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# *Financing the War on Drugs: The Impact of Law Enforcement Grants on Racial Disparities in Drug Arrests*

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

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Abstract

We estimate the effectiveness of the Edward Byrne Memorial State and Local Law Enforcement Assistance Program, a grant program authorized under the 1988 Anti-Drug Abuse Act to combat illicit drug abuse and to improve the criminal justice system, on racial bias in policing. Funds for the Byrne Grant program could be used for a variety of purposes to combat drug crimes, as well as violent and other drug related crimes. The event-study analysis suggests that implementation of this grant resulted in an increase in police hiring and an increase in arrests for drug trafficking. Post-treatment effect implies a 107 percent increase in white arrests for drug sales compared to a 44 percent increase for blacks 6 years after the first grant is received. However, due to historical racial differences in drug arrests, the substantial increase in white drug arrest still results in large racial disparities in drug arrests. This is supported by weighted least squares regression estimates that show, for every $100 increase in Byrne Grant funding, arrests for drug trafficking increased by roughly 22 per 100,000 white residents and by 101 arrests per 100,000 black residents. The results provide strong evidence that federal involvement in narcotic control and trafficking lead to an increase in drug arrests; disproportionally affecting blacks.



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## I. Introduction

Over the past forty years, we have seen an exponential growth in the US incarceration rate. The most current research has found that this increase is attributable to public policies that increased the scope and length of incarceration for various offenses (Raphael and Stoll 2013, Neil and Rick, 2014). Most research focuses on state and federal policies that directly impacted sentencing, such as enhanced sentences, moving from indeterminate to determinant sentencing, and truth in sentencing laws. However, Pfaff (2011a, 2011b) argues that prison admissions have a greater impact on incarceration rates, not harsher sentencing policies, fueled by decisions of local actors (e.g., prosecutors and police). Nonetheless, Neal and Rick (2014), using the same data and a similar method, find that more punitive punishments account for the majority of the prison growth, and the differential rates of imprisonment for blacks and whites are due to differences in arrest rates. There is also increasing evidence that policies associated with the war on drugs led to increases in incarceration rates, as well as racial disparities within the criminal justice system (Benson and Rasmussen 1996; Blumenson and Nilsen 1998).

Clearly, there is still some debate regarding how mass incarceration happened; specifically, is it attributable to state and federal policies, or is it the result of unchecked actions of local officials (operating under moral hazard), such as prosecutors and police? However, so much focus on finding one culprit might downplay just how interconnected federal, state, and local policies are. That is, it is possible, and likely, that mass incarceration occurred due to both top down and bottom up policies. Specifically, changes in policies at the federal level could have altered the incentive structure of state and local actors causing behavioral responses at the local level that ultimately led to increases in incarceration, such as increases in arrests and felony filings. Nevertheless, fewer studies have focused on understanding how federal policies, which include intergovernmental grants and other incentive based federal programs (e.g., asset forfeiture programs), aimed at redirecting law enforcement's focus to the policy directives of the national government, have affected the behavior of local law enforcement, mass incarceration, and racial disparities in the criminal justice system.

One such policy is the Edward Byrne Memorial State and Local Law Enforcement Assistance Grant Program (EBMGP). The Anti-Drug Abuse Act of 1988 (Public Law 100-690) created the EBMGP, in an effort to reduce drug related crime and support the national agenda on drug control. Through the Byrne program, the Bureau of Justice Assistance creates novel methods

in criminal justice and "encourages replication of effective programs and practices by state and local criminal justice agencies." (BJA Factsheet FY1999). It directly awards discretionary grants to agencies and non-profit organizations. It also awards formula funds to states, which can then award these funds to state and local government units, agencies, or organizations. The discretionary grant program authorizes funds to improve the criminal justice system at state and local levels. They may be used "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 et al. 1996c). The primary goal of the funding was to combat drugs, violent crimes related to drugs, and to improve coordination among the criminal justice systems various parts.

Byrne grants went to state and local law enforcement agencies to improve law enforcement effectiveness in drug related areas, and a large share of federal funding was used to create multijurisdictional drug task forces (Dunworth et al. 1996d). Multijurisdictional drug task forces were rolled out across the United States to assist in the apprehension of offenders of drug related crimes. These task forces funded by Byrne grants have been linked to improvement in police communication and tactical responses as well as a substantial increase in drug arrest (Blumenson and Nilsen 1998; McGarrell and Schlegel 1993; Jefferies et al. 1998). However, these claims have not been supported by rigorous empirical evidence (Smith et al. 2000; Mazerolle 2007).

This paper has three main goals: 1) to re-evaluate the effectiveness of Byrne grants ability to combat illicit drug abuse and trafficking through federal involvement into local policing efforts, 2) to understand the impact of how programs like the Byrne grants might alter policing initiatives and behavior, and 3) to understand the effect of targeted policing policies on racial disparities in the criminal justice system. Using federal expenditure data from the Consolidated Federal Funds Report and arrest data from the Uniform Crime Report, we study the impact of discretionary Byrne grants on drug arrest rates. We focus on arrests for drug possession and drug trafficking for several reasons. First, the Byrne grant program specifically focused on the improvement of the apprehension, prosecution, adjudication, detention, and rehabilitation of drug offenders. Secondly, the Byrne grant may indirectly influence crime rates, but the UCR only record reported criminal offenses 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 where crime rates have been steadily decreasing (Raphael and Stoll 2013; Neal and Rick 2014). Therefore, focusing

on crime rates provide a modicum of information on Byrne grants effectiveness and influences on differential arrest rates as well as the implications for mass incarceration.

Our research design exploits the variation in the timing and location of funding to identify a causal relationship between the Byrne program and drug-trafficking arrests. We use an event-study (Jacobson, LaLonde, and Sullivan 1993) framework to provide a statistical description of the evolution of pre-trends in arrest rates as well as to highlight the dynamics of changing arrest rates after the first grant is received. The event-study framework provides an intertemporal response of the outcomes that are gradual and non-linear. This dynamic model estimates changes in the outcome relative to the date of the first grant, thus, eliminating potential bias from the averaging of outcomes in newly funded locations with those from more established locations. The pre-treatment effects are of importance as it provides a falsification test of pre-treatment, time-varying, city level unobservables that influences the outcome similar to pre-treatment test in the difference-in-difference literature.

Event-study results provide evidence that implementation of this grant resulted in a six percent increase in police hiring and a 126 percent increase in drug trafficking arrests – six years after the first grant is received. There is also evidence of a differential influence of the grant program by race. Event-study results show a cumulative post-treatment effect of a 107 percent increase in drug sales arrests for whites compared to a 44 percent increase in drug sales arrest for blacks. Although the estimated effects are smaller for blacks relative to whites, historically higher arrest rates for blacks implies a much larger impact on black residents. The short run effects imply arrests for drug sales increased by 164 per 100,000 black residents compared to 98 per 100,000 white residents.

Lastly, we provide evidence that drug trafficking arrest by race varied by the intensity of the treatment. Our results indicate that for every $100 increase in Byrne grant funding per capita, arrests for drug trafficking increased by roughly 22 per 100,000 white residents and by 101 arrests per 100,000 black residents. Our results show federal funding for the War on Drugs can be linked directly to the increase in racial disparities in arrest, disproportionally affecting blacks. The results of this analysis in conjunction with Neal and Rick (2014) provide strong support for the narrative linking targeted policing strategies with regards to drug offenses with prison population growth associated with black men.

