IS SHARING REALLY CARING?

ESTIMATING THE EFFECTS OF FEDERAL ASSET FORFEITURE REVENUE SHARING ON LOCAL POLICING OUTCOMES

by

Jadon Jediah Buzzard

A thesis submitted in partial fulfillment
of the requirements for the degree

of

Master of Science

in

Applied Economics

MONTANA STATE UNIVERSITY
Bozeman, Montana

May 2023

©COPYRIGHT

by

Jadon Jediah Buzzard

2023

All Rights Reserved

DEDICATION

I dedicate this work to my loving wife Amelia, who both emotionally and intellectually supported me over the course of this endeavour. My dedication also extends to my young daughter Eleanor Jane, whom I love and cherish.

ACKNOWLEDGEMENTS

This research would not have been possible without the insightful guidance of my thesis chair, Isaac Swensen. From beginning to end, Isaac's helpful comments and ideas allowed me to form a clear picture of how I wanted this work to develop, and for that I am quite thankful. I would also like to acknowledge Wendy Stock and Brock Smith for their assistance during the research process. Their keen intuition and thoughtful remarks substantially improved the quality of the statistical analysis herein.

I am also thankful for my fellow master's students: Brock O'Brien, Katie Flavin, Trevor Vogel, Sadiq Salimi, Laura Sikoski, Hannah Brauch, Justin Reis-Henrie, Taurey Carr, and Olivia Hayes. I will always consider you all to be close friends, and I wish you success in all of your future endeavours.

Lastly, I am thankful to my undergraduate advisor Charles Steele for his formative advice. I would not be in a position to offer my own contributions to economic research without his strong encouragement to pursue the discipline in a more robust way.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF TABLES – CONTINUED

LIST OF FIGURES

ABSTRACT

Civil asset forfeiture, whereby police agencies may profit from seized assets without a criminal conviction, is a contentious practice. Despite high-profile instances of abuse, law enforcement has made strong claims that forfeiture provides a critical funding mechanism for police departments. This paper offers a unique strategy to identify the causal relationship between asset forfeiture revenue and local policing outcomes, measured by crime reports and clearances (a standard proxy for police effort). I estimate the impact of a temporary suspension of Equitable Sharing, a program allowing local police agencies to financially benefit from asset forfeitures done in collaboration with federal law enforcement. The suspension was a plausibly exogenous shock to the forfeiture revenue received by participating police agencies. I exploit pre-suspension variation in program participation to study this interruption as a quasi-experiment; using a difference-in-differences design, my model estimates the differential impact of the suspension on participating agencies (treated) relative to non-participating agencies (control). My results indicate that the suspension led to a 4.7% increase in the number of violent crimes reported within participating agency jurisdictions relative to the baseline mean, but it also offers suggestive evidence of a small (2.5%) decrease in property crime reports as a result of the suspension. These effects appear to cancel out, producing a consistent null effect on total crime reports. While my results for violent crime are quite robust, the results for property crimes are more sensitive to model specification. My results for crime clearances also turn out to be inconclusive; as such, further research is required to determine whether the suspension's impact on crime reports stems from a change in police effort or an alternative explanatory mechanism.

INTRODUCTION

In 2019, single mother Stephanie Wilson had two of her vehicles seized by the Detroit Police Department.[1] In both instances, police stopped Stephanie and seized her vehicle while she was giving a ride to an ex-boyfriend. A miscommunication with the prosecutor's office caused Stephanie to miss the deadline to contest the seizure of her first car, which was never returned to her. Stephanie's second vehicle was returned only after months of legal proceedings, during which time she had to rely heavily on friends and family for transportation. Neither Stephanie nor her ex-boyfriend were charged with a crime in relation to these forfeitures.

Cases such as Stephanie's have incited strong scrutiny of civil asset forfeiture for over twenty years. The standard of proof required to engage in civil asset forfeiture is quite lenient in many states, and most allow police agencies to subsume the cash value of the forfeited property into their own department budgets. Critics argue that this sets up an incentive system that encourages overuse. Despite the pushback, forfeiture has been steadily increasing in the US since the early 2000s (Kelly and Kole 2016), with combined measures of state and federal asset forfeiture more than tripling since 2002 (Knepper et al. 2020). As justification, law enforcement representatives have argued that the revenue generated from asset forfeiture is a critical funding mechanism for police departments, and that regulating the practice would result in poor policing and unsafe communities (Cunningham et al. 2015; Worrall 2001; Snyder 2021). Empirical research testing this claim is limited, since state-level reporting requirements are often non-existent or unenforced. Forfeiture and crime also suffer from a reverse-causality problem, since forfeiture is most often used in high-crime areas, but

[1] https://ij.org/report/policing-for-profit-3/pfp3content/introduction/

crime could also be a function of how frequently forfeiture is used within a given jurisdiction.

This study contributes to the literature by examining the impact of an exogenous interruption in asset forfeiture funds on crime and clearance rates.[2] [3] To do this, I estimate the causal impact a 2016 suspension of Equitable Sharing, which is a federal program that establishes revenue sharing procedures in forfeiture cases involving collaboration between local and federal law enforcement agencies. Forfeiture payments from Equitable Sharing were suspended for approximately the first three months of 2016 due to two Congressional budget cuts to the Department of Justice's Asset Forfeiture Program (AFP) in late 2015. As a result, no local agencies could receive funds from the Equitable Sharing program from January 2016 through March 2016, though they could still collaborate with federal agencies in forfeiture cases. The wide variation in the extent and frequency of police participation in Equitable Sharing allows me to study the suspension as a quasi-experiment, since a police department's response to the suspension is likely a function of 1) whether they had received Sharing funds in the past, and 2) how frequently they participated. I hypothesize that regularly-participating agencies (treated) altered their purchasing due to the decrease in funds available, while non-participating agencies (control) were unlikely to have changed their behavior.

Importantly, no indication was given to local police regarding the temporal extent of the suspension. Police were in the dark as to when, or if, payments would resume. This uncertainty makes it likely that local police observably altered their behavior in order to adapt to the reduction in forfeiture funds, since the suspension could extend indefinitely. In fact, immediately following the suspension announcement, law enforcement groups raised strong concerns that the lack of Equitable Sharing funds would render their communities

[2] Almost all crimes that are cleared by police are done so via arrest; as such, it is helpful to think of a clearance rate as equivalent to an arrest rate.

[3] Within this paper, I use the term "rate" in the context of the dependent variables to mean an aggregate count divided by population.

unsafe (Cunningham et al. 2015). Additionally, the DoJ made it clear that local collaboration with federal agencies would remain open through the course of the suspension period. While avenues of local-federal collaboration remained available for cases involving forfeiture, no monetary compensation was dispersed to local agencies. This allows me to isolate my study to the effects of the negative monetary shock that local police experienced as a result of the interruption in Equitable Sharing funds.

In the baseline model for this paper, I use a difference-in-differences framework to evaluate the average differential impact of the Equitable Sharing suspension on crime and clearance rates within participating agency jurisdictions. Across all models, treated agencies are those that participated at varying levels in the Equitable Sharing program in the six years prior to the suspension. My first specification uses a simple binary treatment variable which indicates whether an agency received Equitable Sharing funds in the pre-suspension period. This will yield the average effect of the Equitable Sharing suspension on participating agencies (treated) relative to non-participating agencies (control). My second baseline specification leverages variation in participation frequency within the treatment group to study heterogeneity in treatment intensity. I use a categorical treatment variable that marks whether an agency is above or below the median in terms of the number of times an agency received money from Equitable Sharing in the pre-suspension period. I also incorporate a dynamic specification to explore treatment timing, agency anticipation, and possible lingering effects after the suspension. My dynamic model examines the months leading up to, within, and after the suspension period, all relative to a pooled average of all months prior to August 2015.

The results demonstrate robust evidence that the population-adjusted number of violent crime reports increased within participating-agency jurisdictions during the suspension relative to the control. Specifically, baseline regression results indicate that treated jurisdictions experienced approximately 4.7% more violent crime reports as a result of the

Equitable Sharing suspension. The models also give suggestive evidence of a small (2.5%) decrease in property crime reports within the suspension, but these effects are more sensitive to model specification. The net result is a statistically insignificant effect of the suspension on total crime in most models. Further, I observe a statistical null effect of the suspension on clearance rates, which is often interpreted as a proxy for policing effort. This is surprising, since the extreme backlash from the law enforcement community might lead one to expect an observable effect on policing effort for participating agencies (Cunningham et al. 2015). Assuming police agencies are economically rational, theory predicts that income disruptions will shift the budget curve inward, reducing the quantity of police services purchased. In Chapter 6, I offer several interpretations for these results, as well as potential areas of future research to assist with identifying which is the most plausible.

BACKGROUND

This chapter will present important background information on asset forfeiture as a practice, as well as on the Equitable Sharing Program in particular.

General Background on Asset Forfeiture

Asset forfeiture is legally delineated between civil asset forfeiture and criminal asset forfeiture. In the United States, civil asset forfeiture is considered *in rem* ("against a thing") proceedings, meaning that the forfeiture action is considered not against a person, but the seized object.[1] The consequence of this designation is that the property owner need not be implicated in the illegal act with which the property is associated. This substantially lowers the legal burden of proof required in most courts for law enforcement to engage in civil asset forfeiture. Because it requires no proof of owner involvement, law enforcement entities need only "probable cause" or "reasonable suspicion" of the seized property's connection to a crime—a much lower legal burden than the "beyond reasonable doubt" that is required in criminal convictions. Criminal forfeiture, on the other hand, is legally considered an action against a property owner, which requires a criminal conviction and the aforementioned legal burden.[2]

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.

[1] These seized assets predominantly take the form of cash, but they can be vehicles, houses, boats, or other items of property as well.

[2] Civil forfeiture is far more common than criminal forfeiture, so I hereafter use the term "asset forfeiture" to mean "civil asset forfeiture" in this paper.

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.[3]

To date, economic research on the relationship between asset forfeiture and policing has been mixed. Several studies have explored the direct effects of forfeiture on aspects of law enforcement behavior such as police effort (Kelly and Kole 2016; Gius 2018), policing expenditures (Benson et al. 1995; Mast et al. 2000), and crime (Campbell et al. 2015). Both Benson et al. and Mast et al. show evidence that asset forfeiture funds strongly incentivize police to engage in drug arrests, but Kelly and Kole, Gius, and Campbell et al. find little-to-no effects of forfeiture on clearances and crime rates. One explanation for these inconsistencies is that municipalities could be adjusting police budgets to account for shortfalls in revenue caused by less forfeiture, a phenomenon documented by Baicker and Jacobson (2007). The authors show that municipal governments may allocate more funding to police in years when asset forfeiture revenue is lower. Their results reveal that police only respond to the net decrease in funds after accounting for changes in their budgets from the response by municipal governments to shortfalls in forfeiture revenue. Similarly, according to Mughan et al. (2020), the incentive effects of forfeiture vary depending on whether the forfeiture is conducted by an elected versus an appointed agency. Analyzing a sample of large police and sheriff departments, the authors find that sheriff departments receive less forfeiture revenues and are less responsive to state forfeiture regulation than police agencies. This indicates that strong forfeiture incentives are likely clustered in urban environments relative to rural environments.

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

[3]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.

entities within a region. The author posits that police confiscation creates capital-inhibiting spillovers which reduces 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 overpoliced to compensate.

My paper contributes the literature by being the first to test the impacts of an exogenous shock to forfeiture receipts on measures of crime and police effectiveness using a nation-wide police agency panel. Most of the current literature uses pooled OLS, lagged forfeiture receipts, or cross-sections, which suffer from issues of reverse causality and unobserved agency heterogeneity. There are wide systematic differences between police agencies in terms of the jurisdictions they serve and their internal procedures. Agency size, budgets, average effort levels, internal politics, corruption, and propensity to use forfeiture are all unobserved characteristics that are plausibly correlated with both policing outcomes and the propensity to participate in asset forfeiture. State or county-level analysis fails to account for these systematic differences between agencies that use asset forfeiture regularly within a single county or state. To resolve these issues, my project uses agency-level monthly data to measure the impact of an exogenous suspension in Equitable Sharing, which will help resolve the reverse causality problem. Additionally, this allows me to incorporate the use of agency-by-month fixed effects, accounting for both unobserved heterogeneity across agencies and the seasonality present in monthly crime data.

Federal Forfeiture and Equitable Sharing

Before exploring the Equitable Sharing program in particular, this section will cover the core institutional details regarding asset forfeiture procedures at the federal level.

The DoJ's Asset Forfeiture Program (AFP)

The Department of Justice established the Asset Forfeiture Program (AFP) pursuant to the Comprehensive Criminal Control Act of 1984. The AFP created the Asset Forfeiture Fund (AFF), which functions as a centralized pool of all monetary returns from asset forfeiture conducted by federal agencies.

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 Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF), the Drug Enforcement Administration (DEA), and the Federal Bureau of Investigations (FBI). 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, 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 seizures of cars, houses, stocks, bank accounts, and other types of property that has 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 Marshalls Service is exclusively tasked with the evaluation of market value and selling of forfeited, non-cash capital assets.

The procedures governing federal asset forfeiture are relatively ubiquitous. According to analysis done by the Institute for Justice, the vast majority of forfeitures conducted by federal agencies are considered administrative (Knepper et al. 2020).[4] Even fewer legal protections for property owners exist under administrative forfeiture than under civil forfeiture. In administrative forfeiture, a federal agency 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 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).

Equitable Sharing Program

Federally instituted in the Comprehensive Criminal Control Act of 1983, the Equitable Sharing program establishes collaboration procedures for local police to work together with federal agencies specifically in cases which involve seized property. When collaboration happens, forfeiture revenue is often shared between federal and state agencies (hence the program's name). After a forfeiture occurs under Equitable Sharing, a local agency may request their shared proportion of forfeiture funds by submitting a DAG-71 form.

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

[4] Administrative forfeiture can best be characterized as a type of civil forfeiture, with fewer restrictions.

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.

A small body of economic scholarship explores the Equitable Sharing program in particular. Several authors document that the Equitable Sharing program may be used as a loophole for local police to skirt state level regulation. For example, both Worrall and Kovandzic (2008) and Holcomb et al. (2018) find that police are more likely to use federal adoptions through the Equitable Sharing program in states where regulation on forfeiture is harsher. Billy (2020) evaluates a change to a processing rule within the Equitable Sharing program that strongly limited sharing adoptions. Contrary to previous results, Billy finds that police did not substitute into other forms of revenue generating activity, measured by state-level asset forfeiture and vehicle speeding tickets. The author argues that this may be due to possible loopholes in state regulation on the practice.[5]

[5] More recent research gives an alternative explanation for the null result. The Institute for Justice reports that adoptions policy change to Equitable Sharing was vaguely worded, not enforced well, and only applied to adoptions, not joint investigations, which make up the bulk of Equitable Sharing payments (Knepper et al. 2020). As such, there may have been no true change to the amount of forfeiture revenue received by local police.

Temporary Suspension of Equitable Sharing Payments

On December 21, 2015, the Department of Justice (DoJ)'s publication called *The Equitable Sharing Wire* announced that that payments from the Equitable Sharing program would be suspended and deferred, effective immediately (DoJ 2015). To justify the policy change, the *Sharing Wire* report cited a combination of both the Bipartisan Budget Act of 2015 (P.L. 114-74), enacted in November of 2015, and the Consolidated Appropriations Act of 2016, enacted on December 18th, 2015. The Bipartisan Budget Act included a provision which slashed funds from the DoJ's Asset Forfeiture Program referenced earlier, including a $746 million-dollar permanent rescission from the Asset Forfeiture Fund. The *Sharing Wire* adds that, after the act passed in November, the DoJ had intended to "[continue] to make equitable sharing payments but at a reduced amount" (DoJ 2015). However, the Consolidated Appropriations Act included an additional $458 million cut to the Asset Forfeiture Program's funds for the fiscal year of 2016, making the combined effect of the two acts to lower the Asset Forfeiture Fund by approximately $1.2 billion. This necessitated the DoJ to defer all Equitable Sharing payments to local agencies until further notice.

The identification strategy used within this paper exploits this suspension of Sharing payments in order to measure the effect of forfeiture funds on measures of crime and police effort. As such, it is important to note several elements of this *Equitable Sharing Wire* report. First, the report explicitly states, "we preserve our ability to resume equitable sharing payments at a later date should the budget picture improve," but it does not give any form of a timeline that could shed light on when, or if, payments would resume (DoJ 2015). The general language of the report indicates that the DoJ did not know when local agencies could expect to receive payments again. The report says there is a possibility that Equitable Sharing requests made in late 2016 could be paid, either partially or in full, but that this is dependent on a variety of budgetary factors that the DoJ cannot predict (DoJ 2015). The report also notes that DAG-71 requests already made before this announcement would be honored, but any future requests after the announcement would not.

