FINANCING THE WAR ON DRUGS: THE IMPACT OF LAW ENFORCEMENT GRANTS ON RACIAL DISPARITIES IN DRUG ARRESTS

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Abstract

This study estimates the impact of the discretionary Edward Byrne Memorial State and Local Law Enforcement Assistance Program (EBMGP) on drug arrests and crime. The 1986 and 1988 Anti-Drug Abuse Act allocated federal funds to state and local municipalities to combat illicit drug use and violent crime associated with drug sales and trafficking. The results show that the implementation of the EBMGP resulted in an increase in police hiring, an increase in drug sales arrest rates for blacks and whites, and a decrease in total crime. Nonetheless, black-white racial disparity in the drug sales arrest rate still significantly increases by approximately 1 for every 1,000 black residents. Our findings highlight the role of federal crime control policies in state and local policing. Although, the EBMGP was a color-blind policy initiative, it was not race neutral in its implementation.

JEL Classification

K4, K42, H76

Keywords

Crime, arrest, drugs, federal grants, incarceration

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Introduction

Over the past 40 years, the United States has experienced exponential growth in its incarceration rate. At the same time, the prevalence of arrests has increased significantly: almost one-third of individuals are arrested by the age of 23, a figure that was closer to one-fifth in 1965 (Brame, Turner, Paternoster, & Bushway, 2012). Although incarceration garners the most attention when it comes to criminal justice reform, the number of individuals arrested and convicted of felonies is more than that of imprisoned individuals. The U.S. Department of Justice (2014) reports in a 2012 survey of state criminal records repositories that over 100 million[1] individuals were found to have a criminal record. In addition, previous research finds that the punitive approach to crime beginning during the 1970s led to an overall increase in arrests, criminal convictions, and incarceration (Neal & Rick, 2016; Raphael & Stoll, 2013), which disproportionately impacted black Americans (Shannon et al., 2017).

As the country adopted a more punitive approach, states implemented distinct changes to the criminal justice system (Phelps, 2017), creating wide variations in incarceration and felony conviction rates across states; in 2010, there were an estimated 18 million Americans with felony convictions (Shannon et al., 2017). Some argue that the increase in arrests and felony convictions is attributable to "…unprecedented federal dollars funneled to local police departments and new policing tactics that condoned arrests for even the smallest offenses" (Fields & Emshwiller, 2014). This coincides with Neal and Rick's (2016) observation that rather than reflecting changes in criminal activity, the dramatic increase in drug arrests after 1985 reflects changes in police conduct.

Researchers have sought to empirically investigate the federal government's involvement in local policing by analyzing federal initiatives such as federal asset forfeiture, Community Oriented Policing (COPS) grants for local police personnel, COPS grants for School Resources Officers, and

[1] Note that this number has not been de-duplicated, thus, one person could have a record in multiple states.

the National Defense Authorization Act (1033 program), which allows law enforcement agencies to acquire excess Department of Defense (DoD) equipment suitable for law enforcement (Baicker & Jacobson, 2007; Bove & Gavrilova, 2017, Evans & Owens, 2007; Harris et al., 2017; Owens 2017; Weisburst, 2019a, 2019b). However, less attention has been paid to the Edward Byrne Memorial State and Local Law Enforcement Assistance Grant Program (EBMGP). According to the Government Accountability Office (1993), the EBMGP was "…the primary source of federal financial assistance for state and local drug law enforcement efforts" (U.S. Government Accountability Office [GAO], 1993, p. 2). One significant innovation that resulted from this funding is the creation of multijurisdictional drug task forces. Nonetheless, few studies have rigorously estimated the influence of the EBMGP on policing behavior. This study aims to fill this gap by estimating the effects of this program on drug arrests and drug arrest rates by race.

The Anti-Drug Abuse Act of 1986 and the Anti-Drug Abuse Act of 1988 (Public Law 100–690) established the EBMGP, initially allocating over $200 million to states and local municipalities in an effort to reduce drug-related crime and support the national agenda concerning drug control. Through the EBMGP, the Bureau of Justice Assistance awarded formula grants to states; the state planning boards then allocated these funds to state and local government units, agencies, and organizations. In addition, the federal government directly awarded discretionary grants to state and local municipalities to improve police effectiveness. By 1994, nearly $500 million had been allocated "to provide personnel, equipment, training, technical assistance, and information systems for widespread apprehension, prosecution, adjudication, detention, and rehabilitation of offenders who violate state and local laws" (Dunworth, Haynes et al., 1996a).

Aimed primarily at combating illicit drug use and violent crimes related to the illegal drug trade in urban areas, the EBMGP also sought to improve coordination among the various components of the criminal justice system (Bureau of Justice Assistance, 1997). A sizable portion of the federal funds

allocated was used to create multijurisdictional drug task force units (MJDTF; Dunworth, Haynes et al., 1996b), which were rolled out across the United States to assist in the apprehension of drug-related criminal offenders. These task forces brought about an improvement in police communication and tactical response, as well as a substantial increase in drug arrests (Blumenson & Nilsen, 1998; McGarrell & Schlegel, 1993; Jefferies, Frank, Smith, Novak, & Travis, 1998). However, these claims are yet to be substantiated by rigorous empirical evidence (Smith et al., 2000; Mazerolle, 2007).

Using the federal expenditure data from the Consolidated Federal Funds Report and the arrest data from the Uniform Crime Report, this paper studies the impact of the EBMGP's *discretionary* grants on drug arrest rates by race, focusing on the arrests for drug possession and drug sales for the following reasons. First, the EBMGP specifically focused on the improvement of the apprehension, prosecution, adjudication, detention, and rehabilitation of drug offenders. Second, while the EBMGP may influence drug crime rates, the Uniform Crime Reporting (UCR) program only records criminal offenses reported for non-drug-related crimes. Third, racial disparities in mass incarceration have been linked directly to changes in policing, arrests, and prosecution during a period that saw a steady decline in crime rates (Neal & Rick, 2016; Raphael & Stoll, 2013). Therefore, focusing solely on crime provides a modicum of information on the impact of the EBMGP and its possible influence on the racial gap in arrests.

The research design exploits variations in the timing and location of funding to identify a causal relationship between the EBMGP and drug arrests. An event-study framework is employed to provide a statistical description of the evolution of pre-trends in arrest rates, as well as to highlight the dynamic response of arrest rates after the first grant is received (Jacobson, LaLonde, & Sullivan, 1993). In addition, the research design, by focusing on program implementation rather than grant size, illustrates how a relatively small grant program can significantly influence policing and crime-related outcomes. The event-study framework provides an intertemporal response to crime-related outcomes that are gradual and nonlinear. The pre-treatment effects provide a falsification test of pre-treatment, time-

varying, and city-level unobservable characteristics that influence crime-related outcomes in a manner similar to the pre-treatment test in the difference-in-differences (DiD) literature.

These event-study results provide evidence that the implementation of the Byrne program resulted in a 1.3% increase in police hiring and a 20% increase in drug sales arrests shortly after receipt of the first grant. In addition, the post-treatment effects vary by race. The implementation of the EBMGP led to an immediate increase in drug sales arrests for blacks. According to the results, drug sales arrests of black Americans increased by 26% one year after treatment, reaching a maximum increase of 30% two years after treatment. Relatedly, the post-treatment effects imply an immediate 9% increase in the number of white Americans arrested for drug sales. The post-treatment effects, which increase over time and are statistically significant, indicate that their arrest rates were 36% above the baseline rate after five years. The percentage change in the arrest rate of blacks is larger in the initial funding period, and then, it falls below that of whites three years post-treatment. There is little evidence that the EBMGP influenced total violent crime; however, the program is associated with a 3% reduction in the overall crime rate, three years after implementation.

We demonstrate that our results are robust to a variety of specification checks that include limiting the sample to those that fully report; limiting the sample to treated only cities; controlling for spillover effects; changing the control group to late adopters; and focusing on cities treated before the implementation of the COPS and 1033 program. Nonetheless, while the qualitative findings of our results hold up across various specifications, some caution should be taken in their interpretation due to significant pretrends in some of our models. Our results are consistent with prior research showing that additional police resources increase police personnel (Evans and Owens, 2007) and reduce crime (Evans and Owens, Bove & Gavrilova, 2017; Harris et al., 2017). We find suggestive evidence that police expenditures increase after grant receipt and, similar to Baicker and Jacobson (2007) and Harris et al. (2017), federal initiatives targeting drug-related crimes resulted in increases in drug-related arrests. Our analysis indicates that the program had a large effect on under-policed communities,

resulting in a substantial increase in drug sales arrests for whites. Despite the substantial increase in white drug sales arrests, racial differences between black and white arrests significantly increase after implementation of the program. Our analysis finds that immediately following the implementation of EBMGP racial disparities in drug sales arrests increase by 1 for every 1000 black residents. Our model suggests that the discretionary EBMGP was responsible for an additional 45,000 black drug sales arrests and an additional 34,000 drug sales arrests for whites.

Brief History of the Byrne Grant Program

In the 1980s, the death of Len Bias, a high-profile All-American collegiate basketball player, as well as greater media coverage of the new crack epidemic, focused national attention on the drug problem that began in the 1960s, creating a bipartisan agreement to establish federal intervention in local policing initiatives (Kerr, 1986; D'Amato, 1986). The State and Local Assistance for Narcotics Control Program of the Anti-Drug Abuse Act of 1986 authorized the use of federal dollars to assist state and local governments in fighting crime and combating drug abuse problems. The mission was clear—to improve the apprehension, prosecution, adjudication, detention, and rehabilitation of drug offenders.[2] The grants of the program were distributed in two parts: (1) block grants to state and local governments, and (2) discretionary grants for demonstration projects to public and nonprofit organizations. The Anti-Drug Abuse Act of 1988 renamed the grant program to the Edward Byrne Memorial State and Local Law Enforcement Assistance Program (EBMGP) in honor of a police officer who was slain in his patrol car while keeping watch outside the home of a witness in a drug case. The 1988 amendment not only strengthened the federal government's commitment to and involvement in local policing, but also authorized the development of multijurisdictional drug control strategies deemed to be vital to the War on Drugs.

[2] See Program Policy and Administrative Guidance.

The EBMGP distributed federal funds to state and local municipalities for additional personnel, equipment, training, and technical assistance for law enforcement. The program required states to match 25% of the expenses for the block grant, creating a three-to-one ratio of federal involvement in local policing initiatives. During the 1987 fiscal year, $178 million was appropriated for Byrne grants to be disbursed as block grants, and $46 million was made available for discretionary grants. By 1990, the program doubled in size, with a total appropriation of $445 million. The next few years saw a slight increase in the budget to a total of $500 million allocated in 1995 (Dunworth, Green et al., 1996).

Byrne grants were distributed to local municipalities for, initially, 21 purpose areas to influence law enforcement effectiveness; however, approximately forty percent of all the funds were used to establish MJDTFs between 1989 and 1993 (Dunworth, Haynes et al., 1996b). MJDTFs were established to integrate state, local, and national drug law enforcement to specifically support domestic drug control priorities. Byrne grants through MJDTFs were championed as being effective in improving communication between law enforcement agencies (Jefferies et al., 1998; McGarrell & Schlegel, 1993). By 1991, 904 MJDTFs had been established; these were responsible for over 250,000 arrests made in that year alone (Blumenson & Nilsen, 1998). Despite MJDTFs coverage of 83% of the population, little evidence exists that the EBMGP actually influenced drug arrests (Mazerolle, 2007; Smith et al., 2000).

The Violent Crime Control and Law Enforcement Act (VCCA) was the next federally funded intervention in local police initiatives, which was passed by Congress and signed into law by President Clinton in 1994. The VCCA reauthorized appropriations for the EBMGP and a shift in focus toward community policing. The Community Oriented Policing Services (COPS) program was established with the intent to support local law enforcement agencies in crime prevention. The COPS initiative was primarily intended to provide grants to local police agencies for hiring new police officers. Much speculation has surrounded the effectiveness of these grant programs at (1) increasing the number of police officers and (2) reducing crime (Muhlhausen & Walsh 2008). However, the extant economics

literature has established a causal link between community policing grants and lower crime rates using an instrumental variables approach (Evans & Owens, 2007; Government Accountability Office, 2005; Weisburst, 2019a). Moreover, other research has established a causal link between increases in police resources and decreases in crime (Bove & Gavrilova, 2017; Chaflin & McCrary, 2018; Harris et al., 2017; Machin & Marie, 2011; Worrall & Kovandzic, 2010).

The Expected Effects of Byrne Grants on Crime and Arrests

We expect the EBMGP to have a direct effect on crime and arrest rates. In the canonical economic model of crime and punishment, homogenous agents make rational decisions based on their utility from committing a crime; for an individual to commit a crime, the expected utility from the commission of that crime must be greater than zero (Ehrlich, 1996). These decisions are based on the net gain from committing a crime and the likelihood of being apprehended by law enforcement. Moreover, local governments face budget constraints in their efforts to minimize the amount of crime in society. Considering criminal behavior, the government's goal is to minimize the net social loss to society from crimes that are likely to be committed. In this context, it is easy to see that federal grants, ceteris paribus, would lower the cost of additional police resources (e.g., the hiring of additional police officers) for local authorities.