## II. Brief History of the Byrne Grant Program

After decades of decline, reported crime in the United States began to rise in the early 1960s. The rise in violent crime, especially homicide, pushed crime to the forefront of political debates (Grimes and Loo 2004). Concerns over the decline of urban communities, eruptions of urban violence, and the assassination of public officials resulted in the establishment of two important public policy interventions. The first was the inclusion of experimental programs within the War on Poverty federal initiative that would reduce crime. The second was the response of legislators to support state and local efforts to prevent crime. In response to the President's Commission on Law Enforcement and the Administration of Criminal Justice appointed by President Johnson, Congress introduced the Omnibus Crime Control and Safe Street Act of 1968 focusing on crime prevention programs. The result was an initial spending spree concentrating on funding of local police programs. The Omnibus Crime Control and Safe Street Act of 1968, under Title I, allocated 50 million dollars in block grants to state and local law enforcement agencies to deal with the rise in crime, riots, and organized crime. These grants provided aid in training, purchasing of new equipment, and the hiring of additional police officers.

*A. The War on Crime*

The crime prevention grants were administered through the newly created Law Enforcement Assistance Administration (LEAA). To the same extent the Office of Economic Opportunity funded programs under the War on Poverty, LEAA funded programs under the "War on Crime." LEAA grants were initially funded under the Omnibus Crime Control and Safe Street Act of 1968 and administered through LEAA within the Department of Justice. This crime prevention endeavor marks the first substantial involvement of the federal government in providing aid to local law enforcement efforts. LEAA called for state planning agencies to coordinate local initiatives and comprehensive planning for improvements to criminal justice. LEAA authorized grants to state and local government units to meet new goals and police tactics introduced under the new structure of the state planning agencies. Although the first wave of funding concentrated on funding local police agencies, amendments in the 1970s expanded funding to correctional purposes, juveniles, anti-crime initiatives, and created the National Institute of Justice and Bureau of Justice Statistics to conduct independent research. For instance, total expenditures appropriated for LEAA in 1968 was 63 million dollars; this number grew to 895 million dollars by 1975. Since 1968, the federal government has been using grants to fund crime prevention programs and projects to build police-community relationships.

Initially, LEAA grants were touted as the center piece on the War on Crime, but support of the program quickly began to wane. The program received heavy criticism for being ineffective in crime prevention (Hinton 2016). The lack of leadership, a clear mission, and bureaucratic red tape were often cited as impediments of the program's effort to fight crime ("U.S. Anticrime" 1976; "Floundering" 1977). Similar to the War on Poverty, the anti-crime effort was viewed as to simplistic to deal with the complexity of urban crime (Horrock 1975; Varon 1975; Diegelman 1982). As crime continued to increase in the seventies, public officials began to scrutinize how LEAA funds were being used by local municipal law enforcement agencies (Hinton 2016). These officials cited that there was little evidence that funds were used to increase the number of officers ("Crime Program" 1972). Funds were often unaccounted for with federal public officials concerned about misappropriation of funds at the local level (Halloran 1971; Varon 1975; C.H. 1976; Diegelman 1982, Hinton 2016). Although in existence until 1982, LEAA did not administer any grants after 1980.[1] The program was eventually terminated in response to the political backlash related to the misappropriation of funds, funding of ineffective programs, and inefficient use of public dollars by state planning agencies (Pear 1980; "Death of an Agency" 1980).

*B. Re-evaluation of federal grants and the War on Drugs*

By the 1980s the War on Crime was supplanted by the War on Drugs.[2] Violence surrounding the crack epidemic garnered national attention creating a bi-partisan agreement for 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 federal dollars to assist state and local governments in efforts to fight crime and drug abuse problems. Learning from the past mistakes of LEAA, the mission was clear – improve the apprehension, prosecution, adjudication, detention, and rehabilitation of drug offenders.[3] The program grants were distributed in two parts 1) block grants to state and local governments and 2) discretionary grants for demonstration projects to public and nonprofit organization. The Anti-Drug Abuse Act of 1988 renamed the grant program the Edward Byrne Memorial State and Local Law Enforcement

[1] LEAA was officially terminated on April 25, 1982 [Source: NCJRS_2(Dec96)]
[2] War on Drugs was officially declared by the Nixon Administration with the creation of the Drug Enforcement Administration (DEA). March 28, 1973 – Message from the President of the United States Transmitting Reorganization Plan No. 2 of 1973, Establishing a Drug Enforcement Administration.
[3] See Program Policy and Administrative Guidance

Assistance Program honoring the life of a police officer slain during a drug arrest.[4] The 1988 amendment not only doubled downed the federal government involvement into local policing but develop multijurisdictional drug control strategies which were deemed vital to the War on Drugs.

The Edward Byrne Memorial State and Local Law Enforcement grants awarded funds to provide additional personnel, equipment, training, and technical assistance to law enforcement. The program required states to match 25 percent of the expenses creating a 3 to 1 federal involvement into local policing initiatives. During the 1987 fiscal year, 178 million dollars were appropriated for Byrne grants under the block grants while 46 million dollars were available for discretionary grants. By 1990 the program doubled in size with a total appropriation of 445 million dollars. The budget slightly increased over the next few years with a total allocation of funds of 500 million dollars in 1995 (Dunworth et al. 1996b).

Byrne grants went to local municipalities for a variety of areas to influence law enforcement effectiveness, but approximately half of all funds were used to establish multijurisdictional drug task forces (MJDTF) between 1989 and 1993 (Dunworth et al. 1996d). MJDTF were created to deal with drug trafficking that often involved multiple jurisdictions. By 1991, there existed 904 MJDTF which were responsible for over 250,000 arrests made in that year (Blumenson and Nilsen 1998). Despite the MJDTF covering 83 percent of the population, there is little evidence that the Byrne grants influenced drug arrest (Smith et al. 2000; Mazerolle 2007). Byrne grants through MJDTF have been championed as effective in improving communication between law enforcement agencies (McGarrell and Schlegel 1993; Jefferies et al. 1998).

The next large federal intervention into local police initiatives was through the Violent Crime Control and Law Enforcement Act (VCCA) passed by congress and signed into law by President Clinton in 1994. The VCCA reauthorized appropriations for the Byrne grant program as well as shifted focus towards community policing. The Community Oriented Police Service (COPS) office was introduced with the goal to support local law enforcement agency in crime prevention. The primary purpose of the COPS initiative was to provide grants to local police agencies to hire new police officers. There has been much speculation as to how effective these grant programs were at 1) increasing the number of police officers 2) reducing crime (Muhlhausen and Walsh 2008). Contrarily, literature in economics links grants for community policing with lower crime rates, using instrumental variables to imply causality over a short sample period with

[4] The program was renamed in the Anti-Drug Abuse Act of 1988

falling crime rates (Evans and Owens 2007). Concerns regarding the effectiveness of COPS and the Byrne grant program stems from the debate about its predecessor – LEAA.

*C. Expected Effects of Byrne Grants on Crime and Arrests*

In the Becker canonical 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 committing the crime has to be greater than zero.[5] These decisions are based on the net gain from committing a crime as well as the likelihood of being apprehended by law enforcement. Moreover, the local government is faced with a budget constraint as they try to minimize the amount of crime in society. Considering the criminal, the government 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 would lower the cost of police resources or the hiring of additional police officers for local authorities.

Providing the police additional resources increases the marginal product of police. Hiring more police increases the probability of detection, and, therefore, the cost of committing a crime. Effective or more policing, in theory, will increase arrests and reduce crime, as current criminals are apprehended, and future criminals are deterred from engaging in illicit activity due to the increase in the cost of criminal behavior. Becker's theory of crime predicts an exogenous increase in police resources will decrease crime, however, empirical research over the years has not confirmed this relationship holds (Cameron 1998; Marvel and Moody 1996).