On January 6th of 2016, the DoJ released a fact sheet further clarifying their expectations regarding the deferral of Equitable Sharing funds (DoJ 2016). The language within this fact sheet is largely the same as it is in the primary announcement, but it does further expound that the DoJ expects the deferral to be temporary. The fact sheet also makes it clear that the deferral of payment from the Equitable Sharing program should not be taken to mean that federal agencies will no longer collaborate with local agencies in cases involving asset forfeiture. The policy change would only affect payments out of the Asset Forfeiture Fund as a part of the Equitable Sharing Program; it would not impact the ability for local police to collaborate, nor would it abolish or end Equitable Sharing entirely.

MOTIVATION FOR STUDY

This chapter will develop several arguments in favor of using the Equitable Sharing suspension as a method for identifying a causal link between asset forfeiture payments and policing behavior.

Law Enforcement Advocacy

Immediately following its announcement in the *Equitable Sharing Wire*, the DoJ received intense pushback from local law enforcement. One of the most significant and strongly-worded advocacy statements against the deferral of Equitable Sharing appears in a letter to President Obama, signed by the executives and presidents of the six major local law enforcement organizations in the United States (Cunningham et al. 2015).[1] The letter, dated just two days after the DoJ's announcement on December 21, 2015, presents itself as representing the aforementioned organizations, and it argues that the DoJ's decision to suspend Equitable Sharing payments "will have a significant and immediate impact on the ability of law enforcement organizations throughout the nation to protect their communities and provide their citizens with the services they expect and deserve" (Cunningham et al. 2015). The letter argues that the payments had allowed local law enforcement to purchase necessary crime-fighting equipment and deprive criminal organizations of their resources. As a result of the lack of payment, the letter indicates that local law enforcement may "reconsider their ability to participate" in future joint endeavors with the federal government (Cunningham et al. 2015).

[1]The individuals who signed this letter include Terrence Cunningham, President of the International Association of Chiefs of Police; William Johnson, Executive Director of the National Association of Police Organizations; Donny Youngblood, president of the Major County Sheriffs' Association; Jonathan Thompson, Executive Director and CEO of the National Sheriffs Association; William Fitzpatrick, president of the National District Attorney's Association; and Thomas Manger, president of the Major Cities Chiefs Association.

Importantly, the letter also expresses disappointment at the DoJ's lack of communication with local law enforcement prior to the suspension. The letter specifically expressed concern at the "failure of Congress and the Department of Justice to consult with its state and local partners before taking this drastic step of a program of such critical importance" (Cunningham et al. 2015). This adds evidence to the assumption that there were no anticipation effects by local agencies, which is important to the legitimacy of a causal interpretation for the results of this paper. If police agencies anticipated a future decline in revenue, they might have taken action that reduced the burden of the budget shortfall in the pre-suspension period. This could wipe out any effects observed within the suspension. Fortunately, because the DoJ did not consult with any local agencies before making their decision, this is unlikely to be a concern.

Other statements echo the sentiments described in that initial letter. For example, in a statement to the Washington Post, the president of the Institute for Justice (Scott Bullock) said, "[w]e are seeing a lot more pushback from law enforcement. Even to the point where they are ... making budgetary appeals saying, 'we need this for our bottom line'" (Ingraham 2016). Bullock indicated that he believed law enforcement agencies to be primarily concerned about the funding they received from Equitable Sharing, at the expense of the lax legal standards that come with working with federal agencies on cases involving asset forfeiture (Ingraham 2016).

The pushback against the Equitable Sharing decision was not limited only to local law enforcement agencies; it included government officials as well. On February 26, 2016, former governor of New Hampshire Maggie Hassan wrote in a letter to then- Attorney General Loretta Lynch, "[w]ithout these payments, local and state law enforcement simply will not have the resources they need to keep our communities safe" (Morris 2016). The former governor's letter focused primarily on drug crimes, arguing that the Equitable Sharing program was key to taking down drug cartels and combatting the opioid epidemic. Other government officials in New Hampshire have echoed Governor Hassan's sentiments. For example, in a bipartisan push to have Equitable Sharing funds reinstated, New Hampshire Sens. Kelly Ayotte (R) and Jeanne Shaheen (D) both wrote letters to the DoJ requesting that payments resume (Morris 2016).[2]

Perhaps, rather than demonstrating the true importance of Equitable Sharing funds to community safety, these statements only reflect the rent-seeking nature of police agencies and those affiliated with them. However, organizing such intense advocacy efforts in such short order is quite difficult. Given that the letter was signed by organizations representing nearly all local law enforcement in the United States, these opponents would have needed to generate costly political energy, to draft, sign, and send a letter to President Obama in a mere two days after the suspension announcement. If, however, police believed that the suspension would hamper their day-to-day activities, it is plausible that these organizations would have been able to procure the energy needed for such drastic action.

Policing and Crime Literature

Regardless of advocates' claims, there may be good theoretical reasons to expect that an exogenous suspension in policing revenue would increase crime. In the standard model, one can consider each policing agency to be perfectly rational, making purchasing decisions to maximize expected utility. Each agency faces a budget constraint, purchasing a bundle composed of policing services (officer salaries, weapons purchases, joint investigations, etc.) and all other purchasing options (such as rents, office supplies, and food). Since the Equitable Sharing program offered supplemental funding to police agency budgets, a sudden reduction would, *certerus paribus*, cause an inward shift of the budget constraint, forcing the police

[2]Note that the concern expressed by New Hampshire government officials is understandable, since the state ranked second in the nation in 2016 for the number of population-adjusted opioid deaths, according to CDC statistics (Leins 2016).

agency to alter the composition of their purchased bundle. Assuming that policing is a normal good (i.e., that policing services and budgets are directly related), the new bundle should contain fewer policing services.

There is a some empirical evidence that police agencies are sensitive to changes in their overall budget. For example, Beck et al. (2023) finds that the decline in misdemeanor arrest rates in the United States after peaking in the 1990s was primarily due to the changes in agency budgets and ensuing staffing shortages. In the context of asset forfeiture in particular, Baicker and Jacobson (2007) use police seizure and expenditures data to demonstrate that police agencies are highly sensitive to net budget changes from forfeiture shortfalls, accounting for adjustments by the local municipality.

Would a reduction in policing services increase criminal behavior? The current empirical evidence robustly answers this question in the affirmative (Chalfin and McCrary 2017; Apel and Nagin 2011). Most of the evidence in favor of the deterrence hypothesis examines whether police presence, as measured by 1) proximity to a crime, and 2) total number of officers on the street, is associated with less criminal behavior. Overall, the crime literature shows a relatively strong inverse association between an officer's proximity to a crime, corresponding response time, and the average level of criminal behavior within a given jurisdiction (Weisburd 2021; Vidal and Kirchmaier 2018; Klick and Tabarrok 2005). The literature also generally shows a strong deterrence effect of funding tied to more police within a given jurisdiction (Mello 2019; Evans and Owens 2007). Thus, if the suspension of Equitable Sharing funds did reduce the amount of policing within the treatment group, it is plausible that it increased local measures of criminal activity as well.

Equitable Sharing Requirements

The legal restrictions on the use of Equitable Sharing funds could increase our confidence that the suspension led to reduced policing. The document that outlines the requirements for local police to participate in the Equitable Sharing program is the DoJ's *Guide to Equitable Sharing for State, Local, and Tribal Law Enforcement Agencies* (DoJ 2018). This twenty-nine-page guidebook contains all of the relevant restrictions that the DoJ places on money spent from Equitable Sharing, including both proper and improper uses, reporting and logging requirements, and audit requirements.

Generally, the DoJ requires that all Equitable Sharing funds "be used by law enforcement agencies for law enforcement purposes only" (DoJ 2018). The *Guide* gives eleven permissible uses for Equitable Sharing funds, including: law enforcement operations and investigations; law enforcement training and education; facilities for law enforcement, public safety, and detention; law enforcement equipment; joint law enforcement public safety operations; contracts for services, law enforcement travel and *per diem* costs (those associated with travel on official law enforcement business); law enforcement awards and memorials; drug, gang, and other prevention or awareness programs; matching grants (DoJ 2018). Each type of permissible use is further expounded upon in the document.

The *Guide* also outlines twelve impermissible uses, including: use of forfeited property by non-law enforcement personnel; creation of endowments or scholarships; uses contrary to state or local laws; personal or political uses of shared assets; food and beverages; extravagant or wasteful expenditures or entertainment; cash on hand, secondary accounts, and stored value cards; transfers to other law enforcement agencies; purchases of items for other law enforcement agencies; costs related to lawsuits, loans, money laundering operations; salaries (DoJ 2018). While the DoJ prohibits the use of Equitable Sharing funds for specific agents' salaries, the document includes several exceptions to this policy. The two primary exceptions are for overtime payments to officers, as well as salaries for filling vacancies within federal-state crime taskforces. The two other exceptions to the salary prohibition include an exception for salaries of officers within specialized (non-standard) programs and matching federal grants.

These restrictions on the usage of forfeiture funds from Equitable Sharing have important implications for the identification strategy within this paper. A response by local police to the deferral of Equitable Sharing funds would likely take the form of reducing purchases that improve crime-fighting within police jurisdictions. Based on the literature cited earlier, I expect that a reduction of these purchases led to an increase in measurable criminal behavior. The second implication is that the reduction in Equitable Sharing money might be more likely to lead to a reduction in policing and an increase in crime than traditional state-level asset forfeiture. Since most states do not have the restrictions on the usage of forfeiture funds that the DoJ details in its *Guide*, it is possible that police officers are more likely to use forfeiture funds that are not acquired via Equitable Sharing in ways that are wasteful or on purchases that do not improve crimefighting capacity.

How would the DoJ know whether police agencies are using their Equitable Sharing funds for these designated uses? If police agencies are systematically failing to comply, then I might observe no change in policing outcomes as a result of the suspension of Equitable Sharing funds, since police may not be using the money for crimefighting purposes. However, the DoJ's *Guide* mitigates these concerns by outlining the accounting procedures that local agencies must follow to maintain compliance. Participating police agencies are required to keep scrupulous logs of how Equitable Sharing funds are spent. According to the *Guide*, they are required to establish "separate Department of Justice and Department of the Treasury accounts or accounting codes to track both revenues and expenditures for each respective Program" (DoJ 2018). Local agencies can be selected to undergo audit at any time by the DoJ or the Treasury, along with a host of other governmental agencies.

Identification Concerns

When evaluating the promise of my identification strategy, it is important to consider the length of the Equitable Sharing suspension. A critic might reasonably suggest that the suspension did not last long enough to noticeably change policing behavior. A three-month interruption in forfeiture funds may not be enough time to observe a substantial effect on policing outcomes. Had agencies been given an indication as to the length of the Equitable Sharing suspension, they may have chosen to simply wait it out instead of altering their behavior. However, as mentioned in Chapter 2, the DoJ did not reveal the length of the suspension period to law enforcement ahead of time. The DoJ eventually announced that the suspension would be lifted in late March, but the announcement came on the day that the suspension was lifted. Police departments were in the dark as to when (and if) payments would be reinstated. As such, despite the short time period, it is plausible that police departments would have altered the amount of policing services purchased to account for a potentially extended cash shortfall.[3] However, note that I cannot rule out that three months was simply too short a time to produce observable effects on crime even if police observably altered their behavior.

An additional concern is that local municipalities might be adjusting the level of funding within these police departments by an amount similar to the shortfall from the Equitable Sharing program. This could nullify the likelihood of observing any effects within the three month suspension window. As mentioned earlier, this is a phenomenon that has been documented by Baicker and Jacobson (2007), and it may be why previous studies using state-level asset forfeiture which did not account for budget adjustments may have produced biased results. However, many local municipalities allocate their budgets yearly. A three-month suspension would not extend long enough in order for many local municipalities to increase police funding as within the normal-means budgeting process. Further, as per the Municipal Research and Services Center (MRSC), budgets are generally planned and drafted in the summer and fall months (MRSC 2022). The December announcement by the DoJ

[3]In addition, other policy changes recently, as well as political changes, were working against asset forfeiture at the time of the suspension. There was good reason to assume that the DoJ was "soft repealing" the policy, in which case the suspension would have lasted indefinitely.

came much too late for most local municipalities to adjust the FY 2016 budget. Another consideration that explicitly mitigates the budgeting issue is that the DoJ's *Guide* mentioned in Chapter 3 disallows local municipalities from adjusting police budgets in response to changes in Equitable Sharing program funds. In a section of the *Guide* titled "General Guidance on Supplantation and Budgeting," the document states that Equitable Sharing funds are meant only to supplement, not supplant, local police budgets. Local municipalities are not allowed to adjust police budgets by the amount of increase (or decrease) in Equitable Sharing money. Assuming that the threat of audit is enough to deter local agencies, then it is unlikely that such supplantation is occuring.

One plausible objection to this identification strategy is that law enforcement may substitute into other revenue-generating forms of policing in order to compensate for the Equitable Sharing shortfalls. This is especially concerning because forfeiture can happen at either a state or a federal level. Local police may substitute away from forfeitures processed under Equitable Sharing and into forfeitures processed within state courts, which could obscure any effects observed within the suspension period. While granular data on other revenue-generating forms of police activity are not available for use in this analysis, some previous literature may mitigate this concern. For example, as mentioned earlier, Billy (2020) finds no substitution into other revenue-generating forms of policing (including state-level asset forfeiture) as a result from a change to adoptions processing which reduced the number of adoptions from the program to zero. This should provide at least some evidence that including these controls may be unnecessary, though future development of this work should include them as a robustness check. An explanation for this may come from Holcomb et al. (2018), which shows that Equitable Sharing is used more in states that have stronger restrictions on revenue-generating practices. Agencies which received Equitable Sharing funds in the pre-suspension period are likely statutorily inhibited from engaging in asset forfeiture within their own states, which is why they are using Equitable Sharing in the first place.

It is true that this paper does not directly control for the substitution activity mentioned above. However, since it is expected that crime increased within treated jurisdictions relative to control during the suspension, the omitted variable bias caused by not including covariates such as other revenue-generating policing should push our estimates away from the expected outcome. As such, the worst-case scenario is that the estimates within this paper suffer from attenuation bias, and they are likely lower bounds on the true effect of the suspension.

DATA

Primary Data Sources

I use two primary data sources for this analysis. The first is the Department of Justice's *Consolidated Assets Tracking System (CATS)* which contains incidence-level data on federal forfeitures from 1993 to the present (DoJ 2021). CATS is a centralized archive for all federal law enforcement agencies to record instances of asset forfeiture, including the FBI, DEA, and US Marshalls Service. The incidence-level data is quite robust, recording almost every piece of information tied to a forfeiture upon entry. It also includes every forfeiture payment associated with the DoJ's Equitable Sharing program, including the name, state, city, and county of the local agency involved, and total monetary amount granted. I filter the CATS database to get agency-level data on the frequency and intensity of local police participation in the Equitable Sharing program. Since forfeiture is recorded at the incidence level, I aggregate this to get total dollar amount and number of receipts received by local agencies by month and year, from 2010 to 2020.[1]

Tables 4.1 through 4.6 present both weighted and non-weighted summary statistics describing the dollar amount of Equitable Sharing payments made before and after the suspension period. Table 4.1 shows that the average monthly payment made per agency during the suspension was approximately \$228.16. Compared to the average monthly payment in the pre-suspension period of \$8,734.23 in Table 4.2, it appears that the suspension reduced the average monthly payment per agency by approximately \$8,505.39, a 97% decrease relative to the pre-period. Table 4.3 shows that the average monthly payment during the months of January through March of the pre-suspension period was even larger than the whole-year average, which indicates that treated agencies were likely expecting their

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

January through March payment of 2016 to be even larger than they would for other months in that year. Tables 4.4 through 4.6 present the unweighted averages of these payments for comparison; I do not observe substantial differences between them.

Figure 4.1 plots the average amount of Equitable Sharing funds that were paid to participating police agencies in the months leading up to, within, and after the suspension period. Figure 4.2 plots the total amount of Equitable Sharing funds dispersed in the same timeframe. Both figures show relatively noisy fluctuation leading up to the suspension, then a steep drop in January, February, and March of 2016. In April, both figures show a large spike in payment before returning to a fluctuation that looks similar to the pre-suspension trend. The spike in payment within April likely appears because the Equitable Sharing suspension functioned as a deferral; agencies which filed DAG-71 forms within the suspension were paid after the suspension was lifted. Further, note that payments do not drop to zero because some agencies with already-existent pending requests were processed within the three months of the suspension. However, none of these agencies are included in my baseline panel.

Table 4.1: Equitable Sharing payments within-suspension: population-weighted

	N	Mean	SD	Min	Max
Avg. Monthly Sharing / Agency	5067	228.84	2992.32	0	84,389.58

Notes: The table above presents weighted summary statistics describing the dollar amount of Equitable Sharing payments paid per agency between January 2016 and March 2016 (the suspension period). All means are weighted by the primary jurisdiction population of the reporting police agency.

