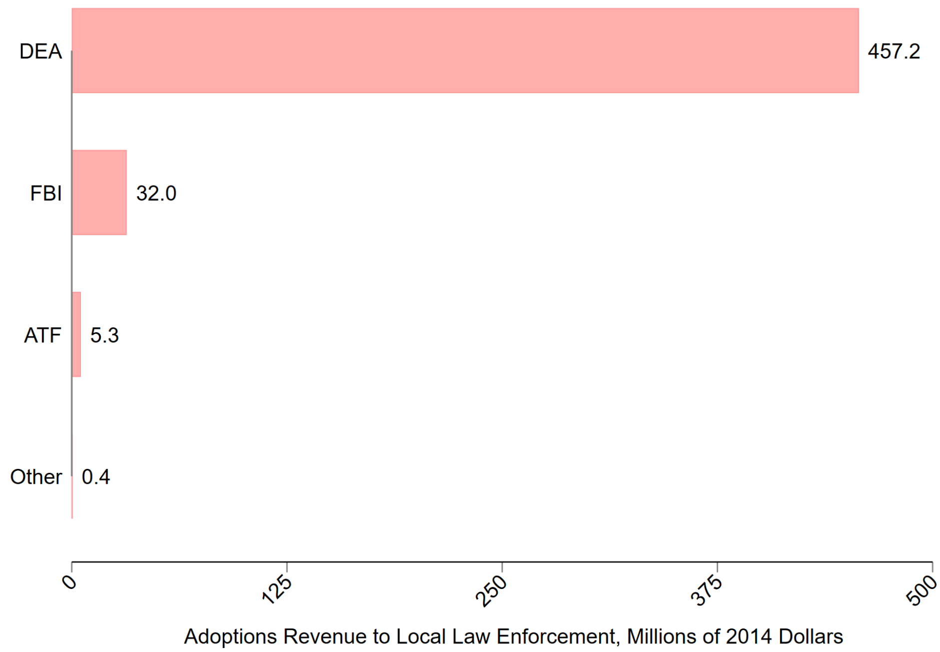
[5] I closely follow the method for sifting through the CATS database that the Institute for Justice uses in their Policing for Profit reports (Knepper et al., 2020).

3.1.1 Link between adoptions revenue and drug policing

One can think of the suspension of adoptions revenue as a shock to the profitability of a particular revenue-generating task for a local agency. A natural question then arises: which police tasks generate adoptions revenue? From available data, it appears that the local agencies generate the most adoptions revenue through the policing of illicit drug trafficking and consumption.

Most adoption of locally seized assets is conducted by the Drug Enforcement Administration. Figure 2 plots total adoptions payments received by local law enforcement from 2006-2020. All revenue is in real dollars, and revenue is categorized by which federal agency received and processed the DAG-71 forms. Over 99% of all adoptions revenue is processed under one of four federal law enforcement agencies: the Drug Enforcement Administration (DEA), the Federal Bureau of Investigation (FBI), the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF), and the United States Postal Service (USPS). As indicated by the magnitude of the red bars in Figure 2, the DEA paid local agencies over $457 million through adoptions, which is about 90% of all adoptions payments since the early 2000s. In order for the DEA to have jurisdiction as the adopting agency, local police conducting the seizures would need to do so under the pretense of enforcing drug laws. The Supplemental Appendix contains more information about the particular statute violations given by local agencies when filling out their DAG-71 forms.

Figure 2: Local adoptions payments by federal agency, 2006–2020



This figure displays adoptions outflows from Equitable Sharing in real 2014 dollars from 2006 through 2020. The acronyms on the y-axis represent the collaborating federal agency listed on the DAG-71 form documenting the Equitable Sharing payment. FBI = Federal Bureau of Investigation, DEA = Drug Enforcement Administration, ATF = Bureau of Alcohol, Tobacco, Firearms and Explosives, USPS = US Postal Service.

3.2 Uniform Crime Reporting (UCR) data

I use the FBI's *Uniform Crime Report (UCR), Arrests by Age, Sex, Race* dataset to measure local police activity via arrests.[6] The UCR is a voluntary reporting system where local police report the total arrest counts made each month to the FBI, across several crime and demographic categories. Each arrest counts once in the FBI's system, so if a single individual is arrested three times in a month, it would count as three separate arrests in that agency's UCR report for the month.

The UCR's arrests data categorize crimes into 29 different offense types, split between Part I offenses (index crimes) and Part II offenses. Part I contains seven violations that are organized according to the Hierarchy Rule, a system that the FBI imposed in the early 1900s and still continues to this day. The Hierarchy Rule basically says that for a given person who is arrested for committing multiple offenses, only the most severe is recorded as the reason for the arrest. The Part I crimes organized according to the Hierarchy Rule are homicide, murder and non-negligent manslaughter, manslaughter by negligence, rape, robbery, aggravated assault, burglary, non-vehicle theft, and

[6] All UCR files I used come from those cleaned and concatenated by Jacob Kaplan (Kaplan 2020b; Kaplan 2020a; Kaplan 2021).

motor vehicle theft. The remaining offenses are categorized as Part II offenses, and these are not organized according to the Hierarchy Rule. These offenses include simple assault, arson, forgery and counterfeiting, fraud, embezzlement, buying/receiving/selling stolen property, vandalism, weapons offenses, prostitution, other sex offenses, drug abuse violations (possession, manufacturing, sale, etc.), gambling, offenses against family and children, DUI, liquor law violations, drunkenness, disorderly conduct, vagrancy, suspicion, curfew/loitering, runaways, and all other offenses.

The UCR data suffers from two problems: potential measurement error and agency nonreporting. First, for agencies that do report arrest counts in a given month, many of their observations are zero. There is currently little consensus as to whether these are so-called true zeros or merely placeholders for months when the agency did not complete a full report. It is possible that some agencies are lumping most of their arrest counts into certain months, then reporting zeros for the rest of the months. Since some agencies report large numbers of crimes at the end of the calendar year, lumping measurement error would not be idiosyncratic. To address this, I aggregate all UCR data to the agency-by-year level, focusing again on the years 2010–2019. So long as all crimes within a year eventually get reported to the UCR in some month of the year, this should resolve lumping mismeasurement. Of course, there may be miscounting errors within these reports, but so long as these errors are random they should not induce bias in my estimates.

The second issue is that because the UCR system is voluntary, for a given agency that submits UCR reports in a given year, they may not submit reports for every month of that year. Hence, when aggregating to the year level, I limit my sample to include only regularly reporting agencies, which are those that report every month of a given year. Since my analysis focuses on drug policing behavior, I require that an agency submit drug arrest reports for all 12 months of a given year in order for that agency's year of data to be included. Of course, this means that my panel is naturally unbalanced, since if an agency reported all 12 months of drug crime in 2015 but failed to report in February of 2016, they would not be included in 2016. Since there is no heterogeneity in treatment timing, this is not a major issue for identification. Even if treatment induces attrition from the sample, so long as it is random across high/low arresting agencies, it should not affect my estimates.

In addition to arrests data, I also use 2 other dataset from the UCR system. I use the *Law Enforcement Officers Killed and Assaulted (LEOKA)* program to obtain non-arrest characteristics for each agency in my sample. The LEOKA data files document agency jurisdiction populations, agency type (police, sheriff, state, special jurisdiction), hiring composition, and the number of officers killed in the line of duty. I also incorporate various measures of local crime reports using the *Offenses Known and Clearances by Arrest* data. Local law enforcement self-reports this information to the FBI along with their standard UCR submission. Note that these two data sources may suffer from the same misreporting and measurement error issues as the arrest measures. By limiting my sample to regularly reporting agencies, I think it is unlikely that any remaining measurement errors are systematic.

3.3 County demographics

I use county-level demographic estimates from the American Community Survey (ACS) as controls in my primary specifications outlined in Section 5 (American Community Survey 2024). The ACS is a yearly survey sent to a subset of residents within every US county in order to infer demographic statistics for that county. More than 3 million addresses are surveyed each year according to the US Census Bureau's website. While the primary unit of analysis in my models is the agency jurisdiction, there is currently no reliable way to obtain jurisdiction-level demographic shares. As a next-best option, I infer jurisdiction demographic shares from the county within which an agency is located. To generate real-dollar values for adoptions revenue, I also use the Consumer Price Index (CPI) deflator provided by IPUMS (IPUMS 2024).

3.4 Sample Selection

I begin with all local law enforcement agencies recorded in the CATS database. This means that every agency in my baseline sample has participated in Equitable Sharing at least once since the data recording process began in the late 70s. This is reasonable to ensure comparability of the agencies, but I will conduct a synthetic-DiD exercise later in the paper that uses UCR data from all never-participating agencies as a check. Next, I restrict the sample to agencies that consistently report drug arrests within a given year. That is, agency i is recorded in year t if i reports all 12 months of drug arrests in year t. Finally, I aggregate arrests data to the agency-year level, which resolves some concerns with lumping.

Following the literature that uses UCR data, I drop very large outcome outliers (anything above 3 standard deviations from the mean) in my baseline specification. I am left with an agency-by-year panel of local sheriffs and police departments from 2010 through 2019. My panel of agencies is naturally unbalanced due to variation in reporting years, but since there is no heterogeneity in treatment timing, the unbalanced nature of the panel should not directly affect the internal validity of the design.

Table 1 gives pre-2015 summary statistics using for the full sample of law enforcement agencies. Statistics weighted by jurisdiction population are given in Table A.1. Agencies report on average around 62 drug-related arrests and 400 non-drug-related arrests per 10,000 jurisdiction residents. The average police department's jurisdiction contains 67,000 residents, though with wide variation across departments. Note that 73% of the sample is composed of local police agencies, while the remaining 27% are sheriffs departments.

Across all agencies, the total amount of revenue received from adoptions is about \$53,000, although this estimate is substantially diluted by the over 60% of agencies which never received revenue from adoptions in the pre-period. Conditioning on agencies which received positive adoption revenue in the pre-period, the average yearly adoption revenue in the pre-period is \$26,448.49, and the average total revenue is \$132,242.4.