Moreover, while Byrne grants may seem like an innocuous method to increase drug policing efforts, within a public choice framework it is possible that more police resources do not necessarily lead to lower crime rates. On the contrary, such allowances may provide police with motives to keep crime/arrest rates high since arrests rates are a measure of their efficacy and demand, and are now tied to federal funding (Blumenson and Nilson 1998; Benson, Rasmusen, and Sollars 1995). One would expect that this would not only lead to greater arrests rates for crime, but also greater racial disparities in arrest since there might be lower costs to implement adverse changes in policing policies within the most disadvantaged communities (e.g., minority

[5] See Isaac Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life and Death, 65 Am. Econ. Rev. 397 (1975);

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communities), which have fewer resources to protect themselves from this type of predatory behavior.

There is some evidence that asset forfeiture laws lead to increases in drug arrests over this period, even after accounting for funding offsets from state and local government (Baicker and Jacobson 2007). Moreover, it is quite possible that racial disparities in arrest rates are in part explained by policies, such as the Byrne grants, that may have led to an increase in predatory policing. It is also possible that due to racial differences in the operation of drug markets and prosecution, African American's who sell drugs are easier targets for police. Therefore, rational police officers might target black suspects first if they are easier to apprehend and/or charges and convictions are easier to obtain. Considering the two dialectical theories, the Byrne Grants – through MJDTFs and additional police officers – should lead to an increase in arrest, especially for drug related offenses, disproportionately affecting African Americans (direct effect). However, the effects on actual crime are possibly ambiguous (indirect effect).

## III. Data on the Edward Byrne Memorial Grant Program and Crime Data

Analysis of the Edward Byrne Memorial State and Local Law Enforcement Assistance Program begins with data from the Consolidated Federal Funds Report (CFFR) files. CFFR provides information on federal expenditures to state, county, and local municipalities and entities in the United States. The CFFR files provide expenditure data for grants-in-aid, direct loans, government purchases, and other direct payments from 1982 to 2007. Our analysis focuses on the ***discretionary*** grants to take advantage of the variation in timing of the EBMGP.[6] Agency police data are constructed from the Uniform Crime Reporting (UCR) Program's annual publication entitled "Law Enforcement Officers Killed of Assaulted" (LEOKA) which contains monthly accounts of law enforcement officers who are killed or assaulted while on duty. The number of civilian and sworn officers as of October 31 of the reporting year are also included in LEOKA. This data is publicly available at Inter-University Consortium for Political and Social Research for the years after 1974. Information regarding arrests come from the UCR's "Arrests by Age, Sex,

[6] We focus on discretionary grants for two reason. First, block grants went to state capitols and were distributed to local municipalities based on the discretion of state planning boards. This often resulted in funds being used for political purposes and not crime prevention, resulting in the over funding of rural municipalities. Secondly, 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).

and Race" (AASR) which categorize monthly arrests by sex, race, age, and offense. Combining the CFFR files with the UCR's LEOKA and AASR results in a data set that links federal expenditures on public safety and narcotic control to local crime and arrest rates. The analysis focuses on the relationship between federal grants from the EBMGP and local police behavior between 1980 and 2005. The final sample includes a total of 407 cities, in which 199 cities receive Byrne grants between 1986 and 2000 (treatment group) and 208 cities that were unfunded (control group).[7] Every city in the final sample has a population of over 25,000 residents in every sample year.[8]

To isolate the effect of the EBMGP, we restrict the sample to include only cities funded between 1987 and 2004.[9] Our methodological approach focuses on the implementation of the EBMGP to estimate a causal relationship between grants and police behavior, and we also supplement our analysis by assessing the effectiveness of the initial expansion of the program in treated cities. Figure 1 displays the estimated probability of receiving a Byrne grant for those that were treated relative to cities that never participated in the Byrne Grant Program. The figure captures how estimated funding propensities change after initial treatment.[10] 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 government that received a Byrne grant were likely to be treated more than once. After initial treatment, the estimated probability of receiving additional funding is between 25 and 35 percent. Figure 2 presents the average size of a Byrne grant over the first 5 years of treatment. According to Figure 2, the initial grant typically was the

[7] See Appendix Figure 2A for details of the city level roll-out of discretionary grants by year for our sample. Note that Figure 2A does not account for every city that received a grant but captures a subset of cities that received discretionary grants.

[8] The Census County and Data Books provide demographic data for cities and places with more than 25,000 residents. This determined our sample restriction for our main analysis.

[9] The 1986 Anti-Drug Abuse Act provided funding to state and local government agencies to fight, deter, and prevent drug related offenses. The grant program was named after Edward Byrnes, and officer killed in the line of duty in the 1988 Anti-Drug Abuse Act. 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). Therefore we focus our analysis on the pre-2005 grant program

[10] Figures 1 and 2 report coefficient 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 one if the city ever received a Byrne grant and $1(\cdot)$ is an indicator variable equal to one if the observation year is $\pm\tau$ years from the date that the Byrne grant is received. Section IV outlines this empirical approach in detail.

largest; approximately $900 thousand. Subsequent grants were typically smaller, but funding increased over time.

Table 1 reports the sample's average characteristics in 1980. Demographic data is provided by the United State Census Bureau County and City Data Books publicly available at the Inter-University Consortium for Political and Social Research (Haines 2005; U.S. Census 1995; U.S. Census 2002). In 1980, the average sample population was 107,514 and roughly 11 percent of the residents in these cities were black. Treated cities are typically larger and poorer which is reflected in median income and the percentage of the population with a female head of households. Treated cities also have a larger proportion of black residents. Treated cities also experience higher crime rates, have a slightly larger police force, and more drug arrest rates compared to the control group. Despite having a smaller share of black residents, cities in the control group have higher drug arrest rates for black residents. Also, relative black/white drug arrest rates differ dramatically between the treated and control group. Drug arrest rates are 3 times larger than white drug arrest in the control group compared to black drug arrest being twice as large as white drug arrest in the control group. This difference is driven by pre-treatment drug arrest of black residents.

## IV. Empirical Strategy

The empirical strategy in this analysis will take advantage of the differential timing of the implementation of the EBMGP. Although there are key cross-sectional differences between funded and unfunded cities, the identification strategy is dependent on how crime and arrest rates evolve before a city receives a grant. According to table 1, cities that received Byrne grants have much higher average crime rates between 1980 and 1985. This is not surprising, considering cities that received Byrne grants are typically larger, more urban, and have a larger proportion of black residents. Important for our research design, however, is that crime *evolved* similarly in *treated* and *untreated* cities prior to the implementation of the EBMGP. Our analysis will account 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 will help estimate how crime and arrest rates are *evolving* over time and provide a *control* group for how crime and arrest rates are expected to *evolve* after treatment. The untreated cities in this sample provide a plausible control group if city and year fixed effects capture the difference in how arrest rates *evolve* in treated cities versus untreated cities before the implementation of the grant program. A test of this assumption is

embedded within the difference-in-difference 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 due to the introduction of the EBMGP.

The empirical strategy will also take advantage of the variation in the roll-out 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 1960 demographic characteristics that are determinants of crime and arrest rates on the year the first grant was received. Table 2 reports weighted and unweighted estimates from ordinary least squares (OLS) regressions for the year grants were received and the probability of treatment. Columns 2 and 4 reports estimates from a weighted least squares regression (weighted by 1980 city population) including the change in crime rates between 1980 and 1985.[11] Similar to Table 1, key demographic variables are a strong predictor of treatment however female head of household is the only predictor of timing. According to Table 2, changing crime rates fail to predict when a city first received a grant.