Theoretically, hiring additional police officers will increase the probability of detection, and, consequently, will raise the cost of committing a crime. Innovations in policing techniques and surveillance technology, however, increases the marginal productivity of police officers and ultimately the probability of detection and conviction. Theoretically, more effective or greater policing increases arrests and reduces crime as current criminals are apprehended and future criminals are deterred from engaging in illicit activity because of the increased cost of criminal behavior. This theory of crime predicts that increases in police resources, improvements in policing techniques, and technological

advances in policing will lead to an increase in arrests in the short-run (with potentially no effect on crime) but decrease crime—ceteris paribus—and arrests in the long-run.

Although empirical research over the years has been mixed regarding whether increasing police resources lead to a greater number of arrests and lower crime rates (Cameron, 1998; Marvell & Moody, 1996), Chaflin and McCrary (2018) highlight the importance of measurement error in the ambiguous findings on the relationship between police and crime. After accounting for measurement error, they find the police elasticity of cost-weighted crime to be negative and statistically different from zero. In addition, as previously mentioned, research assessing the COPS program, as well as the 1033 Program, using an instrumental variables framework has found these programs to have a statistically significant and negative effect on crime (Bove & Gavrilova, 2017; Evans & Owens, 2007; Government Accountability Office [GAO], 2005; Harris et al., 2017; Weisburst, 2019a).

Nonetheless, it is possible that additional funding from the federal government (or other outside agencies) leads to funding offsets from state or local government officials (Baicker and Jacobson 2007). Consequently, total police revenues and expenditures can remain unchanged after grant receipt. Nevertheless, federal initiatives such as asset forfeitures have been found to increase arrests even after accounting for any such funding offsets (Baicker & Jacobson, 2007). Therefore, it is possible that federal or outside funding can change local police behavior (through incentives) even if total allocated resources remain unchanged. Likewise, changes in police incentives may lead to increases in arrests, but this may not necessarily translate into crime reduction (Benson, Kim, Rasmussen, & Zuehlke, 1992; Benson & Rasmussen, 1996). Specifically, incentive-driven policing could lead to increases in arrests without any impact on crime, if these incentives lead to a relative shift in focus to other crimes (e.g., drug crimes) with traditionally lower rates of enforcement or to more false arrests (Benson, Rasmussen, & Sollars, 1995; Cox, 2015). Previous research also indicates that changes in police behavior or the size of the police force may actually increase the reporting of crime even if the actual rate of crime is decreasing or unchanged (Cunningham, 2016; Levitt, 1998).

This paper contributes to the literature on policing by investigating the impact of intergovernmental (federal to local) transfers, through the EBMGP, on local policing outcomes (i.e., arrests and crime) in general, and by race. Given the theory and prior research, it is expected that the EBMGP—by promoting anti-drug policing, encouraging innovations in police tactics (e.g., MJDTFs), and enabling the hiring of additional police officers—will lead to an initial increase in arrests for drug-related offenses that diminishes over the long-run as criminals respond to changes in the costs of committing a crime. However, the effects on reported crime are ambiguous because the presence of more police could increase reported crime, while simultaneously lowering the actual crime rate.

Additionally, we also explore the impact of the EBMGP on arrests rates by race. There are several common explanations for racial disparities in drug sales arrests, which provide reasons to believe this funding may have a differential impact on arrest rates by race: 1) blacks and whites may commit violent and drug sales crimes at different rates; 2) the types of drugs consumed and, therefore, the setup of drug markets may vary between blacks and whites; 3) the program explicitly targeted categories of drugs disproportionately consumed and sold by blacks; 4) the availability of additional resources possibly allows for the expansion of law enforcement efforts to under-policed communities; and 5) the program may provide financial incentives for heavier handed policing within more vulnerable communities.

Heterogenous effects in arrests rates by race may exists because of the concentration and segregation of poverty (Firebaugh & Acciai, 2016); highly visible open drug markets in black communities (Beckett, Nyrop, & Pfingst 2006; Johnson, Petersen, & Wells, 1977); and selective enforcement by police departments and leaders (Beckett et al., 2006; Bulman, 2019). Moreover, the EBMGP program at one point had a Crack-Focused Substance Enforcement Program for MJDTFs (U.S. Department of Justice, 1990) and had provisions for cracking down on drugs in public housing–selectively policing the black and poor (Fagan, Davies, & Carlis, 2012; Goetz, 2013). All of this would suggest that additional funding may lead to an increase in the arrest rate for blacks. However, the

EBMGP focused law enforcement's attention on a type of crime for which blacks experience a disproportionate share of the arrests. As a result, the shift in attention toward a perceived "black crime type" may also lead to a greater number of arrests not only of blacks but also of whites who participate in this type of crime (Bulman, 2019). It is also possible that additional resources would allow police to pursue more costly criminals. To the extent that whites have higher apprehension costs, then we would expect to see an increase in white arrest rates when additional resources are made available.

Data on the Edward Byrne Memorial Grant Program and Crime Data

Our analysis of the EBMGP begins with data from the Consolidated Federal Funds Report (CFFR) files, which provides information on federal expenditures of state, county, and local municipalities and entities within the United States. The CFFR provides expenditure data on grants-in-aid, direct loans, government purchases, and other direct payments. The analysis focuses on discretionary grants to take advantage of the variation in timing of the EBMGP.[3] Agency police data are constructed from the UCR program's annual publication, entitled "Law Enforcement Officers Killed and Assaulted" (LEOKA), which contains monthly counts of law enforcement officers killed or assaulted while on duty, as well as the number of civilian and sworn officers as of October 31 of the reporting year. These data are publicly available at the Inter-University Consortium for Political and Social Research (ICPSR) for the years after 1974. Information regarding arrests is gathered from the UCR's "Arrests by Age, Sex, and Race" (AASR) statistics, which categorize monthly arrests by sex, race, age, and offense. Combining the CFFR files with the UCR's LEOKA and AASR, a data set was created that links federal expenditures on public safety and narcotics control to local crime and arrest rates. The analysis focuses on the relationship between federal grants from the EBMGP and local police

[3] We focused on discretionary grants for two reasons. First, the block grants went to state capitals and were distributed to local municipalities based on the discretion of state planning boards. This may have resulted in funds being used for political purposes and not for crime prevention, resulting in the overfunding of rural municipalities. Second, the discretionary grants went directly to local municipalities. These grants emphasized the initiatives directly related to the mission of the grant program and allowed federal influence over local policing matters (Hinton, 2016).

behaviors between 1987 and 2004. In 2005, the Violence Against Women and Department of Justice Reauthorization Act combined the Byrne Grant Program and the Local Law Enforcement Block Grant Program and created the Edward Byrne Memorial Justice Assistance Grant Program (JAG). This reauthorization provides a clear break in policy to analyze the *initial implementation* of the program. Therefore, we focus our analysis on the pre-2005 grant program.

The voluntary nature of the UCR means that many prominent cities do not provide monthly crime information. To address this problem, and to keep our sample as close to recent research (Chaflin & McCrary, 2018) on police and crime as possible, we restrict our sample to agencies that report in 1980 and fully report arrest information (all 12 months) for at least 27 years between 1980 and 2009. We also restrict our sample to cities that have populations greater than or equal to 50,000 residents in every year. The final sample consists of 223 cities that report arrests for most of the sample years.[4] Appendix Table A1 lists every city in the sample and identifies the 93 cities that report to the UCR every month over the entire sample period. We supplement our analysis by reporting results for the 93 fully reporting cities.

The final sample includes 135 cities that received discretionary Byrne grants (treatment group) and 88 cities that were never funded (control group).[5] The treatment group consists of cities that received federal grants between 1987 and 2004. We examine trends in arrests and crime five years before and five years after a city receives its first grant. As a result, the final sample consists of crime, arrests, and demographic information from 1982 to 2009.

Our methodological approach focuses on the implementation of the EBMGP to estimate a causal relationship between grants and police behavior. We also augment our analysis by assessing the

[4] We chose 27 years of reporting to keep as many cities in the sample as possible. Appendix Table B1 provides summary statistics for all cities with populations greater than 50,000 residents, cities in the sample, and cities that report every year between 1980 and 2009. Cities that fully report are slightly larger than the national average and the sample average but not statistically different. Moreover, the cities in the sample are demographically similar to those that fully report and all the cities in the country with populations greater than 50,000 residents. Also, it is important to note that our sample accounts for 57 percent of all arrest made in 1980 for cities with a population greater than 50,000.

[5] The control group did not receive grants between 1987 and 2004 or between 2005 and 2009 after the reauthorization act of 2005.

impact of the initial expansion of the program in treated cities. Figure 1A depicts the estimated probability of receiving a Byrne grant for treated locations relative to cities that never participated in the program. The figure captures the manner in which estimated funding propensities changed after the initial treatment.[6] The initial treatment year is normalized to year 0 for all treated cities. As expected, the probability of being treated in the initial year is equal to 1. Municipalities and local governments that received grants were likely to be treated more than once. After the initial treatment, the estimated probability of receiving additional funding is between 25% and 35%. Figure 1B presents the average size of a Byrne grant over the first five years of treatment. According to Figure 1B, the initial grant typically was the largest (over $500,000); subsequent grants were smaller, but funding increased over time.[7]

Table 1 reports the sample's average characteristics in 1980. The United States Census Bureau County and City Data Books, which are publicly available at ICPSR, provide the demographic data (Haines, 2005; U.S. Census, 1995; U.S. Census, 2002). In 1980, the average sample population was 177,297, and approximately 13% of the residents in these cities were black. Typically, treated cities are larger and poorer, which is reflected in the median income and percentage of the population with female heads of households. In addition, the treated cities have a larger proportion of black residents, experience more crime (as reflected in the crime rate), and have a slightly larger police force with more drug arrests when compared with the control group. Despite possessing a smaller share of black residents, cities in the control group have higher drug arrest rates for black residents. Furthermore, the relative drug arrest rates of blacks and whites differ dramatically between the treated and control groups. Drug arrest rates for blacks are three times greater than the drug arrest rates for whites in the

[6] Figures 1 and 2 report coefficients from $Y_{i,t} = \gamma_i + \sum_{\tau=1}^{5} \pi_{-\tau} D_i 1(t - T^* = -\tau) + \sum_{\tau=1}^{5} \delta_\tau D_i 1(t - T^* = \tau) + \varepsilon_{i,t}$ where $Y_{i,t}$ is an indicator variable equal to 1 if a city received a Byrne grant and the size of the grant, respectively. D_i is an indicator variable equal to 1 if the city ever received a Byrne grant, and $1(\cdot)$ is an indicator variable equal to 1 if the observation year is $\pm\tau$ years from the date that the Byrne grant was received. Section IV outlines this empirical approach in detail.

[7] The sample does not include every city that received funding because of crime data limitations. It is important to note that roughly 50% of total funding went to cities in the sample in any given year. See Appendix Figure B2.

control group, compared to being twice as large as in the treated group, despite the fact that whites typically report higher drug use than blacks (McCabe et al., 2007; Vaughn et al., 2018).

Empirical Strategy

The empirical strategy adopted in this analysis utilizes the differential timing in the implementation of the EBMGP displayed in Table 2. We identify the impact of the program by focusing on when the city received its first discretionary Byrne grant. The treatment, therefore, reflects participation in the program. Even if a city received additional grants, we can compare trends in crime and arrest rates before and after the first grant to estimate a causal relationship between the program and crime related outcomes.[8]

Using the variation in the timing and location of funding, we can identify causality within an event-study framework (Jacobson et al., 1993). The event-study design lends itself well to testing the effects of an outcome before and after exposure to the treatment and provides falsification of pre-treatment, time-varying, city-level unobservables that influence the outcomes. The pre-treatment effects test whether changes in the outcomes occur before the implementation of the treatment. In addition, the event-study approach provides a statistical description of the evolution of pre-trends in the outcome variable as well as the dynamics of changes in the outcome variable after the first grant arrives. We estimate the impact of the EBMGP by using the following linear regression:

$$(1)\ Y_{i,t} = \gamma_i + \alpha_{t,s(i)} + \beta_{t,u(i)} + \sum_{\tau=1}^{q} \pi_{-\tau} D_i 1(t - T^* = -\tau) + \sum_{\tau=1}^{p} \delta_\tau D_i 1(t - T^* = \tau) + \varepsilon_{i,t}$$

where $Y_{i,t}$ is the outcome of interest in city *i* in the year *t (t = 1982, 1983,…2009)*; γ_i is a set of city effects that control for unobservable city characteristics that are time invariant; $\alpha_{t,s(i)}$ are year and

[8] This research design groups cities that received one grant with cities that received multiple grants. We ignore the intensity of the treatment.

state-by-year effects; and $\beta_{t,u(i)}$ are urban-by-year effects.[9] Year effects absorb policies that will impact crime nationally. State-by-year effects capture time-varying state-level changes, such as those related to the business cycle or policy changes (e.g., punishment, enforcement), which can, in turn, influence the supply of criminal activity. Urban-by-year effects capture time-varying changes that influence cities of a certain size or urbanicity. D_i is an indicator variable equal to 1 if the city ever received a Byrne grant. $1(t - T^* = -\tau)$ is an indicator variable equal to 1 if the observation year is $-\tau$ years from the date that the Byrne grant was received or $1(t - T^* = \tau)$ is equal to 1 if the observation year is τ years after the date on which the Byrne Grant Program was first implemented in city i. $1(t - T^* = 0)$ is omitted because of collinearity where T^* is the funding year for the Byrne grant; q refers to the number of lags or years before the first Byrne grant; and p is the lead or years after receiving the first Byrne grant. To ensure that the coefficients are well estimated, event time for $\tau > 5$ and $\tau < -5$ are grouped into endpoints $q = 6$ and $p = 6$. The endpoint coefficients are not estimated using a balanced sample of cities and give unequal weight to cities that have received federal grants very early or late according to the sample. Therefore, these endpoints are omitted from the presentation of results.