Table 1: Pre-2015 Summary Statistics

	Mean	Std. Dev.	Min	Max
Local agency characteristics				
Drug arrests / 10k jurisd. residents	61.73	49.63	1.97	795.96
Nondrug arrests / 10k jurisd. residents	400.73	256.86	15.24	1,433.01
Total crime / 10k jurisd. residents	324.90	197.43	0.00	1,820.74
Violent crime / 10k jurisd. residents	33.45	32.38	0.00	451.87
Property crime / 10k jurisd. residents	291.45	176.21	0.00	1,660.87
Total Adoptions Revenue	52,729.56	257,845.64	0.00	4,958,514.29
Average Yearly Adoptions Revenue	10,545.91	51,569.13	0.00	991,702.88
Jurisdiction population	67,826.93	155,435.84	742	4,029,741
Sworn officers employed	130.37	405.32	0	10,002
Male sworn officers	114.12	336.95	0	8,166
Female sworn officers	16.24	70.55	0	1,929
Total agency employees	185.43	584.09	0	16,990
Officers killed	0.01	0.14	0	8
Officer assaults	15.30	50.85	0	1,372
Local police indicator	0.73	0.44	0	1
Sheriff indicator	0.27	0.44	0	1
County characteristics				
Total population	613,768.89	1,379,216.30	1,745.00	10,105,708.00
Age 0–4 share	0.06	0.01	0.03	0.12
Age 5–17 share	0.17	0.02	0.08	0.29
Age 18–24 share	0.10	0.03	0.04	0.47
Age 25–44 share	0.26	0.03	0.14	0.45
Age 45–64 share	0.26	0.03	0.08	0.42
Age 65+ share	0.15	0.04	0.04	0.40
Male share	0.49	0.01	0.44	0.68
Female share	0.51	0.01	0.33	0.55
White share	0.81	0.13	0.18	0.99
Black / African-American share	0.11	0.12	0.00	0.79
American Indian / Alaska Native share	0.01	0.03	0.00	0.65
Asian share	0.04	0.05	0.00	0.44
Native Hawaiian / Pacific Islander share	0.00	0.00	0.00	0.13

This table displays pre-period summary statistics for the baseline sample described in Section 3.4. See Table A.1 for summary statistics weighted by jurisdiction population.

4 Methodology

This section describes the baseline difference-in-differences strategy I use to study the 2015 suspension of federal adoption payments. Treatment is determined by whether a given agency participated in the adoptions program between 2010 and 2014 (about 30% of the sample).[7] As a first pass, I estimate baseline regressions of the form:

$$Y_{i,t} = \alpha_i + \delta_t + \beta(Participating_i \times Post_t) + \gamma \mathbf{X}_{i,t} + \varepsilon_{i,t}. \tag{1}$$

$Y_{i,t}$ is the outcome for agency i in year t, $Participating_i$ indicates agencies that received positive adoptions revenue during 2010–2014, $Post_t$ marks years after 2015, and $\mathbf{X}_{i,t}$ is a vector of time-varying agency and demographic covariates. Agency fixed effects α_i absorb all time-invariant differences across agencies (e.g., baseline size, organizational capacity, and persistent local characteristics). The year fixed effects δ_t absorb any shocks shared by all agencies in a given year, such as national economic conditions. $\mathbf{X}_{i,t}$ controls for total officers per jurisdiction resident and total joint-investigations revenue per jurisdiction resident. Controlling for total officers captures simultaneous employment shocks that differentially hit large agencies, while controlling for joint-investigations revenue ensures that effects are not being driven by a surge in non-adoption DOJ collaboration.[8] I conduct inference using heteroskedasticity-robust standard errors that are clustered at the the agency level.

I focus on two sets of outcomes. First, I examine agency arrest rates (drug and non-drug arrests per 10,000 jurisdiction residents), which are intended to capture the department's behavioral response to the suspension of adoption payments. Second, I study crime rates (total, violent, and property crimes per 10,000 residents), which capture the reduced-form response of criminal activity to changes in policing intensity and composition. Because crime is an equilibrium outcome shaped by both police behavior and offender responses, I interpret the arrest results as the most direct test of the incentives mechanism, and view the crime results as complementary evidence on downstream effects. I also re-estimate baseline results using a population-adjusted Poisson model rather than a rate.

The coefficient of interest β on the interaction term gives the differential effect of the adoptions suspension on participating agencies (as defined above) relative to nonparticipating agencies. If agencies' drug arrest behavior, for example, is sensitive to changes in revenue-generating capability, β will be negative and statistically significant. The two identifying assumptions under this framework are standard: parallel trends and no anticipation. That is, we need nonparticipating agencies (as defined above) to accurately represent the counterfactual trend in the post-period outcomes for participating agencies.

One identification concern might be that participation status is correlated with jurisdiction population size (see Figures A.1a and A.1b) – so if there is a non-adoptions shock that differentially

[7] I do not exploit variation in adoption revenue received before 2010 for two reasons. First, forfeiture behavior farther from the suspension period is a worse proxy for treatment intensity at $t = 2015$. Second, adoptions revenue in the late 2000s may reflect transitory fiscal pressures and policing responses associated with the Great Recession, generating confounding variation.

[8] If the adoptions suspension itself impacted employment or participation in joint investigations, these controls could induce collider bias. However, I do not believe this is a concern. Agencies are not allowed by the DOJ to use adoptions revenue to pay for agency salaries (with the exception of overtime hours). Joint investigations are also usually DOJ-instigated, and the coefficient on this variable is consistently small statistically insignificant.

impacts agencies with large jurisdictions after 2015, this will confound my estimate of β. While no specification can entirely rule out simultaneous shocks, I can rule out place-based shocks that are correlated with population jurisdiction by including region-by-time fixed effects $\delta_{r,t}$, where r indexes either state or county. Put differently, $\delta_{r,t}$ ensures identification comes from comparing participating and non-participating agencies *within the same region-year*, rather than from differences across counties that may experience different contemporaneous shocks around 2015 (for example, local policy/economic shifts). Any threat to identification would need to come from *within*-region shocks differentially impact population jurisdiction size, which I argue is unlikely, especially given the agency-level controls in $\mathbf{X}_{i,t}$. Of course, this will absorb much of my variation, but if effects survive the inclusion of region-year FEs then we can be relatively confident that they are not spurious.

My preferred specifications are unweighted because my primary estimand is the effect of the adoptions suspension on the average law-enforcement agency. For arrest outcomes in particular, β captures a change in agency behavior, so each agency should contribute equally to the estimate. For completeness, I also report specifications weighted by an agency's average jurisdiction population in 2010–2014. These estimates can be interpreted as the effect of the adoptions suspension on the *average resident* (i.e., a population-weighted average treatment effect).

I also estimate dynamic specifications of the form:

$$Y_{i,t} = \alpha_i + \delta_t + \sum_{k \neq 2014} \beta_k \Big(Participating_i \times \mathbf{1}\{Year_t = k\} \Big) + \gamma \mathbf{X}_{i,t} + \varepsilon_{i,t} \tag{2}$$

where the coefficients $\{\beta_k\}$ give the average difference between participating and nonparticipating agencies in year k relative to that difference in 2015. This allows me to test whether nonparticipating agency trends are evolving similarly to participating agencies in the years leading up to the suspension.

I also examine alternative specifications, including a continuous treatment measure that uses variation in pre-period participation among participating agencies. Specifically, I estimate variants of Equation 1 that replace the binary $Participating_i$ indicator with a continuous "dose" measure equal to an agency's (rescaled) average adoptions revenue in 2010–2014. I also split the participating agency group into terciles based on average pre-period adoptions revenue to explore treatment dynamics. This specification tests whether agencies that relied most heavily on adoptions prior to 2015 exhibit the largest post-suspension changes in arrest outcomes. This dose-response design assumes that, absent the suspension, high- and low-dose participating agencies would have followed parallel trends after conditioning on fixed effects and controls.

5 Results

This section presents baseline results for the estimation questions in the previous section. It also presents robustness checks and a basic heterogeneity analysis.

5.1 Arrest Outcomes

5.1.1 Participating vs nonparticipating

The main results are reported in Table 2, which includes difference-in-differences estimates for arrest outcomes comparing participating agencies to nonparticipating agencies. The outcome in Panel A is drug arrests per 10,000 jurisdiction residents, while the outcome for Panel B is the nondrug arrest rate, defined analogously. I report both weighted and population-weighted estimates for completeness.

Across unweighted specifications, the estimated effect of the suspension on drug arrests (the coefficient on $Participating_i \times Post_t$) is negative and statistically significant. The baseline specification in Column 2 estimates that the suspension caused a decline of -2.91 drug arrests per 10,000 jurisdiction residents, a 4.7% decline relative to the pre-period mean. The estimate also survives the successive addition of state-year and county-year FEs, indicating that the observed effect is likely not due to simultaneous local policy shifts or economic shocks correlated with agency participation in adoptions. In the population-weighted specifications (Columns 4–6), the point estimates for drug arrests are nearly double the magnitude of the unweighted estimates and mostly statistically significant. While the inclusion of county-year FEs reduces precision in the weighted model, the event-studies I present later still show suggestive evidence of a decline.

I find no evidence that the adoptions suspension impacted nondrug arrests; if anything, I can rule out small positive deviations. Across unweighted specifications, the difference-in-differences estimate is quite small – only around a 1% deviation from the mean in the highest magnitude estimate in Column 3 – and all are statistically indistinguishable from zero. The weighted estimates are larger on average and consistently negative, and while the state-year FE specification is statistically significant, the inclusion of county FEs inflates the standard error and lowers the estimate substantially.