The second test of the identifying assumption is to compare the timing of the LSP with the pre-program *growth* in crime. Figure 3 plots the changes in total crime from 1980 to 1985. According to Figure 3, the timing of discretionary grants is uncorrelated with changes in pre-period crime rates.[12] These two tests provide statistical evidence that the variation in the timing of grant was not determined by pre-period crime rates or predictors of crime. As a result, the timing of the first grant will identify a causal relationship between the Byrne grants and crime and arrest rates if one exists.

Using the variation in the timing and location of funding, we can identify causality within an event-study framework (Jacobson, LaLonde, and Sullivan 1993)[13]. The event-study design lends itself well to testing the effects of an outcome before and after exposure to the treatment and

[11] Weights are used to give more weight to cities that contribute more to the population descriptive statistics used in the regression analysis.

[12] The slope in Figure 3 is -0.0000229 (0.00244). The slope is calculated from a univariate regression of the crime rates on the year the first grant was received. Should dependent variable be change in crime rates?

[13] See Bailey and Goodman-Bacon (2015), Cunningham (2016), and McCrary (2007) for other studies using a similar identification strategy.

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 treatment. The event-study also provides a statistical description of the evolution of pre-trends in the outcome variable as well as the dynamic of changes in the outcome variable after the first grant arrives. We estimate the effects of the EBMGP by using the following linear regression:

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

where $Y_{i,t}$ is the outcome of interest in city *i* in year *t (t= 1980, 1981,...2009)*; $\gamma_i$ is a set of city effects which control for unobservable city characteristics that are time invariant; $\alpha_t$ is either a set of year effects or state-by-year effects ($\alpha_{s(i),t}$). Year effects will absorb policies that will impact crime nationally. State-by-year effects captures time-varying state-level changes such as the business cycle or policy changes (e.g., punishment, enforcement), which may influence the supply of criminal activity. $D_i$ is an indicator variable equal to one if the city ever received a Byrne grant. $1(t - T^* = -\tau)$ is an indicator variable equal to one if the observation year is $-\tau$ years from the date that the Byrne grant is received or $1(t - T^* = \tau)$ is equal to one if the observation year is $\tau$ years after the date Byrne program was first implemented in city *i*. $1(t - T^* = 0)$ is omitted due to 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 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 will give unequal weight to cities that receive federal grants very early or late in the sample. These endpoints, therefore, are omitted from the presentation of results.

In the final sample, cities receive federal grants between 1987 and 2000. A balanced event panel using UCR data from 1980 to 2009 will focus on five years before and five years after federal funds are received. The coefficients of interest are $\pi_{-\tau}$, which are pre-treatment effects, and post-treatment effects $\delta_\tau$. These estimates describe the dynamics of 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, $\delta_\tau$, is the average change in the difference in outcome variable of interest $\tau$ years after the city received the grant.

Since the econometric model includes city fixed effects, the pre-treatment $\pi$ and post-treatment effects $\delta$ will be unbiased even in the presence of pre-existing and permanent differences in crime and arrest rates between cities that receive funding and those that do not, despite the key cross-sectional differences. Also, the event-study framework provides an intertemporal response of the outcomes that are gradual and non-linear. This dynamic model estimates changes in the outcome relative to the date of the first grant; thus, eliminating potential downward bias from the averaging of outcomes in low crime communities with those that are more drug infested and display higher crime and arrest rates. Lastly, this framework estimates a causal relationship between Byrne grants and police behavior. Because the model captures changes in outcome variable of interest that are unrelated to crime and arrests, the post-treatment effects will capture any *trend break* in the outcome variable of interest due to the implementation of the Edward Byrne Memorial Grant Program.

The event-study estimates can be summarized in a difference-in-difference (DiD) specification using 2 year intervals for post-treatment effects (Bailey and Goodman-Bacon, 2015):

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

where the notation remains as defined above and q indexes the group of all years more than 5 years before grants and years -5 to -1 and p indexes each of the periods for 1 to 2, 3 to 4, 5 to 6, and 6 and later. This specification is less connected with the timing of changes compared to the event study approach in Equation 1, but it has the advantage of summarizing the estimates and their joint statistical significance. Following Bailey and Goodman-Bacon (2015) approach, we also analyze heterogeneous treatment effects by city characteristics using the following specification:

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

where, $D_i^k$ is equal to 1 if a city ever received a grant under the Byrne Memorial Grant Program and belongs to group *k*.

## V. Results

Using the estimates from Equation 1, we plot pre-treatment effects and post-treatment effects from a balanced panel. Figure 4 plots the estimates from three different specifications of Equation 1. Model 1 is plotted in the solid line with no markers. It contains only city and year effects. Model 2 is plotted with a solid line and circle markers and includes city and state-by-year effects. Model 3, similar to Model 2 but includes demographic variables from Table 1 – interpolated between census years – is plotted with a solid line and square markers. We present 95-percent confidence intervals for models 2 and 3 with dashed lines. The confidence intervals are constructed from heteroscedasticity-robust standard errors clustered by city. The sample consists of cities with a population greater than 25,000 residents in every year. Presented are estimates for the outcome of interest $Y_{i,t}$ for Equation 1. All regressions are estimated using the 2000 population as weights to correct for heteroskedasticy related to city size in the error term.[14]

Figure 4 plots pre-treatment and post-treatment effects for Byrne grants on sworn police per 1,000 residents. According to all three models, the point estimates for $\pi_{-\tau}$ are indistinguishable from zero and statistically insignificant. After the first year a grant is received, post-treatment effects are positive and statistically significant. Sworn police officers per 1,000 residents increases over the next four years and is statistically significant. Using model 3 estimates, sworn police officers per 1,000 residents increase by 2.4 (.0399/1.6719) percent three years after treatment. Four and five years after treatment, the size of law enforcement is steadily growing. The results are consistent with the grant utilization for *additional* personnel to conduct wide-spread apprehension of persons in violation of local and state substance abuse laws (U.S. Bureau of Justice Assistance, 1987). Both the Anti-Drug Abuse Act of 1986 which introduce federal grants to state and local institutions and the Anti-Drug Abuse Act of 1988 which formally introduce the Edward Byrne Memorial State and Local Law Enforcement Assistance Programs outlined the grant purposes which included additional personnel, equipment, training, technical assistance, and information for the more widespread apprehension, prosecution, and rehabilitation of persons who violate drug and crime laws (Anti-Drug Abuse Act, Pub.L 100-690, 1988).

[14] Weighted least squares is used to make the error term homoscedastic.

*A. Event-study Results for Arrests*

Figure 5 presents pre-treatment and post-treatment effects for drug arrests per 1,000 residents. Once again, pre-treatment effects are indistinguishable from zero and statistically insignificant in all three specifications for all but one pre-period. The post-treatment effects are statically insignificant in all three specifications. Post-treatment effects in Model 2 and Model 3, are positive and increasing over the first 3 years but does not provide evidence that the Byrne grant program increases drug arrests. Although treated cities typically increased the size of law enforcement, additional police officers are not necessarily resulting in higher drug arrest rates. A possible explanation for the insignificant post-treatment effects seen in Figure 5 is the 1986 and 1988 anti-drug acts possibly changed police behavior nationally by expanding the War on Drugs. Although the EBMGP provides additional funding, it only represents a small fraction of total expenditures on public safety. It is also possible that Byrne grants had spillover implications for nearby untreated cities through the establishment of multi-jurisdictional drug task forces. These multi-jurisdictional drug task forces could increase drug arrest in both treated and nearby untreated cities and the full sample specifications would under estimate the effect of the Byrne Grant Program. This is especially important considering the results in Figure 5 which imply the federal grant program did not result in a wide-spread increase in apprehension, regardless of specification.