Table 4.2: Equitable Sharing payments pre-suspension: population-weighted

	N	Mean	SD	Min	Max
Avg. Monthly Sharing / Agency	115689	8,734.23	2.5e+05	0	59938076.00

Notes: The table above presents weighted summary statistics describing the dollar amount of Equitable Sharing payments paid per agency between January 2010 and December 2015 (the pre-suspension period). All means are weighted by the primary jurisdiction population of the reporting police agency.

Table 4.3: Equitable Sharing payments for Jan-Mar pre-suspension: population-weighted

	N	Mean	SD	Min	Max
Avg. Monthly Sharing / Agency	27533	12,606.20	5.1e+05	0	59938076.00

Notes: The table above presents weighted summary statistics describing the dollar amount of Equitable Sharing payments paid per agency in only the months of January, February, and March between January 2010 and December 2015 (the pre-suspension period). All means are weighted by the primary jurisdiction population of the reporting police agency.

Table 4.4: Equitable Sharing payments within-suspension: non-weighted

	N	Mean	SD	Min	Max
Avg. Monthly Sharing / Agency	9656	396.16	6448.80	0	419,756.60

Notes: The table above presents non-weighted summary statistics describing the dollar amount of Equitable Sharing payments paid per agency between January 2016 and March 2016 (the suspension period).

Table 4.5: Equitable Sharing payments pre-suspension: non-weighted

	N	Mean	SD	Min	Max
Avg. Monthly Sharing / Agency	221632	9,895.67	2.5e+05	0	59938076.00

Notes: The table above presents non-weighted summary statistics describing the dollar amount of Equitable Sharing payments paid per agency between January 2010 and December 2015 (the pre-suspension period).

Table 4.6: Equitable sharing payments for Jan-Mar pre-suspension: non-weighted

	N	Mean	SD	Min	Max
Avg. Monthly Sharing / Agency	52810	13,948.02	5.0e+05	0	59938076.00

Notes: The table above presents non-weighted summary statistics describing the dollar amount of Equitable Sharing payments paid per agency in only the months of January, February, and March between January 2010 and December 2015 (the pre-suspension period).

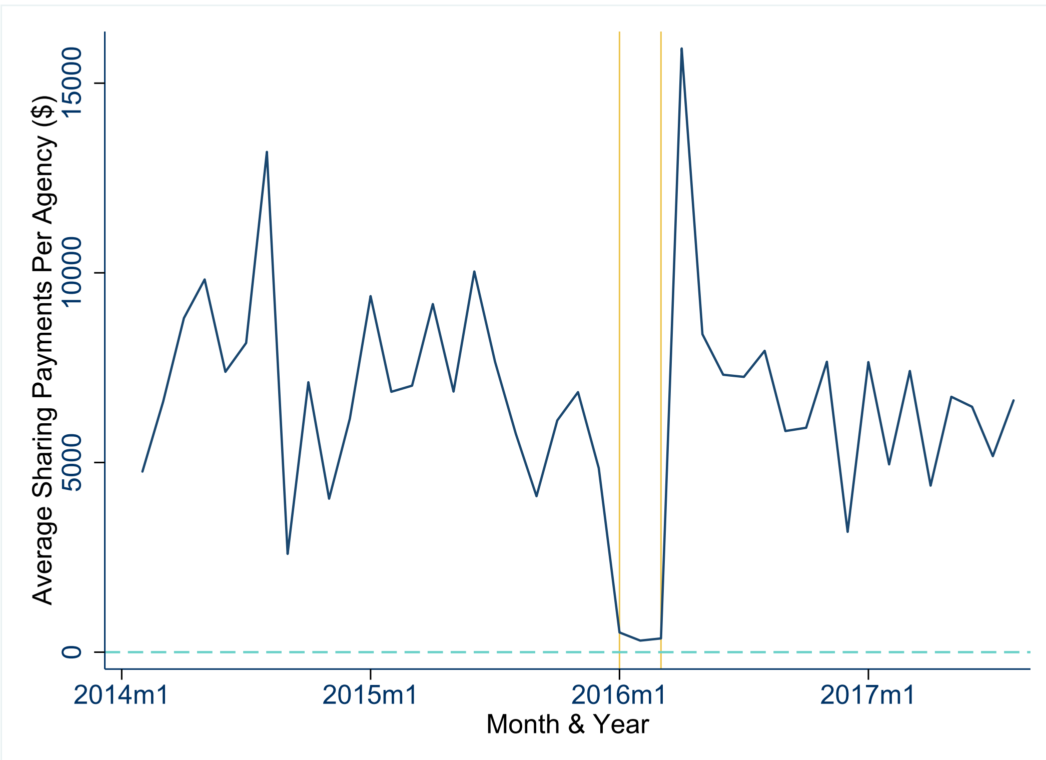

Figure 4.1: Average monthly Equitable Sharing payment made per agency

Notes: The graph above displays the average dollar amount of Equitable Sharing payments made per agency from the Asset Forfeiture Fund between the January 2014 and December 2017. The vertical lines mark roughly the beginning and the end of the Equitable Sharing suspension period, during which time no new Equitable Sharing requests could be processed.

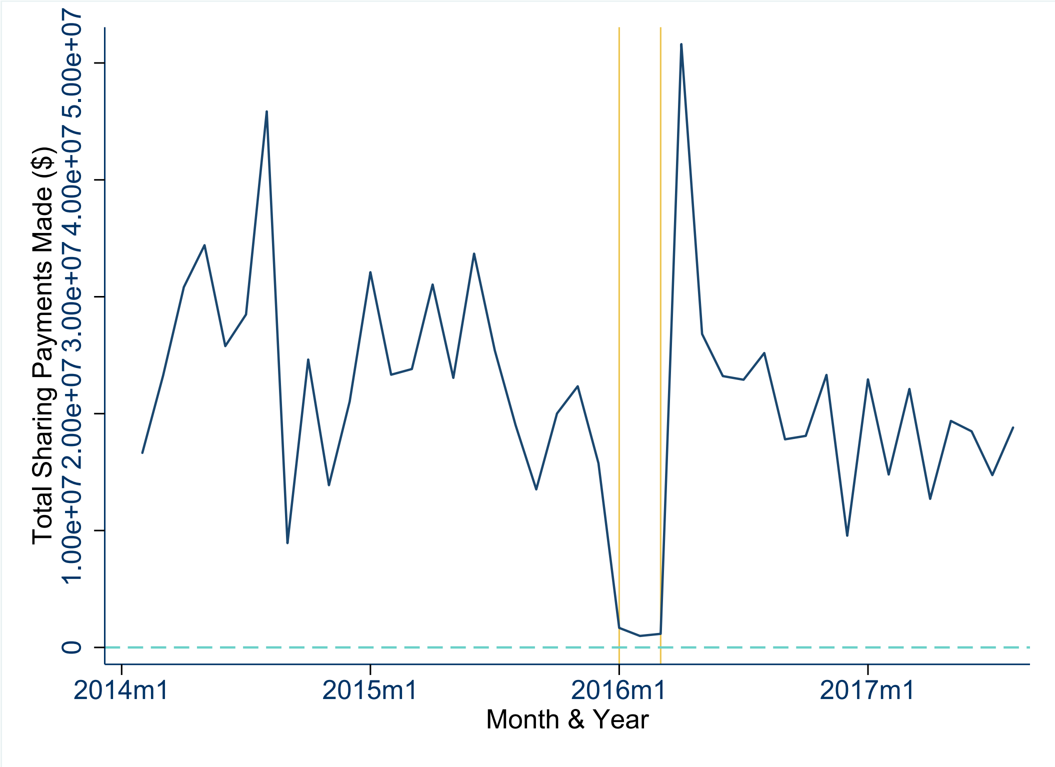

Figure 4.2: Total monthly Equitable Sharing payments made

Notes: The graph above displays the total dollar amount of Equitable Sharing payments made from the Asset Forfeiture Fund between the January 2014 and December 2017. The vertical lines mark roughly the beginning and the end of the Equitable Sharing suspension period, during which time no new Equitable Sharing requests could be processed.

The second primary data source I use is the FBI's *Uniform Crime Reporting (UCR) Program Data: Offenses Known and Clearances by Arrest* (Kaplan 2020).[2] The UCR database includes agency-level monthly data on total crimes reported and total crimes cleared from local and federal law enforcement in the US. Not every agency in the United States participates in the UCR program, and some that do report fail to do so every month. The types of crime that the UCR records are homicide, manslaughter, rape, robbery, assault, burglary, larceny-theft, motor vehicle theft, and arson. I limit the UCR sample to include the years 2010 through 2020, approximately six years before and five years after the Equitable Sharing suspension. I then filter the data to only include local police agencies that report for all twelve months of every year in the sample. I aggregate total crime reports and total clearances for each agency into three categories: total (all violations above), violent (homicide, manslaughter, rape, robbery, assault) and property (burglary, larceny-theft, motor vehicle theft, arson). Splitting my outcome variables between these crime categories is warranted because it is plausible that the suspension could have heterogeneous effects across different types of violations. Within my results section, I include heterogeneity analysis to determine whether the effects are driven by specific types of violations. Each agency in the UCR database has a unique identifier, which is also included in the CATS data above. This code allows me to merge the UCR and CATS databases to get an agency-by-month panel on crime reports and clearances for the categories above and monthly Equitable Sharing payments across my sample period. The UCR program also reports the number of residents in the agency's primary jurisdiction each month. I divide the crime reports and clearances by population to obtain crime and clearance rates, which are the dependent variables I use within this paper.

I use the CATS data in conjunction with the Bureau of Justice Statistics' *Law*

[2] I use the version of this data that was compiled and concatenated by Jacob Kaplan; see citation for project details.

Enforcement Agency Identifiers Crosswalk (BJS 2012) to split my treatment and control groups. The Crosswalk includes ORI codes, demographic information, and geographical details for all of the local police agencies in the FBI UCR database. To obtain a list of agencies that did not receive Equitable Sharing funds pre-suspension, I merged the Crosswalk with the CATS database for the years 2010 through 2015. Agencies without a match in the CATS data serve as my control, while agencies that do show a match participated at least once in the DoJ's Equitable Sharing program before the suspension, so they serve as my treatment group. Importantly, participation frequency in Equitable Sharing during the sample is likely an indicator for intensity of treatment. High-participation agencies could be more dependent on Equitable Sharing funds. If so, the 3-month suspension will have heterogeneous effects across the treatment group. I create a categorical treatment variable based the frequency of agency participation, which I use in additional specifications.

I include county-by-year economic and demographic controls in most of my model specifications. For this, I use data on real GDP per-capita in chained dollars and unemployment rates from the Bureau of Economic Analysis (BEA 2022). I also use county-year data on the proportion of citizens of various age and race demographics from the US Census Bureau.

My panel is naturally unbalanced because not all agencies that participated in Equitable Sharing reported to the UCR program during all 12 months of every year between 2010 and 2020. As mentioned earlier, I force-balance the panel in my baseline regressions by dropping all treatment and control agencies that do not have data reports for all 12 months of every year. Because many agencies report zeros for certain months of the year, I believe it is appropriate to only include agencies which consistently report through the whole panel. It is plausible that the zeros reported by these agencies are more likely to be "true zeros" than those reported by agencies which report inconsistently. However, this could pose a problem if agencies which are dropped are systematically different from those that are included in other ways related to the variables in my regressions. Chapter 6 includes robustness checks that run the model using the unbalanced panel.

Tables 4.1 and 4.2 give population-weighted and unweighted summary statistics for the whole panel, respectively. In each table, descriptive statistics are calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). The top panel presents summary statistics for police agencies which did not participate in Equitable Sharing during the pre-suspension period (control). The second panel presents summary statistics for agencies which did participate in Equitable Sharing during the pre-suspension period (treatment). The final panel presents whole-sample summary statistics. I present both weighted and unweighted statistics here to assist with the interpretation of my results. Since I weight my estimates by jurisdiction population, the weighted dependent variable means are the most appropriate for interpreting the magnitude of my estimates relative to their baselines. The unweighted summary statistics are useful for observing raw averages of variables such as the treatment and control indicators, average pre-period sharing, and population means.

Data Limitations

The UCR crime data suffers from some ambiguity, which could make interpretation of the results of this paper difficult. For example, many agencies indicate that they have reported their crime statistics to the FBI for all 12 months of the year, and yet report zero values for monthly total crime and clearance counts for certain months. It is unclear whether these zeros are placeholders for missing values or true zeros. Many police agencies within the United States are very small, so it would not be outlandish to assume that some of them did not experience any crime reports within certain months of the year. Further, nothing in the UCR documentation indicates that missing values should be logged as 0s, and there are missing values that are not logged as zeros from agencies which did not report for certain months of the year. Agencies which report missing values for crime reports or clearances are not included my analysis, since I limit the panel to only include agencies which have reported for all twelve months of every year between 2010 and 2020. Since nothing within the UCR documentation indicates the zeros are missing values, I treat reported zeros as non-missing.[3]

The other way that the UCR data is ambiguous originates from the method by which crime data is reported. This study uses crime reports as a measure of criminal activity and crime clearances as a measure of police effort. However, the FBI documentation that accompanies the UCR data indicates that police agents themselves occasionally report crimes. If a police officer is the primary witness to a crime, as would potentially be the case when the officer is on patrol, it would be reported by the officer themselves. As such, it is plausible that a higher level of effort from local police could result in increased crime reporting. Similarly, a reduction in police effort could result in lower crime reports. Unfortunately, the UCR data aggregates crime reports at the month level, so there is no information about the proportion of crimes that are reported by officers relative to those reported by the general public. As such, I cannot directly test whether my reduced-form results for crime reports indicate a true increase or decrease in the number of crimes committed, or an increase or decrease in the level of police effort. However, the results for crime clearances can partially guide the interpretation. If crime reports increase while I observe a tight zero for crime clearances, for example, that would indicate police effort is not driving the effects observed for crime reports.

[3]There are also a few instances where a monthly crime report total is negative, the meaning of which is unclear. I do not include any agency which reports a negative crime rate, nor do I include agencies for which population data is missing.

Table 4.7: Summary statistics by treatment and control: population-weighted

	N	Mean	SD	Min	Max
Control					
Avg. Pre-Period Sharing	451968	0.00	0.00	0	0.00
Tot. Crime Per 10k Residents	451968	21.69	25.42	0	27,759.61
Vio. Crime Per 10k Residents	451968	2.28	3.05	0	2,000.00
Prop. Crime Per 10k Residents	451968	19.41	24.07	0	27,759.61
Tot. Clearances Per 10k Residents	451968	5.11	20.98	0	63,079.78
Vio. Clearances Per 10k Residents	451968	1.14	1.74	0	784.31
Prop. Clearances Per 10k Residents	451968	3.97	20.73	0	63,079.78
Treatment					
Avg. Pre-Period Sharing	212256	4686693.63	8.6e+06	20.56	62549463.03
Tot. Crime Per 10k Residents	212256	29.62	19.36	0	98,750.00
Vio. Crime Per 10k Residents	212256	4.37	3.55	0	2,500.00
Prop. Crime Per 10k Residents	212256	25.25	17.23	0	98,750.00
Tot. Clearances Per 10k Residents	212256	5.61	10.04	0	98,750.00
Vio. Clearances Per 10k Residents	212256	1.67	1.39	0	2,500.00
Prop. Clearances Per 10k Residents	212256	3.93	9.71	0	98,750.00
Total					
Avg. Pre-Period Sharing	664224	3490384.63	7.7e+06	0	62549463.03
Tot. Crime Per 10k Residents	664224	27.59	21.35	0	98,750.00
Vio. Crime Per 10k Residents	664224	3.84	3.55	0	2,500.00
Prop. Crime Per 10k Residents	664224	23.76	19.37	0	98,750.00
Tot. Clearances Per 10k Residents	664224	5.48	13.69	0	98,750.00
Vio. Clearances Per 10k Residents	664224	1.54	1.51	0	2,500.00
Prop. Clearances Per 10k Residents	664224	3.94	13.42	0	98,750.00

Notes: Weighted descriptive statistics are calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). All means are weighted by the primary jurisdiction population of the reporting agency.