Table 2: Effect of the Adoptions Suspension on Arrest Rates

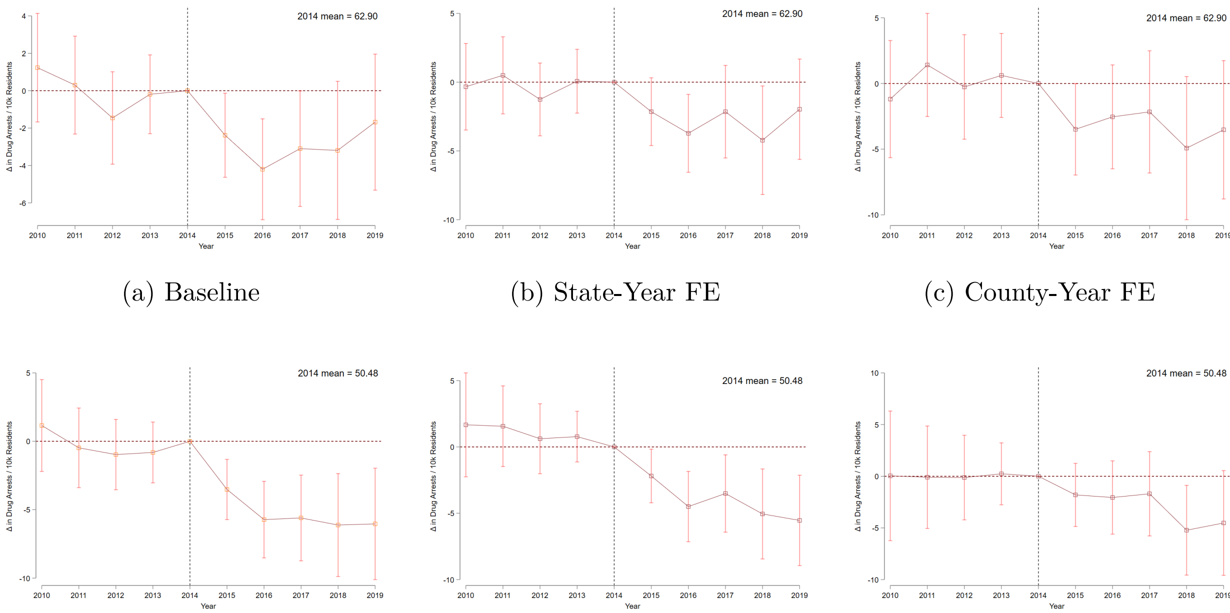
	Unweighted				Population-Weighted			
	TWFE	Baseline	+StateYr FE	+CountyYr FE	TWFE	Baseline	+StateYr FE	+CountyYr FE
Panel A: Drug Arrest Rate								
Participating × Post	-3.41***	-2.91**	-2.66**	-3.42**	-6.62***	-5.15***	-5.01***	-3.01
	(1.21)	(1.20)	(1.26)	(1.73)	(1.84)	(1.52)	(1.65)	(2.31)
Officers Per 10k Residents		0.26***	0.28***	0.26***		0.13***	0.15***	0.16***
		(0.08)	(0.08)	(0.10)		(0.04)	(0.03)	(0.03)
JI Revenue Per 10k Residents		-0.00	0.00	-0.00		-0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Panel B: Nondrug Arrest Rate								
Participating × Post	-2.90	-3.60	-5.35	2.59	-9.78	-6.94	-10.35*	-5.90
	(4.40)	(4.43)	(4.54)	(5.72)	(7.38)	(6.56)	(6.10)	(7.78)
Officers Per 10k Residents		0.79*	0.80*	0.83		0.61***	0.54***	0.63***
		(0.41)	(0.42)	(0.53)		(0.10)	(0.10)	(0.11)
JI Revenue Per 10k Residents		0.00	0.00	-0.00		0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Agency FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
State-Year FE	No	No	Yes	No	No	No	Yes	No
County-Year FE	No	No	No	Yes	No	No	No	Yes
Covariates	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Pop. Weights	No	No	No	No	Yes	Yes	Yes	Yes
Obs.	24,773	24,773	24,751	18,600	24,773	24,773	24,751	18,600
Agencies	3,391	3,391	3,389	2,643	3,391	3,391	3,389	2,643

This table presents baseline estimates + variations of Equation 1. The estimates in the first four columns are unweighted, while the estimates in the last four columns are weighted by the reporting agency's average jurisdiction population from 2010-2014. Within each heading, the first column includes only agency and year fixed effects, the second column controls for agency-level covariates and county-level demographics, the fourth column replaces year FEs with state-by-year FEs, and the fourth column replaces year FEs with county-by-year FEs. The dependent variable in Panel A is drug arrests per 10,000 jurisdiction residents, and the dependent variable in Panel B is nondrug arrests per 10,000 jurisdiction residents. All standard errors are robust and clustered at the agency level. *, **, and *** denote p < .10, p < .05, and p < .01., respectively.

To explore treatment dynamics and check for parallel pre-trends, I estimate event-study specifications following Equation 2. The event study results for drug arrests are included in the main body of the paper, while the nondrug arrest event studies can be found in Appendix Figure A.2.

For the drug arrest rate outcome, Figure 3 shows that across the baseline, state-year FE, and county-year FE models, participating and nonparticipating agencies follow a similar trend during the pre-period, but participating agencies experience a sharp relative decline in drug arrest rates in 2015, which is sustained through the rest of the panel. The weighted estimates are similar, but the baseline specification portrays a sharper and more sustained drop at 2015. While the ATT estimate is not statistically significant on the weighted county-year FE specification, the graphical evidence does show a slight decline which picks up in 2018. Overall, the event-study evidence suggests that the drop in the drug arrest rate temporally aligns with the adoptions suspension in 2015, strengthening the paralell trends and no-anticipation assumptions required for a causal interpretation of the estimates in Table 2.

Figure 3: Dynamic Effect of Adoptions Suspension on Drug Arrests



(a) Baseline (b) State-Year FE (c) County-Year FE

(d) Baseline (weighted) (e) State-Year FE (weighted) (f) County-Year FE (weighted)

These figures display event study coefficients following Equation 2, where the dependent variable is drug arrests per 10,000 jurisdiction residents. All results include agency and county controls, as well as robust standard errors that are clustered at the agency level. Dots represent point estimates, bars represent 95% confidence intervals. Weighted estimates are weighted by the average jurisdiction population size in the pre-period (2010–2014).

5.1.2 Continuous Treatment

Next, instead of a simple participation binary, I explore effects of the suspension using a continuous "dose" variable measured by pre-period participation intensity, defined using each agency's average annual adoptions revenue over 2010–2014.[9] Because the distribution of pre-period adoptions revenue is highly skewed, I construct a transformed dose measure using the inverse hyperbolic sine transformation,

$$Dose_i = \text{asinh}\left(\frac{1}{5}\sum_{t=2010}^{2014} Adoptions_{i,t}\right) \tag{3}$$

and then demean $Dose_i$ relative to the mean dose among participating agencies in the pre-period.

Table 3 reports estimates following the baseline specification in Equation 1, replacing the original interaction with $Dose_i \times Post_t$. Columns labeled "Participating" restrict the sample to agencies that participated at least once during 2010–2014, so identification comes from variation in pre-period reliance on adoptions within the treated group. The columns labeled "Full" include

[9]Using average adoptions revenue as a proxy for participation intensity is useful because it captures both the level and regularity of participation. Under this measure, an agency that received \$20,000 in 2010 and nothing thereafter is treated as less exposed than an agency that received \$5,000 in each year.

both participating and nonparticipating agencies and therefore combine variation on the extensive and intensive margins. In Panel A, the coefficient on $Dose_i \times Post_t$ is negative and statistically significant across specifications, both weighted and unweighted, indicating that agencies with higher pre-period reliance on adoptions reduce drug arrests more after 2015. The estimates are fairly stable across specifications, although the participating-only estimates are larger in magnitude. In Panel B, the corresponding estimates for nondrug arrests are generally small and statistically indistinguishable from zero, which is consistent with the baseline results in Table 2. While there is no direct way to interpret the magnitudes on these estimates due to the application of the inverse hyperbolic sine transformation, it seems that agencies with higher pre-period participation in adoptions experience larger negative declines.

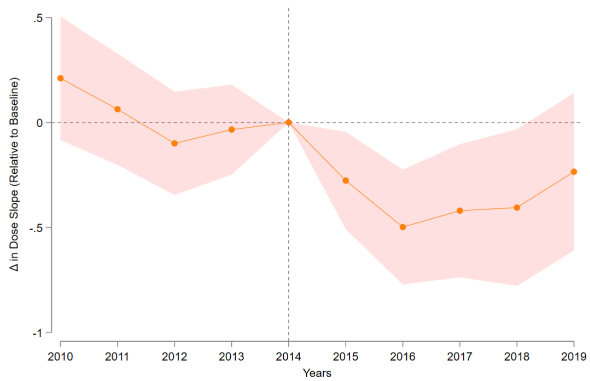
Table 3: Dose Effects of Suspension on Arrest Rates

	Unweighted		Population-Weighted	
	Full	Participating	Full	Participating
Panel A: Drug Arrest Rate				
Dose × Post	-0.40***	-1.67***	-0.67***	-2.71***
	(0.13)	(0.55)	(0.18)	(0.81)
Officers Per 10k Residents	0.26***	0.42***	0.13***	0.52***
	(0.08)	(0.13)	(0.04)	(0.13)
JI Revenue Per 10k Residents	-0.00	-0.00	-0.00	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)
Panel B: Nondrug Arrest Rate				
Dose × Post	-0.47	-1.89	-0.86	-3.15
	(0.47)	(1.98)	(0.69)	(2.54)
Officers Per 10k Residents	0.79*	1.75***	0.61***	1.63***
	(0.41)	(0.50)	(0.10)	(0.42)
JI Revenue Per 10k Residents	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Agency & Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Pop. Weights	No	No	Yes	Yes
Obs.	24,773	9,595	24,773	9,595
Agencies	3,391	1,234	3,391	1,234

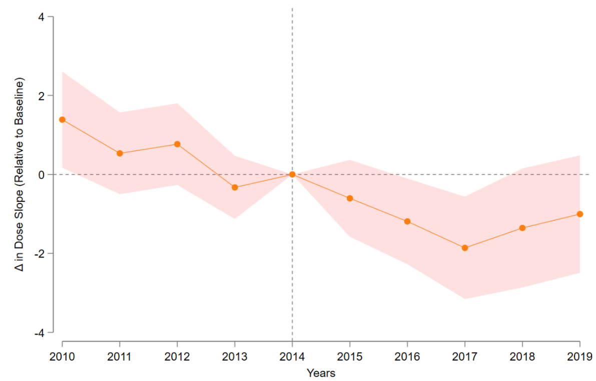
This table shows baseline estimates of a variation of Equation 1, where the binary Participating indicator is replaced with the continuous Dose treatment defined in Equation 3. Estimates in the first two columns are unweighted, while estimates in 3rd and 4th columns are weighted by the reporting agency's average jurisdiction population from 2010-2014. Columns labelled use the full sample, while columns labelled use only the participating agency sample. The dependent variable in Panel A is drug arrests per 10,000 jurisdiction residents, and the dependent variable in Panel B is nondrug arrests per 10,000 jurisdiction residents. All standard errors are robust and clustered at the agency level. *, **, and *** denote $p < .10$, $p < .05$, and $p < .01$., respectively.

I also estimate a dynamic event study specification interacting the continuous $Dose_i$ measure with year dummies, similar to Equation 2. Figure 4 presents results using the outcome of drug arrest rates across the four main dose specifications given in Table 3. Pre-trends appear quite are parallel, and the event study estimates show a clear drop in drug arrests immediately following the 2015 suspension of adoptions payments. Findings here appear even stronger than those in Figure 3.