To a great extent mass incarceration is driven primarily by rigid punitive punishments for drug related crimes (Alexander 2010; Raphael and Stoll 2013); moreover, incarceration rates for drug related criminal offenses are differentiated by race (Beckett et al. 2006; Cox 2015). Disparities in incarceration by race may stem from targeted policing strategies in high crime areas where narcotic trafficking occurs outdoors; creating the opportunity for wide-spread apprehension for drug offenses (Johnson et al. 1977) [15]. This policing strategy in conjunction with historically higher arrest rates for blacks suggests the possibility of heterogeneous treatment effects. Although previous results suggest that the EBMGP had minimal influence on arrest rates, the implementation of the program could exacerbate differences in incarceration if apprehension of drug offenders differ by race due to treatment.

[15] It is unclear whether selective enforcement behavior for drug crimes are due to police officers targeting suspects that are easily apprehended or whether suspects are targeted because of race. Becket et al.'s (2006) analysis provides evidence that this is not the case.

Figures 6 & 7 plots pre-treatment and post-treatment effects for the total drug arrest per 1,000 residents by race. Figure 6 plots weighted least squares estimates from Equation 1 for the white drug arrest per 1,000 white residents and Figure 7 focuses on black drug arrests per 1,000 black residents.[16] Similar to Figure 5, pre-treatment effects in both figures are statistically insignificant except for earlier periods. Also, there is little evidence that the EBMGP influenced arrest rates of white or black residents. Post-treatment effects are near zero, statistically insignificant, and show no visible trend break in each specification. Drug arrest rates are driven by arrest for drug possession. According to Table 1, drug possession represented roughly four-fifths of all drug arrests. Therefore, the results in figures 5 through 7, were driven by police arrest for drug possession.

Table 3 report estimates from Equation 2 for arrest for drug possession. Columns 1 and 2 provide pre-treatment and post-treatment effects for the influence of the first Byrne grants on arrest for drug possession per 1,000 residents. Column 1 report estimates for Model 2 while column 2 report estimates when the sample is restricted to only treated cities. Pre-treatment effects in both columns are statistically insignificant. Post-treatment effects are negative, statistically insignificant, and generally increasing in magnitude over time. Columns 3 through 6 report pre-treatment and post-treatment effects on arrest for drug possession per 1,000 for white and black residents. In general, pre-treatment effects are negative and statistically insignificant except for column 4, where pre-treatment effects are positive. Post-treatment effects are also negative for black and white arrests in all columns except column 4. Post-treatment effects in column 3 are smaller than post-treatment effects in Figure 6, providing suggestive evidence that the Byrne grants may have a strong influence on drug sales. This is also true for column 5, where post-treatment effects are larger in magnitude.

Similar to Table 3, Table 4 report estimates from Equation 2 for arrest for drug sales. According to columns 1 & 2, treatment is associated with an increase in drug sale arrests. The post-treatment effects are positive and statistically significant across sample restrictions. The joint treatment effect in year 1 and 2 indicates a 43 (0.416/0.956) percent increase in drug sales arrest

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

after the first discretionary grant is received.[17] Arrest for drug sales increases by 9 percent 3 to 4 years after treatment. The cumulative post-treatment effect implies a 126 (1.2/0.956) percent increase in drug sales arrests 6 years after the first Byrne grant. The post-treatment effects differ dramatically by race. According to column 3, the cumulative post-treatment effect implies a 107 (0.98/0.916) percent increase in drug sales arrests for whites compared to a 44 (1.643/3.708) percent increase in drug sales arrest for blacks in column 5. The difference in the post-treatment effects by race can be attributed to 1) the huge increase in arrest for blacks in year 1 and 2 (not statistically significant) and 2) historically high arrest rates for blacks. According to Table 4, there is a steady increase in white drug sales arrests compared to an immediate one-time increase in black drug sales arrests. The point estimate for year 1 and 2 for black drug sales arrest is larger than the cumulative effect for whites. But the huge treatment effects are partly due to historical policing efforts in these communities which are reflected in the smaller cumulative effect.

Tables 5 & 6 analyzes the heterogeneous treatment effects by city characteristics. Table 4 focuses on white drug sale arrests per 1,000 white residents and Table 5 report joint treatment effects for black drug sale arrests per 1,000 black residents. Although the pretreatment effects in the event-study framework provide a falsification test for time-varying cross sectional differences influence on arrest rates, columns 1 & 2 examines how 1980 crime rates influence arrest rates. We test if places with higher (lower) crime rates before the implementation of the grant program are more effective with apprehending drug trafficking offenders. Columns 1 & 2 reports pre-treatment effects and post-treatment effects for cities with crime rates below and above the median crime rate in 1980.[18] Columns 1 & 2 provide evidence of heterogeneous treatment effects on drug sale arrests of whites by previous crime rates. The point estimates in column 2 are statistically significant in later periods and much larger than those reported in Table 4. Cities with a high 1980 crime rate experienced a 166 (1.529/0.916) percent increase in drug sale arrest of white residents over 6 years. These cities also experience a 64 (2.367/3.708) percent increase in drug sale arrests of black residents although the post-treatment effects are not statistically significant.

There is evidence that the police apprehended more white drug sale offenders in cities with a larger black population compared to cities that had smaller black population. The cumulative

[17] Mean drug sales arrest rate at event year 0 is 0.956 and is 0.916 for white drug sales arrests and 3.708 for black drug sales arrests.

[18] All estimates in Tables 3 and 4 are produced using untreated cities in the sample as the comparison group instead of comparing treated cities above the median with treated cities below the median characteristic.

effect of the Byrne grant program on white drug sale arrests in cities with the proportion of the black population is larger than the median is 113percent larger than the below median cities. According to column 4, cities with the proportion of black residents above the median black population experienced a 141 percent increase white drug sale arrest rates over six years compared to cities that never received a Byrne grant. In comparison, cities with a proportion of black residents below the median experienced a 66 percent increase in white drug sale arrests compared to untreated cities. Similar to white drug sales arrests, cities with the share of black residents above the median produce larger post-treatment effects for black drug sale arrests. However, none of the post-treatment effects are statistically significant in the above median group while the post-treatment effect for 1 to 2 years after treatment is marginally statistically significant in the below median group. According to columns 3 and 4 of Table 6, black drug sales arrest increase by 35 percent in the below median group and 82 percent in the above median group six years after treatment.

Columns 5 through 7 of Tables 5 and 6 present estimates by city size. Column 5 compare treated cities with the population between 50,000 and 100,000 residents to untreated cities. Column 6 compare treated cities with the population between 100,000 and 250,000 residents to untreated cities and column 7 contrast large treated cities with untreated cities. Post-treatment effects are positive in columns 5 and 7 for both black and white drug sales arrest but are not statistically significant. Heterogeneous treatment effects for black and whites are the largest when comparing large treated cities to untreated. However, the joint pre-treatment effect in column 4 of Table 6 is negative and marginally statistically significant suggesting the black drug sale arrests were lower relative to untreated cities before treatment.

Table 5 and 6 provide evidence that treatment effects on drug sales arrest are driven by 1980 crime rates, the proportion of black residents, and city size. Average treatment effect on the treated for white drug trafficking arrest is driven by pre-period crime rates and cities with a large proportion of black residents. This provides suggestive evidence that Byrne Grants lead to policing in communities that were typically under policed. High crime rates in low income communities probably lead to a relatively strong police presence in low income black communities. Marginal return to additional policing in these communities is likely low compared to white communities where crime occurs.