Table 4.8: Summary statistics by treatment and control: non-weighted

	N	Mean	SD	Min	Max
Control					
Avg. Pre-Period Sharing	451968	0.00	0.00	0	0.00
Population	451968	12,011.17	20088.44	5	454,353.00
Pop. Adj. Sharing	451968	0.00	0.00	0	0.00
Tot. Crime Per 10k Residents	451968	22.59	100.79	0	27,759.61
Vio. Crime Per 10k Residents	451968	2.08	7.44	0	2,000.00
Prop. Crime Per 10k Residents	451968	20.51	96.56	0	27,759.61
Tot. Clearances Per 10k Residents	451968	5.83	98.11	0	63,079.78
Vio. Clearances Per 10k Residents	451968	1.11	4.70	0	784.31
Prop. Clearances Per 10k Residents	451968	4.73	97.34	0	63,079.78
I[FundsReceived > 0]	451968	0.00	0.00	0	0.00
I[Susp]	451968	0.02	0.15	0	1.00
I[PreSusp]	451968	0.55	0.50	0	1.00
I[PostSusp]	451968	0.43	0.50	0	1.00
Treatment					
Avg. Pre-Period Sharing	212256	470,059.44	2.6e+06	20.56	62549463.03
Population	212256	74,621.30	2.8e+05	8	8616333.00
Pop. Adj. Sharing	212256	228,428.00	4.4e+06	7.078863	1.79e+08
Tot. Crime Per 10k Residents	212256	44.63	899.31	0	98,750.00
Vio. Crime Per 10k Residents	212256	3.06	15.16	0	2,500.00
Prop. Crime Per 10k Residents	212256	41.57	893.22	0	98,750.00
Tot. Clearances Per 10k Residents	212256	22.52	873.84	0	98,750.00
Vio. Clearances Per 10k Residents	212256	1.46	13.70	0	2,500.00
Prop. Clearances Per 10k Residents	212256	21.05	868.58	0	98,750.00
I[FundsReceived > 0]	212256	1.00	0.00	1	1.00
I[Susp]	212256	0.02	0.15	0	1.00
I[PreSusp]	212256	0.55	0.50	0	1.00
I[PostSusp]	212256	0.43	0.50	0	1.00
Total					
Avg. Pre-Period Sharing	664224	150,209.77	1.5e+06	0	62549463.03
Population	664224	32,018.54	1.6e+05	5	8616333.00
Pop. Adj. Sharing	664224	72,995.28	2.5e+06	0	1.79e+08
Tot. Crime Per 10k Residents	664224	29.63	515.23	0	98,750.00
Vio. Crime Per 10k Residents	664224	2.40	10.55	0	2,500.00
Prop. Crime Per 10k Residents	664224	27.24	511.27	0	98,750.00
Tot. Clearances Per 10k Residents	664224	11.16	500.62	0	98,750.00
Vio. Clearances Per 10k Residents	664224	1.22	8.66	0	2,500.00
Prop. Clearances Per 10k Residents	664224	9.94	497.58	0	98,750.00
I[FundsReceived > 0]	664224	0.32	0.47	0	1.00
I[Susp]	664224	0.02	0.15	0	1.00
I[PreSusp]	664224	0.55	0.50	0	1.00
I[PostSusp]	664224	0.43	0.50	0	1.00

Notes: Unweighted descriptive statistics are calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations).

IDENTIFICATION STRATEGY

The strategy I employ in this paper treats the suspension of Equitable Sharing funds as an exogenous shock to the asset forfeiture revenue received by local police. This allows me to isolate the causal effect of asset forfeiture payments on measures of crime and police effort using the data mentioned in the previous chapter. Due to the variation in program participation within the pre-period, I study the suspension as a quasi-experiment, examining whether agencies which frequently participated in Equitable Sharing are more intensely affected by the interruption.

I use three primary specifications to explore the causal effects of the suspension. The first employs a binary treatment, where treated agencies are those which participated at least once in the Equitable Sharing program prior to January 2016. In general, I use the term "treated" to refer to this group. The binary model will identify the average differential effect of the suspension on crime reports and clearances for treated agencies relative to the control group, which consists of police agencies which did not participate in Equitable Sharing during the pre-period. The second specification explores intensity of treatment within the suspension window. This models splits up the treatment group between above and below median in terms of the frequency of participation in the Equitable Sharing program. Results for this model will identify any higher-intensity effects for the above-median treatment group. My final specification employs a dynamic model which examines whether months leading up to and after the suspension period show evidence of anticipation or lingering effects.

Binary Treatment

My first baseline approach models one of six policing outcomes as a function of interactions between a treatment indicator marking whether an agency received Sharing revenue pre-suspension and time indicators marking whether an observation occurs within or after the suspension period.

$$
\begin{aligned}
Outcome_{i,m,y} = &\beta_0 + \beta_1 I[FundsReceived_i > 0] \times I[Susp_{m,y}] \\
&+ \beta_2 I[FundsReceived_i > 0] \times I[PostSusp_{m,y}] \\
&+ \beta_3 \chi_{i,m,y} + \alpha_i \times \theta_m + \lambda_{m,y} + \epsilon_{i,m,y}
\end{aligned}
\tag{5.1}
$$

The $Outcome_{i,m,y}$ dependent variable represents either crime reports or crime clearances for total crime, property crime, or violent crime per 10,000 residents in agency i's primary jurisdiction in month m, year y. The indicator $I[FundsReceived_i > 0]$ marks whether an agency received money from Equitable Sharing pre-suspension, the indicator $I[Susp_{m,y}]$ marks whether an observation occurs within the suspension period, and the indicator $I[PostSusp_{m,y}]$ marks whether the observation occurs after the suspension period. The interaction $I[FundsReceived_i > 0] \times I[Susp_{m,y}]$, then, equals one if an observation is treated (the reporting agency participated in Equitable Sharing prior to 2016) within the suspension period of January through March of 2016. Similarly, $I[FundsReceived_i > 0] \times I[PostSusp_{m,y}]$ equals one for observations of treated agencies after the suspension period. The $\chi_{i,m,y}$ represents a matrix of county-year covariates that could impact the outcome variables. Specifically, the model controls for the proportion of county populations that are white, black, Hispanic, male, and of various age groups. I also include county-year controls for unemployment rates and real GDP per capita in chained dollars.

In my baseline model, agency-by-month fixed effects $\alpha_i \times \theta_m$ account for agency characteristics that are fixed within each month of the calendar year, such as policing intensity, propensity to use forfeiture, and average budget size. Interacting these with month fixed effects is appropriate to capture seasonality within the data, since more crime tends to happen in warmer months and crime fluctuates greatly within seasons. Fixed agency characteristics can easily influence how an individual agency responds to the seasonal nature of crime. As such, the interacted fixed effects account for seasonality relative to each agency within the panel. I also include a month-by-year fixed effect $\lambda_{m,y}$ to account for time-varying shocks to crime and police effort across the whole panel that occur within months of a particular year and across years. Estimates are weighted by jurisdiction population and standard errors are clustered at the agency-level and robust to heteroskedasticity.

The coefficient β_1 on the interaction $I[FundsReceived_i > 0] \times I[Susp_{m,y}]$ is the coefficient of interest. It gives the differential effect of the suspension on the treated agencies relative to the control. It is plausible that my estimate for this coefficient will be negative and significant when the dependent variable is a clearance rate, given that agencies which are accustomed to participating in Equitable Sharing would likely need to reduce their consumption bundle of policing services in response to a reduction in funds. When the dependent variable is a crime rate, I expect that the coefficient β_1 is positive and significant, given that a large literature has now documented the deterrence effects of police presence and funding on crime. The second interaction, $I[FundsReceived_i > 0] \times I[PostSusp_{m,y}]$, accounts for any systematic differences between treatment and control for the post-suspension period relative to the pre-suspension period. A significant coefficient here could mean any number of things; for example, it could represent permanent effects of the suspension on the treated group relative to the control, or it could represent other shocks in the post-suspension period that affect the treatment group differently from the control. Given that Equitable Sharing funds were reinstated at the end of March 2016, this coefficient is unlikely to be statistically different from zero due to lingering effects of the suspension. Note, however, that if this coefficient is significant, it does not automatically implicate the identification strategy used within this paper. Equitable Sharing funds are highly variable and decreasing over time. As such, if there are any future shocks to Equitable Sharing payments in the post-suspension period, those might appear in the coefficient β_2.

Above and Below Median Specification

In order to explore this potentially-causal relationship further, my next model splits the treatment into two groups to explore heterogeneity in intensity within the suspension window. In this model, I evaluate the causal effect of the Equitable Sharing suspension separately on treated agencies that are either above or below the median in terms of the number of times an agency participated in the Equitable Sharing program pre-suspension. My model specification is as follows:

$$\begin{aligned} Outcome_{i,m,y} = &\beta_0 + \beta_1 I[AboveMedian_i] \times I[Susp_{m,y}] \\ &+ \beta_2 I[AboveMedian_i] \times I[PostSusp_{m,y}] \\ &+ \beta_3 I[BelowMedian_i] \times I[Susp_{m,y}] \\ &+ \beta_4 I[BelowMedian_i] \times I[PostSusp_{m,y}] \\ &+ \beta_5 \chi_{i,m,y} + \alpha_i \times \theta_m + \lambda_{m,y} + \epsilon_{i,m,y} \end{aligned} \tag{5.2}$$

Note that the county-year controls and fixed effects are exactly the same as they appear in the binary treatment specification. Heteroskedasticity-robust standard errors are still clustered at the agency level and estimates are weighted by jurisdiction population. The $Outcome_{i,m,y}$ variable also represents the same six dependent variables as in the binary treatment. The $I[AboveMedian_i]$ and $I[BelowMedian_i]$ indicators mark agencies that fall above and below the median in terms of the distribution of participation frequency, respectively. The $I[Susp_{m,y}]$ and $I[PostSusp_{m,y}]$ are also identical to how they appear in the binary treatment model: they mark observations within the suspension period and after the suspension period, respectively.

There are two coefficients of interest in the model above. The coefficient β_1 on the $I[AboveMedian_i] \times I[Susp_{m,y}]$ interaction represents the difference-in-differences estimator for above-median treated agencies. β_1 may be interpreted as the causal effect of the Equitable

Sharing suspension on agencies that were "highly treated" in terms of how frequently they participated in the Equitable Sharing program. The coefficient β_3 is my second coefficient of interest. Its interpretation is similar to that of β_1, in that it gives the differential effect of the Equitable Sharing suspension on the below-median treated agencies. The coefficients β_2 and β_4 account for any systematic differences between above- and below-median treated agencies and the control group in the second half of the panel relative to the first. As I stated above, in an ideal world this coefficient would be insignificant across all specifications in order for a strong causal interpretation to hold. However, this is not a necessary element of a causal interpretation; a significant estimate here could be capturing the effects of other changes in Equitable Sharing funds in the post-suspension period.

If participation frequency accurately captures intensity of treatment, then I would expect the β_1 coefficient on the above-median interaction to be larger in magnitude than the β_3 coefficient on the below-median interaction. Since my expectation is that more of the effect will be centered in treated agencies that participate more frequently in the Equitable Sharing program, the β_1 should certainly be significantly different from zero; however, the β_3 coefficient may or may not be significant. The lower half of the below-median group may contain agencies that are not much different from the control group (for example, agencies that received a single payment from the Equitable Sharing program in the pre-period). As such, the β_3 may or may not be statistically different from zero.

Dynamic Model

My final specification is a variation on an event study in order to determine whether the effects observed in the previous two models are robust to the timing of the Equitable Sharing suspension. This is warranted because it is an open question as to which month I should use as the beginning of the suspension period. As mentioned in Chapter 2, the DoJ's announcement of the suspension came on December 21, 2015, which means that the last ten days of December technically occur within the suspension period. The dynamic model should determine when effects began to appear, and allow me to identify lingering effects of the suspension after payments were reinstated at the end of March 2016. Note that the binary and categorical treatment specifications give the average effect of the suspension within the 3-month window. The dynamic specification explored here will determine whether effects are observable for the individual months themselves. It is possible that I will not observe any statistically significant estimates for individual months even if the previous models do yield jointly-significant result.

Since the primary effect this paper will document is for the dependent variable of crime reports rather than crime clearances, my specification below only uses the dependent variables of total, violent, and property crimes reported per 10,000 residents in the police agency's primary jurisdiction. I run two iterations of this specification: one that employs a binary treatment, and another that employs a categorical treatment similar to those given in the previous section. The equation for the first model can be found below.

$$\begin{aligned} CrimeRate_{i,m,y} =& \beta_0 + \sum_{t=-5}^{6} \mu_t I[FundsReceived_i > 0] \times I[t\_MonthsSinceSusp_{m,y}] \\ & + \beta_1 I[FundsReceived_i > 0] \times I[AfterJuly2016_{m,y}] \\ & + \beta_2 \chi_{i,m,y} + \alpha_i \times \theta_m + \lambda_{m,y} + \epsilon_{i,m,y} \end{aligned} \tag{5.3}$$

$CrimeRate_{i,m,y}$ represents either total, violent, or property crime reports per 10,000 residents in the primary jurisdiction of the reporting agency. The summation $\sum_{t=-5}^{6} \mu_t I[FundsReceived_i > 0] \times I[t\_MonthsSinceSusp_{m,y}]$ represents twelve interactions between the indicator marking the treated agencies and month dummies. These are the difference-in-differences estimators for the five months before and including January 2016, as well as the six months after. Note that the omitted time category here is all months before August 2016; as such, the estimators on each month represent the difference-in-differences across treatment and control comparing the month in question to a pooled average of all months prior to August 2016. The interaction $I[FundsReceived_i > 0] \times I[AfterJuly2016_{m,y}]$ marks all observations of treated agencies that appear after July of 2016. Similarly to previous specifications, the coefficient β_1 will capture any systematic differences between treatment and control in the post-July 2016 period relative to a pooled average of all months before August 2015.

I expect that the coefficients on all interactions that mark treated agencies within the suspension period to be positive and significant for the reasons stated earlier. If there are no lingering effects after the suspension, I should not observe significant estimates on any interaction that marks treated agencies in any period after March of 2016. If there are no anticipation effects, then I should not observe significant estimates on any interaction that marks a treated agency in any month prior to January 2016.

In order to bolster a causal interpretation, my final model determines whether the observed effects are centered in the above-median group or the below-median group.

$$\begin{aligned} CrimeRate_{i,m,y} =& \beta_0 + \sum_{t=-5}^{6} \mu_t I[AboveMedian_i] \times I[t\_MonthsSinceSusp_{m,y}] \\ & + \sum_{t=-5}^{6} \mu_t I[BelowMedian_i] \times I[t\_MonthsSinceSusp_{m,y}] \\ & + \beta_1 I[AboveMedian_i] \times I[AfterJuly2016_{m,y}] \\ & + \beta_2 I[BelowMedian_i] \times I[AfterJuly2016_{m,y}] \\ & + \beta_3 \chi_{i,m,y} + \alpha_i \times \theta_m + \lambda_{m,y} + \epsilon_{i,m,y} \end{aligned} \tag{5.4}$$

The model above shows similar difference-in-differences estimators for above- and below-median treatment agencies separately. Controls, fixed effects, standard error clustering, and population weights are the same as they were in previous models. If, as stated previously, participation frequency captures treatment intensity, then I expect significant and larger (in magnitude) estimates for the above-median interactions relative to the belowmedian interactions in all months within the suspension period. If there are no lingering or anticipatory effects of the policy shift, then I expect no statistical differences between these estimators. The estimators on all interactions that mark below-median treated agencies within the suspension may or may not be significant, depending on how intense treatment is for these agencies.

RESULTS

This chapter will present the results of the models offered in the previous chapter. The first section will present results for all baseline models, while the second will explore sensitivity to different controls and panel specifications. The final section will present a brief heterogeneity analysis.

Baseline Results

Table 6.1 gives the results for my baseline binary treatment specification. Panel A gives estimates for total, violent, and property crime reports per 10,000 jurisdiction residents, while Panel B presents results for total, violent, and property crime clearances per 10,000 jurisdiction residents. For crime reports, the within-suspension difference-in-differences estimate on total crime is negative and insignificant, indicating that agencies which received Equitable Sharing funds prior to the suspension did not experience a spike in overall crime relative to the control agencies. However, the table also shows a statistically significant and positive increase in the population-adjusted number of violent crimes relative to the control, and a statistically significant decrease in property crimes. Specifically, it appears that the suspension increased the number of violent crimes reported by about 0.18 additional reports per 10,000 jurisdiction residents, and it decreased the number of property crimes reported by about 0.6 crimes per 10,000 residents. Both estimates are significant at the 95% confidence level. Recall from Table 4.1 that the weighted whole-sample mean for violent crime reports is 3.84 per 10,000 residents, and the weighted whole-sample mean for property crime reports is 23.76 per 10,000 residents. As such, I find that the suspension increased violent crime by approximately 4.7% and decreased property crime by approximately 2.5% relative to the weighted baseline means. Since the UCR's measure of total crime reports is just a summation of violent and property crime reports, it is likely that these two effects are counteracting each other, resulting in the null effect shown for total crime. Note that none of the DiD estimates are significant for the interaction marking treated agencies in the post-period, which indicates that there may be no systematic differences in the post-period relative to the pre-period across treatment and control.

In Panel B, difference-in-differences estimates are presented for the dependent variables of crime clearances per 10,000 jurisdiction residents, split between total, violent, and property crimes. The table shows no statistically significant effect of the suspension on treated agencies relative to control. None of the within-suspension estimates are statistically significant, but the post-suspension DiD estimates for total and property crime are both significant. This could elicit concerns that there are systematic differences between treatment and control for total and property crime clearances, but as mentioned earlier, this estimator is difficult to interpret. A significant effect could be a result of one-time unobserved policy shifts or other changes in the post-suspension period.