The coefficients of interest are $\pi_{-\tau}$, which are the pre-treatment effects, and δ_τ, the post-treatment effects. These estimates describe the dynamics of the outcome variable of interest in funded cities before and after Byrne grants are received. If the econometric model captures the pre-Byrne program evolution of the dependent variable, the pre-treatment effects should be indistinguishable from zero. The treatment effects, δ_τ, reflect the average change in the difference in the outcome variable of interest τ years after the city received the grant.

[9] Urban-by-year effects are year fixed effects interacted with indicator variables for six bins of the 1980 urban share. This restricts the comparison group to similar urban cities within the same state (using state-by-year fixed effects).

Significant key cross-sectional differences exist between funded and unfunded cities. Nonetheless, the identification strategy depends on the manner that crime and arrest rates evolved before a city received their initial grant. According to Table 1, cities that receive discretionary Byrne grants have higher average crime rates between 1980 and 1985, which is not surprising, considering that cities that receive Byrne grants are typically larger, more urban, and are poorer. Important for our research design, however, is that crime evolves similarly in treated and untreated cities before the implementation of the EBMGP. Appendix Figure B1 displays crime rates over time for the treated and control groups, and provides evidence that crime rates are indeed evolving similarly over time. Our analysis accounts for key cross-sectional differences by using city fixed effects to capture differences that are unobservable but constant over time. Untreated cities in this analysis help estimate the evolution of crime and arrest rates over time, and provide a control group to represent how crime and arrest rates would have evolved in the absence of treatment. Untreated cities in this sample provide a plausible control group if city and year fixed effects capture differences in the evolution of arrest rates in treated cities versus untreated cities before the implementation of the grant program. A test of this assumption is embedded within the DiD approach used in this analysis. If crime and arrest rates evolve similarly in treated and untreated cities before the implementation of the program, our analysis will capture any trend break in crime and arrest rates caused by the introduction of the EBMGP.

Our empirical strategy utilizes the variations in the rollout of discretionary Byrne grants. The key identifying assumption is that the timing of the discretionary grants is uncorrelated with other determinants of changes in crime and arrest rates. The first test of this assumption is a regression of crime determinants from the year 1980 on the year the first grant was received. Table 3 reports estimates from ordinary least squares (OLS) regressions for the year in which grants were received and the probability of treatment. Columns 2 and 4 report estimates from the aforementioned OLS regressions including the change in crime rates between 1980 and 1985. Similar to Table 1, key demographic variables are strong predictors of treatment. However, the only predictor of timing is

female heads of households. According to Table 3, changing crime rates fail to predict when a city first receives a discretionary grant.

The second test of the identifying assumption compares the timing of Byrne grants with the pre-program growth rates in crime. Figure 2A plots the changes in total crime from 1980 to 1985 (timing) against the year of first treatment, and Figure 2B plots the coefficients on year-by-treatment fixed effects from a regression of crime rates on treatment status. According to Figure 2A and 2B, the timing of the discretionary grants is uncorrelated with changes in pre-period growth in crime rates. These tests provide statistical evidence that pre-period crime rates and determinants of crime do not predict the variation in the timing of the initial grant. Consequently, the timing of the first grant will identify a causal relationship between the Byrne grants and the crime and arrest rates, if one exists. Because the model captures changes in the outcome variable of interest that are unrelated to prior trends in crime and arrests, the post-treatment effects will capture any trend break in the outcome variable of interest that is caused by the implementation of the EBMGP.

The event-study estimates can also be summarized in a DiD specification represented by the following equation (Bailey & Goodman-Bacon, 2015):

$$(2)\quad Y_{i,t} = \gamma_i + \alpha_{t,s(i)} + \beta_{t,u(i)} + \sum_q \tilde{\pi}_{-\tau} D_i 1(t - T^* \in q) + \sum_p \tilde{\delta}_\tau D_i 1(t - T^* \in p) + \varepsilon_{i,t}$$

where the notation remains as defined above, and q indexes the group of all the years more than five years before the receipt of grants and years −5 to −1; p indexes each of the periods for the years 1–5 and 6 and later. This specification is less connected to the timing of the changes when compared with the event-study approach in Equation 1; however, it has the advantage of summarizing the estimates and their joint statistical significance.[10]

[10] Following Bailey and Goodman-Bacon's (2015) approach, we analyze the heterogeneous treatment effects by city characteristics using the following specification:

$$Y_{i,t} = \gamma_i + \alpha_{t,s(i)} + \sum_k \{\sum_q \tilde{\pi}^k_{-\tau} D^k_i 1(t - T^* \in q) + \sum_p \tilde{\delta}^k_\tau D^k_i 1(t - T^* \in p)\} + \varepsilon_{i,t}$$

where, D^k_i is equal to 1 if a city ever received a grant under the EBMGP and belongs to group k. We will reference these results, and they are included in the appendix.

Event-Study Results for Police Hiring and Drug Arrests

Using the estimates from Equation 1, we plot the pre- and post-treatment effects from a balanced panel. Estimates from Equation 1are plotted with a solid line with circular markers identifying the marginal effects for each event-year. The 95% confidence intervals for Equation 1 are indicated by dashed lines. These confidence intervals are constructed from heteroscedasticity-robust standard errors clustered by city.

A. Event-study Results for Police and Drug Arrests

Figure 3 plots the pre- and post-treatment effects for Byrne grants on sworn police per 1,000 residents. The point estimates for $\pi_{-\tau}$ are negative and statistically insignificant, except for in event-year 3. An F-test reports that the pre-treatment effects are jointly statistically insignificant.[11] After the initial grant is received, the post-treatment effects are positive but are only statistically significant in event-year 2 and are marginally statistically significant in event-year 3. According to the estimates from Equation 1, sworn police officers per 1,000 residents increased to 1.3% above the baseline rate (event-year 0) in event-year 2 and event-year 3.[12] The increase in law enforcement after the initial treatment is consistent with the utilization of federal funds for acquiring additional personnel to apprehend individuals in violation of local and state substance-abuse laws. The Anti-Drug Abuse Act of 1986, which introduced federal grants to state and local institutions, and the Anti-Drug Abuse Act of 1988, which formally introduced the EBMGP, outlined the purposes of the grant. Approved expenditures included additional personnel, equipment, training, technical assistance, and information for widespread apprehension, prosecution, and rehabilitation of persons violating drug and crime laws (Anti-Drug Abuse Act, 1988). Nonetheless, we should be cautious in interpreting the results. The

[11] Appendix C reports pre- and post-treatment effects for every event-study regression in the manuscript as well as an F-test of the joint pre-treatment effects. See Table C1 for estimates of pre- and post-treatment effects for Figure 3.
[12] Average sworn police officers per 1,000 residents at event-year zero is 2.027.

event-study provides suggestive evidence that treated places increased the number of police relative to the control group.

Figure 4A shows the pre- and post-treatment effects for drug arrests per 1,000 residents. Here, the pre-treatment effects are indistinguishable from zero and statistically insignificant. The post-treatment effects are positive and are marginally statistically significant in event-year 2. The post-treatment effects provide suggestive evidence that the EBMGP increased drug arrests. According to post-treatment effects, two years after the treatment, drug arrests are 7.3% above the baseline rate.[13] However, when we analyze drug arrests by type of arrests, then we see that this result is largely driven by the large proportion of drug possession arrests that constitute total drug arrests. Specifically, Figure 5A provides pre- and post-treatment effects for the influence of the first Byrne grants for drug possession arrests per 1,000 residents. The pre-treatment effects for drug possession are statistically insignificant, whereas the post-treatment effects are zero or near zero and statistically insignificant. Nonetheless, Figure 6A reports estimates from Equation 1 for drug sales arrests and shows that the EBMGP is associated with an increase in total drug sales arrests. The post-treatment effects are positive and statistically significant or marginally statistically significant in each of the event-years 1–5. The post treatment effect in event-year 1 indicates a 19.8% increase in drug sales arrests after the first discretionary grant. The post-treatment effects in event-years 2–5 are at least 20% above the baseline rate (event-year 0). The results suggest that the 1986 and 1988 anti-drug acts changed police behavior nationally in support of the War on Drugs, and this is largely due to an increase in arrests for drug sales.

[13] Average total drug arrests per 1,000 residents in event-year 0 is 7.503.

B. Event-study Results for Drug Arrests by Race

Largely, mass incarceration has been driven by increases along the extensive and intensive margins of punishment (Alexander, 2010; Raphael & Stoll, 2013), and racial differences in incarceration are largely caused by racial disparities in arrests (Neal & Rick, 2016). Moreover, incarceration rates for drug-related criminal offenses are differentiated by race (Beckett et al., 2006; Cox, 2015). As previously mentioned, differences in drug arrests rates by race could stem from targeted policing strategies in disadvantaged areas where narcotic sales are more likely to occur outdoors, thereby creating the opportunity for widespread apprehension for drug offenses (Johnson et al., 1977).[14] Racial differences in arrests rates could also stem from racial biases (either explicit or implicit) in law enforcement's organizational structure or leadership (Beckett et al., 2006; Bulman, 2019). In conjunction with historically higher arrest rates for blacks, a greater focus on a perceived "black crime type" caused by EBMGP suggests the possibility of heterogeneous treatment effects by race (Bulman, 2019).

As previously discussed, Figure 4A shows that the EBMGP has a marginal influence on total drug arrest rates. Figures 4B and 4C plot the pre- and post-treatment effects for the total drug arrests per 1,000 residents for whites and blacks, respectively. Figure 4B plots event study estimates from Equation 1 for drug arrests for whites per 1,000 white residents, and Figure 4C focuses on drug arrests for blacks per 1,000 black residents.[15] In Figure 4B, the pre-treatment effects are statistically insignificant except for event-year 5. After the first event-year, the post-treatment effects are positive

[14] It is unclear whether selective enforcement behavior for drug crimes are caused by police officers targeting suspects that are easily apprehended or whether suspects are targeted because of race. Beckett et al.'s (2006) analysis provides evidence that blacks make up a disproportionate share of the drug delivery arrests for indoor and outdoor drug transactions, and this is a result of a focus on drugs disproportionately sold by blacks and targeted enforcement in racially diverse outdoor spaces. They conclude, implicit biases may shape who is viewed as a drug offender, which then impacts the organizational aspects of anti-drug policing and law enforcement behavior.

[15] Arrests rates are calculated as $Y_{itr} = \frac{\sum_j Arrest_{itrj}}{Population_{itr}}$, where Y_{itr} is the arrest in city *i*, in year *t*, for race *r* (*r* = 0 if white, *r* = 1 if black). In addition, *j* refers to the Uniform Crime Report index for violent, property, or drug related offenses. $Population_{itr}$ comes from decennial census data with intercensal years calculated by interpolating between census years.

and generally increasing in magnitude until event-year 4. Additionally, the post-treatment effects are statistically significant in event-year 3 and marginally statistically significant in event-year 4. According to post-treatment effects, drug arrests for whites are 11 and 11.4 % above the baseline rate in event-year 3 and 4, respectively. In Figure 4C, which reports drug arrests for blacks, the pre-treatment effects are statistically insignificant in all earlier periods. The post-treatment effects are positive and relatively large but not statistically significant.[16]

However, similar to the total drug arrest analysis, a different story emerges when we disaggregate drug arrest rates by type of arrest (i.e., possession versus sales). Figure 4B,C report pre- and post-treatment effects on drug possession arrests per 1,000 white and black civilians, respectively. For drug possession arrests for whites, the pre-treatment effects are negative and statistically insignificant; the post-treatment effects are positive, increasing in magnitude until event-year 4, but statistically insignificant. Similarly, the pre- and post-treatment effects are statistically insignificant for black drug possession arrests. According to Figure 5, there is little evidence that the EBMGP increased drug possession arrests of white or black civilians.