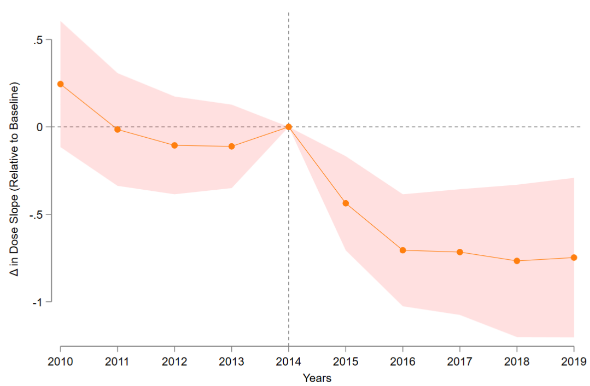
Figure 4: Dose-Slope Dynamics for Drug Arrests



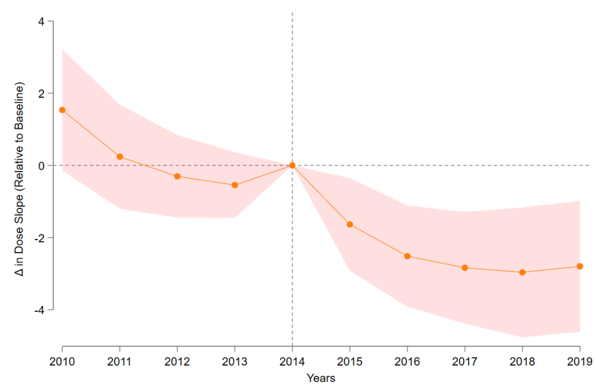
(a) Full sample, unweighted



(b) Participating sample, unweighted



(c) Full sample, weighted



(d) Participating sample, weighted

These figures display event study coefficients estimating a variant of Equation 2 with the previously defined $Dose_i$ measure interacted with year dummies. The dependent variable across specifications is drug arrests per 10,000 jurisdiction residents. Results captioned "All" include all agencies in the sample, while subgraphs captioned "Participating" use only variation within the participating agency group. All results include agency FEs, county-year FEs, agency covariates, and heteroskedasticity-robust standard errors that are clustered at the agency level. Dots represent point estimates, and the shaded region represents 95% confidence intervals.

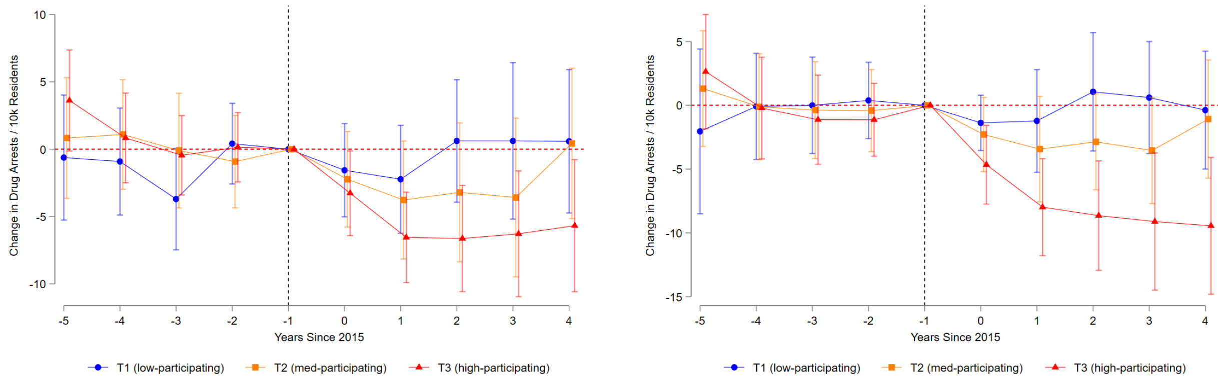
Finally, to explore heterogeneity in the drug arrest outcome within the participating agency group, I bin the participating agencies into terciles based on the distribution of $Dose_i$ and regress an agency's drug rate on tercile-by-year interactions. Table 4 gives pooled difference-in-differences estimates based on this specification, and Figure 5 gives the event study estimates. As is evident in the the first column of the Table, there is a clear striation in effect of the suspension by participating agency tercile – agencies within the highest-participation tercile reduce arrests by about -7.68 drug arrests per 10,000 residents relative to the controls, whereas agencies in the lowest tercile seem to not have changed at all. Differences across terciles are also pronounced for the weighted estimates. The tercile-by-post interactions are statistically distinct in both columns at the 1% significance level. The event studies in Figure 5 show that the nonparticipating agency group follows a similar trend to all 3 participation terciles during the pre-period, strengthening the parallel trends assumption.

Table 4: Effect of Suspension on Drug Arrest Rates by Pre-Period Participation Terciles

	Unweighted	Population-Weighted
T1 × Post	0.32	1.68
	(2.00)	(2.32)
T2 × Post	-1.86	-0.60
	(1.68)	(1.85)
T3 × Post	-7.68***	-10.04***
	(1.67)	(2.19)
Officers Per 10k Residents	0.26***	0.13***
	(0.08)	(0.04)
JI Revenue Per 10k Residents	-0.00	-0.00
	(0.00)	(0.00)
Agency FE	Yes	Yes
Year FE	Yes	Yes
Agency Covariates	Yes	Yes
Pop. Weights	No	Yes
p-val: T1=T2=T3	0.002	0.000
Obs.	24,773	24,773
Agencies	3,391	3,391

This table reports estimates from a TWFE specification with agency and year fixed effects, where treatment is captured by tercile-by-post interactions (terciles defined over participating agencies using the continuous Dose treatment). The dependent variable is drug arrests per 10,000 jurisdiction residents. Column (2) is weighted by the agency's average jurisdiction population (2010–2014). Standard errors are clustered at the agency level. *, **, and *** denote $p < .10$, $p < .05$, and $p < .01$., respectively.

Figure 5: Dynamic Treatment Effects By Participation Terciles (Drug Arrest Rates)



(a) Unweighted

(b) Pop. Weighted

These figures present event study estimates where treatment is captured by tercile-by-year interactions (terciles defined over participating agencies using the continuous Dose treatment). I run separate regressions comparing each tercile to the never-participating group, then overlay the tercile-year estimates for ease of comparison. The dependent variable in both subfigures is drug arrests per 10,000 jurisdiction residents. Estimates in Subfigure a) are unweighted, while estimates in Subfigure b) are weighted by the agency's average jurisdiction population (2010–2014). All specifications include agency-by-year FEs, agency and county covariates, and robust SEs clustered at the agency level. Dots represent point estimates, bars represent 95% confidence intervals.

5.1.3 Heterogeneity

I also explore heterogeneity for the drug arrest outcome by offense type, age, and sex of the arrested person (see Table A.2). It appears the suspension reduced arrests for the possession of a controlled substance rather than for the sale of a controlled substance. In the context of revenue incentives, this suggests that the primary cause of the initial adoptions case is suspicion that the property owner is consuming an illicit substance rather than selling one. This is consistent with anecdotal evidence documented by the Institute for Justice (Knepper et al. 2020). In addition, the effect seems to center on a reduction in arrests for adults rather than for juveniles. The last two columns of Table A.2 show a breakdown of the effect across sex of the arrestee. Participating agencies appear to have differentially reduced arrests for both males and females after the suspension, but the effect is more than twice as strong for males. This makes sense, since males make up a larger portion of the composition of overall arrests (both drug and nondrug) relative to females.

Table A.3 subsets my original sample across local police departments and sheriff offices using the UCR's LEOKA data file, which provides agency type identifiers. Consistent with Mughan et al. 2020, difference-in-differences estimates for police departments are precisely estimated and substantially larger in magnitude, indicating that police departments are more sensitive to changes in their ability to retain forfeiture funds. Estimates for both drug arrest rates and nondrug arrest rates using the sample of sheriff offices are consistently smaller in magnitude than the police department sample and imprecisely estimated.

5.2 Crime Outcomes

Table 5 reports auxiliary difference-in-differences estimates for crime outcomes comparing jurisdictions served by agencies that participated in adoptions before 2015 to jurisdictions served by nonparticipating agencies. Panel A reports results for the total crime rate, while Panels B and C split crime into violent and property components. Columns (1)–(4) report unweighted estimates, while Columns (4)–(6) weight by an agency's average pre-2015 jurisdiction population (effects for the average resident). As in the arrest analysis, the baseline specification is in Column 2, which includes agency fixed effects, year fixed effects, and agency/county controls.

The results for the total crime rate are mostly null. In the baseline specification, the estimate implies a decline of 3.38 total crimes per 10,000 residents (a mere 1% decline relative to pre-period mean), but the estimate does not approach statistical significance. The population-weighted estimate in the baseline model (Column 6) is similarly small in magnitude. However, the diff-in-diff estimate with county-year FEs (both weighted and unweighted) is negative and statistically significant at the 10% significance level.

In Panel B, I observe a consistent positive effect of the suspension on the violent crime rate in most specifications, although my pooled estimate loses statistical significance upon the addition of county-year FEs (and state-year FEs in the unweighted model). Despite this, event studies in Figure 6 offer graphical evidence suggesting a moderate increase in violent crime immediately following the suspension. Note that in Subfigures c) and f), which show event study estimates with the inclusion of county-year FEs, the lag estimate in 2015 is positive and marginally significant, but the effect appears to dissipate over the rest of the panel.

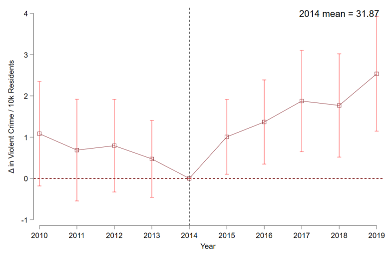
The results for property crime in Panel C are more inconsistent − the difference-in-differences estimate is consistently negative once county-by-year fixed effects are included and are statistically significant in both the unweighted and population-weighted preferred specifications, but the event studies in Figure 7 show evidence of a pre-trend.

Table 5: Effect of the Adoptions Suspension on Crime Rates

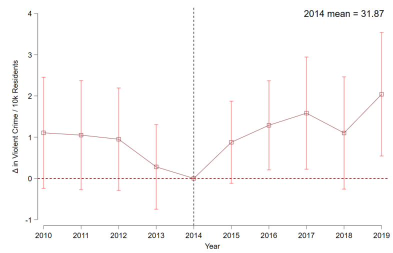
	Unweighted				Population-Weighted			
	TWFE	Baseline	+StateYr FE	+CountyYr FE	TWFE	Baseline	+StateYr FE	+CountyYr FE
Panel A: Total Crime Rate								
Participating × Post	-1.93	-3.34	-2.38	-6.34*	2.75	1.97	-1.16	-6.13*
	(2.62)	(2.58)	(2.61)	(3.40)	(6.02)	(5.10)	(3.87)	(3.66)
Officers Per 10k Residents		0.34	0.31	0.33		-0.06	-0.05	-0.03
		(0.23)	(0.21)	(0.27)		(0.13)	(0.09)	(0.07)
JI Revenue Per 10k Residents		0.00	-0.00	-0.00		0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Panel B: Violent Crime Rate								
Participating × Post	1.04**	1.09**	0.69	0.26	2.24*	2.50**	2.03*	1.56
	(0.49)	(0.49)	(0.53)	(0.70)	(1.24)	(1.09)	(1.04)	(1.30)
Officers Per 10k Residents		0.03	0.04	0.05		-0.01	-0.00	-0.00
		(0.03)	(0.03)	(0.04)		(0.02)	(0.02)	(0.01)
JI Revenue Per 10k Residents		-0.00	-0.00**	-0.00		0.00	-0.00**	-0.00
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Panel C: Property Crime Rate								
Participating × Post	-2.97	-4.43*	-3.07	-6.60**	0.51	-0.53	-3.19	-7.68***
	(2.43)	(2.39)	(2.40)	(3.06)	(5.33)	(4.49)	(3.25)	(2.94)
Officers Per 10k Residents		0.30	0.28	0.29		-0.06	-0.04	-0.03
		(0.21)	(0.19)	(0.24)		(0.12)	(0.08)	(0.06)
JI Revenue Per 10k Residents		0.00	-0.00	-0.00		0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Agency FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
State-Year FE	No	No	Yes	No	No	No	Yes	No
County-Year FE	No	No	No	Yes	No	No	No	Yes
Covariates	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Pop. Weights	No	No	No	No	Yes	Yes	Yes	Yes
Obs.	24,773	24,773	24,751	18,600	24,773	24,773	24,751	18,600
Agencies	3,391	3,391	3,389	2,643	3,391	3,391	3,389	2,643