Similar to Table 1, cities that are larger are more urbanized, dense, have higher crime rates, and a larger proportion of black residents. Average treatment effect on the treated for black drug arrest is driven by a large increase in drug sales arrest in large cities. The EBMGP is associated with differential arrest rates by race, and cumulative effects show a relatively large increase in the arrest of whites for drug sales. However, the post-treatment effects are large for black drug sales arrests relative to whites. According to Table 4, black drug sale arrests where 68 percent larger than whites 6 years after treatment. According to column 7 in Table 5 and 6, the increase in drug sales arrest of blacks was 243 percent larger than whites.

Table 7 report estimates from Equation 2 for the number total and violent crimes reported as well as violent crime arrests for black and white residents. Violent crime aggregates include murder, manslaughter, rape, assault, and robbery whereas Total crime includes violent crimes as well as property crimes – burglary, larceny, and motor vehicle theft. Both the 1986 and 1988 Anti-Drug Abuse Acts targeted violent crimes related to drug trafficking. Although property crimes may be an externality of drug trafficking, it is reasonable to believe that the Byrne program would influence crime rates. Columns 1 and 2 provide pre-treatment and post-treatment effects for the influence of the first Byrne grants on violent crimes per 1,000 residents while columns 3 and 4 report treatment effects for total crime per 1,000 residents. Pre-treatment effects are generally negative and not statistically significant except for in column 4 where the joint treatment effect is marginally statistically significant. Post-treatment effects are also statistically insignificant in all four columns. Joint treatment effects for violent crime displays no visible pattern across sample restrictions. Post-treatment effects for total crime are generally positive and move in the same direction across sample restrictions. Columns 5 through 8 provide evidence of differential treatment effects by race. According to columns 5 and 7, white arrest rates increased significantly after a Byrne grant was received compared to a decrease in violent arrest for black residents. Byrne grants are associated with a 16 (1.067/6.474) percent increase in the arrest of white violent crime offenders, six years after treatment.

*B. Difference in Difference Results by Grant Size*

The implementation of the Edward Byrne Memorial Grant Program (EBMGP) is associated with a 6 percent increase in the number of sworn law enforcement officers and 126 percent increase in drug sale arrests after six years. Cities that receive a Byrne grant also arrest

more black drug sale offenders; however, there is evidence that the increase in white drug arrest rates is relatively larger. These results vary by underlying baseline characteristics and the size of the city. The event-study methodology provides insight on how the implementation of the EBMGP changed local institutional behavior with regards to drug related offenses. Seemingly, the effectiveness of the program may vary by the size of the grant. To investigate the effects of federal funding on police performance, we estimate the following reduced form regression:

$$Y_{i,t} = \gamma_i + \alpha_{t,s(i)} + \theta Byrne_{i,t} + \varepsilon_{i,t}$$

where $Y_{i,t}$ is the outcome of interest in city *i* in year *t*. Again, the parameter $\gamma_i$ is city fixed effects and $\alpha_{t,s(i)}$ refers to state-by-year fixed effects which were previously discussed. Lastly, $Byrne_{i,t}$ measures the size of the federal grant per capita (in hundreds of dollars).[19] This empirical strategy takes advantage of the variation in geography, the timing of the treatment, and the intensity of the treatment measured by the size of the grant.

Table 8 presents the estimates of $\theta$ from Equation 4 for the effects of federal funding on drug arrest per 1,000 residents. Equation 4 is estimated using the city population in 2000 as weights to perform a weighted least squares regression. The standard errors are constructed from heteroskedastic robust standard errors clustered by city. Column 1 report estimates using city and state-by-year effects, column 2 adds demographic variables interpolated between census years; and column 3 restricts the sample to only treated cities. Columns 4 through 6 report estimates of $\theta$ for white drug arrests and columns 7 through 9 report estimates for black drug arrests. Panel A refers to the total arrest for drug possession and Panel B refers to arrest for drug sales. In Panel A, estimates of $\theta$ are generally positive but marginally statistically significant only in column 7. Panel A is consistent with Table 3, highlighting a weak or no relationship between Byrne grants and drug possession arrests. Similar to Table 4, panel B provide evidence of a relationship between the size of the grant and arrest for drug sales. According to column 1, for every $100 increase in the size of the grant per capita, arrests for drug trafficking increase by .424 per 1,000 residents or by 32 (.424/1.32) percent.

Participation in the Byrne Grant Program increases arrest for drug trafficking, but the size of the grants is associated with differential arrest rates. Cities that receive larger grants are not only

[19] The size of the grant is adjusted for inflation and measured in the year 2000 dollars.

associated with higher arrest for drug sales, but results vary by race. Although not statistically significant, according to column 4, for every $100 increase in the size of the grant per capita, the total arrest of whites for drug trafficking increases by 22 per 100,0000 rates increase or by 21 (.220/1.05) percent. In contrast, according to column 7, for every $100 increase in the size of the grant per capita, the total arrest of blacks for drug trafficking increase by 101 per 100,000 black residents or by 24 (1.008/4.24) percent. Although the reduce-form estimates do not imply causality, the difference in drug sales arrests rates by race are substantial. Considering arrest rates for blacks are historically higher than whites, federal funding of the War on Drugs can be linked directly to the increase in incarceration of blacks. Table 8 provides suggestive evidence that federal involvement in narcotic control and trafficking lead to an increase in the apprehension of drug offenders, primarily driven by the arrests of blacks.

## VI. Discussion

The results of this study suggest that federal funding for the War on Drugs increased the hiring of police personnel but, overall, did not improve police effectiveness in program target areas as measured by total and violent crime rates, as well as arrests for both drugs and violent crimes. However, analyzing overall crime statistics mask the heterogeneous effects of federal grant programs on the arrest by type of crime, race, and city characteristics. Specifically, while there is no impact on federal funding on overall drug or drug possession arrests rates, intergovernmental transfers influence drug sales arrests, and this impact is primarily driven by increases in the arrests of African Americans in years one and two and increases in white drug sales arrests in years three to six post grant receipt. Contrary to our hypotheses, the general trend in the data is for the change in black drug sales arrests to have a marginally significant[20] increase in years one and two after grant funding, but the impact, while generally positive, is decreasing in magnitude and insignificant thereafter. The change in white drug sales arrests is positive and increasing in magnitude over time (after funding), but only significantly so for years three to six. This pattern persists in the data regardless of city characteristics. Likewise, violent crime arrests had a similar pattern, except changes in violent crime arrests rates are concentrated in the increase in white violent crime arrests post grant funding receipt, and these changes also increase over time.

[20] The increase in black drug sale arrests is significant at the 10% level using a one-tailed test that is consistent with our initial hypothesis regarding black arrests.

Interestingly, receipt of this discretionary award did not impact overall or violent crime rates, providing evidence that hiring additional police did not improve deterrence or effectiveness when measured by change in crime rates.