Nonetheless, drug sales arrest rates are statistically significant for whites and blacks. Specifically, Figure 6B shows that drug sales arrests for whites increased by 10% in event-year 1, although not statistically significant; by event-year 5, drug sales arrests for whites were a statistically significant 36% above the baseline rate. For blacks, the post-treatment effects are positive and statistically or marginally statistically significant for every event-year following the initial funding. Arrest rates for drug sales are approximately 26% above baseline in event-year 1, increase as high as 30% above baseline in event-year 2, and then decrease to 19% above baseline by event-year 5. Appendix Figure B4 shows that the change in the black arrests rate is increasing at a decreasing rate, whereas the change in the white arrest rate is increasing at an increasing rate. It is also interesting to

[16] The mean drug arrests for black and white Americans per 1,000 residents at event-year 0 are 20.36 and 6.613, respectively.

note that drug sales arrest rates begin to significantly increase for whites (year 2 post-treatment) when the changes in black drug sales arrests rates begin to diminish. Overall, it appears that the EBMGP led to large increases in both black and white drug sales arrest rates, with no significant changes in drug possession arrest rates by race.

C. Event-study Results for Violent Crime and Arrests

The 1986 and 1988 Anti-Drug Abuse Acts also targeted violent crimes related to drug trafficking. Figure 7 reports estimates from Equation 1 for the overall total crimes and violent crimes reported, as well as violent crime arrests for black and white civilians. Violent crime aggregate counts include only incidences of murder, manslaughter, rape, assault, and robbery, whereas total crime includes violent crimes and property crimes, such as burglary, larceny, and motor vehicle theft. Figure 7A displays pre- and post-treatment effects for the influence of the first discretionary Byrne grants on violent crimes per 1,000 residents; Figure 7B reports treatment effects for total crime per 1,000 residents. For both violent and total crime, the pre-treatment effects are negative and not statistically significant. For violent crime, the post-treatment effects increase and subsequently decrease but are never statistically significant.[17] The post-treatment effects for total crime are negative and statistically significant after event-year 3. According to Figure 7B, crime is 3% below the baseline rate after three event-years and 4% below the baseline rate after five event-years. Figure 7C and 7D report treatment effects by race for violent crime arrests. For both black and white violent crime arrest rates, there are no significant trend breaks, and post-treatment effects are near zero.

In terms of crime prevention, results show that crime dropped by 4% five years after treatment relative to the baseline rate. This drop is driven primarily by a decrease in property crime as the violent

[17] Appendix Figure B3 reports pre- and post-treatment effects for murder, robbery, and assault rates. The pre- and post-treatment effects for murder are nearly zero and show no visible trend break. Likewise, pre- and post-treatment effects for assault are not statistically significant. However, while pre-treatment effects are largely insignificant, post-treatment effects show a significant decline in robbery after implementation of EBMGP.

crime rate in general, and the assault and murder rate in particular, remain relatively unchanged. It should be noted that, in addition to property crime, the EBMGP appears to have had a negative effect on robbery, which may have also contributed to the declining crime rates (see Appendix Figure B3).

D. Sensitivity Analyses

Although we find significant changes in many crime related variables, it is reasonable to be concerned about pre-period trends. Appendix Tables C1-C3 report the coefficients for Figures 3–7. The table also provides an F-test for the joint significance of the pre-treatment effects. Similar to the earlier test for pre-trends, the joint F-Test shows that the pre-treatment effects are jointly statistically insignificant in each of the aforementioned regression results. Moreover, there are various threats to the identification strategy used in the analysis, and we examine several alternative explanations. Figure 8 plots the joint pre- and post-treatment effects from Equation 2 for the impact of the Byrne program on drug sales arrest rates by race for these alternative specifications.[18] Columns 1 and 2 provide pre-treatment and post-treatment effects for white drug sales arrest rates, while columns 3 and 4 report treatment effects for black drug sales arrest rates. Row 1 of Figure 8 reports joint treatment effects for the main specification. For white drug sales arrests, pre-treatment effects are negative and statistically insignificant, and post-treatment effects are positive and statistically significant. For black drug sales arrests, pre-treatment effects are statistically insignificant and post-treatment effects are positive and generally statistically significant.[19]

The aforementioned Table 1 highlights key cross-sectional differences between treated and control groups. In our analysis, we attempted to control for key differences by restricting the control

[18] Appendix Tables C5-C9 provide joint-treatment effects and event-study estimates for each robustness check in Figure 8. The appendix also reports event-study estimates for drug possession arrests.

[19] In addition to Figure 8, we estimated our model using the log of arrest rates. Regression estimates finds a statistically significant increase in both arrests rates of white and black residents for drug sales. It is important to note that these models do not include state-by-year fixed effects. The inclusion of state-by-year fixed effects resulted in statistically significant pre-treatment effects.

group to cities in the same state that have similar urbanicity by using state-by-year and urban-group-by-year fixed effects. Row 2 also reports results when demographic characteristics, interpolated between census years, are included in Equation 2. For both white and black drug sales arrest rates, the treatment effects remain essentially unchanged. This is unsurprising because of the use of urban-group-by-year fixed effects in our main specification.

Because of the voluntary nature of the FBI's UCR program, the data suffer from unit non-response; therefore, we limited our data to the 223 cities that report drug arrests in 27 of the 30 sample years. However, Row 3 reports the joint treatment effects from limiting our sample to the 93 cities that report drug arrests in every year (hereinafter referred to as the fully reporting sample). This sample produces statistically significant post-treatment effects for white drug sales arrest rates. The joint post-treatment effect for white drug sales arrest rates in the fully reporting sample is 73% larger than the original estimate. The joint post-treatment effect for black drug sales arrest rates is 60% smaller than the original estimate and not statistically significant. The different results for black drug sales arrest rates is not due to demographic differences between the two samples; the fully reporting sample closely resembles the original sample in terms of city size, median income, the percentage of the population black, and many other demographic characteristics.[20] Nonetheless, the slight difference in the results could be explained by some unobservable factor associated with fully reporting. Although the post-treatment effect for black drug sales arrest rates is no longer statistically significant, it is still positive and large. However, we cannot rule out that arrests of black civilians for drug sales did not increase after treatment in the fully reporting sample.

It is possible that the Byrne grants had spillover effects for nearby untreated cities through the establishment of Multijurisdictional Drug Task Force Units (MJDTFs), which could have increased

[20] Appendix Table A1 lists all the cities in the sample and denotes the cities that are in the fully reporting sample. Appendix Table B1 reports summary statistics for all cities with populations greater than 50,000 residents in 1980, the 223 cities used in the sample, and the 93 cities in the fully reporting sample. The three samples are statistically similar (columns 3 and 5 report p-values for differences), but the fully reporting sample has a slightly larger population and is denser (not statistically different).

drug arrests in both treated and neighboring untreated cities, wherein the full sample specifications would underestimate the EBMGP's effect. Row 4 restricts the sample to only cities that received at least one Byrne grant between 1987 and 2004. Once again, only the post-treatment effects are statistically significant for drug sales arrest rates. The effects are larger, which provides suggestive evidence of possible spillover effects because of the MJDTF. However, if drug arrests are increasing all over the country, the larger estimates are overstating the impact of Byrne grants. To better identify the Byrne grant's impact, we incorporate a spatial measurement of treatment to account for possible spillover effects. Row 5 changes the treatment status of cities in the control group that are within 25 miles of a treated city. For drug sales arrest rates, using a broader definition of treatment produces estimates that are slightly larger than the original effects but smaller than the treated only sample. The slightly larger estimates provide suggestive evidence that a spillover effect produces downward biased estimates and the true effect of the Byrne grant program is possibly larger. If this is the case, the event-study is providing a lower bound for the program's impact.

Given the spillover effect and key cross-sectional differences, it is reasonable to question whether the control group used herein is appropriate. In an attempt to construct an alternative control group, we exploit the variation in the timing of the participation in the program across cities. Row 6 provides pre and post-treatment effects when places that are treated earlier are compared to places that are treated later (stacked DiD). In order to compare early adopters to late adopters, cities that are treated between 1987 and 1993 are in the treatment group and cities treated between 1999 and 2004 are in the control group. Cities treated between 1994 and 1998 and cities that were never treated are removed from the sample. Using the stacked DiD approach, the pre-treatment effects are statistically insignificant and the post-treatment effects are larger (nearly twice as large) and statistically significant (black drug sales arrest rates) or marginally statistically significant (white drug sales arrest rates).

Finally, it is possible that other funding initiatives, either local or federal, could undermine the validity of our results.[21] Specifically, the 1994 Violent Crime Control and Safe Streets Act (VCCA) and the 1997 National Defense Authorization Act, which established the 1033 Program, provided additional funds and resources to state and local governments to combat crime. These funds would also bias our estimates upward if COPS grants and the 1033 Program were complementary to the EBMGP. To address this issue, we focus on cities that were treated prior to 1991. Row 7 reports pre- and post-treatment effects when only cities that were treated before 1991 and non-treated cities are in the sample, and all years after 1996 are excluded. This creates a sample with 41 treated cities that have at least five event-years that are not confounded by the impact of the 1994 VCCA and COPS grants or the 1033 program. Restricting the sample in this manner, produces post-treatment effects that are larger than the treated only sample. However, the pre-treatment effects are larger and marginally statistically significant for black drug sales arrests, although the sign of the coefficient indicates black drug sales arrests were decreasing prior to the grant. Nevertheless, row 7 provides evidence that the Byrne grant influenced policing behavior.

E. Test for Racial Disparities in Arrests

To analyze the Byrne Grant Program impact on racial disparities in drug arrests we implement a triple difference strategy (DDD) within the event-study framework. We estimate the following equation:

$$(3)\ Y_{r,i,t} = \gamma_i + \alpha_{t,s(i)} + \beta_{t,u(i)} + Black_r + Black_r\alpha_t + Black_r D_i + \sum_{\tau=1}^{q} \pi_{-\tau} D_i 1(t - T^* = -\tau) + \sum_{\tau=1}^{p} \delta_\tau D_i 1(t - T^* = \tau) + \sum_{\tau=1}^{q} \lambda_{-\tau} D_i 1(t - T^* = -\tau) Black_r + \sum_{\tau=1}^{p} \sigma_\tau D_i 1(t - T^* = \tau) Black_r + \varepsilon_{i,t}$$

[21] As previously mentioned, because it is difficult to know the exact pass-through amount from the state to local jurisdictions for the formula grant (also see GAO, 2005), our analysis focuses on the discretionary portion of the EBMGP. As a result, an upward bias could be present in the estimation of the discretionary program's treatment effect on the treated if block grants are complementary to discretionary grants. However, to the extent that cities in the control group received some EBMGP funding through the formula grant program, this would create a downward bias on our results.

where the notation remains as previously defined in equation 1. However, now each city is characterized by their white or black drug arrest rate. This is captured by a binary variable $Black_r$ which is equal to one when referencing the black drug sales arrest rate and zero for the white drug sales arrest rate. In addition, $\lambda_{-\tau}$ will capture the pre-treatment effects and σ_τ will capture the post treatment effects. Pre-treatment effects will capture how racial disparities are evolving before a city receives their initial grant and the post-treatment effects will capture changes in racial disparities associated with the implementation of the program. As stated earlier if the specification correctly captures permanent pre-existing differences between the treated and the control group, the pre-treatment effects, $\lambda_{-\tau}$, will be indistinguishable from zero. This will occur if racial disparities in drug arrests are evolving similar in both groups. The post-treatment effects, σ_τ, will capture changes in the racial composition of drug arrests if the program is actually contributing to racial disparities. If the post-treatment effects are zero, the Byrne Grant Program may not be driving racial disparities in drug arrests. However, if the post-treatment effects are negative, then the program is reducing racial disparities; and if the post-treatment effects are positive, then the program is compounding racial disparities in drug arrests.