Table 6.2 gives the baseline results for the above and below median categorical treatment specification. Beginning with the crime report results in Panel A, I observe a significant and consistent effect of the suspension in the above-median treatment group for both violent and property crime, but no discernible effect for total crime rates. Given that violent crimes appear to increase and property crimes appear to decrease, it is likely that the null effect for total crime is a result of these two effects cancelling out, just as they do in the binary treatment specification. Specifically, the table indicates that agencies which participated more frequently in the DoJ's Equitable Sharing program in the pre-period experienced a statistically-significant differential increase of about 0.24 more violent crimes per 10,000 jurisdiction residents and a decrease of 0.69 property crimes per 10,000 jurisdiction residents. Relative to the weighted whole-sample means presented in Table 4.1, this is an increase of about 6.25% in violent crime reports and a decrease of approximately 2.9% in property crime reports. Note that these estimates are larger in magnitude than those reported in Table 6.1

Table 6.1: Effect of the Equitable Sharing suspension: binary treatment

	(1) Total	(2) Violent	(3) Property
Panel A: Crime Reports per 10k Residents			
I[FundsReceived]xI[Susp]	-0.414	0.184**	-0.599**
	(0.308)	(0.073)	(0.284)
I[FundsReceived]xI[PostSusp]	-0.261	0.114	-0.374
	(0.297)	(0.074)	(0.273)
Agency-Month FE	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes
Panel B: Crime Clearances per 10k Residents			
I[FundsReceived]xI[Susp]	-0.036	0.078	-0.114
	(0.116)	(0.053)	(0.086)
I[FundsReceived]xI[PostSusp]	-0.223**	0.016	-0.239***
	(0.102)	(0.035)	(0.086)
Agency-Month FE	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Panel A presents results for the outcome variables of total, violent, and property crime reports per 10,000 residents in the police agency's primary jurisdiction. Panel B presents results for the outcome variables of total, violent, and property crimes cleared by police per 10,000 residents in the police agency's primary jurisdiction. Each model includes agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

for the binary treatment specification.

Moving to the estimates for the below-median treatment group within the suspension (also in Panel A), note that none of the estimates are statistically significant. As such, the data do not rule out a null effect of the suspension on agencies which participated less frequently in the Equitable Sharing program during the pre-suspension period. These estimates are smaller in magnitude than those present for the above-median treatment group, which is evidence that participation frequency captures intensity of treatment. None of the DiD estimates for the post-period are statistically significant in Panel A.

Panel B in Table 6.2 gives the results for crime clearances using the above and below median specification. Consistent with the results for the binary treatment model, I do not observe evidence of an effect of the suspension on clearance rates. Interestingly, even though the estimates are not significant at the 90% confidence level, I do observe larger estimates for the above-median group relative to the below-median group. Also consistent with the binary treatment specification, the DiD estimates for both treatment groups in the post-suspension period are statistically significant for total and property crime clearances.

Figure 6.1 shows the dynamic specification results for total crime reports using a binary treatment. Recall that the motivation behind this specification involved determining whether there were any anticipation effects before or lingering effects after the Equitable Sharing suspension was implemented at t=0. Figure 6.1 shows estimates and confidence intervals for the months leading up to and after the suspension period. While a bit messy, consistent with our average difference-in-differences results in Table 6.1, the event study indicates no statistically-significant effects of the suspension within or after the suspension period. However, two of the months leading up to the suspension show a negative differential between treatment and control in terms of total crime: December 2015 and September 2015. Since the last ten days of December 2015 technically occur within the suspension, it is possible that the estimate is capturing a true effect of the Equitable Sharing suspension (likely driven

Table 6.2: Effect of the Equitable Sharing suspension: above & below median

	(1) Total	(2) Violent	(3) Property
Panel A: Crime Reports per 10k Residents			
I[AboveMedian]xI[Susp]	-0.451	0.240***	-0.690**
	(0.352)	(0.085)	(0.322)
I[AboveMedian]xI[PostSusp]	-0.242	0.136	-0.378
	(0.336)	(0.088)	(0.310)
I[BelowMedian]xI[Susp]	-0.274	-0.040	-0.234
	(0.311)	(0.056)	(0.290)
I[BelowMedian]xI[PostSusp]	-0.319	0.007	-0.327
	(0.320)	(0.052)	(0.297)
Agency-Month FE	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes
Panel B: Crime Clearances per 10k Residents			
I[AboveMedian]xI[Susp]	-0.041	0.098	-0.139
	(0.134)	(0.062)	(0.097)
I[AboveMedian]xI[PostSusp]	-0.213*	0.016	-0.229**
	(0.115)	(0.041)	(0.097)
I[BelowMedian]xI[Susp]	-0.016	0.026	-0.042
	(0.122)	(0.037)	(0.110)
I[BelowMedian]xI[PostSusp]	-0.209*	0.024	-0.233**
	(0.114)	(0.029)	(0.100)
Agency-Month FE	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Panel A presents results for the outcome variables of total, violent, and property crime reports per 10,000 residents in the police agency's primary jurisdiction. Panel B presents results for the outcome variables of total, violent, and property crimes cleared by police per 10,000 residents in the police agency's primary jurisdiction. Each model includes agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

by a decrease in property crime). However, since I only have monthly aggregates of crime reports, I cannot rule out whether another unobserved factor is influencing the crime rates of the treatment group differently from the control. The estimate for September 2015 could be a result of an anticipation effect, but it is strange that an effect appears only for this month and not October or November 2015. Despite these anomalies, the dynamic results appear to show that the suspension did not affect the total crime rates in any substantial way.

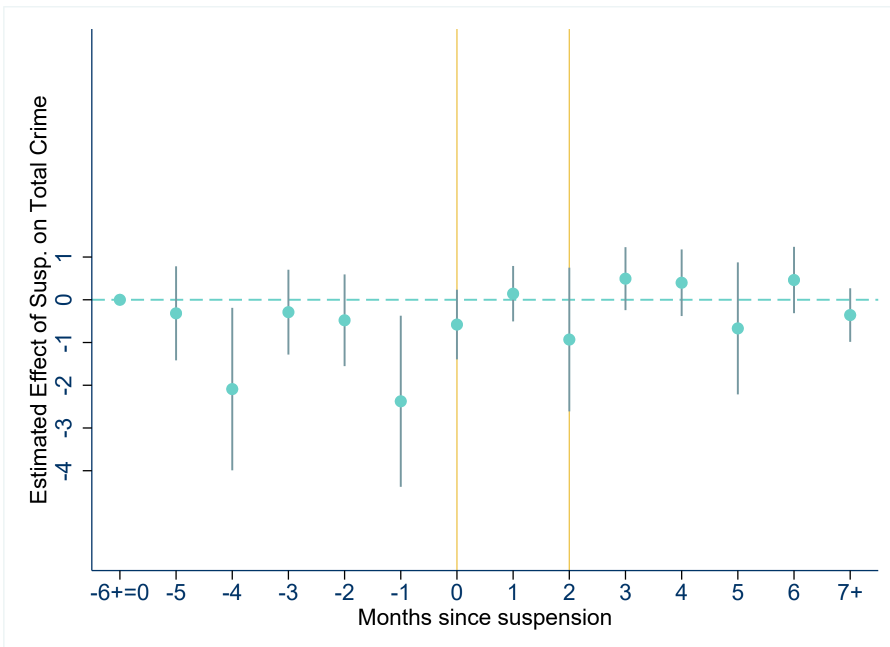

Figure 6.1: Event study using binary treatment: total crime

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). The graph presents estimates and confidence intervals of the effects of the Equitable Sharing suspension on total crime rates by month leading up to and after the suspension at t=0. Each estimate is comparing the differential effect of the suspension on crime rates relative to a pooled average of all months prior to August 2015. Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

Figure 6.2 presents results of the binary treatment event study using violent crime reports per 10,000 jurisdiction residents as the dependent variable. Again, while a bit messy, I observe a clear spike in crime rates immediately following the suspension in January of 2016. This effect persists in February and appears to dissipate in March. However, it is worth noting that the standard error on the estimate for March 2016 is quite large relative to the others; as such, the data do not rule out a true positive effect of the suspension on violent crime within this month. The months after the suspension do not show evidence of a lingering effect, and the months leading up to it do not show strong evidence of anticipation effects.

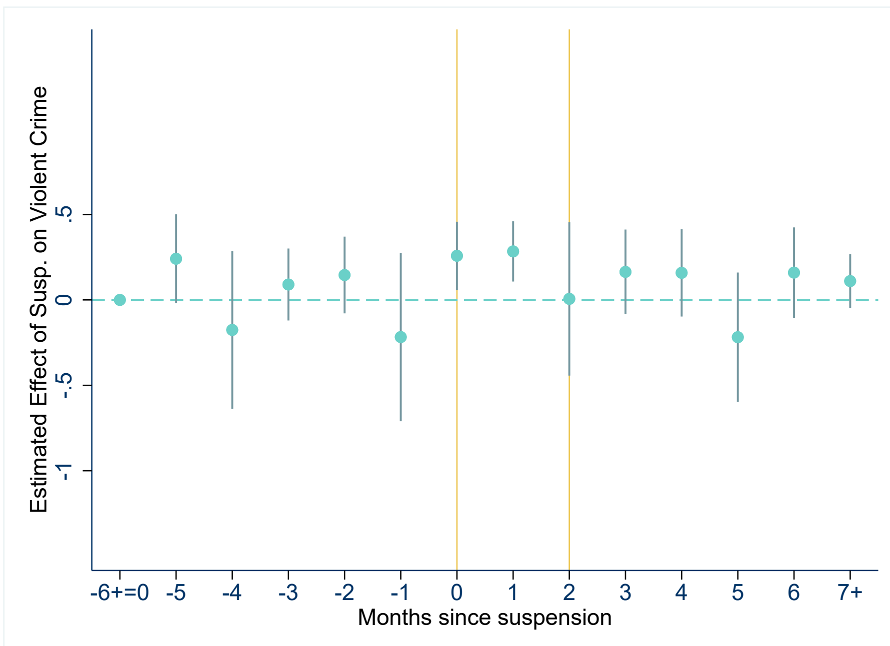

Figure 6.2: Event study using binary treatment: violent crime

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). The graph presents estimates and confidence intervals of the effects of the Equitable Sharing suspension on violent crime rates by month leading up to and after the suspension at t=0. Each estimate is comparing the differential effect of the suspension on crime rates relative to a pooled average of all months prior to August 2015. Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

Figure 6.3 presents results of the binary treatment event study with property crime

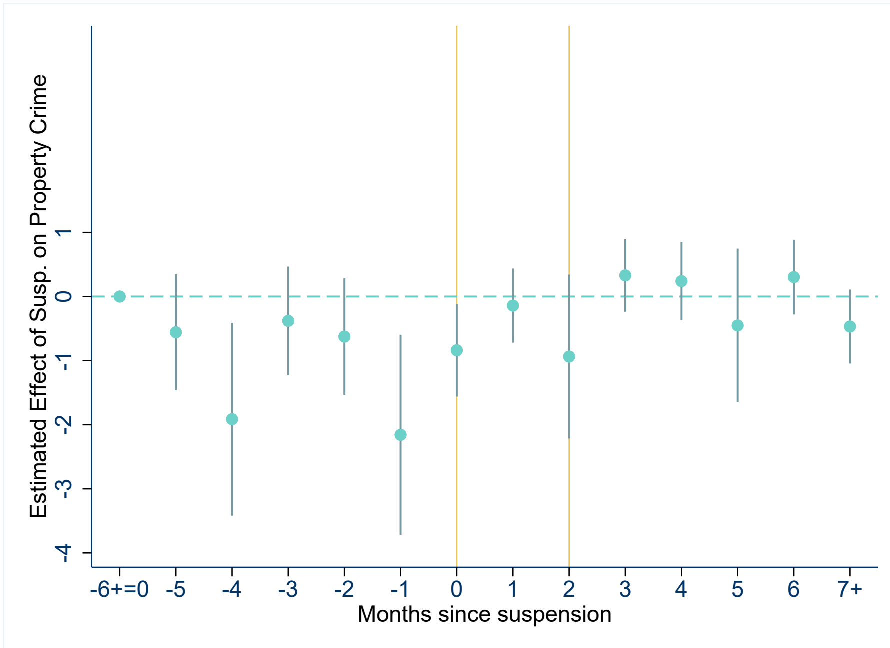

Figure 6.3: Event study using binary treatment: property crime

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). The graph presents estimates and confidence intervals of the effects of the Equitable Sharing suspension on property crime rates by month leading up to and after the suspension at t=0. Each estimate is comparing the differential effect of the suspension on crime rates relative to a pooled average of all months prior to August 2015. Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

reports per 10,000 jurisdiction residents as the dependent variable. Results here nearly mirror the plot for total crime rates. Both estimates for both November and December are significant, as well as the estimate for January, the first month of the suspension. Again, since total crime rates are simply an aggregation of violent and property crime, the stronger effects of property crimes are likely influencing the results observed for total crime reports. One primary difference between Figure 6.3 and Figure 6.1 is that I do observe a statistically significant DiD estimator for the month of January when property crime rates are used as the dependent variable. Paired with the estimate for December 2015, the results begin to appear as though the effects of the Equitable Sharing suspension may have started in December of 2015, but quickly dissipated leading into February. However, given the anomalous estimator for September of 2015, I cannot state this with confidence.

Figures 6.3, 6.4, and 6.5 all show dynamic (event study) results for the above and below median specification. Admittedly, the results for all of the event study graphs range in their interpretability and neatness. But one can easily deduce that all potential effects of the suspension, both within the suspension period and before it, are centered in the above-median treatment group. That is, the results consistently demonstrate that the effects are strongest for agencies which participated more frequently in the Equitable Sharing program in the the pre-period.

Overall, it appears that my baseline models demonstrate a consistent positive effect of the suspension on violent crime reports and suggestive evidence of a negative effect on property crime reports. These results seem economically small, which is consistent with previous literature (Kelly and Kole 2016; Gius 2018). However, given that this is a per-month estimate, the positive effect on violent crimes becomes much more qualitatively significant when aggregated up to the year-level. Once I explore the timing of the suspension, the results for total and property crime become somewhat suspect given the possible anticipation effects in the months leading up to January 2016. However, the results for violent crime across all model specifications are consistent with timing. As soon as funding was cut off, treated agencies experienced a spike in violent crime that immediately subsided as the suspension was lifted. This effect was strongest for agencies which participated more frequently in Equitable Sharing.

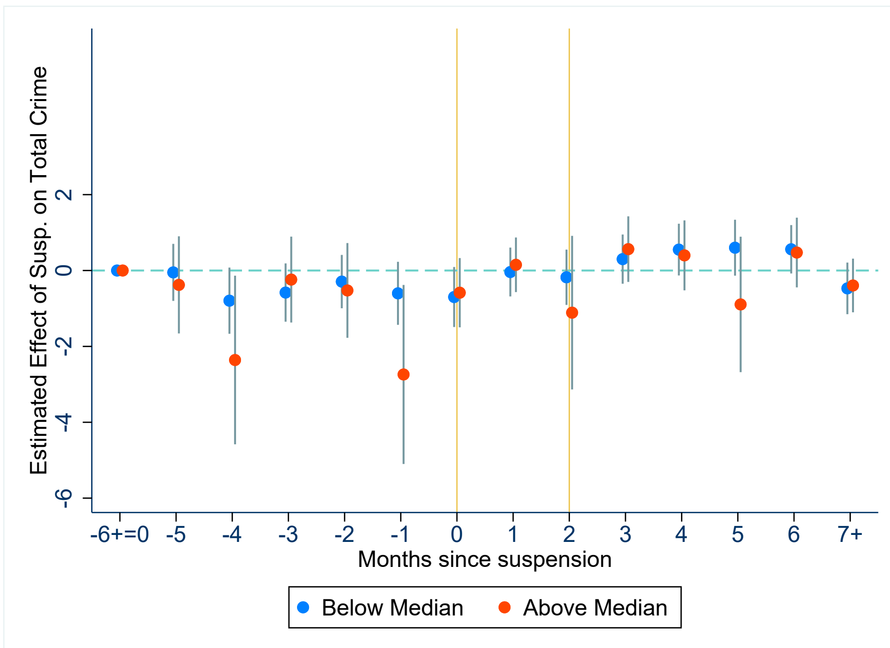

Figure 6.4: Event study using categorical treatment: total crime

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). The graph presents estimates and confidence intervals of the effects of the Equitable Sharing suspension on total crime rates by month leading up to and after the suspension at t=0. The the blue dots represent the effect of the suspension on treated agencies below median in terms of participation frequency, and the red dots show the effect of the suspension on above-median agencies. Each estimate is comparing the differential effect of the suspension on crime rates relative to a pooled average of all months prior to August 2015. Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