However, these findings may offer some support for the theoretical findings within a public choice framework that increasing police resources do not necessarily translate into an increase in police effectiveness and greater deterrence, as measured by lower crime rates. On the contrary bureaucratic supervisors in pursuit of discretionary profits (partially made possible through the availability of federal funds), have an incentive to take a wait and see approach by focusing on measurable outputs like response times and arrest rates, versus crime prevention, to show both their productivity and importance at the same time (see for a discussion Cox 2015, Benson et al. 1996, and Andreozzi 2004). However, surprisingly, evidence of increases in

While a cursory interpretation of these results might suggest that white drug traffickers were targeted by police officers after receipt of federal funding, not African Americans, a more comprehensive look at the data reveals that this is not the case. Prior to receiving the grant, both treated and control cities had exorbitant racial disparities in drug arrests for both sales and possession. Specifically, African Americans were arrested at 2.57 the rate of whites in treated cities and 3.83 times the rate of whites in non-treated cities for drug sales. Similarly, blacks were arrested at 2.03 and 3.17 times their white counterparts in treated and control cities, respectively, for drug possession arrests. In light of the substantial racial disparities in arrest, a more plausible explanation of our results is that there are diminishing returns to selective enforcement of African Americans for both drugs and violent crimes. Nonetheless, we do find that in general, selective enforcement among African Americans for drug sales and drug possession is positively associated with increases in discretionary grant funding, but this effect does not persist after controlling for selection of grant recipients. These findings taken together with the general pattern of marginally significant increases in African American drug sales arrests in years one and two post grant receipt, but no significant changes when white drug sales arrests begin to significantly increase in years three through six, suggest that, due to limited resources, police officers may have first selectively enforced laws within black communities, only to expand enforcement among whites once additional resources from the federal government became available through the Byrne grant program. Nonetheless, it should be noted that the evidence is only suggestive because we cannot rule out the scenario that there was no increase in black drug arrests over this time period.

This begs the question: Does federal grant funding help to combat selective racial law enforcement? In the case of the Byrne grant program, this is probably not likely because there are extremely large increases in black drug sales arrests in years one and two of funding (1.161 arrests per 1000 black residents) relative to the increase in white drug arrests in years three through six (1.025 arrests per 1000 white residents) that racial disparities likely increased slightly or remained the same. This finding remains unchanged regardless of city characteristics. In fact, when we look at results by city characteristics, we see that overall African Americans experience relatively large increases in drug sales arrests rates in years one and two after grant receipt compared to the cumulative impact on whites for years three through six. It is also interesting to note that while cities with above median 1980 crime rates, cities with above median black populations in 1980, and cities funded before 1995 responded to Byrne grant funding by increasing both black and white drug sales arrest rates, cities with below median 1980 crime rates, cities with below median black populations, and larger cities (population of at least 250,000) increased black drug sales arrest rates but did not increase white drug sales arrest rates. Therefore, larger cities, cities with below median crime rates, and cities with below median black populations solely focused on the apprehension of suspected black drug traffickers, but the significant increase happened only in years one and two. Thus, it is likely that racial disparities increased in cities with these characteristics. Given that one of the program priorities was "…to strengthen urban enforcement and prosecution efforts targeted at street drug sales" (Bureau of Justice Assistance 1997), and that African Americans are concentrated in urban areas, these findings are not surprising.

## VII. Conclusion

This study investigates the impact of a federal intergovernmental transfer program, Edward Byrne Memorial Grant Program (EBMGP), on police resources, police effectiveness, and selective law enforcement. Selective racial policing policies could be attributable to efficiency enhancing behavior (i.e., differences in offending behavior could lead police to target groups that are more easily detectable), implicit bias, or racial animus. EBMGP is a grant program authorized under the 1988 Anti-Drug Abuse Act to combat illicit drug abuse and to improve the criminal justice system. Funds for the Byrne Grant program could be used for a variety of purposes to combat drug crimes, as well as violent and other drug related crimes. The event-study analysis suggests that implementation of this grant resulted in an increase in police hiring and an increase in arrests for drug trafficking. Post-treatment effect implies a 107 percent increase in white arrests for drug sales compared to a 44 percent increase for blacks 6 years after the first grant is received. However, due to historical racial differences in drug arrests, the substantial increase in white drug arrest still results in large racial disparities in drug arrests. This is supported by weighted least squares regression estimates that show, for every $100 increase in Byrne Grant funding, arrests for drug trafficking increased by roughly 22 per 100,000 white residents and by 101 arrests per 100,000 black residents. The results provide strong evidence that federal involvement in narcotic control and trafficking lead to an increase in drug arrests; disproportionally affecting blacks.

In addition, the change in African American drug sales arrest rates increases at a diminishing rate, while that of whites increases at an increasing rate. Moreover, the grant program only marginally significantly impacts African American drug sales arrest rates in years one and two post grant receipt, and whites in years three through six, suggesting that black drug sales arrests and white drug sales arrests might be substitutes. We also find that the size of the grant is positively associated with drug arrests (both possession and sales) only for African Americans, but this effect

seems to be driven by the decision to participate in the program. We not only find heterogenous treatment effects by race, but we also find that the impact of EBMGP varies by city characteristics and race. However, the pattern of the results mirrors those from the analysis solely focusing on racial differences. Overall, the evidence suggests an increase in racial disparities due to the EBMGP. So far, this analysis has shed light on one possible mechanism through which policies associated with the war on drugs may have contributed to racial disparities in arrests and possibly incarceration.

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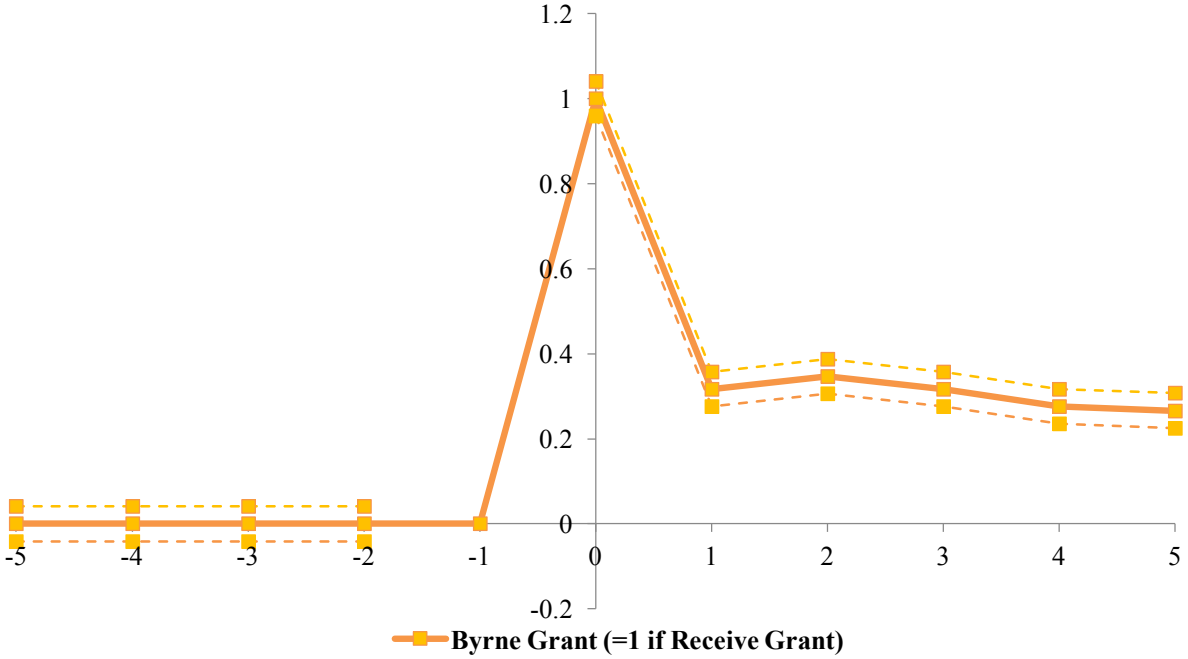
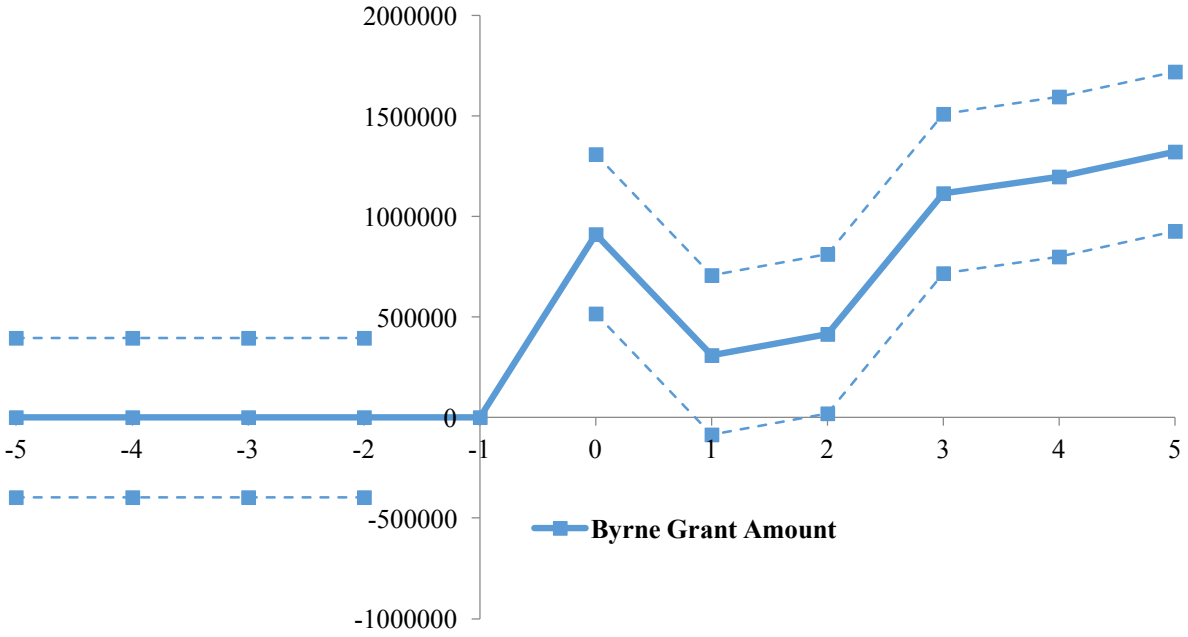
## TABLES AND GRAPHS