Figure 9, plots pre-treatment effects and post-treatment effects from the DDD model. We focus solely on drug sales arrest because we have established a robust relationship between the Byrne Grant Program and drug sales arrest rates for both white and black residents. The pre-treatment effects, $\lambda_{-\tau}$, are statistically insignificant and close to zero. The lack of relationship does not suggest that treated cities did not have racial disparities in drug arrest before treatment. It is clear from Table 1, that both cities in the treated and control group have significant racial disparities in drug sales arrest rates, with the disparity being more pronounced in the control group. The pre-treatment effects provide statistical evidence that the racial disparities in drug sales arrests are evolving similarly in both groups. After treatment, the post-treatment effects are positive and statistically significant in event year 1, 2 and 4. According to the results, racial disparities increased by one additional arrest per 1,000 black residents,

one year after the Byrne program is implemented. The mean black arrest rate is 4.7 at the time of treatment and 1.3 for white drug arrest, which implies racial disparities increased by 30 percent – one year after the program was implemented. Appendix Figure B5 plots the evolution of racial disparities in drug sales arrests that is attributable to the Byrne Grant Program. Prior to 1987, the racial gap attributable to the program is near zero as expected. Immediately following the program implementation, the racial gap in drug sales arrests increases, peaking at almost 4,000 additional arrests of black residents for drug sales prior to 1990. After 1990, the racial gap in drug sales arrests is consistently close to 1,000 additional black arrests. Although both white and black arrests greatly increase[22], it is clear that the program disproportionally impacted black residents. According to our model, over the five years immediately following the initial grant, treated cities are associated with approximately 34,000 additional white drug sales arrests and close to 45,000 additional black drug sales arrests.[23]

Discussion

We find evidence that the EBMGP increased the number of sworn police officers after the first grant was received. Additionally, we find suggestive evidence that the EBMGP led to an increase in police expenditures. Event-study estimates using data from the Annual Survey of Governments report positive post-treatment effects for the impact of treatment on city police expenditures. Although the point estimates are not statistically significant, a clear trend break occurs following treatment and point-estimates increase over time, whereas changes in city revenues remain flat after treatment.[24] The lack of statistically significant post-treatment effects may have stemmed from the discretionary grants being relatively small compared to total spending on police. Furthermore, state planning boards might have

[22] See Appendix Figure B18
[23] See Appendix Figure B19.
[24] See Appendix Figure B14

distributed block grant funds to other municipalities, increasing police expenditures in the control group. Finally, local governments may have reallocated financial resources in response to the grant, resulting in less spending on police than if local offsets did not occur (Baicker & Jacobson, 2007).

Moreover, we find evidence that the EBMGP led to a decrease in total crime.[25] This decrease in total crime was generally driven by lower property crimes because we find no evidence that the program reduced total violent crime, even though violent crime was also a stated goal of the program. Our results on violent crime stand in contrast to the aforementioned research on the COPS program, which finds that COPS significantly reduce violent crime (Evans & Owens, 2007; GAO, 2005; Weisburst, 2019a; Worrall & Kovandzic, 2010).[26] Our findings also slightly differ from those of the 1033 program. Similar to the 1033 program, we find a significant decrease in robbery when we disaggregate violent crime.[27] However, unlike the 1033 program, we do not find the EBMGP to significantly affect assault (Bove & Gravilova, 2017; Harris et al., 2017).

Eventually, property crime became one of the program purposes for EBMGP, which may be why there is a lag in the significant decrease in property crime rates after receipt of the initial grant. Similar to the 1033 program, we also find a decrease in larceny and burglary, which seems to be driving the overall decrease in property crimes. Contrary to the 1033 program, EBMGP did not have an effect on auto-theft (Bove & Gravilova, 2017; Harris et al., 2017), even though the discretionary program at one point had a program entitled Auto-Theft Deterrence, Investigation, and Prosecution Program.[28] However, caution should be taken when interpreting these results given the the presence of significant

[25] See Appendix Figure B20 for the estimated number of crimes prevented due to the Byrne Grant Program overtime. It is important to note, that we do not find a trend-break in the estimated number of crimes prevented until 8 and 5 years after the COPS and 1033 programs were implemented nationally, respectively. However, this does not rule out confounding effects due to the complementarity of the COPS and 1033 programs to the Byrne Grant Program.

[26] It should be noted that the GAO (2005) analysis of the COPS program controls for the discretionary EBMGP program but they do not show EBMGP to significantly impact crime rates. However, EBMGP is not the primary focus of their analysis, thus, they do not causally model the program. Nonetheless, Worrall and Kovandzic (2010) exclude the EBMGP as an instrument because they find it to have a direct relationship with their second stage crime models (i.e., EBMGP had a direct effect on crime that was not solely operating through increases in police).

[27] See Appendix Figure B3

[28] See Appendix Figure B17

pre-trends for some years for motor vehicle theft and burglary. We also find evidence that the total property arrest rate is decreasing, suggesting that the impact of the program on property crime could be a secondary effect stemming from an increase in the arrest rate for drug offenses.[29]

The results also show that the drug sales arrest rate increases for blacks and whites. Specifically, the black drug sales arrest rate increases in every year after treatment. However, it increases at a diminishing rate. Conversely, the post-treatment effects for the white drug sales arrest rate does not become statistically significantly until two years after treatment, coinciding with the significant increase in the number of sworn police officers. The difference in the black drug sales arrest rate from baseline is, on average, 3.8 times that of the white drug sales arrest rate in the post-treatment period. It appears that initial resources translated into a focus on areas where drug arrests were prominent, which is reflected in the immediate increase in black drug sales arrests. Following the hiring of additional police, we see a significant increase in the white drug sales arrests rate and a decrease in property crime. This suggests that white communities may have been previously under-policed for drug crimes and increases in enforcement in these communities led to an increase in white drug sales arrests.

The question remains: What led to the initial increase in black drug sales arrests? One plausible explanation is that blacks have lower apprehension costs and are easier to arrest (see Beckett et al. 2006 for a discussion), thereby resulting in more arrests of black drug dealers until new resources materialized. This seems plausible, given the lag time between receiving a grant, hiring additional police officers, and training new hires. The timing also coincides with the increase in white drug sales arrests and lower crime rates over time. It is also possible that there are disproportionately more black drug dealers, and the EBMGP, either via the development of multijurisdictional drug task forces or via the targeted funds made available to police, allowed for the initial expansion of resources to be used to

[29] See Appendix Figure B16

apprehend black offenders. However, prior research suggests that this explanation is insufficient in explaining black-white racial disparities in arrests (Beckett et al., 2006; Gaston, 2019; Mitchell & Caudy, 2015).

An alternative hypothesis is that the grant program may have provided an incentive for law enforcement agencies to focus on drug sales and trafficking arrests, which may have exacerbated incentive-driven and heavier handed policing practices (Benson et al., 1992; Cox, 2015). Within this more entrepreneurial approach to policing (i.e., policing-for-profit), it is plausible that law enforcement selectively enforced drug laws among vulnerable populations, such as black communities and the poor. This hypothesis is also consistent with results showing substantial increases in black drug sales arrest rates followed by increasingly large increments in white drug sales arrest rates over time.

In fact, as previously mentioned, the discretionary EBMGP had the Crack-Focused Substance Enforcement Program, which was a MJDTF program focusing on policing crack-cocaine in urban locations. In some places, there have been documented ratcheting effects of the racially biased implementation of this program that extended beyond urban cities. An anecdotal example of the aforementioned can be found in the case of Texas where the rampant misconduct of drug task forces throughout the state, was attributed to the funding made available through the EBMGP program (see Carman & McVicker, 2001). In their article, Carman and McVicker (2001) state the following:

> Those task forces are as addicted to the federal cash injections as the junkies are to their dope. And according to critics, they're more concerned with making as many busts as possible to keep their arrest numbers up, and their funding high than they are on concentrating on time-consuming investigations that might net large-scale dealers.

A former member of multiple task forces in Texas, Barbara Markham, also discussed the rampant corruption within Texas task forces in the same article. In her interview, she noted: "'The thing I started noticing was that they were only going after blacks'" (Carman & McVicker, 2001). Later she stated:

> 'If you were white, you didn't have to worry much about task forces, because they were going after crack. But it doesn't take any skill to make a crack bust. All you have to do is drive up and roll down your window. It's like shooting fish in a barrel. But the drug problems in these various counties do not just involve black people, and it's not just crack. But that's about all they're turning out now. It's just ridiculous' (Carman & McVicker, 2001).

Her statement provides some support of selective enforcement and targeting of drug crimes typically committed by blacks at the organizational and incident levels. In addition to the early focus on crack-cocaine by the discretionary EBMGP, part of the motivation for targeting drugs disproportionately used and sold by blacks could have been lower apprehension costs of blacks, as alluded to by Markham. However, two well-known egregious cases of racial targeting of African Americans also took place in Hearne, TX (American Civil Liberties Union, 2003; Carman & McVicker, 2001) and Tulia, TX (Blakeslee, 2000), providing some evidence that targeting may have also stemmed from racial discrimination (Beckett et al., 2006; Bulman, 2019).

While we find evidence beyond the anecdoctal case of Texas that racial disparities in black-white arrests increase after initial grant receipt, we also find a substantial increase in white arrests as a result of the Byrne Grant Program. One explanation for the increasing rate of change for the white drug sales arrests rate is that the EBMGP caused an overall shift in focus to a perceived "black crime type" (i.e., drug crimes), which subsequently led to an increase in the arrest rate for whites committing drug crimes (Bulman, 2019). Another explanation, which is not mutually exclusive from the aforementioned, is that law enforcement began shifting attention away from drugs typically consumed and sold by blacks (e.g., crack-cocaine) towards drugs normally consumed and sold by whites. A decrease in the focus on typical black drug crimes, could also explain the decreasing change in arrests for blacks (Bulman, 2019). Coinciding with the increase in white drug sales arrests is the explosion in methamphetamine laboratories that occurred during the 1990s. There is anecdotal evidence that law

enforcement managers may have shifted focus from crack-cocaine to methamphetamines (see Carman & McVicker, 2001).

Conclusion

This study investigates how additional resources for policing drug and violent crimes through the discretionary Edward Byrne Memorial State and Local Law Enforcement Grant Program (EBMGP) impacted total drug arrests by type of drug crime committed and by race. We also estimated the effect of EBMGP on police hiring and crime rates. The results show that the EBMGP significantly increased police hiring, but there was no overall impact on violent crime, except for robbery. Nonetheless, treated cities experienced an increase in drug sales arrests for blacks *and* whites and a decrease in crime that was primarily driven by lower property crime. Although the increase in the black drug sales arrest rate is relatively large, it is diminishing over time. Conversely, white drug sales arrest rates gradually rise over time at an increasing rate. Despite the substantial increase in white drug sales arrests, our DDD specification finds that racial differences between black and white arrests rates significantly increase post grant receipt: One year after grant implementation, racial disparities increase by 1 per 1,000 black residents, or 30% above baseline. According to our model, the discretionary EBMGP was responsible for an additional 45,000 black drug sales arrests and an extra 34,000 white drug sales arrests over the observed post-treatment period. Caution should be taken when interpreting the results due to the significance of some pre-trends in certain specifications.

Our findings indicate that federal intergovernmental transfer programs, such as the discretionary EBMGP, may have played a role in expanding the net of the criminal justice system and the growth of felony convictions and incarceration in the United States, primarily through increases in drug sales arrests. Although funding for the discretionary EBMGP may not have been substantial, it should be considered within the body of the larger legislative initiatives of the War on Drugs passed between 1984 and 1989 (Benson & Rasmussen, 1996). The EBMGP, which was established after the

1984 federal asset forfeiture law, may have been used to provide seed money to develop the local infrastructure needed (i.e., multijurisdictional drug task force) to efficiently expand local policing agencies budget via asset forfeitures.

The results of this study have two policy relevant implications. First, although the EBMGP was a color-blind policy initiative, it was not race neutral in its implementation, which led to a sharp increase in the black drug sales arrest rate and racial disparities in drug sales arrests after receipt of the initial grant. Nonetheless, because the EBMGP encouraged state and local agencies to shift their focus to crimes with historically high black arrest rates, such as drugs, there was also an expansion of arrests for whites who participated in such crimes. Second, effective criminal justice reform should not only consider addressing state and local policies that have driven the criminal justice system's expansion, but also federal policies aimed at altering the behavior of state and local authorities to support national crime control policies. Further research should investigate the role of federal policies on racial disparities in convictions and incarceration.