This table presents baseline estimates and variations of Equation 1. The estimates in the first four columns are unweighted, while the estimates in the last four columns are weighted by the reporting agency's average jurisdiction population from 2010-2014. Within each heading, the first column includes only agency and year fixed effects, the second column controls for agency-level covariates and county-level demographics, the third column replaces year FEs with state-by-year FEs, and the fourth column replaces year FEs with county-by-year FEs. The dependent variables are (Panel A) total index crimes per 10,000 jurisdiction residents, (Panel B) violent index crimes per 10,000 jurisdiction residents, and (Panel C) property index crimes per 10,000 jurisdiction residents. All standard errors are robust and clustered at the agency level. *, **, and *** denote p < .10, p < .05, and p < .01., respectively.

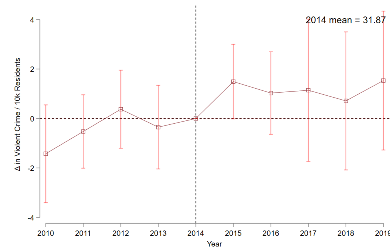
Figure 6: Dynamic Effect of the Adoptions Suspension on Violent Crime



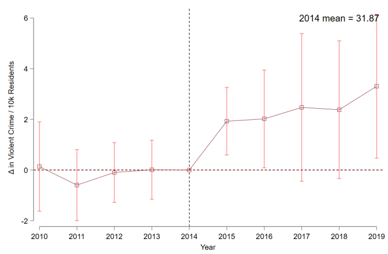
(a) Baseline



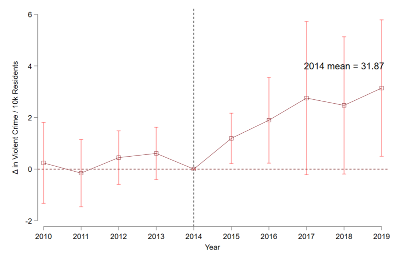
(b) +StateYr FE



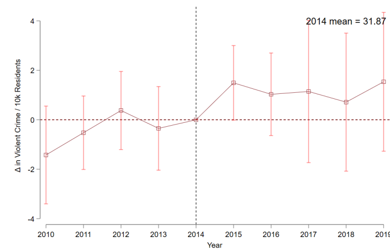
(c) +CountyYr FE



(d) Baseline (weighted)



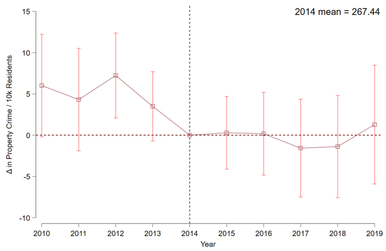
(e) +StateYr FE (weighted)



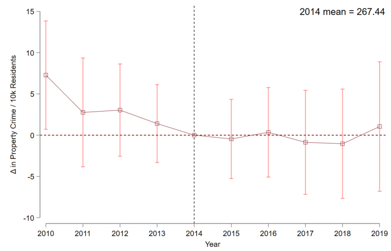
(f) +CountyYr FE (weighted)

This figure displays event study graphs following variations of Equation 2. The dependent variable in each graph is violent index crimes reported per 10,000 jurisdiction residents. Figures a) - c) display unweighted estimates, while Figures d) - f) display estimates weighted by average jurisdiction population in the pre-period (see Table 5 for specifics on each specification). Dots represent point estimates, bars represent 95% confidence intervals.

Figure 7: Dynamic Effect of the Adoptions Suspension on Property Crime

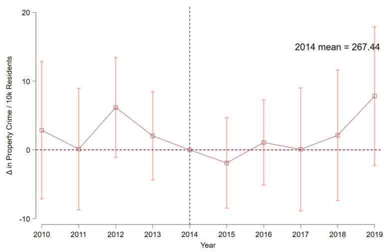


(a) Baseline

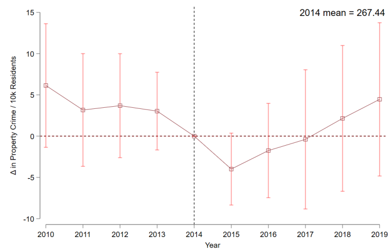


(b) +StateYr FE

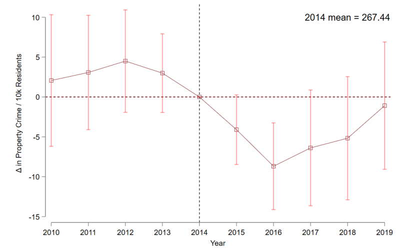
(c) +CountyYr FE



(d) Baseline (weighted)



(e) +StateYr FE (weighted)



(f) +CountyYr FE (weighted)

This figure displays event study graphs following variations of Equation 2. The dependent variable in each graph is property index crimes reported per 10,000 jurisdiction residents. Figures a) - c) display unweighted estimates, while Figures d) - f) display estimates weighted by average jurisdiction population in the pre-period (see Table 5 for specifics on each specification). Dots represent point estimates, bars represent 95% confidence intervals.

6 Conclusion

The 2015 suspension of federal adoptions created a plausibly exogenous reduction in the share of forfeiture proceeds that police departments could retain. By tracing that budget shock through administrative data on Equitable-Sharing receipts, arrests, and crime reports, I show that agencies exposed to the policy adjusted their arrest bundles in response to changes in their fiscal incentives.

Agencies which had participating in the Equitable Sharing adoptions program curtailed drug-arrest activity by about 5% in response to the suspension. The fall is concentrated in departments that had relied most heavily on adoptions beforehand and in possession (not trafficking) arrests, while non-drug arrests move little and imprecisely. If anything, I can rule out small-to-moderate positive effects of the suspension on nondrug arrests. Together with the theoretical framework, these patterns indicate that forfeiture proceeds function as a salient price signal: when the marginal return to drug enforcement falls, agencies shift away from the activity. The suggestive small dips in nondrug arrest rates could be evidence of cross-subsidization of other police activity, but given the relatively small amount of revenue that adoptions in particular generates, it makes sense that we do not observe strong effects along this margin.

These behavioral adjustments carry clear welfare implications. A drop in low-level drug enforcement frees officer time and may reduce the collateral consequences of arrest, such as court costs, criminal records, and community distrust. Municipal governments can and sometimes do back-fill forfeiture shortfalls through the general fund (Baicker and Jacobson 2007); if such fiscal offsets are widespread, the measured enforcement elasticity understates the true social cost of tying police revenue to seizure activity. Identifying whether there are any fiscal spillovers remains an open question.

Were these changes in police behavior meaningfully associated with changes in criminal activity? The results are more complicated – I document suggestive evidence that violent crime rises immediately following the suspension of adoption payments. This could be evidence of a deterrence effect, in that if police shift their locational presence out of high crime areas in response to the financial return to policing, individuals may feel more at liberty to offend. This is complicated by the suggestive *decline* in property crimes in the weighted models (admittedly more sensitive to specification). However, these results could be explained by the reduction in police activity itself, since both residents and police can report criminal behavior. Less policing activity could reduce the likelihood that police uncover and report property crimes. Alternatively, allowing criminals to retain more of their assets could reduce the need to engage in theft or burglary to generate more income.

Several limitations point the way for future work. First, jurisdiction-level arrest counts cannot reveal how chiefs reassign officers or how residents experience those reallocations. Linking budget shocks to officer-level deployment data, complaints, or use-of-force incidents would help substantiate welfare analysis. Second, longer panels are needed to gauge whether reduced drug-possession enforcement influences long-run drug markets or recidivism. Finally, the logic here extends to other self-financing mechanisms, such as traffic fines or fee-based policing, and comparative evidence across funding sources would help legislators design incentive-compatible budgets.

Taken together, the results in this paper demonstrate that even modest revenue streams can distort the allocation of police effort in ways that matter for both efficiency and justice. In particular, state policies that aim to restrict law enforcements' use of asset forfeiture revenue are likely to be most effective when accounting for Equitable Sharing loopholes.

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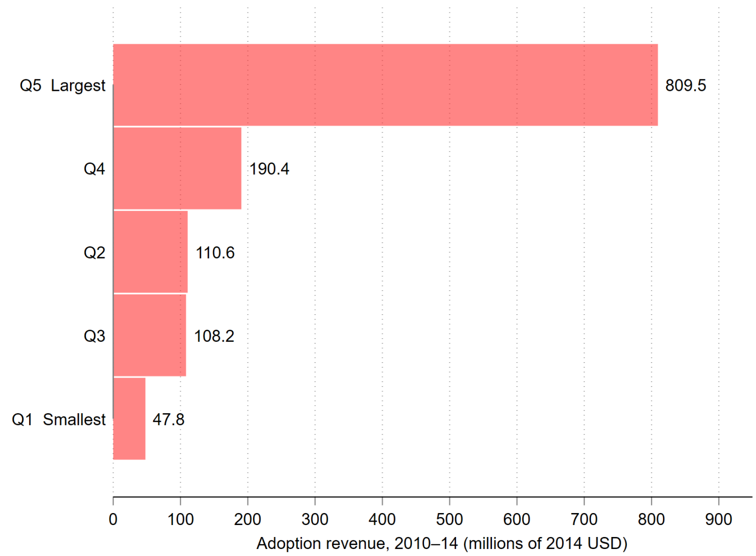
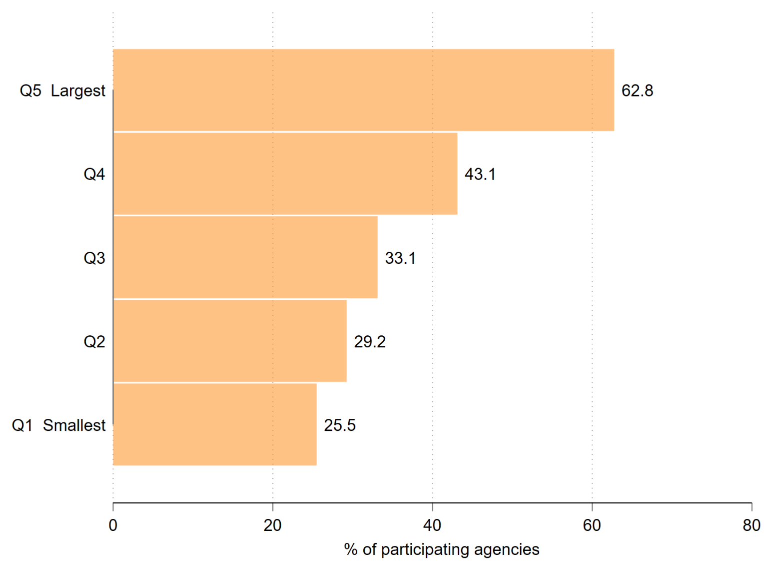
A Appendix