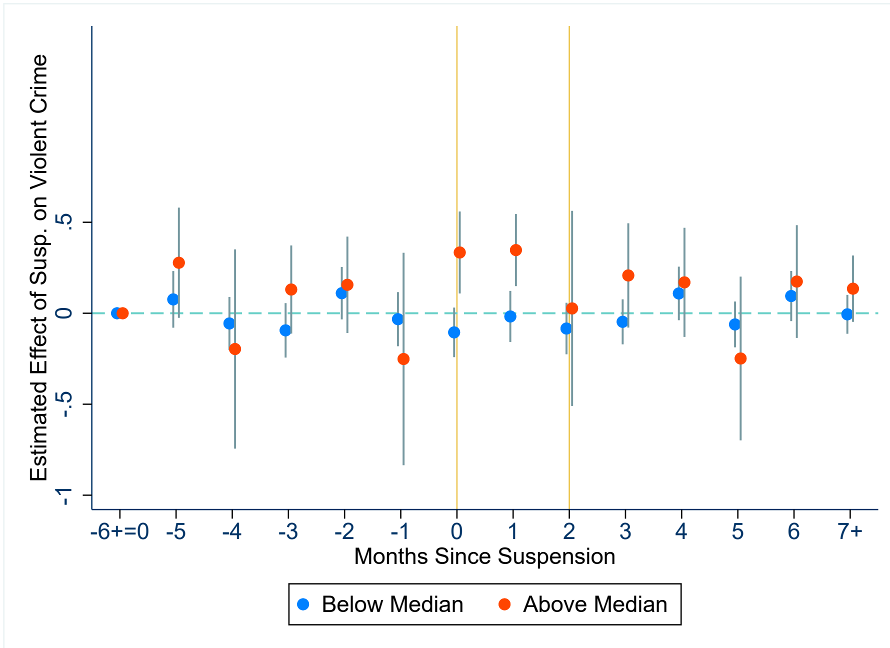

Figure 6.5: Event study using categorical treatment: violent crime

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). The graph presents estimates and confidence intervals of the effects of the Equitable Sharing suspension on violent crime rates by month leading up to and after the suspension at t=0. The the blue dots represent the effect of the suspension on treated agencies below median in terms of participation frequency, and the red dots show the effect of the suspension on above-median agencies. Each estimate is comparing the differential effect of the suspension on crime rates relative to a pooled average of all months prior to August 2015. Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

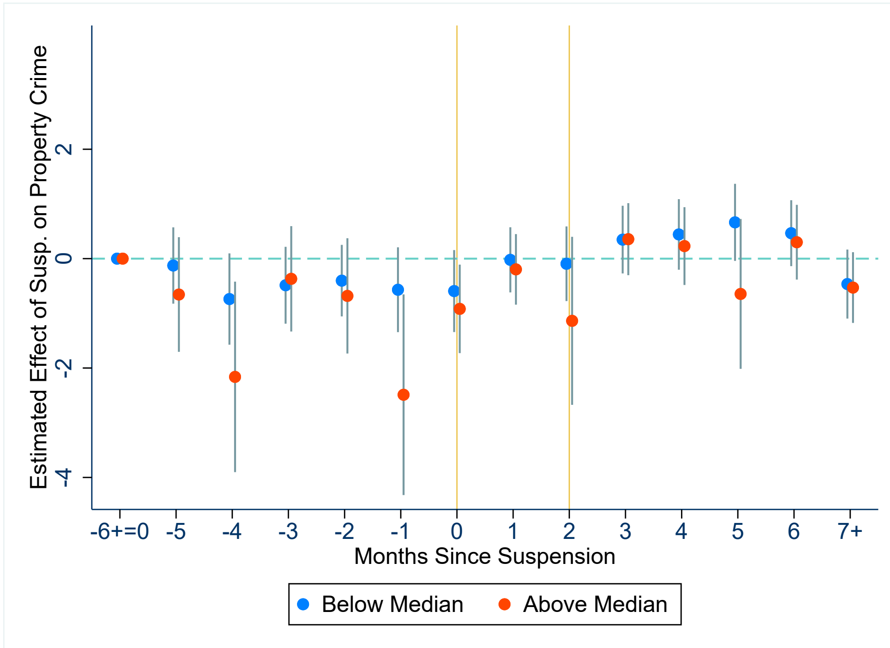

Figure 6.6: Event study using categorical treatment: property crime

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). The graph presents estimates and confidence intervals of the effects of the Equitable Sharing suspension on property crime rates by month leading up to and after the suspension at t=0. The the blue dots represent the effect of the suspension on treated agencies below median in terms of participation frequency, and the red dots show the effect of the suspension on above-median agencies. Each estimate is comparing the differential effect of the suspension on crime rates relative to a pooled average of all months prior to August 2015. Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

Robustness and Sensitivity

This section will explore different variations on the binary and categorical treatment models discussed in the previous two sections. Overall, I find relatively robust and consistent results for the increase in violent crimes shown in the previous section. Most models also show a null effect for total crimes within the suspension, as well as a decrease in property crimes within the suspension. Results for crime clearances are somewhat less consistent; most models show a null effect of the suspension on clearance rates, but some specifications show statistically significant results.

Binary Treatment Model Variations

Table 6.3 presents results for various regression models estimating the effects of the Equitable Sharing suspension on the population-adjusted number of total crime reports and clearances. The results for total crime reports can be found in Panel A. For easy comparison, Column 1 in each of the tables that follow in this section present regression results for the baseline specification shown in the previous section, which include agency-by-month and month-by-year fixed effects, county-year controls, and agency-level clustering of the standard errors. Column 2 runs the original regression with only year fixed effects rather than month-year fixed effects; Column 3 presents results for a regression that includes agency, month, and year fixed effects separately; Column 4 presents the baseline results without county-year demographic and economic controls; Column 5 presents results using agency-month and agency-year fixed effects instead of county-year controls; Column 6 presents results using agency-month, agency-year, and month-year fixed effects; Column 7 presents baseline results while clustering standard errors at the state level rather than at the agency level. The within-suspension DiD estimates are statistically insignificant across all control specifications except for the one presented in Column 5, which shows an increase in total crime within the suspension for treated agencies relative to control. However, once month-year fixed effects are added to this model (results presented in Column 6), the statistical significance dissipates and the results become consistent with previous model results. I observe this phenomenon for the majority of the results that follow: when agency-year and agency-month fixed effects are used, the presented result will differ from the others in the panel; when month-year fixed effects are added to the model, the results conform more robustly. This indicates that there are are likely strong shocks to the dependent crime variables that occur within years that must be accounted for.[1]

There are a few more elements to Table 6.3 that are worthy of mention. Note that none of the DiD estimates for the post-suspension period are statistically significant, which indicates that I may not need to be worried about shocks affecting treatment differently from control in the post-suspension period. In Panel B, I do not observe a statistically significant effect of the suspension on crime clearances for most models (barring the same exception for Column 5 explained in the previous paragraph). Many of the estimates for the post-suspension period are significant for total crime clearances, which is also consistent with the results from the baseline model.

Table 6.4 shows the same set of model specifications using violent crime reports and clearances as the dependent variable. As with the previous set of tables, Panel A shows results for crime reports per 10,000 jurisdiction residents, and Panel B shows results for crime clearances per 10,000 jurisdiction residents. Beginning in Panel A, I observe that in the vast majority of model specifications, the within-suspension DiD estimates are significant and positive, indicating that treated agencies experienced more violent crime in the suspension period relative to the control. The only exception is for the Column 6 specification, which includes agency-month, agency-year, and month-year fixed effects. Note, however, that

[1] In general, I am suspicious of any model that does not include month-year fixed effects, given that crime data fluctuates substantially within-years.

Table 6.3: Effect of the Equitable Sharing suspension on total crime and clearances: variations on fixed effects and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Total Crime Reports per 10k Residents							
I[FundsReceived]xI[Susp]	-0.414	0.238	0.081	-0.209	0.574***	-0.197	-0.414
	(0.308)	(0.281)	(0.295)	(0.312)	(0.146)	(0.184)	(0.346)
I[FundsReceived]xI[PostSusp]	-0.261	-0.296	-0.287	-0.035			-0.261
	(0.297)	(0.294)	(0.293)	(0.428)			(0.450)
Agency FE	-	-	Yes	-	-	-	-
Month FE	-	-	Yes	-	-	-	-
Year FE	-	Yes	Yes	-	-	-	-
Agency-Month FE	Yes	Yes	-	Yes	Yes	Yes	Yes
Agency-Year FE	-	-	-	-	Yes	Yes	-
Month-Year FE	Yes	-	-	Yes	-	Yes	Yes
County-Year Controls	Yes	Yes	Yes	No	No	No	Yes
SE Clustering	Agency	Agency	Agency	Agency	Agency	Agency	State
Panel B: Total Crime Clearances per 10k Residents							
I[FundsReceived]xI[Susp]	-0.036	0.141	0.142	0.071	0.334***	-0.020	-0.036
	(0.116)	(0.098)	(0.098)	(0.248)	(0.049)	(0.069)	(0.107)
I[FundsReceived]xI[PostSusp]	-0.223**	-0.232**	-0.233**	-0.085			-0.223*
	(0.102)	(0.100)	(0.100)	(0.245)			(0.115)
Agency FE	-	-	Yes	-	-	-	-
Month FE	-	-	Yes	-	-	-	-
Year FE	-	Yes	Yes	-	-	-	-
Agency-Month FE	Yes	Yes	-	Yes	Yes	Yes	Yes
Agency-Year FE	-	-	-	-	Yes	Yes	-
Month-Year FE	Yes	-	-	Yes	-	Yes	Yes
County-Year Controls	Yes	Yes	Yes	No	No	No	Yes
SE Clustering	Agency	Agency	Agency	Agency	Agency	Agency	State

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for models which include variations on fixed effects and controls. Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Columns display results for the dependent variable of total crime reports and clearances per 10,000 jurisdiction residents. All estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity.

including all three sets of fixed effects soaks up a substantial amount of variation, which could be the reason why I observe a null effect here. Much of what the agency-year fixed effects captures is unlikely to be associated with the dependent variable, besides what is already being controlled for in the county-year matrix of covariates. As such, I prefer the results in the baseline specification to the one given in Column 6. Additionally, none of the DiD estimates for the post-suspension period are significant, and in the Column 5 and Column 6 specifications, this variable is perfectly co-linear with the fixed effects.

Moving briefly to Panel B, results in Columns 2, 3, and 5 are inconsistent with that which was previously observed. The within-suspension DiD estimators in these columns are statistically significant and positive, while none of the DiD estimators for the post-suspension period are significant. Note, however, that the effects documented in Panel B are very small, and there are decent reasons to prefer the baseline specification given that it controls for both agency-specific seasonality as well as within-year and across-year shocks. The positive result in and of itself is surprising; I would expect that a reduction in funds for the treatment agencies would lead to less policing of violent crimes relative to the control. Given that the estimates for clearances are almost universally the same sign as the estimates for reports, one might suspect that the crime clearance data is not truly capturing a measure of police effort.

Table 6.5 shows the same seven variations on fixed effects and controls using property crime reports and clearances as the dependent variable. In Panel A, it appears that the the estimates on the variable of interest (the within-suspension DiD estimate) are more sensitive to different model specifications than the other dependent variables have been. Most estimates are negative which is consistent with the baseline, but the estimates within Columns 3, 4, and 6 are not statistically different from zero. Further, the estimates in Columns 2 and 5 are positive. This is potentially concerning, but note that these models do not control for panel-wide month shocks that happen within-year. Whenever the model fails to control for month-by-year fixed effects, the results for property crime seem to indicate a positive differential increase within the suspension period for treated agencies. There are likely within-year month shocks in the panel that are biasing the coefficient of interest. Briefly, Panel B within Table 6.5 gives results using crime clearances as the dependent variable. The DiD estimator for within the suspension is insignificant in all specifications besides the one presented in Column 5, which is likely inferior to the baseline.

Tables 6.6, 6.7, and 6.8 present results using various panel specifications in order to test these models' sensitivity to panel setup. In all three tables, Column 1 presents baseline results for ease of comparison. Recall from the descriptive statistics that one difference between treatment and control is the size of the jurisdictions that these agencies serve. The control group agencies are significantly smaller than those in the treatment group. Column 2 drops the lower 75% of the control group in terms of population jurisdiction in order to test whether results are being driven by a large number of small agencies clustered within the control group.[2] Column 3 similarly tests whether extremely large agency outliers within the treatment group are driving the baseline results by dropping the largest 1% of the treatment group. I am hesitant to drop more of the larger agencies within the treatment group since these agencies are more intensely treated. If I drop more than the 1% here, then I may observe a null effect simply because I am dropping highly treated agencies from the panel. In order to test whether results are being driven by low-frequency participating agencies in the treatment group, Column 4 presents results for a regression that only includes treated agencies with an average participation frequency of greater than one per month in the pre-suspension period. In order to determine whether results are driven by seasonality not captured by the fixed effects, Column 5 presents results which only keep observations that occur within December, January, February, or March in all years of the panel. Finally,

[2]This specification leaves the median agency jurisdiction population very similar across treatment and control.

Table 6.4: Effect of the Equitable Sharing suspension on violent crime and clearances: variations on fixed effects and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Violent Crime Reports per 10k Residents							
I[FundsReceived]xI[Susp]	0.184**	0.220***	0.131*	0.196**	0.108***	0.062	0.184**
	(0.073)	(0.072)	(0.068)	(0.084)	(0.032)	(0.039)	(0.077)
I[FundsReceived]xI[PostSusp]	0.114	0.112	0.116	0.127			0.114
	(0.074)	(0.073)	(0.074)	(0.093)			(0.090)
Agency FE	-	-	Yes	-	-	-	-
Month FE	-	-	Yes	-	-	-	-
Year FE	-	Yes	Yes	-	-	-	-
Agency-Month FE	Yes	Yes	-	Yes	Yes	Yes	Yes
Agency-Year FE	-	-	-	-	Yes	Yes	-
Month-Year FE	Yes	-	-	Yes	-	Yes	Yes
County-Year Controls	Yes	Yes	Yes	No	No	No	Yes
SE Clustering	Agency	Agency	Agency	Agency	Agency	Agency	State
Panel B: Violent Crime Clearances per 10k Residents							
I[FundsReceived]xI[Susp]	0.078	0.078**	0.071**	0.134	0.045**	-0.005	0.078
	(0.053)	(0.035)	(0.036)	(0.113)	(0.018)	(0.024)	(0.048)
I[FundsReceived]xI[PostSusp]	0.016	0.016	0.016	0.074			0.016
	(0.035)	(0.034)	(0.035)	(0.080)			(0.040)
Agency FE	-	-	Yes	-	-	-	-
Month FE	-	-	Yes	-	-	-	-
Year FE	-	Yes	Yes	-	-	-	-
Agency-Month FE	Yes	Yes	-	Yes	Yes	Yes	Yes
Agency-Year FE	-	-	-	-	Yes	Yes	-
Month-Year FE	Yes	-	-	Yes	-	Yes	Yes
County-Year Controls	Yes	Yes	Yes	No	No	No	Yes
SE Clustering	Agency	Agency	Agency	Agency	Agency	Agency	State

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for models which include variations on fixed effects and controls. Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Columns display results for the dependent variable of violent crime reports and clearances per 10,000 jurisdiction residents. All estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity.

Table 6.5: Effect of the Equitable Sharing suspension on property crime and clearances: variations on fixed effects and controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Property Crime Reports per 10k Residents							
I[FundsReceived]xI[Susp]	-0.599**	0.018	-0.050	-0.405	0.465***	-0.258	-0.599*
	(0.284)	(0.265)	(0.277)	(0.289)	(0.132)	(0.169)	(0.322)
I[FundsReceived]xI[PostSusp]	-0.374	-0.407	-0.404	-0.162			-0.374
	(0.273)	(0.270)	(0.269)	(0.410)			(0.416)
Agency FE	-	-	Yes	-	-	-	-
Month FE	-	-	Yes	-	-	-	-
Year FE	-	Yes	Yes	-	-	-	-
Agency-Month FE	Yes	Yes	-	Yes	Yes	Yes	Yes
Agency-Year FE	-	-	-	-	Yes	Yes	-
Month-Year FE	Yes	-	-	Yes	-	Yes	Yes
County-Year Controls	Yes	Yes	Yes	No	No	No	Yes
SE Clustering	Agency	Agency	Agency	Agency	Agency	Agency	State
Panel B: Property Crime Clearances per 10k Residents							
I[FundsReceived]xI[Susp]	-0.114	0.063	0.071	-0.062	0.289***	-0.015	-0.114
	(0.086)	(0.082)	(0.081)	(0.147)	(0.044)	(0.062)	(0.087)
I[FundsReceived]xI[PostSusp]	-0.239***	-0.248***	-0.248***	-0.159			-0.239**
	(0.086)	(0.085)	(0.085)	(0.173)			(0.094)
Agency FE	-	-	Yes	-	-	-	-
Month FE	-	-	Yes	-	-	-	-
Year FE	-	Yes	Yes	-	-	-	-
Agency-Month FE	Yes	Yes	-	Yes	Yes	Yes	Yes
Agency-Year FE	-	-	-	-	Yes	Yes	-
Month-Year FE	Yes	-	-	Yes	-	Yes	Yes
County-Year Controls	Yes	Yes	Yes	No	No	No	Yes
SE Clustering	Agency	Agency	Agency	Agency	Agency	Agency	State

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for models which include variations on fixed effects and controls. Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Columns display results for the dependent variable of property crime reports and clearances per 10,000 jurisdiction residents. All estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity.

recall that the baseline models in this paper use a strongly balanced agency-by-month panel, which I achieve by dropping agencies which do not report crimes or clearances for all months within the panel. However, it is possible that agencies which do not report consistently are systematically different from agencies which do in some way that biases the coefficient of interest. As such, Column 6 displays regression results for the baseline model using the unbalanced panel.