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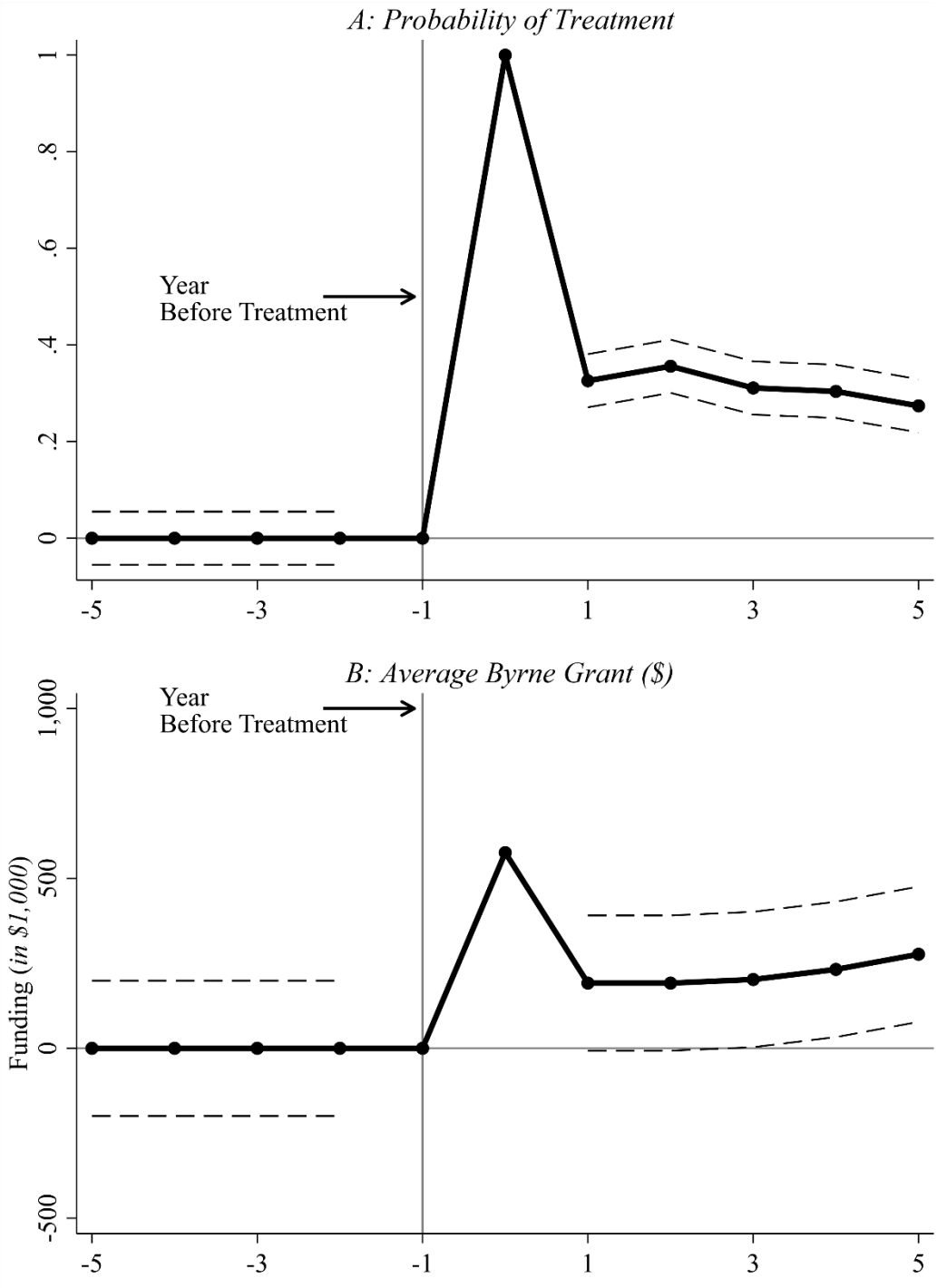
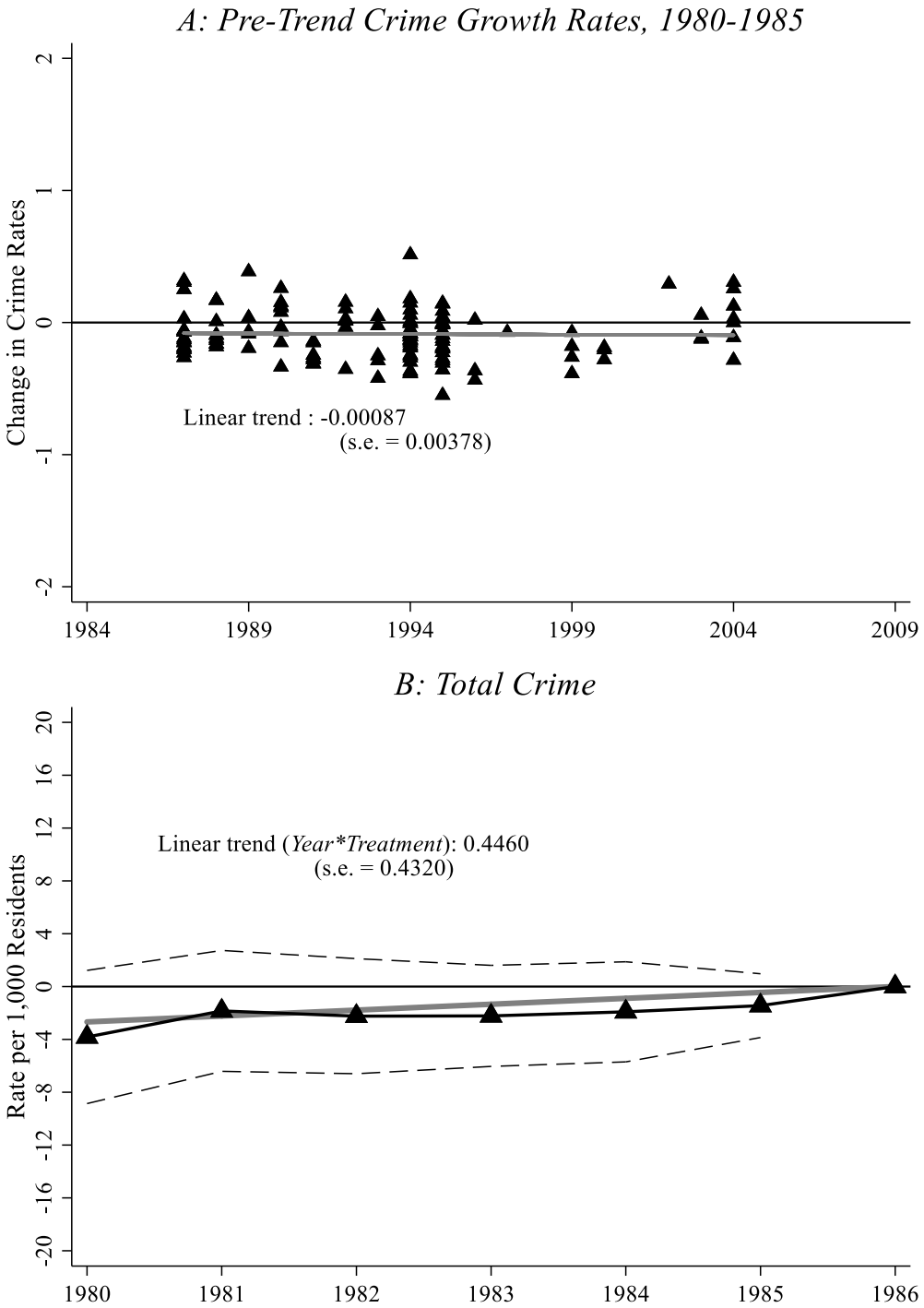
TABLES AND GRAPHS

Figure 1. Funding and Funding Propensities



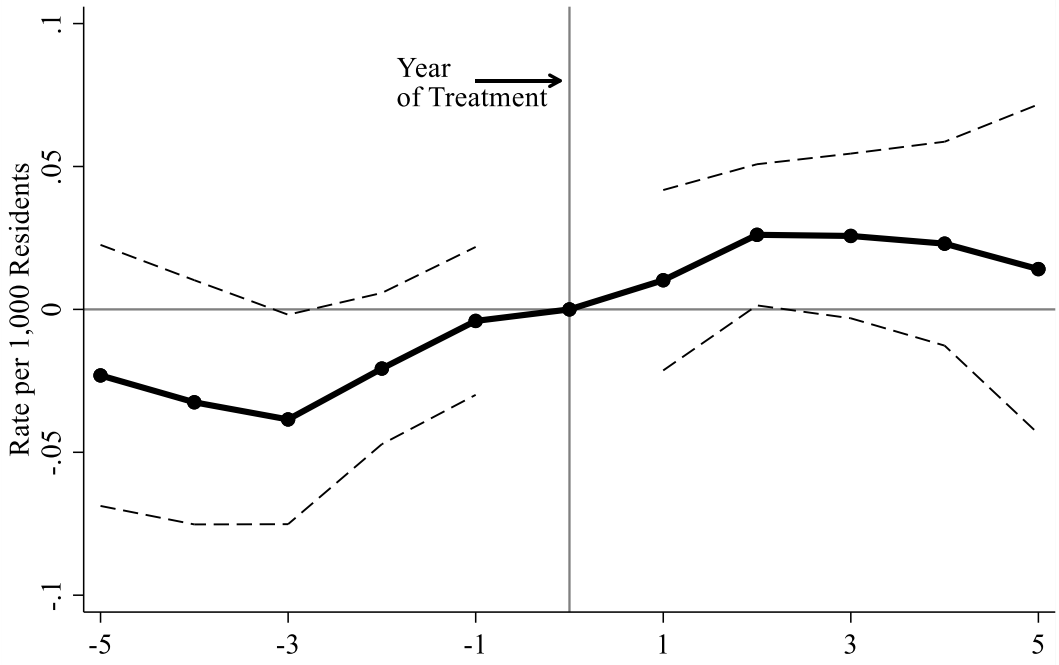
Note: **[Panel A]** Dependent variable is an indicator variable for (0/1) for receiving a discretionary Byrne grant. **[Panel B]** Dependent variable is the amount of the Byrne grant. In both panels, the horizontal axis corresponds to the years before and after receipt of the first Byrne grant. Heteroskedasticity-robust standard errors clustered by city are presented.

Figure 2. Pre-trend Crime Growth Rates



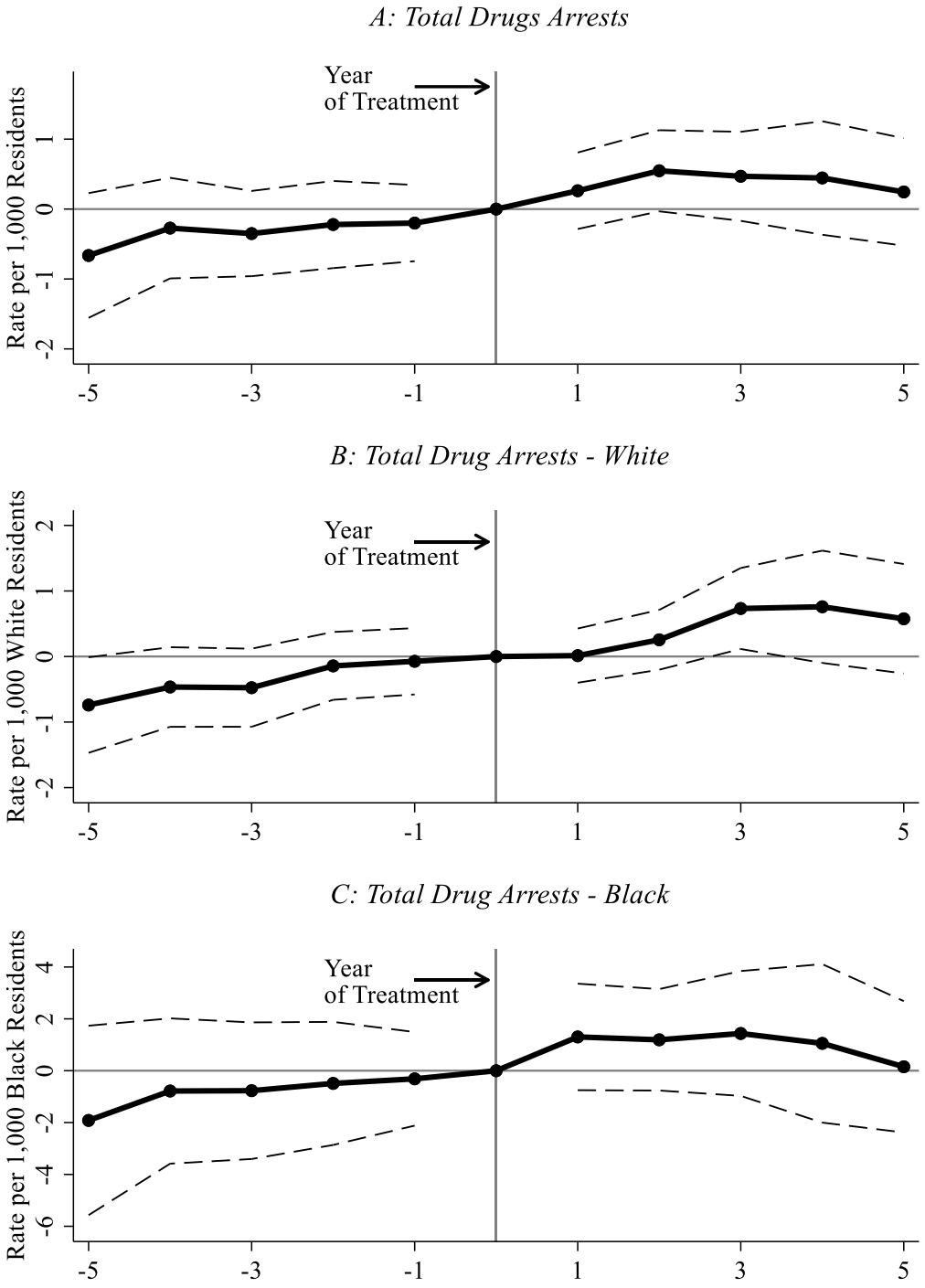
Notes: **[Panel A]** Regression coefficients and predicted values are from a univariate regression of the dependent variable changes in the crime rate on the year a city received its first discretionary Byrne grant. **[Panel B]** The dependent variable is crime per 1,000 residents. The independent variables are year fixed effects (1980-1985) – Y, treatment indicator (0/1 if ever treated) –T, and year by treatment effects T×Y. The coefficients plotted are the coefficients on the interaction terms.