Figure A.1: Pre-suspension adoptions participation by jurisdiction population quintile



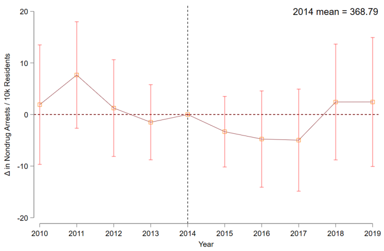
(a) Total adoptions revenue



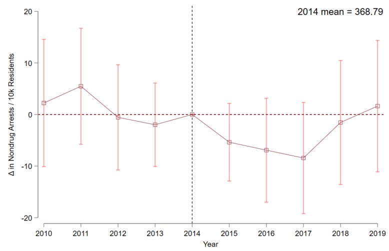
(b) Proportion of participating agencies

These figures show pre-suspension participation in adoptions (both in real dollars and likelihood of receiving adoption funds pre-suspension) by jurisdiction population quintile for the baseline sample described in Section 5.1. In both figures, the y-axes display population quintile markers Q1 through Q5. The x-axis on Subfigure A.1a is adoptions revenue in millions of 2014 dollars, and the x-axis on Subfigure A.1b is the percentage of agencies in the quintile that participated (received adoptions revenue in the 5 years before the suspension).

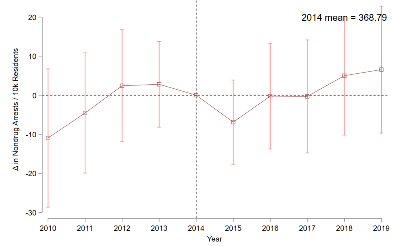
Figure A.2: Dynamic Effect of the 2015 Adoptions Suspension on Nondrug Arrests



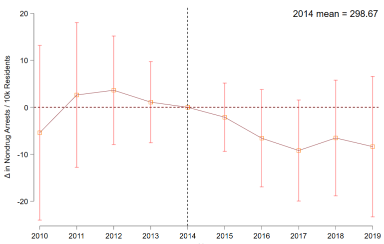
(a) Baseline



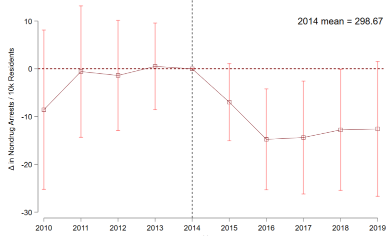
(b) State-Year FE



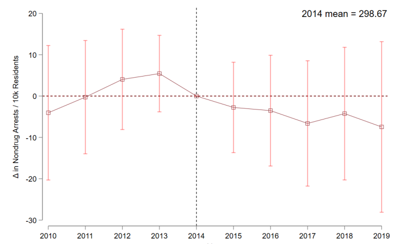
(c) County-Year FE



(d) Baseline (weighted)



(e) State-Year FE (weighted)



(f) County-Year FE (weighted)

These figures display event study coefficients following Equation 2, where the dependent variable is nondrug arrests per 10,000 jurisdiction residents. The specifications include controls for county demographics, and all control for agency employment and joint-investigation revenue. All estimates use heteroskedasticity-robust standard errors clustered at the agency level. Dots represent point estimates, bars represent 95% confidence intervals. Weighted estimates are weighted by the average jurisdiction population size in the pre-period (2010–2014).

Table A.1: Pre-2015 Summary Statistics Weighted by Pre-2015 Jurisdiction Population

	Mean	Std. Dev.	Min	Max
Local agency characteristics				
Drug arrests / 10k jurisd. residents	51.59	36.21	1.97	795.96
Nondrug arrests / 10k jurisd. residents	335.73	201.86	15.24	1,433.01
Total crime / 10k jurisd. residents	348.66	186.80	0.00	1,820.74
Violent crime / 10k jurisd. residents	43.13	36.34	0.00	451.87
Property crime / 10k jurisd. residents	305.53	160.49	0.00	1,660.87
Total Adoptions Revenue	206,158.14	500,415.17	0.00	4,958,514.29
Average Yearly Adoptions Revenue	41,231.63	100,083.04	0	991,703
Sworn officers employed	929.77	1,888.06	0	9,992
Male sworn officers	784.77	1,542.31	0	8,063
Female sworn officers	144.99	352.57	0	1,929
Total agency employees	1,267.57	2,591.65	0	16,990
Officers killed	0.07	0.32	0	3
Officer assaults	100.49	191.00	0	1,372
Local police indicator	0.75	0.44	0	1
Sheriff indicator	0.25	0.44	0	1
County characteristics				
Total population	1,348,057.13	2,195,921.97	1,745.00	10,040,072.00
Age 0–4 share	0.07	0.01	0.03	0.12
Age 5–17 share	0.18	0.02	0.08	0.29
Age 18–24 share	0.10	0.03	0.04	0.47
Age 25–44 share	0.27	0.03	0.15	0.45
Age 45–64 share	0.26	0.03	0.08	0.42
Age 65+ share	0.13	0.03	0.04	0.36
Male share	0.49	0.01	0.45	0.67
Female share	0.51	0.01	0.33	0.55
White share	0.77	0.14	0.18	0.99
Black / African-American share	0.13	0.13	0.00	0.79
American Indian / Alaska Native share	0.01	0.02	0.00	0.65
Asian share	0.06	0.07	0.00	0.44
Native Hawaiian / Pacific Islander share	0.00	0.01	0.00	0.13

This table displays weighted pre-period summary statistics for the baseline sample described in Section 3.4. All estimates are weighted by the pre-period average jurisdiction population.

Table A.2: Effect on drug rates: heterogeneity by offense type, age, sex

		Offense		Age		Sex	
	All	Sale	Possession	Adult	Juvenile	Male	Female
Unweighted							
Participating × Post	-2.914**	-0.442	-2.696**	-2.951**	0.036	-2.045**	-0.870**
	(1.204)	(0.311)	(1.108)	(1.173)	(0.104)	(0.889)	(0.356)
Weighted							
Participating × Post	-5.151***	-0.328	-5.140***	-4.975***	-0.175	-3.718***	-1.432***
	(1.524)	(0.400)	(1.411)	(1.463)	(0.129)	(1.223)	(0.333)
Obs.	24,773	24,773	24,773	24,773	24,773	24,773	24,773
Agencies	3,391	3,391	3,391	3,391	3,391	3,391	3,391

This table displays drug arrest heterogeneity results for the baseline difference-in-differences estimates comparing participating agencies to nonparticipating agencies. The dependent variable across columns is category X drug arrests per 10,000 jurisidiction residents, where category X can refer to the offense type (sale or possession), age of offender (adult or juvenile), or sex of offender (male or female). All estimates follow my preferred specification, which includes agency and year fixed effects, county-year demographic shares, agency-level covariates, and heteroskedasticity robust standard errors clustered at the agency level. The top panel reports unweighted estimates and the bottom panel reports estimates weighted by average pre-period population. *, **, and *** denote p < .10, p < .05, and p < .01., respectively.

Table A.3: Heterogeneity by Agency Type: Police vs. Sheriffs

	Unweighted		Weighted	
	Police	Sheriff	Police	Sheriff
Panel A: Drug arrest rate				
Participating × Post	-3.124**	-1.329	-7.308***	1.175
	(1.427)	(2.165)	(1.793)	(2.218)
Panel B: Nondrug arrest rate				
Participating × Post	-6.430	-2.614	-9.352	3.567
	(5.341)	(7.245)	(7.946)	(6.168)
Agency FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Pop weighted	No	No	Yes	Yes
Cluster	Agency	Agency	Agency	Agency
Obs.	18,099	6,657	18,099	6,657
Agencies	2,444	965	2,444	965

This table reports difference-in-differences estimates of the effect of the 2015 Equitable Sharing adoptions suspension on policing outcomes separately for police and sheriff departments. All specifications use the baseline two-way fixed effects model with agency fixed effects and year fixed effects. Panel A uses drug arrests per 10,000 jurisdiction residents as the dependent variable, and Panel B uses nondrug arrests per 10,000 jurisdiction residents. Columns (1)–(2) report unweighted estimates for police departments and sheriffs offices, respectively. Columns (3)–(4) report estimates weighted by average pre-period population. All models include agency-level covariates for officers per 10,000 residents and joint investigation revenue per 10,000 residents, as well as county demographic controls. Standard errors are clustered at the agency level. *, **, and *** denote $p<0.10$, $p<0.05$, and $p<0.01$, respectively.

B Supplemental Appendix

B.1 Simple Theoretical Framework

This section presents a partial equilibrium model of revenue-generating behavior in law enforcement. To abstract from the principal–agent problem, I model from the perspective of a police commissioner allocating funds to various law enforcement activities, some of which are revenue generating. The goal here is to demonstrate how changes in the return of revenue-generating activities alter law enforcement behavior.