Overall, I find my results quite robust to different panel specifications. Beginning with total crime in Table 6.6, all estimates on the within-suspension interaction variable for total crime reports and clearances are negative, and some of the results indicate a decrease in total crime reports as a result of the suspension. All within-suspension estimates for clearances are not statistically different from zero. For violent crime reports in Table 6.7 (Panel A), I observe that panel specifications do not seem to influence the within-suspension results at all. While the magnitudes of the estimators vary from around 0.11 to 0.33, results consistently demonstrate a statistically significant increase in violent crime reports per 10,000 residents within the suspension period. Most of the post-suspension DiD estimates are likewise consistently insignificant. Finally, Table 6.8 gives results for property crimes and clearances per 10,000 residents. In Panel A, results are mostly consistent with baseline. It appears that most models show a negative and significant effect of the suspension on property crime reports, though the statistical significance does seem to be somewhat sensitive to model specification. In addition, several of the post-suspension DiD estimates are significant at the 90% and 95% confidence levels. In Panel B, I mostly observe null results for property crime clearances, except for the estimate in Column 3 which is negative and statistically significant. Additionally, all of the post-suspension estimates are negative and significant, which is consistent with the baseline specification.

Lastly, Table 6.12 displays results for several falsification tests which help determine whether the data show similar effects to the baseline in other years of the sample. The

Table 6.6: Effect of the Equitable Sharing suspension on total crime and clearances: various panel specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total Crime Reports per 10k Residents						
I[FundsReceived]xI[Susp]	-0.499	-0.123	-0.774**	-0.333	-0.197	-0.510**
	(0.338)	(0.297)	(0.384)	(0.272)	(0.272)	(0.251)
I[FundsReceived]xI[PostSusp]	-0.263	-0.415	-0.119	-0.430	-0.121	-0.505*
	(0.345)	(0.299)	(0.392)	(0.300)	(0.271)	(0.262)
Agency-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Total Crime Clearances per 10k Residents						
I[FundsReceived]xI[Susp]	-0.078	-0.048	-0.150	-0.043	-0.014	-0.099
	(0.127)	(0.094)	(0.150)	(0.120)	(0.106)	(0.102)
I[FundsReceived]xI[PostSusp]	-0.228*	-0.323***	-0.270*	-0.268***	-0.193**	-0.229***
	(0.120)	(0.088)	(0.153)	(0.104)	(0.092)	(0.088)
Agency-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for various panel specifications. Columns display results for the dependent variable of total crime reports and clearances per 10,000 jurisdiction residents. All models include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level. Column 1 presents estimates using the baseline panel; Column 2 drops the lowest 75% of the control agencies in terms of population jurisdictions; Column 3 drops the upper 1% of the treatment agencies in terms of population jurisdiction; Column 4 only includes treatment agencies with an average forfeiture rate of over 1 per month in the pre-suspension period; Column 5 only includes observations within the months of December through March for the whole panel; Column 6 drops the first and last years in the sample (2010 and 2020); Column 7 presents estimates using the naturally unbalanced panel.

Table 6.7: Effect of the Equitable Sharing suspension on violent crime and clearances: various panel specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Violent Crime Reports per 10k Residents						
I[FundsReceived]xI[Susp]	0.180**	0.113**	0.329***	0.198***	0.199***	0.130**
	(0.078)	(0.056)	(0.110)	(0.066)	(0.071)	(0.061)
I[FundsReceived]xI[PostSusp]	0.117	0.073	0.246**	0.070	0.115	0.037
	(0.085)	(0.061)	(0.118)	(0.078)	(0.070)	(0.066)
Agency-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Violent Crime Clearances per 10k Residents						
I[FundsReceived]xI[Susp]	0.063	0.007	0.100	0.086	0.079	0.036
	(0.059)	(0.032)	(0.069)	(0.056)	(0.051)	(0.046)
I[FundsReceived]xI[PostSusp]	0.015	-0.005	-0.003	0.026	0.027	-0.023
	(0.042)	(0.027)	(0.065)	(0.034)	(0.032)	(0.031)
Agency-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for various panel specifications. Columns display results for the dependent variable of violent crime reports and clearances per 10,000 jurisdiction residents. All models include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level. Column 1 presents estimates using the baseline panel; Column 2 drops the lowest 75% of the control agencies in terms of population jurisdictions; Column 3 drops the upper 1% of the treatment agencies in terms of population jurisdiction; Column 4 only includes treatment agencies with an average forfeiture rate of over 1 per month in the pre-suspension period; Column 5 only includes observations within the months of December through March for the whole panel; Column 6 drops the first and last years in the sample (2010 and 2020); Column 7 presents estimates using the naturally unbalanced panel.

Table 6.8: Effect of the Equitable Sharing suspension on property crime and clearances: various panel specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Property Crime Reports per 10k Residents						
I[FundsReceived]xI[Susp]	-0.679**	-0.236	-1.103***	-0.531**	-0.396	-0.640***
	(0.312)	(0.282)	(0.338)	(0.258)	(0.248)	(0.231)
I[FundsReceived]xI[PostSusp]	-0.380	-0.488*	-0.365	-0.500*	-0.235	-0.542**
	(0.313)	(0.272)	(0.356)	(0.267)	(0.242)	(0.237)
Agency-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Property Crime Clearances per 10k Residents						
I[FundsReceived]xI[Susp]	-0.141	-0.055	-0.250**	-0.129	-0.093	-0.135*
	(0.096)	(0.083)	(0.109)	(0.087)	(0.080)	(0.075)
I[FundsReceived]xI[PostSusp]	-0.243**	-0.318***	-0.267**	-0.295***	-0.220***	-0.206***
	(0.099)	(0.078)	(0.120)	(0.089)	(0.078)	(0.073)
Agency-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for various panel specifications. Columns display results for the dependent variable of property crime reports and clearances per 10,000 jurisdiction residents. All models include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level. Column 1 presents estimates using the baseline panel; Column 2 drops the lowest 75% of the control agencies in terms of population jurisdictions; Column 3 drops the upper 1% of the treatment agencies in terms of population jurisdiction; Column 4 only includes treatment agencies with an average forfeiture rate of over 1 per month in the pre-suspension period; Column 5 only includes observations within the months of December through March for the whole panel; Column 6 drops the first and last years in the sample (2010 and 2020); Column 7 presents estimates using the naturally unbalanced panel.

motivation here stems from a concern that the treatment and control agencies may be systematically different from each other, which could impact the way they report criminal behavior to the FBI. If the results observed for 2016 are only attributable to the Equitable Sharing suspension and not due to systematic reporting error, then I would expect that running the same regression for the months of January through March of previous years would yield substantially different results. In Table 6.12 Panel A, regression results using January through March of 2015 as a "false suspension" are displayed. The DiD estimates on total crime and property crime are both significant and negative, which is similar to what I observe in the baseline models. However, the estimate for violent crime reports is not statistically different from zero. Panel B elicits further concern for property crimes, showing both "fake suspension" estimates to be negative and significant for these months of 2014 as well, although at a lower confidence level. Here, the estimate on violent crime is negative and significant, certainly different from what is shown in the baseline results. In Panel C, none of the fake within-suspension DiD estimates are statistically significant at 90% confidence. While it is possible that other unobserved factors are driving the significant estimates observed for property crimes in the pre-suspension period, these tests should make us concerned that the results observed for property crime reports in 2016 may be driven by reporting error and not the Equitable Sharing suspension. However, it appears that the results for violent crime are robust to timing and are not replicated in earlier years of the sample.

Categorical Treatment Model Variations

This section will explore tests to the robustness of the categorical treatment models presented in the previous chapter. The models within this section will help determine whether there are stronger effects for agencies which participated more frequently in

Table 6.9: Falsification tests for crime reports: binary model

	(1) Total	(2) Violent	(3) Property
Panel A: False Year 2015			
I[FundsReceived]xI[Susp]	-1.109***	-0.077	-1.033***
	(0.341)	(0.060)	(0.308)
I[FundsReceived]xI[PostSusp]	-0.502	0.099	-0.600**
	(0.326)	(0.072)	(0.302)
Agency-Month FE	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes
Panel B: False Year 2014			
I[FundsReceived]xI[Susp]	-0.794**	-0.169***	-0.625*
	(0.355)	(0.064)	(0.319)
I[FundsReceived]xI[PostSusp]	-0.687*	0.050	-0.737**
	(0.350)	(0.065)	(0.331)
Agency-Month FE	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes
Panel C: False Year 2013			
I[FundsReceived]xI[Susp]	-0.057	-0.040	-0.017
	(0.346)	(0.046)	(0.335)
I[FundsReceived]xI[PostSusp]	-0.641	0.015	-0.656*
	(0.396)	(0.057)	(0.379)
Agency-Month FE	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The panels above display regression results for falsification tests using the baseline binary model presented in this paper. Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Panel A presents results using a fake suspension dummy which marks observations within January through March of 2015. Panel B does the same, but for the year 2014. Panel C likewise uses a fake suspension dummy for these months in 2013. Each model includes agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

Equitable Sharing before the suspension.[3]

Tables 6.9, 6.10, and 6.11 present variations on the categorical treatment model. As with previous specifications, Column 1 displays the DiD estimators for the baseline above- and-below-median specification already covered. Column 2 runs a model which splits treatment into the upper quartile and lower quartile in terms of frequency of participation, dropping all treated agencies in between. Column 3 displays results for a model that compares the above-median treatment group to the below-median treatment group, using the below median agencies as control. Column 4 displays results for a model that compares the upper quartile treated group to the lower quartile treated group, using the lower quartile agencies as control.

Beginning with the results for total crime reports in Table 6.9, I immediately observe that Column 2 shows that more intensely treated agencies experience a stronger negative effect of the suspension. Consistent with the categorical treatment model, the effect is centered in the upper-quartile treated agencies rather than the lower-quartile treated agencies. Since I do not observe a statistically-significant effect of the suspension on above-median agencies relative to control (as shown in Column 1), it seems that only very intensely-treated agencies experience an overall effect of the Equitable Sharing suspension. Moving to Column 3, I observe a null effect of the suspension on above-median agencies relative to the below-median agencies. In Column 4, however, I observe a statistically significant negative effect of the suspension on the upper quartile treated agencies relative to the lower quartile agencies. Interestingly, the results in Column 3 seem to be consistent with those presented in Column 1, and the results in Column 2 seem to be consistent with the results in Column 4.

Table 6.10 displays results for the population-adjusted number of violent crime reports. In Column 2, I observe a significant effect of the suspension on the upper-quartile treatment

[3]The analysis within this section will center on models with crime reports as the independent variable, since most models do not give evidence of an effect of the suspension on clearances.

Table 6.10: Effect of the Equitable Sharing suspension on total crime reports using categorical treatment: various treatment specifications

	(1)	(2)	(3)	(4)
Total Crime Reports per 10k Residents				
I[AboveMedian]xI[Susp]	-0.451		-0.199	
	(0.352)		(0.430)	
I[AboveMedian]xI[PostSusp]	-0.242		0.106	
	(0.336)		(0.387)	
I[BelowMedian]xI[Susp]	-0.274			
	(0.311)			
I[BelowMedian]xI[PostSusp]	-0.319			
	(0.320)			
I[UpperQuartile]xI[Susp]		-0.756**		-0.787**
		(0.374)		(0.360)
I[UpperQuartile]xI[PostSusp]		0.140		0.088
		(0.366)		(0.357)
I[LowerQuartile]xI[Susp]		0.270		
		(0.409)		
I[LowerQuartile]xI[PostSusp]		0.439		
		(0.368)		
Agency-Month FE	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for various regression models which explore heterogeneity within the treatment group. All columns display results for the dependent variable of total crime reports per 10,000 jurisdiction residents. All models include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

agencies relative to the control, and no effect of the suspension on the lower quartile treatment agencies. This is relatively consistent with the baseline, except that the DiD estimate for both lower and upper quarterly treatment agencies in the post-suspension period is significant. This could be concerning, but note that the magnitude of the within-suspension estimator is larger at around 0.30 relative to the post-suspension estimator of 0.24 for the above-quartile agencies. As such, even if there are systematic differences between treatment and control, the results indicate a larger effect within the suspension period. Column 3 shows that the above-median DiD estimate for the within-suspension period is positive and significant, which is consistent with previous results. Column 4 also shows a similar within-suspension DiD estimate for the upper quartile treated group relative to the lower quartile group, but also with a significant DiD estimate for the post-suspension period.

Table 6.11 displays results for the population-adjusted number of property crime reports per 10,000 jurisdiction residents. Column 2 shows that, for the upper-quartile treatment group relative to the control, I observe a significant negative effect of the suspension on property crime reports. This effect dissipates for the lower quartile group, however, which is consistent with the above and below median specification. Column 3 shows no effect of suspension on the above-median treatment group when the below-median group is used as control, but I do observe a consistent negative effect when comparing the upper-quartile treatment group to the lower-quartile treatment group. None of the post-suspension DiD estimates are significant within any of these specifications for property crime.

Heterogeneity Analysis

The previous two sections detailed the results of my regression models as well as their robustness to control and panel specifications. Ultimately, I observe a robust effect of the Equitable Sharing suspension on violent crime reports and suggestive evidence of an effect on property crime reports. These move in opposite directions and ultimately lead to statistical

Table 6.11: Effect of the Equitable Sharing suspension on violent crime reports using categorical treatment: various treatment specifications

	(1)	(2)	(3)	(4)
Violent Crime Reports per 10k Residents				
I[AboveMedian]xI[Susp]	0.240***		0.284***	
	(0.085)		(0.092)	
I[AboveMedian]xI[PostSusp]	0.136		0.118	
	(0.088)		(0.088)	
I[BelowMedian]xI[Susp]	-0.040			
	(0.056)			
I[BelowMedian]xI[PostSusp]	0.007			
	(0.052)			
I[UpperQuartile]xI[Susp]		0.301***		0.289***
		(0.100)		(0.099)
I[UpperQuartile]xI[PostSusp]		0.237**		0.221**
		(0.107)		(0.106)
I[LowerQuartile]xI[Susp]		0.096		
		(0.062)		
I[LowerQuartile]xI[PostSusp]		0.129**		
		(0.057)		
Agency-Month FE	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for various regression models which explore heterogeneity within the treatment group. All columns display results for the dependent variable of violent crime reports per 10,000 jurisdiction residents. All models include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

Table 6.12: Effect of the Equitable Sharing suspension on property crime reports using categorical treatment: various treatment specifications

	(1)	(2)	(3)	(4)
Property Crime Reports per 10k Residents				
I[AboveMedian]xI[Susp]	-0.690**		-0.483	
	(0.322)		(0.397)	
I[AboveMedian]xI[PostSusp]	-0.378		-0.012	
	(0.310)		(0.362)	
I[BelowMedian]xI[Susp]	-0.234			
	(0.290)			
I[BelowMedian]xI[PostSusp]	-0.327			
	(0.297)			
I[UpperQuartile]xI[Susp]		-1.056***		-1.077***
		(0.333)		(0.319)
I[UpperQuartile]xI[PostSusp]		-0.096		-0.133
		(0.338)		(0.331)
I[LowerQuartile]xI[Susp]		0.174		
		(0.394)		
I[LowerQuartile]xI[PostSusp]		0.310		
		(0.351)		
Agency-Month FE	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table above displays coefficients for various regression models which explore heterogeneity within the treatment group. All columns display results for the dependent variable of property crime reports per 10,000 jurisdiction residents. All models include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

null result for total crime reports. This section will briefly explore heterogeneity analysis to determine which violations are be driving these results within the violent and property crimes report aggregates.

Table 6.12 gives both within-suspension and post-suspension DiD estimates using the five aggregated violent crime report categories as dependent variables: murder, manslaughter, rape, robbery, and assault. For the within-suspension DiD estimator, I observe two statistically significant estimates for the dependent variables of rape and assault. The estimate for rape is statistically significant at the 99% confidence level, whereas the assault estimate is significant at the 90% confidence level. Note that the estimate for assault is 0.203, much larger than the estimate for rapes which is 0.032. Next, consider the DiD estimates for the post-suspension period for these dependent variables. Almost all of these are significant and positive. This is strange, considering the post-suspension DiD estimates are mostly statistically insignificant within my baseline models. One explanation could be that the negative post-suspension estimate on robbery, which is negative, cancels out the positive estimates on assault, rape, and murder. Either way, this may implicate the design of my strategy if treatment and control are somehow systematically different from one another through the whole panel. However, what is more likely is that as I dis-aggregate my dependent variables, their variation becomes substantially less consistent. For example, since robbery and murder are relatively rare to begin with compared to other crime types, it could be that in the post-suspension period, some agencies in the treatment group experience differential changes relative to the control for any number of reasons. Even if just for several months, this differential effect would result in a significant estimate for the pooled average of those crime types relative to the pre-period. What would implicate the identification strategy in this paper is if some factor besides the suspension was uniquely influencing the the treatment group in the months of January, February, and March of 2016.