Figure 3. Estimated Effects of the First Byrne Grant on Numbers of Sworn Police



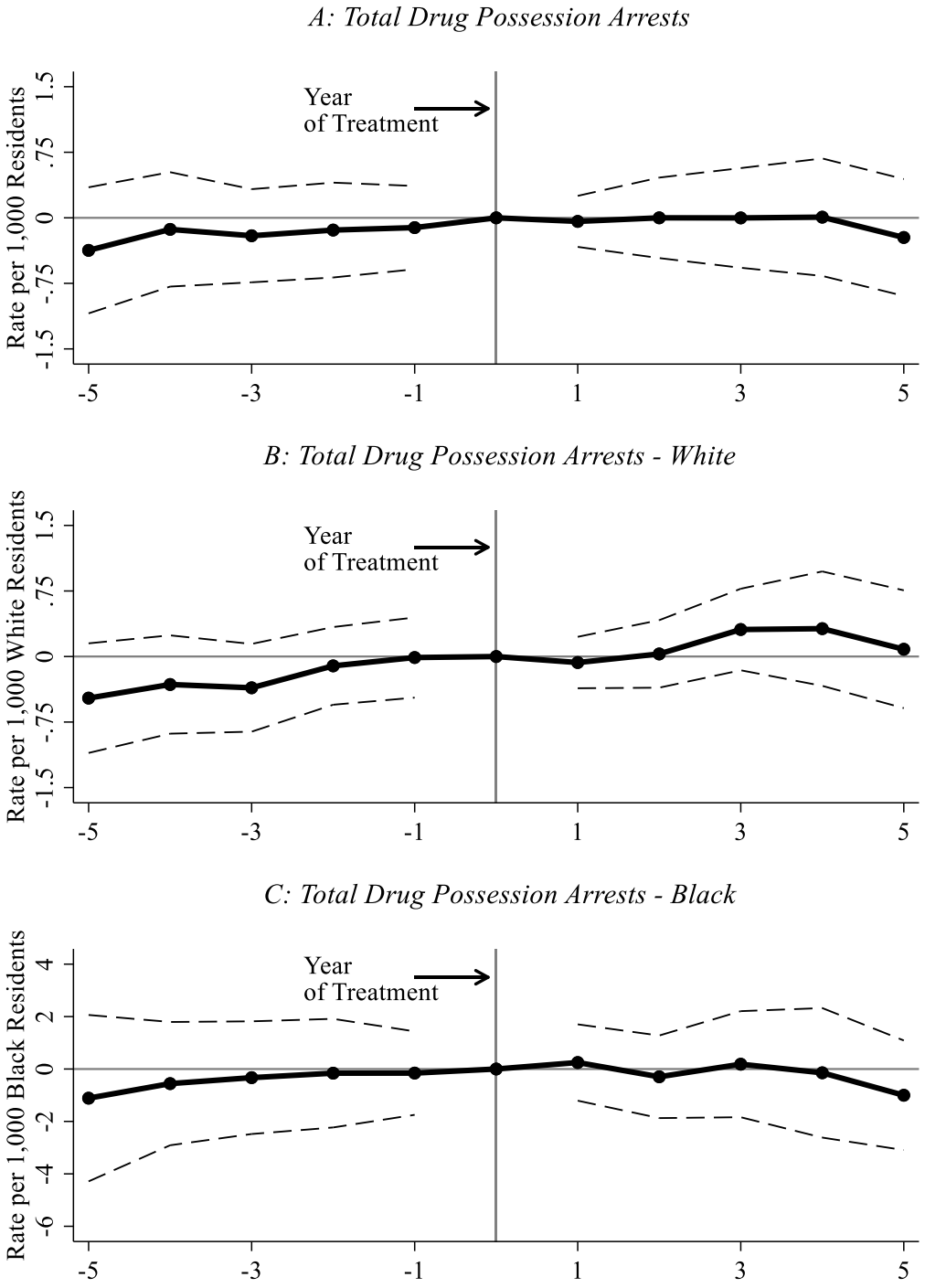
Notes: Police data come from the Uniform Crime Report: Law Enforcement Killed or Assaulted Files. The regression specification includes city, C, and year, Y, effects, state-by-year S-Y, effects, and urban-group-by-year U-Y, effects. Heteroskedasticity-robust standard errors clustered by city. The horizontal axis represents event-years (years before and after the first grant).

Figure 4. Estimated Effects of the First Byrne Grant on Total Drug Arrests



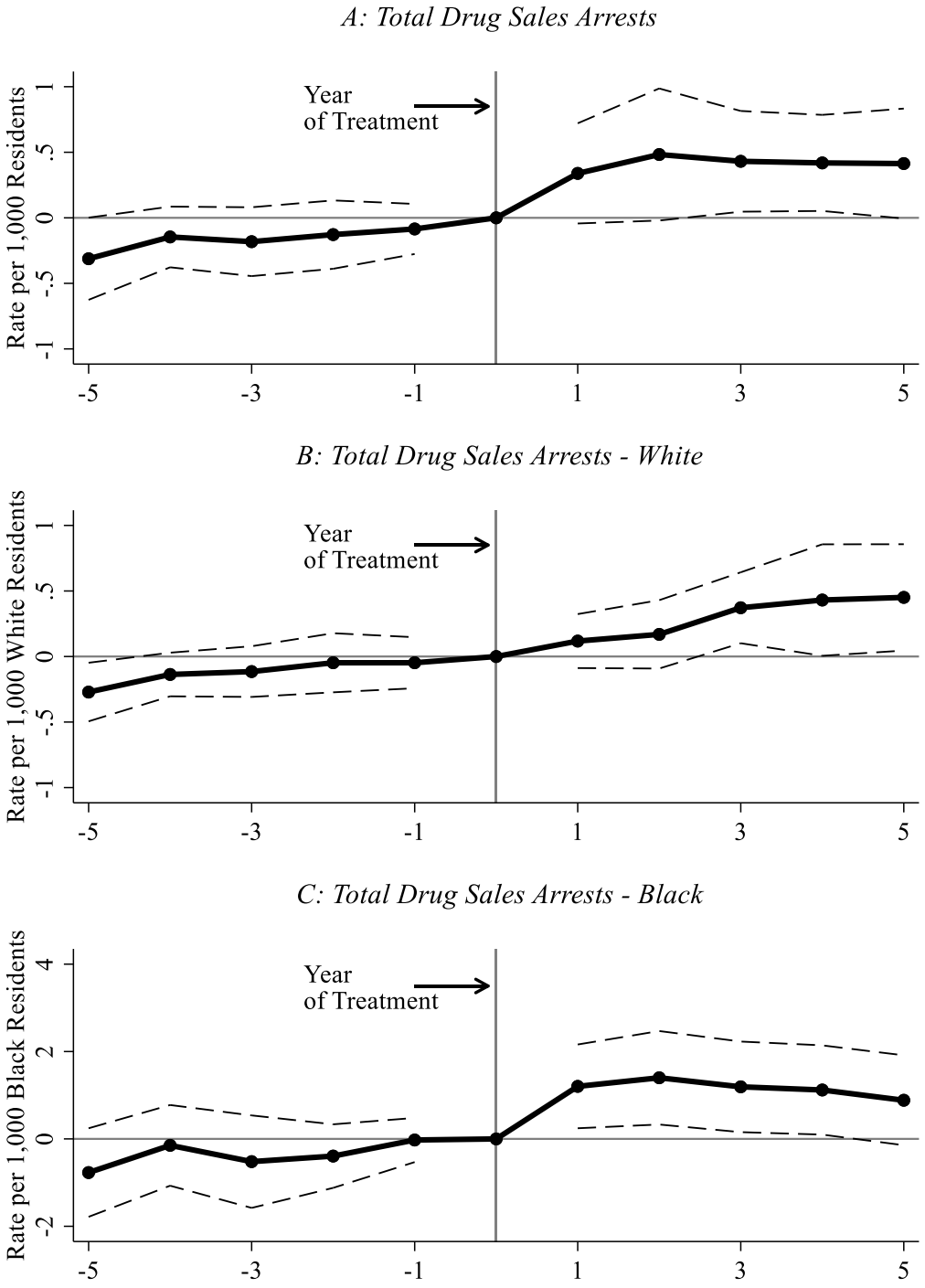
Notes: Arrests Data come from the Uniform Crime Report: Gender, Age, and Race Supplement. The regression specification includes city, C, and year, Y, effects, state-by-year S-Y, effects, and urban-group-by-year U-Y, effects. Heteroskedasticity-robust standard errors clustered by city. The horizontal axis represents event-years (years before and after the first grant).

Figure 5. Estimated Effects of the First Byrne Grant on Drug Possession Arrests



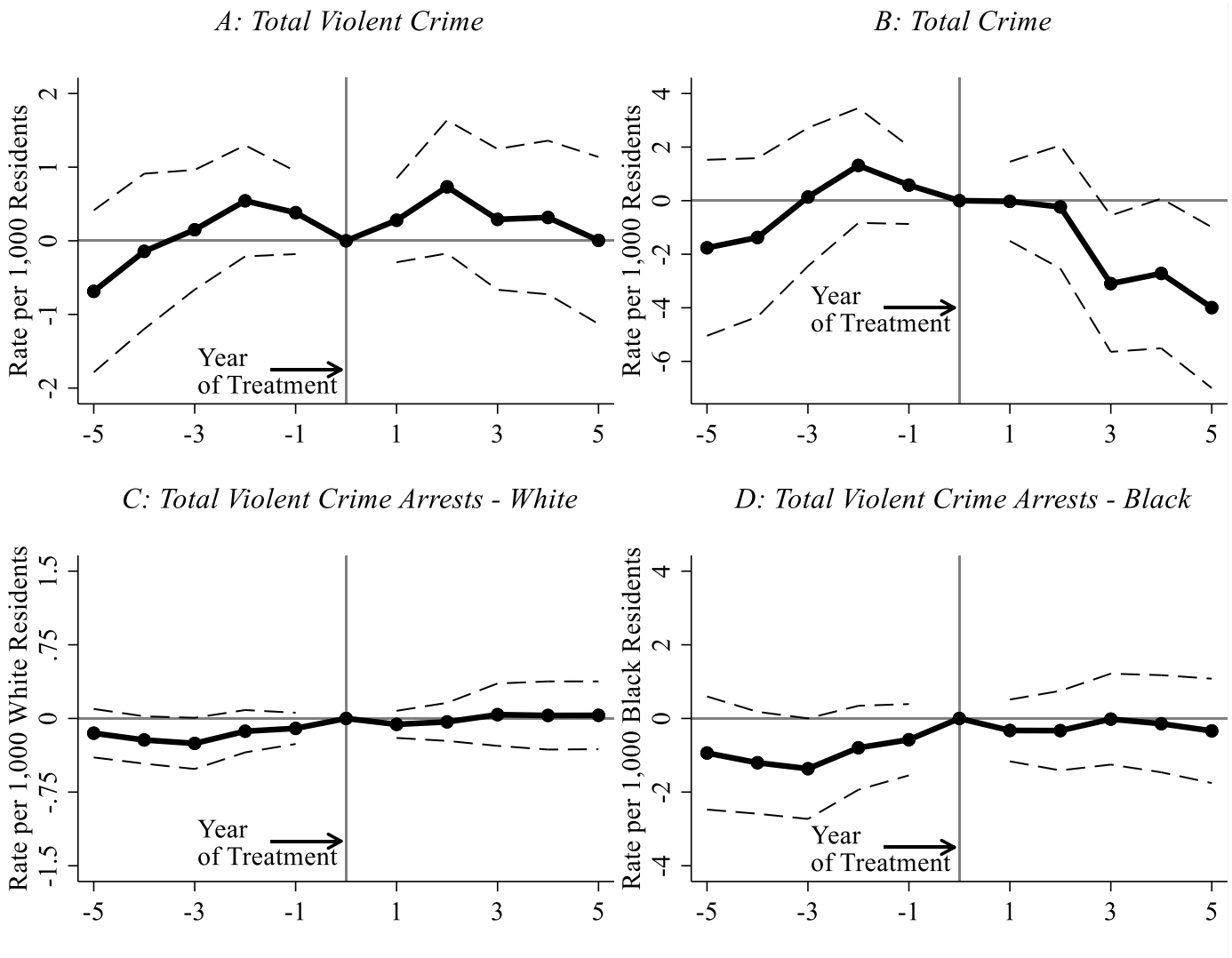
Notes: Arrests Data come from the Uniform Crime Report: Gender, Age, and Race Supplement. The regression specification includes city, C, and year, Y, effects, state-by-year S-Y, effects, and urban-group-by-year U-Y, effects. Heteroskedasticity-robust standard errors clustered by city. The horizontal axis represents event-years (years before and after the first grant).