Consider the simple case where a police commissioner allocates resources across two tasks: D and C. Task D is an index for revenue-generating activity (ticketing, asset forfeiture, political lobbying, etc.) and C is an index for all non-revenue-generating activity. Allocating funds to D is revenue-generating according to the linear function $r(D) = \pi D$, with $\pi < 1$. The commissioner has a total budget of M allocated by the local municipality, which also determines the proportion of revenue $1 - \tau$ that the local agency can keep from allocating funds to D. The commissioner maximizes over a quasi-linear objective:

$$\max_{C,D} \quad \Phi C + \frac{D^{1-\frac{1}{\sigma}}}{1-\frac{1}{\sigma}}$$

$$\text{subject to:} \quad C + D \leq M + \pi D(1-\tau)$$

We know the constraint binds because the objective is strictly increasing. Setting up the Lagrangian:

$$\mathcal{L} = \Phi C + \frac{D^{1-\frac{1}{\sigma}}}{1-\frac{1}{\sigma}} - \lambda[C + D - M - \pi D(1-\tau)]$$

First-order conditions:

$$\frac{\partial \mathcal{L}}{\partial D} = D^{-\frac{1}{\sigma}} - \lambda[1 - \pi(1-\tau)] = 0$$

$$\frac{\partial \mathcal{L}}{\partial C} = \Phi - \lambda = 0$$

Combining, we solve for D^*:

$$D^* = \left(\frac{1}{\Phi[1-\pi(1-\tau)]}\right)^{\sigma} \tag{4}$$

Rearranging the budget constraint (2) and plugging in for D^*, we obtain

$$C^* = M + D^*[\pi(1-\tau) - 1]$$

$$\Leftrightarrow C^* = M + \frac{\pi(1-\tau) - 1}{(\Phi[1-\pi(1-\tau)])^{\sigma}}$$

$$\Leftrightarrow C^* = M - \frac{[1-\pi(1-\tau)]^{1-\sigma}}{\Phi^{\sigma}} \tag{5}$$

The first observation is that the quantity $1 - \pi(1-\tau)$ is the *relative price* of investing a dollar in task D.[10] Hence, an upward shock to τ is analogous to a price increase for the revenue-generating

[10] This is clear from rearranging the initial budget constraint as $C + [1 - \pi(1-\tau)]D = M$.

task. Given the structure of D^*, own-price effects are clearly negative:

$$\frac{\partial D^*}{\partial \tau} < 0$$

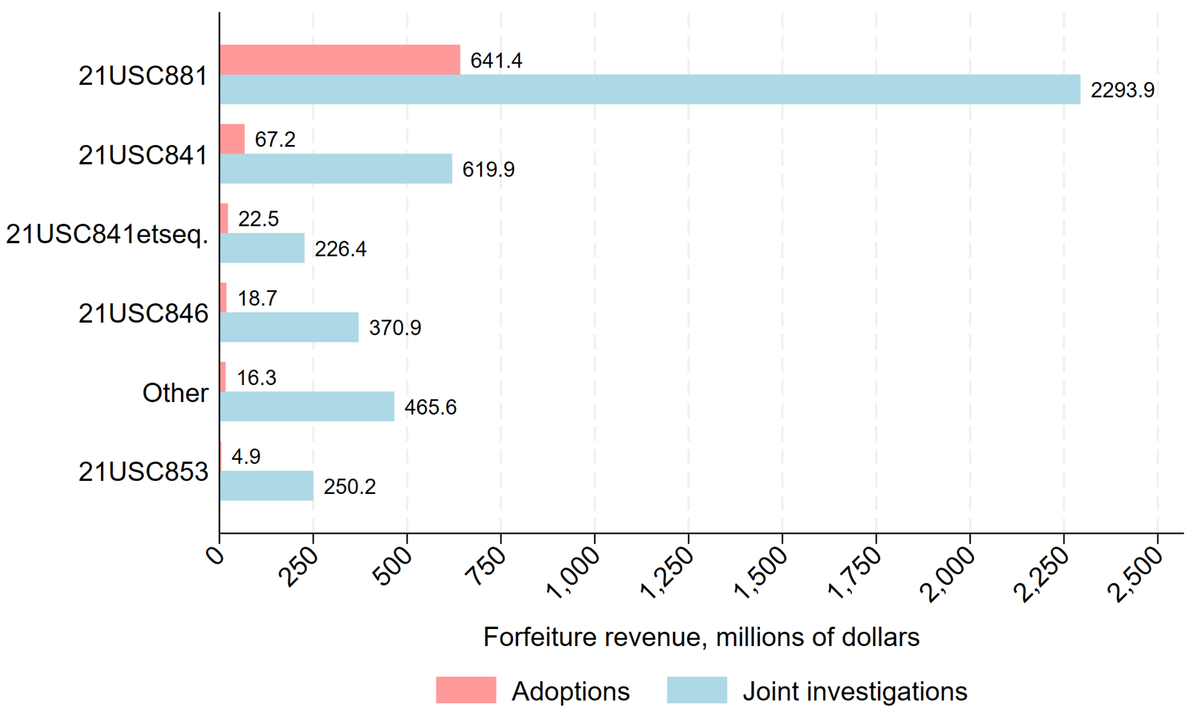
But cross-price effects depend on whether tasks C and D are gross substitutes or gross complements, which is controlled by the elasticity of substitution across tasks σ. Specifically:

$$\frac{\partial C^*}{\partial \tau} \text{ is } \begin{cases} > 0 \text{ if } \sigma > 1 \\ = 0 \text{ if } \sigma = 1 \\ < 0 \text{ if } \sigma < 1 \end{cases}$$

The sign on the comparative static thus depends on the strength of the substitution effect versus the "wealth" effect. If σ is large, a relative price shock induces substitution away from D and into C, so the substitution effect from the price increase is larger than the income effect. If $\sigma = 1$, substitution and wealth effects exactly cancel out. Finally, if σ is small, investment in task D is not sensitive to changes in price, so the wealth effects are stronger than the substitution effects and investment in C falls.

B.2 Forfeiture Statute Codes

Figure B.1: Local equitable sharing revenue by statute violation, 2000-2024



This figure displays Equitable Sharing revenue outflows from adoptions and joint investigations to local agencies in real 2014 dollars from 2000 through 2024. Payments are categorized by the statute violation cited as justification invoking forfeiture, provided in the CATS database described in Section 3. Descriptions of each statute are provided in Table B.1.

Table B.1: Key drug-related statutes cited in federal asset-forfeiture "adoptions"

Statute	LII Link	Brief description	Why it matters for drug-law forfeiture / "adoptions"
21 U.S.C. § 881	https://www.law.cornell.edu/uscode/text/21/881	Lists categories of property that are automatically subject to *civil (in-rem)* forfeiture when linked to any federal drug offense—e.g. drugs, cash proceeds, vehicles, real estate.	Primary hook for DEA administrative "adoptions." Local police seize currency; DEA adopts it and forfeits it under § 881(a)(6) without needing a criminal conviction.
21 U.S.C. § 841	https://www.law.cornell.edu/uscode/text/21/841	Defines federal crimes of manufacturing, distributing, or possessing with intent to distribute controlled substances; sets sentencing tiers.	When a seizure is tied to a trafficking indictment, the asset record is tagged with § 841 to show the predicate crime; forfeiture later proceeds under § 853 but CATS keeps the § 841 flag.
21 U.S.C. § 846	https://www.law.cornell.edu/uscode/text/21/846	Makes attempts or conspiracies to commit any drug-trafficking crime a separate offense carrying the same penalties as the completed act.	Assets seized during conspiracy investigations are coded § 846; once the conspiracy is charged, forfeiture follows criminal-forfeiture procedures, keeping path aligned with the conspiracy count.
21 U.S.C. § 853	https://www.law.cornell.edu/uscode/text/21/853	Provides the procedure for forfeiting a defendant's property *after conviction* for major drug felonies. The forfeiture order is part of the sentence.	Local seizures that turn into federal criminal cases shift from § 881 to § 853. DOJ then distributes the proceeds back to the seizing agency through Equitable Sharing.

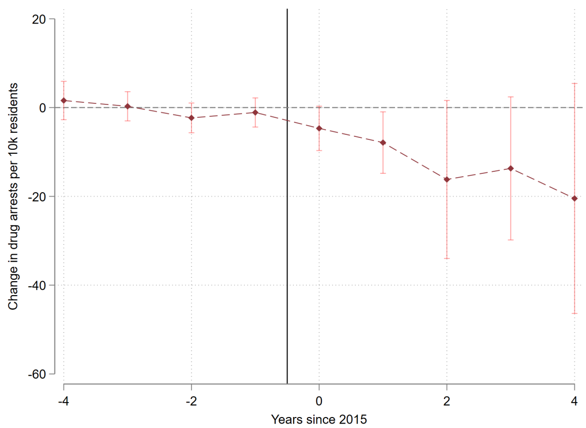
This table provides descriptions of the legal violation codes used to invoke civil asset forfeiture through Equitable Sharing displayed in Figure B.1. Each of the links in the second column direct to the Legal Information Institute's (LII) page describing the statute. The second and third columns provide a brief description of the statute and its connection to adoptions through Equitable Sharing.

B.3 Synthetic Difference-in-differences

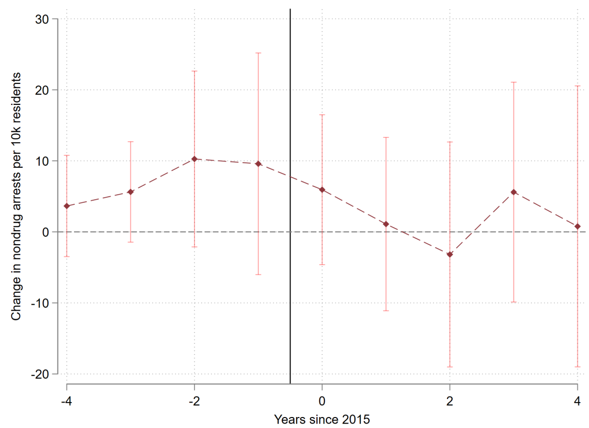
I re-estimate the effect of the adoptions suspension using a synthetic difference-in-differences design following Arkhangelsky et al. (2021). Since synthetic difference-in-differences requires a balanced agency-year panel, I make two tweaks to the baseline models presented in the Methodology section to try to preserve power. First, I also only include agency and year FEs rather than county-by-year FEs (which sweeps out substantial variation). Second, I relax my sub-sampling technique to allow for the inclusion of "never-participating" agencies, which are those not found in the CATS database. These are agencies which never received an Equitable Sharing payment since the data recording process began. By focusing on fewer years, I can balance the sample without losing too many agencies. Additionally, the inclusion of never-participating agencies reassures us that my results are not dependent on using only agencies in the CATS database, which might systematically differ from agencies not included in the database. For completeness, I run the specification on a longer panel using years 2010 through 2019, as well as a shorter panel using data from years 2012 through 2017. The results are sumilar, but my preferred estimation sample is the shorter panel due to the increased power (see Appendix A).

Figure B.3 present synthetic DiD event study coefficients and trend graphs using the shorter panel. Results for the longer panel are displayed in Figure B.2. Table B.2 gives ATT estimates for all synthetic DiD specifications. All standard errors across these designs are bootstrapped and I adjust for coefficients using the optimized method (Arkhangelsky et al. 2021). My preferred results using the shorter panel appear materially similar to my baseline event studies: I observe an immediate and sustained decline in drug arrest rates following the suspension and no detectable effect on nondrug arrest rates. Results generated using the longer panel of agencies are overall consistent with baseline, but they are estimated with much less precision because there are fewer consistently reporting agencies.