Table 6.13 presents results which further disaggregate rape and assault into forced

Table 6.13: Heterogeneity analysis for violent crime rates: murder, manslaughter, rape, robbery, assault

	(1) Murder	(2) Manslaughter	(3) Rape	(4) Robbery	(5) Assault
I[FundsReceived]xI[Susp]	0.003	-0.000	0.032***	-0.016	0.208*
	(0.003)	(0.000)	(0.010)	(0.024)	(0.120)
I[FundsReceived]xI[PostSusp]	0.004*	0.000	0.045***	-0.167***	0.482***
	(0.002)	(0.000)	(0.008)	(0.031)	(0.157)
Agency-Month FE	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

and attempted rape, as well as assault with a gun, knife, other weapon, no weapon, and simple assaults. The results give evidence that, within rape and assaults, the effects of the suspension are likely centered within forced rape and assault with a gun. This is consistent with a black market capital effect story, where police do not engage in asset forfeiture as much as a result of the suspension. This results in more black market capital, which could be in the form of weapons or in the form of cash which can be used to purchase weapons. If this were true, then I might expect that that the availability of illegal weapons would lead to an increase in reports of crimes which involve weaponry. However, as mentioned previously, I cannot disentangle this from increased reports due to lower policing, market capital, or some other factor. It should also be noted that most of the DiD estimates for the post-suspension period are significant within this table, consistent with what was previously observed.

Finally, Table 6.14 gives the heterogeneity analysis results for the four aggregated types of property crime reports: burglary, non-vehicle theft, grand theft auto, and arson. Results indicate a statistically significant decrease in reports of burglary and grand theft auto.

Interestingly, the table also indicates that arson slightly increased within the suspension period for the treated agencies relative to the control, which is somewhat unexpected. Note that the post-suspension DiD estimate for burglary is statistically significant and negative as well. These results could be consistent with a "theft substitution" story, wherein asset forfeiture incentivizes criminals to engage in more theft of private property in order to account for depletion of illegal capital stocks. More analysis will be presented regarding the interpretation of these reduced-form results in the next chapter.

Table 6.14: Heterogeneity analysis for violent crime rates: dis-aggregating rapes and assaults

	Rape		Assault				
	(1) Forced	(2) Attmpt	(3) Gun	(4) Knife	(5) Othr Weap.	(6) Unarmed	(7) Simple
I[FundsReceived]xI[Susp]	0.031***	-0.002	0.105***	0.020	0.027	0.035	0.043
	(0.009)	(0.001)	(0.022)	(0.013)	(0.017)	(0.022)	(0.102)
I[FundsReceived]xI[PostSusp]	0.041***	-0.001	0.144***	0.025**	0.022	0.050***	0.250*
	(0.007)	(0.001)	(0.034)	(0.010)	(0.022)	(0.018)	(0.137)
Agency-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

Table 6.15: Heterogeneity analysis for property crime rates: burglary, theft, grand-theft auto, arson

	(1) Burglary	(2) Theft	(3) GTA	(4) Arson
I[FundsReceived]xI[Susp]	-0.351***	-0.133	-0.107*	0.004*
	(0.087)	(0.227)	(0.060)	(0.002)
I[FundsReceived]xI[PostSusp]	-0.481***	0.176	-0.072	0.002
	(0.107)	(0.199)	(0.062)	(0.004)
Agency-Month FE	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates calculated based on a strongly-balanced agency-by-month panel from 2010 through 2020 for 5,032 local US police agencies (664,224 observations). Regression results include agency-by-month fixed effects, month-by-year fixed effects, and county-year demographic and economic controls. Estimates are weighted by jurisdiction population, and standard errors are robust to heteroskedasticity and clustered at the agency-level.

CONCLUSION

It is important to evaluate both the costs and the benefits of any tool used frequently by law enforcement. The potential costs of asset forfeiture have been documented in the form of wrongful seizures, poor legal standards, and corrupting incentives, but not much research tests the benefits of asset forfeiture funds in the context of policing outcomes. My results contribute to the literature by providing that key test.

Within this paper, I have offered a novel strategy to identify the effects of asset forfeiture funds on measures of criminal activity and police effort. In order to isolate a causal link, this paper studies the three month suspension of the Equitable Sharing program from January through March 2016. This paper is the first within the literature to examine the effects of the Equitable Sharing program on these outcomes, and it is also the first within the asset forfeiture literature to utilize a nationally-representative, agency-by-month policing sample. Using administrative data from the DoJ and FBI, I find that the suspension plausibly increased violent crime reports, but some suggestive evidence that it decreased property crime reports. When aggregated, these effects seem to cancel out, leaving a null effect for total crime reports. While the results for violent crime are robust across almost all specifications, the decrease in property crime is sensitive to model specification, and I observe similar effects in other years of the sample. Further, the vast majority of the within-suspension DiD estimates for crime clearances were statistically insignificant, which means that I cannot reject the null hypothesis that the true estimate is zero.

These results are somewhat difficult to interpret. All else held equal, theory holds that a reduction in forfeiture revenue should cause the agency budget curve to contract, thereby decreasing the amount of policing services purchased. Why might there be an increase in violent crime reports as a result of the Equitable Sharing suspension without a corresponding effect on clearance rates?

Since the estimates on most of the models that use clearance rates as a dependent variable are statistically insignificant, I cannot reject the null hypothesis that the Equitable Sharing suspension did not effect the number of crimes police cleared. However, this does not imply an acceptance of the null. The Equitable Sharing suspension may have impacted policing effort by reducing the amount of funds available for local police to develop task forces, engage in overtime service for their department, or purchase necessary equipment. The UCR clearance data may itself be flawed, which could also produce a false null effect. This could be the case if clearances are systematically misreported, or if there is a very long delay between when a crime is committed and when the crime is cleared by police. As such, the lack of of statistically significant estimates on the clearance rate models cannot disprove the hypothesis that the Equitable Sharing suspension impacted police effort.

If the Equitable Sharing suspension did impact clearance rates within participating-agency jurisdictions, this would influence the interpretation of this paper's reduced-form results for crime reports. One could explain the effect on violent crime as a kind of deterrence effect, in that if police are clearing fewer crimes, criminals may feel that the risk of being apprehended is lower. This might lead criminals to engage in more illegal behavior, thereby increasing the number of crimes reported to police within the suspension period.

The decrease in property crimes is more difficult to explain with a police effort story. Recall that these results are not as robust as those observed for violent crime, so the correlation may be spurious. However, if there was a true negative effect on property crime, one might explain it by appealing to the concern about the UCR reports mentioned in the Data chapter: crime reports can be filed by police officers as well as citizens. If policing decreases, officers may be less likely to report certain types of crimes because they do not observe them occurring. But why would this primarily affect property crime and not violent crime? One explanation arises from the incentive system created by asset forfeiture. It is plausible that, as a result of the suspension of Equitable Sharing money, police agencies substantially reduced the type of policing that is likely to uncover assets that can be forfeited, since it is more difficult to profit from acquisitions. Instances of asset forfeiture are very likely reported by the police agencies themselves since forfeiture often happens during routine traffic stops based on behavior directly observed by the officer. As such, police may record cases of asset forfeiture within their UCR property crime reports, but not within their UCR clearance reports. This is because forfeiture's legal designation as an action against an object reduces the likelihood that an arrest is ever made.[1] The reduction in forfeiture funds likely decreased the incentive to engage in forfeiture through Equitable Sharing, which could in turn reduce the number of property crimes involving asset forfeiture reported by police in the UCR data. Given that very few of the forfeitures made by police are associated with a criminal charge or an arrest, there would be no corresponding change in arrest rates. An easy way to picture this is to consider the anecdote of Stephanie Wilson provided at the beginning of the paper. Police may have initially reported a property crime, but no charge or arrest was ever made. As such, it is possible that the incident was logged as a report, but not as a clearance.

It is certainly plausible, however, that there is a true null effect on clearance rates since the Equitable Sharing funds cannot directly be used for paying officer salaries besides for overtime payments. It may be that many agencies use the funds only to purchase better policing equipment, and the suspension merely reduced the amount of additional equipment that could be purchased. In this case, police may be somewhat less safe or efficient, but it may not have a direct impact on the number of crimes cleared. Or, since Equitable Sharing funds can also be spent on task forces and other kinds of concerted crime efforts, the formation of these teams may have drawn resources away from other policing activity; as such, clearances might have decreased as much as they increased for treated agencies within the suspension.

[1] Recall that a clearance rates is very similar to an arrest rate, which means that a crime not associated with an arrest would not be logged as a clearance.

Explanations which assume a null effect on police effort are available, but somewhat implausible. One way to explain these results is that criminal agents, if they had knowledge of the suspension and believed it to be harmful to police agencies, might have acted "as if" police agencies were altering their policing bundle when they really were not. In other words, criminals may have committed more violent crime due to a perceived decrease in overall police presence, and less property crime due to a perceived decrease in asset forfeiture done by police agencies. As more information became available in the coming months, criminals may have realized that the suspension did not affect policing behavior and thus adjusted accordingly. This could explain how the effects documented within this paper seem to subside by March of 2016. Or, after the suspension, police may have reduced the number of purchases of high-quality firearms and vehicles, which may have increased the number of violent crimes through a kind of "reverse" deterrence mechanism. If criminal agents believe they have superior weapons relative to police agencies, then they may have a higher propensity to engage in violent crime because they think police are less likely to intervene in a violent situation. This explanation requires that criminal agencies knew about the reduced quality or quantity of these purchases, which is a shaky assumption. Additionally, the reduction in property crimes as a result of the suspension is more difficult to explain in this context.

In summary, this paper documents a reduced-form effect of the Equitable Sharing suspension on crime reports within police jurisdictions, one that is difficult to explain. Future research could inform the interpretation of this work. For example, as more states and agencies migrate into using the National Incident-Based Reporting System (NIBRS), other papers could use incidence-level data to further tease out the effect of the Equitable Sharing suspension on crime and clearance rates. While this paper uses the aggregate count data sets reported in the FBI's UCR system, incidence-level data would be particularly be helpful to identify the suspension's effect on police effort. Such papers could use a measure of time-to-clearance as the dependent variable, which would identify whether the suspension in

Equitable Sharing funds increased the amount of time it takes a police agency to clear crimes within its jurisdiction. Additionally, as more states pass laws requiring the documentation of state-level asset forfeiture, future work could better control for potential substitution into other forms of revenue-generating behaviors.

REFERENCES CITED

Apel, R. and Nagin, D. S. (2011). General deterrence: A review of recent evidence. *Crime and public policy*, 4:411–436.

Baicker, K. and Jacobson, M. (2007). Finders keepers: Forfeiture laws, policing incentives, and local budgets. *Journal of Public Economics*, 91(11-12):2113–2136.

BEA (2022). Bureau of economic analysis data. *Bureau of Economic Analysis, United States, https://www.bea.gov/data*.

Beck, B., Holder, E., Novak, A., and Kaplan, J. (2023). The material of policing: Budgets, personnel and the united states' misdemeanour arrest decline. *The British Journal of Criminology*, 63(2):330–347.

Benson, B. L., Rasmussen, D. W., and Sollars, D. L. (1995). Police bureaucracies, their incentives, and the war on drugs. *Public Choice*, 83(1):21–45.

Billy, A. (2020). Policing for profit and asset forfeiture: Evidence from the equitable sharing suspension. *Available at SSRN 3594881*.

BJS (2012). Major information systems - consolidated asset tracking system. *Bureau of Justice Statistics; Law Enforcement Agency Identifiers Crosswalk, United States, https://doi.org/10.3886/ICPSR35158.v2*.

Campbell, D., Johnson, S., et al. (2015). The effect of municipal drug fund revenues on crime rates". *Economics Bulletin*, 35(4):2439–2442.

Chalfin, A. and McCrary, J. (2017). Criminal deterrence: A review of the literature. *Journal of Economic Literature*, 55(1):5–48.

Cunningham, T., Johnson, W., Youngblood, D., Thompson, J., Fitzpatrick, W., and Manger, T. (2015). Joint letter. *Washington Post Archive, https://www.washingtonpost.com/blogs/wonkblog/files/2015/12/JointAFESletter_Obama.pdf*.

DoJ (2015). Assets forfeiture fund rescission impact on equitable sharing program. *Equitable Sharing Wire, https://www.justice.gov/criminal-afmls/file/801381/download*.

DoJ (2016). Deferral of department of justice equitable sharing payments: Fact sheet. *Equitable Sharing Wire, https://www.justice.gov/criminal-afmls/file/811836/download*.

DoJ (2018). Guide to equitable sharing for state, local, and tribal law enforcement agencies. *Department of Justice, https://www.justice.gov/criminal-afmls/file/794696/download*.

DoJ (2021). Major information systems - consolidated asset tracking system. *US Department of Justice, https://www.justice.gov/jmd/major-information-systems-consolidated-asset-tracking-system*.

Evans, W. N. and Owens, E. G. (2007). Cops and crime. *Journal of public Economics*, 91(1-2):181–201.

Gius, M. (2018). The effects of civil and criminal forfeiture on drug-related arrests. *Justice Policy Journal*, 15(1).

Holcomb, J. E., Williams, M. R., Hicks, W. D., Kovandzic, T. V., and Meitl, M. B. (2018). Civil asset forfeiture laws and equitable sharing activity by the police. *Criminology & Public Policy*, 17(1):101–127.

Ingraham, C. (2016). The feds have resumed a controversial program that lets cops take stuff and keep it. *The Washington Post, https://www.washingtonpost.com/news/wonk/wp/2016/03/28/the-feds-have-resumed-a-controversial-program-that-lets-cops-take-stuff-and-keep-it/.*

Kaplan, J. (2020). Jacob kaplan's concatenated files: Uniform crime reporting program data: Offenses known and clearances by arrest (return a), 1960-2020. *Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], https://doi.org/10.3886/E100707V17.*

Kelly, B. D. and Kole, M. (2016). The effects of asset forfeiture on policing: a panel approach. *Economic Inquiry*, 54(1):558–575.

Klick, J. and Tabarrok, A. (2005). Using terror alert levels to estimate the effect of police on crime. *The Journal of Law and Economics*, 48(1):267–279.

Knepper, L., McDonald, J., Sanchez, K., and Pohl, E. S. (2020). Policing for profit: The abuse of civil asset forfeiture, 3rd addition. *Institute for Justice.*

Leins, C. (2016). New hampshire: Ground zero for opioids. *US News, https://www.usnews.com/news/best-states/articles/2017-06-28/why-new-hampshire-has-one-of-the-highest-rates-of-opioid-related-deaths.*

Makowsky, M. D., Stratmann, T., and Tabarrok, A. (2019). To serve and collect: the fiscal and racial determinants of law enforcement. *The Journal of Legal Studies*, 48(1):189–216.

Mast, B. D., Benson, B. L., and Rasmussen, D. W. (2000). Entrepreneurial police and drug enforcement policy. *Public Choice*, 104(3):285–308.

Mello, S. (2019). More cops, less crime. *Journal of public economics*, 172:174–200.

Morris, A. (2016). N.h. top politicians want state to continue to collect federal forfeiture funds. *The Concord Monitor, https://www.concordmonitor.com/Archive/2016/03/From-Archives/forfeiture-CM-030816.*

MRSC (2022). Introduction to budgeting. *Municipal Research and Services Center, https://mrsc.org/explore-topics/management/financial-management/introduction-to-budgeting.*

Mughan, S., Li, D., and Nicholson-Crotty, S. (2020). When law enforcement pays: Costs and benefits for elected versus appointed administrators engaged in asset forfeiture. *The American Review of Public Administration*, 50(3):297–314.

Operti, E. (2018). Tough on criminal wealth? exploring the link between organized crime's asset confiscation and regional entrepreneurship. *Small Business Economics*, 51(2):321–335.

Snyder, R. (2021). Police, prosecutors oppose proposed bill changing asset forfeiture for low-level drug crimes. *The Nevada Independent*.

Vidal, J. and Kirchmaier, T. (2018). The effect of police response time on crime clearance rates. *The Review of Economic Studies*, 85(2):855–891.

Weisburd, S. (2021). Police presence, rapid response rates, and crime prevention. *The review of economics and statistics*, 103(2):280–293.

Worrall, J. L. (2001). Addicted to the drug war: The role of civil asset forfeiture as a budgetary necessity in contemporary law enforcement. *Journal of Criminal Justice*, 29(3):171–187.

Worrall, J. L. and Kovandzic, T. V. (2008). Is policing for profit? answers from asset forfeiture. *Criminology & Public Policy*, 7(2):219–244.