Figure 6. Estimated Effects of the First Byrne Grant on Drug Sales Arrests



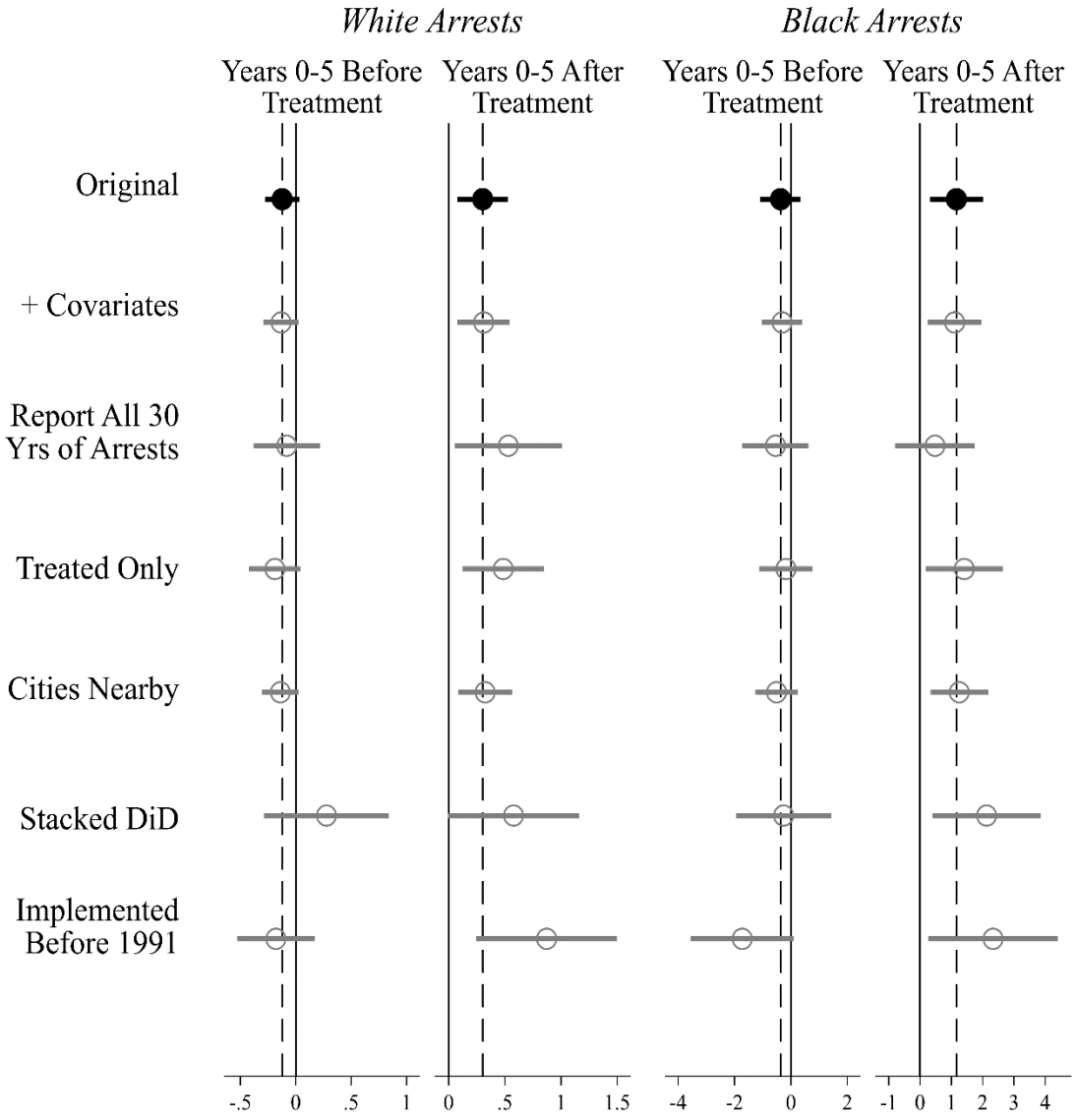
Notes: Arrests Data come from the Uniform Crime Report: Gender, Age, and Race Supplement. The regression specification includes city, C, and year, Y, effects, state-by-year S-Y, effects, and urban-group-by-year U-Y, effects. Heteroskedasticity-robust standard errors clustered by city. The horizontal axis represents event-years (years before and after the first grant).

Figure 7. Estimated Effects of the First Byrne Grant on Violent Crime and Arrests



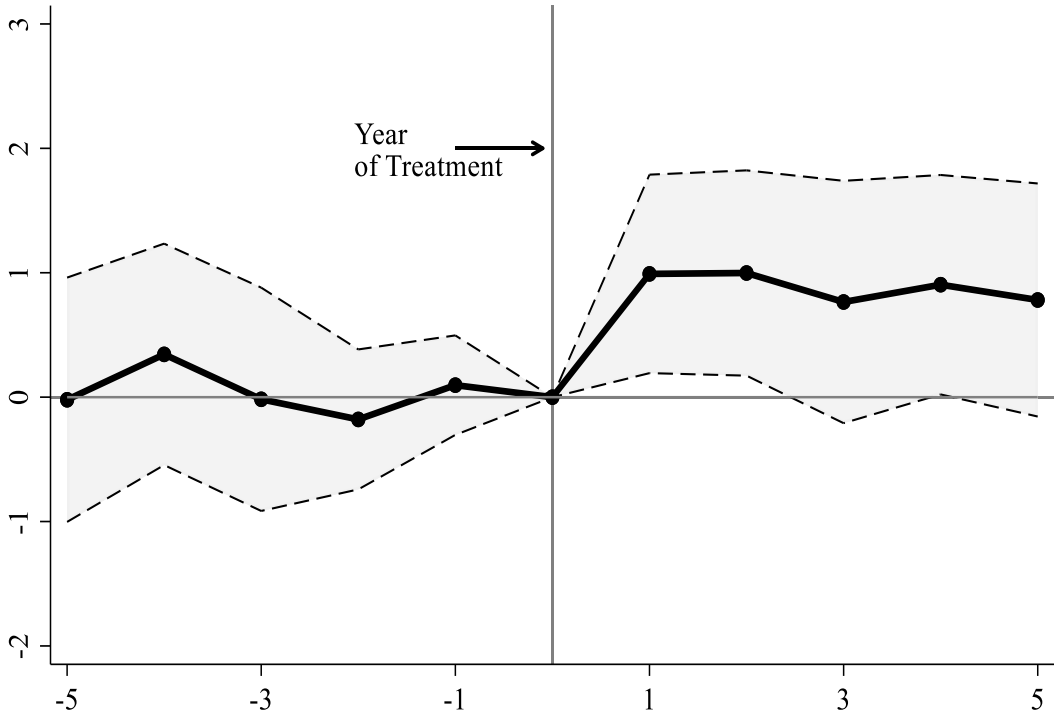
Notes: Arrests Data come from the Uniform Crime Report: Gender, Age, and Race Supplement. Crime Data come from the Uniform Crime Report: Offenses Known and Cleared by Arrests. The regression specification includes city, C, and year, Y, effects, state-by-year S-Y, effects, and urban-group-by-year U-Y, effects. Heteroskedasticity-robust standard errors clustered by city. The horizontal axis represents event-years (years before and after the first grant).

Figure 8. DiD Estimates of the Byrne Grant Program – Drug Sales Arrests



Notes: The figure displays least-squares estimates obtained from estimating Equation 2 by grouping years before and after treatment. All columns include city, C, year,Y, state-by-year, S-Y, and urban-group-by-year, U-Y, effects. Heteroskedasticity-robust standard errors clustered by city are presented by bold line. Joint lest-square coefficient is presented by the circle marker.

Figure 9. Estimated Effect of the First Byrne Grant on Racial Disparities in Drug Sales Arrest Rates



Notes: Arrests Data come from the Uniform Crime Report: Gender, Age, and Race Supplement. The regression specification includes city, C, and year, Y, effects, state-by-year S-Y, effects, and urban-group-by-year U-Y, effects. Heteroskedasticity-robust standard errors clustered by city. The horizontal axis represents event-years (years before and after the first grant). The coefficients are estimates are from a DDD model. Point estimate show the relative increase of black drug sales arrest rate relative to white drug sales arrest rate in a treated city.

Table 1. Characteristics of Cities between 1980 and 1985

	All Cities (N =223)	Received Grant between 1987-2004 (N = 135)	Control Group (N = 88)	T-Test of Difference	Standardized Difference
A. Average Characteristics 1980					
Population	177,297	238,449	83,484	*<0.01*	0.54
Population Per Square Mile	447	469	413.4	0.33	0.14
Median Age	29.6	29.2	30.2	*<0.05*	-0.32
Median Income	17,394.80	15,818.90	19,812.4	*<0.01*	-0.93
Percent of the Population					
with 12 or more years of education	69.5	67.8	72.3	*<0.01*	-0.39
with female head of households	17	19.2	13.6	*<0.01*	0.93
Black	12.6	17.5	5.1	*<0.01*	0.81
joint F-test				28.47	
p-value				*<0.01*	
B. Average Characteristics 1980-1985					
Crime Rates (per 1,000 Residents)					
Total Crime	82	91.5	67.1	*<0.01*	0.91
Personnel					
Sworn Police (per 1,000 Residents)	1.8	1.9	1.5	*<0.01*	0.63
Drug Arrest Rates (per 1,000 Residents)					
Total	3.9	4.1	3.5	*<0.01*	0.22
Sales	0.6	0.8	0.5	*<0.01*	0.41
Possession	3.2	3.4	3.0	*<0.05*	0.14
Drug Arrest Rates by Race					
Black (per 1,000 Black Residents)	9.9	8.9	11.4	*<0.01*	-0.15
White (per 1,000 White Residents)	3.9	3.9	3.8	0.36	0.05
Drug Sales Arrest Rates by Race					
Black (per 1,000 Black Residents)	1.7	1.6	1.8	0.38	-0.05
White (per 1,000 White Residents)	0.6	0.6	0.5	*<0.01*	0.25
Drug Possession Arrest Rates by Race					
Black (per 1,000 Black Residents)	8.1	7.2	9.5	*<0.01*	-0.15
White (per 1,000 White Residents)	3.3	3.3	3.3	0.89	0.01
joint F-test				62.44	
p-value				*<0.01*	

Notes for Table 1: Arrests Data come from the Uniform Crime Report: Gender, Age, and Race Supplement. Crime Data come from the Uniform Crime Report: Offenses Known and Cleared by Arrests. City demographic information was collected from the City and County Data Books.

Table 2. The Implementation of the Byrne Program across Cities in the Sample

Treatment Status	Number of Cities	Percent of Cities	Percent of 1980 Population
Treated	135	60.5	81.4
Year Treated			
1987	17	7.6	23.7
1988	10	12.1	36.9
1989	5	14.3	38.5
1990	9	18.4	46.0
1991	7	21.5	49.1
1992	7	24.7	52.9
1993	5	26.9	58.5
1994	29	39.9	67.0
1995	24	50.7	74.2
1996	3	52.0	76.3
1997	1	52.5	76.7
1999	4	54.3	77.8
2000	3	55.6	78.3
2002	1	56.1	78.6
2003	3	57.4	79.6
2004	7	60.5	81.4
Untreated	88	39.5	18.6

Note: Table provide the number of cities treated for the first time each year in the sample.

Table 3. Relationship between First Grants and 1980 City Demographics

	(1)	(2)	(3)	(4)
	Year Received First Grant		0/1 Receive Byrne Grant	
Population per Square Mile	-0.900	-0.899	-0.0542	-0.0526
	[1.119]	[1.121]	[0.0640]	[0.0649]
Median Age	-2.298	-2.298	-0.570	-0.542
	[5.190]	[5.221]	[0.357]	[0.361]
Median Income	-5.893	-5.885	-0.358	-0.330
	[4.568]	[4.647]	[0.266]	[0.270]
Log of the Proportion of Residents				
12 or more years of schooling	-0.493	-0.496	0.411	0.397
	[4.272]	[4.228]	[0.289]	[0.290]
Female head of household	-11.14***	-11.14***	0.592***	0.595***
	[3.812]	[3.832]	[0.225]	[0.225]
Black	0.320	0.320	0.0699**	0.0696**
	[0.495]	[0.498]	[0.0321]	[0.0325]
Change in Crime Rates		0.0252		0.148
		[3.730]		[0.196]
Observations	135	135	223	223
R-squared	0.350	0.350	0.468	0.470

Note: Each column reports estimates from a separate least squares regressions. The dependent variable in Columns 1 and 2 is the year a city first received a grant. The dependent variable in Columns 3 and 4 is an indicator equal to 1 if a city received a grant between 1987 and 2004. Heteroskedasticity-robust standard errors are corrected for clustering with state and presented in brackets. The city demographic variables are from the 1980 Decennial Census. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$