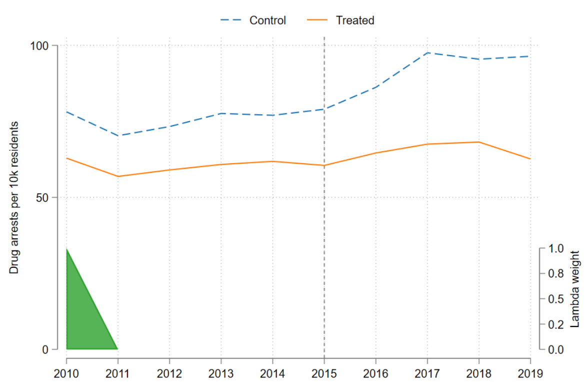
Figure B.2: Synthetic difference-in-differences results: event studies and trends - long panel



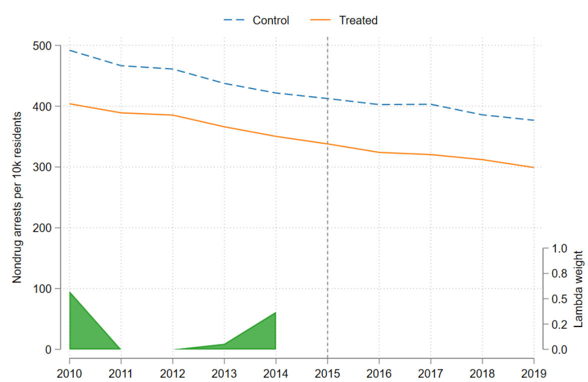
(a) Drug arrests, event study



(b) Nondrug arrests, event study



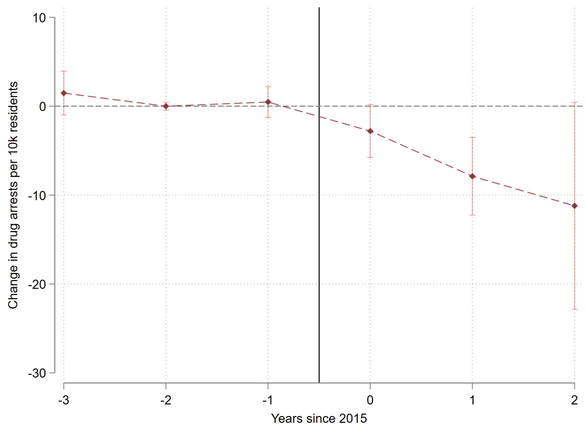
(c) Drug arrests, synthetic DiD trends



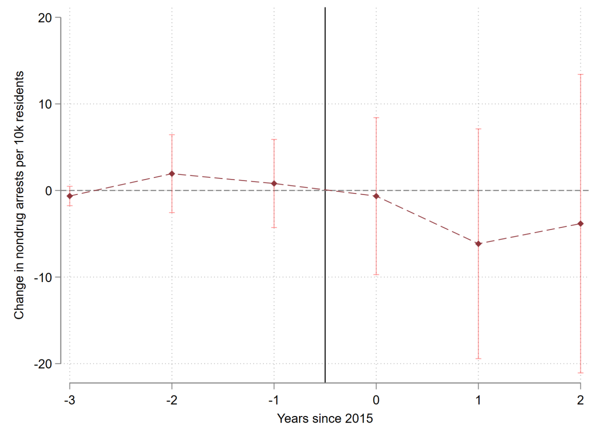
(d) Nondrug arrests, synthetic DiD trends

These figures display synthetic difference-in-differences results following methodology outlined in Arkhangelsky et al. (2021) using a longer panel of agencies from 2010 through 2019 (1,189 agencies for a total of 11,890 observations). Figures (a) and (b) display synthetic DiD event study coefficients estimating the effect of the 2015 suspension of adoptions on drug arrests and nondrug arrests per 10,000 jurisdiction residents, respectively. Dots represent point estimates, bars represent 95% confidence intervals. Figures (c) and (d) show time trends for treatment and control groups (defined as participating vs nonparticipating agencies as per Section 4) for the synthetic difference-in-differences results displayed in columns 6 and 8 in Table B.2, as well as time weights for each model. Standard errors are estimated via bootstrap with 50 iterations, and all specifications adjust for county-year demographic co-variates using the optimized method. ATT estimates for all models are displayed in Table B.2.

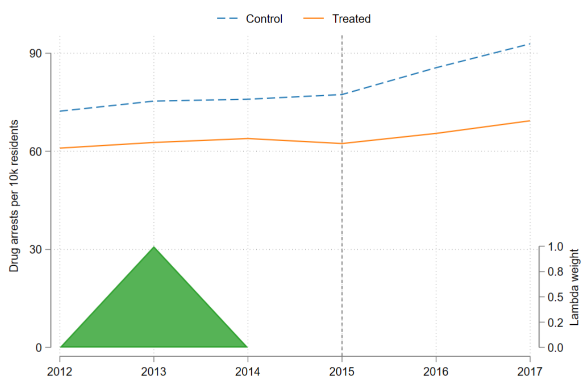
Figure B.3: Synthetic difference-in-differences results: event studies and trends



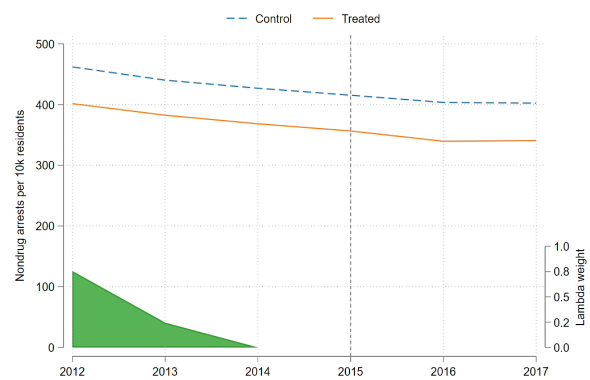
(a) Drug arrests, event study



(b) Nondrug arrests, event study



(c) Drug arrests, synthetic DiD trends



(d) Nondrug arrests, synthetic DiD trends

These figures display synthetic difference-in-differences results following the methodology outlined in Arkhangelsky et al. (2021) using a shorter balanced panel of agencies from 2012 through 2017 (1,752 agencies for a total of 10,512 observations). Figures (a) and (b) display synthetic DiD event study coefficients estimating the effect of the 2015 suspension of adoptions on drug arrests and nondrug arrests per 10,000 jurisdiction residents, respectively. Dots represent point estimates, bars represent 95% confidence intervals. Figures (c) and (d) show time trends and weights for treatment and control groups (defined as participating vs nonparticipating agencies as per Section 4) for the synthetic difference-in-differences results displayed in columns 2 and 4 of Table B.2. Standard errors are estimated via the bootstrap with 50 iterations, and all specifications adjust for county-year demographic covariates using the optimized method. ATT estimates for all models are displayed in Table B.2.

Table B.2: Synthetic difference-in-differences results: ATT estimates

	Short panel				Long panel			
	Drug	Drug	Nondrug	Nondrug	Drug	Drug	Nondrug	Nondrug
Diff-in-diff	-6.909**	-6.969*	-1.809	-2.058	-11.000*	-11.678	3.442	2.955
	(2.895)	(3.951)	(5.989)	(4.376)	(5.935)	(7.559)	(7.016)	(7.009)
Covariates?	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	10512	10512	10512	10512	11890	11890	11890	11890
Agencies	1752	1752	1752	1752	1189	1189	1189	1189

This table displays average-treatment-on-the-treated (ATT) estimates of the adoptions suspension using a synthetic difference-in-differences design (Arkhangelsky et al. 2021). The first four columns display results using a shorter agency-year panel from 2012 through 2017. The last four columns displays results using a longer panel of agencies from 2010 through 2019. Within each short vs. long panel heading, the first two columns display results using drug arrests per 10,000 jurisdiction residents as the dependent variable, and the last two columns display results using nondrug arrests per 10,000 residents as the dependent variable. All panels are strongly balanced. Covariates are adjusted for using the optimized method, and standard errors are bootstrapped using 50 replications. *, **, and *** denote $p < .10$, $p <.05$, and $p < .01$., respectively.

B.4 Poisson Estimates

I re-estimate the baseline specifications in the paper using a Poisson model for arrest and crime counts, including log jurisdiction population as an exposure (offset) term, rather than modeling outcomes as per-capita rates (Table B.3). The resulting treatment effects closely track the baseline estimates for drug arrests arrests, while nondrug arrests also exhibit a similar dip using the baseline and state-year fixed effects, but it disappears upon the addition of county-year fixed effects. This pattern suggests the main findings for drug arrests are not an artifact of the linear-in-rates functional form, while the null effect observed for nondrug arrests is more sensitive to functional form.

Table B.3: Poisson Estimates: Participating vs Nonparticipating

	Baseline	+StateYr FE	+CountyYr FE
A: Drug Arrests			
Participating × Post	-0.111***	-0.099***	-0.064**
	(0.031)	(0.032)	(0.033)
Officers Per 10k Resident	0.000	0.000	-0.000
	(0.002)	(0.001)	(0.001)
JI Revenue Per 10k Residents	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
B: Nondrug Arrests			
Participating × Post	-0.044**	-0.048***	-0.033
	(0.020)	(0.018)	(0.023)
Officers Per 10k Resident	-0.001	-0.002	-0.002*
	(0.001)	(0.001)	(0.001)
JI Revenue Per 10k Residents	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
C: Violent Crime			
Participating × Post	0.043*	0.038**	0.039
	(0.022)	(0.017)	(0.028)
Officers Per 10k Residents	-0.004**	-0.004***	-0.004***
	(0.001)	(0.001)	(0.001)
JI Revenue Per 10k Residents	-0.000	-0.000*	-0.000*
	(0.000)	(0.000)	(0.000)
D: Property Crime			
Participating × Post	0.013	0.011	-0.019**
	(0.015)	(0.010)	(0.009)
Officers Per 10k Residents	-0.003**	-0.003**	-0.004***
	(0.002)	(0.001)	(0.001)
JI Revenue Per 10k Residents	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Agency & Year FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	No
County-Year FE	No	Yes	Yes
Covariates	Yes	Yes	Yes
Obs.	24,769	24,747	18,596
Agencies	3,390	3,388	2,641

Each column reports difference-in-differences estimates from a Poisson pseudo-maximum likelihood (PPML) regression using agency-by-year data. The dependent variable is the count listed in the panel heading: (A) drug arrests, (B) nondrug arrests, (C) violent crime incidents, and (D) property crime incidents. All specifications include an exposure term equal to log population, so coefficients can be interpreted as effects on per-capita rates. All models control for officers per 10,000 residents, joint investigation revenue per 10,000 residents, and county demographic controls (see text). Column (1) includes agency-year fixed effects; column (2) adds state-by-year fixed effects; column (3) adds county-by-year fixed effects. Standard errors are clustered at the agency level. *, **, and *** denote <0.10, < 0.05, and <0.01