### Figure 1 Estimated Byrne Grant Propensities



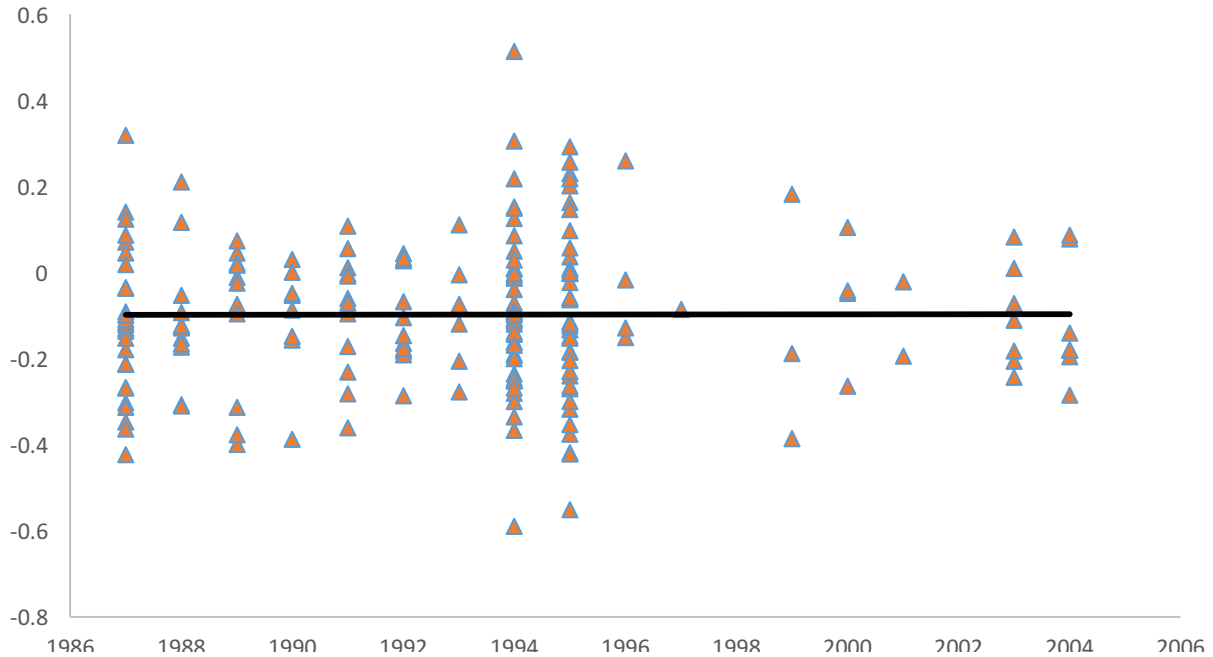

*Notes:* Dependent variable is an indicator equal to 1 if the city received a Byrne Grant. The horizontal axis corresponds to the years before and after the first Byrne grant is received. Heteroskedasticity-robust standard errors clustered by city are presented.

### Figure 2. Estimated Average Byrne Grant



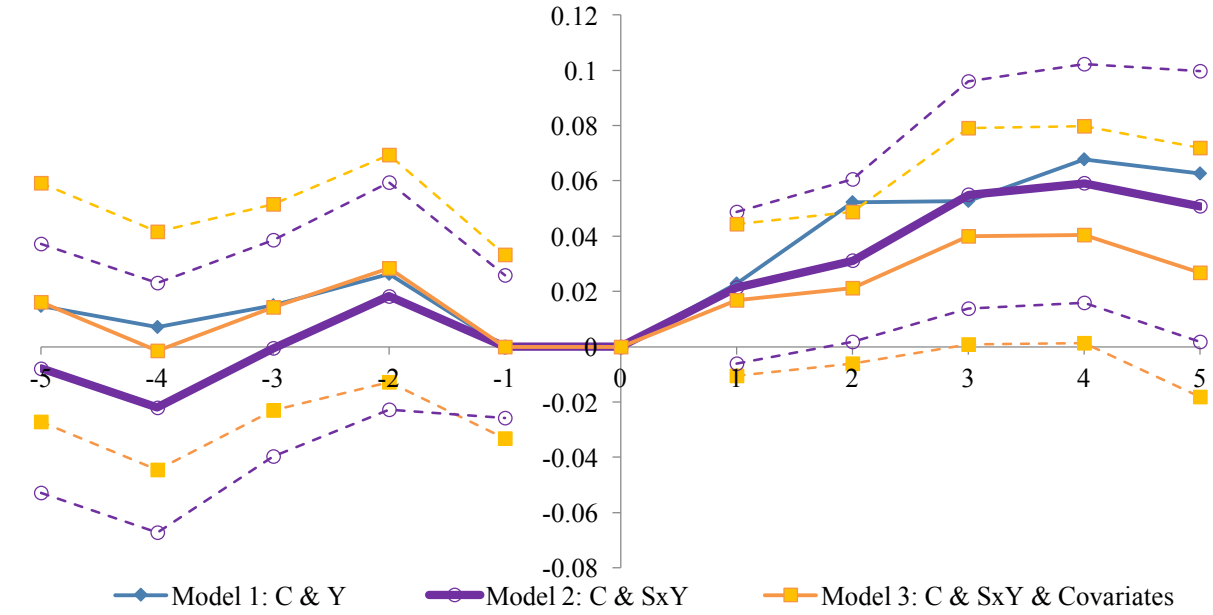

*Note:* Dependent variable is the amount of the Byrne grant. The horizontal axis corresponds to the years before and after the first Byrne grant is received. Heteroskedasticity-robust standard errors clustered by city are presented.

**Figure 3. Pre-trend Crime Growth Rates**



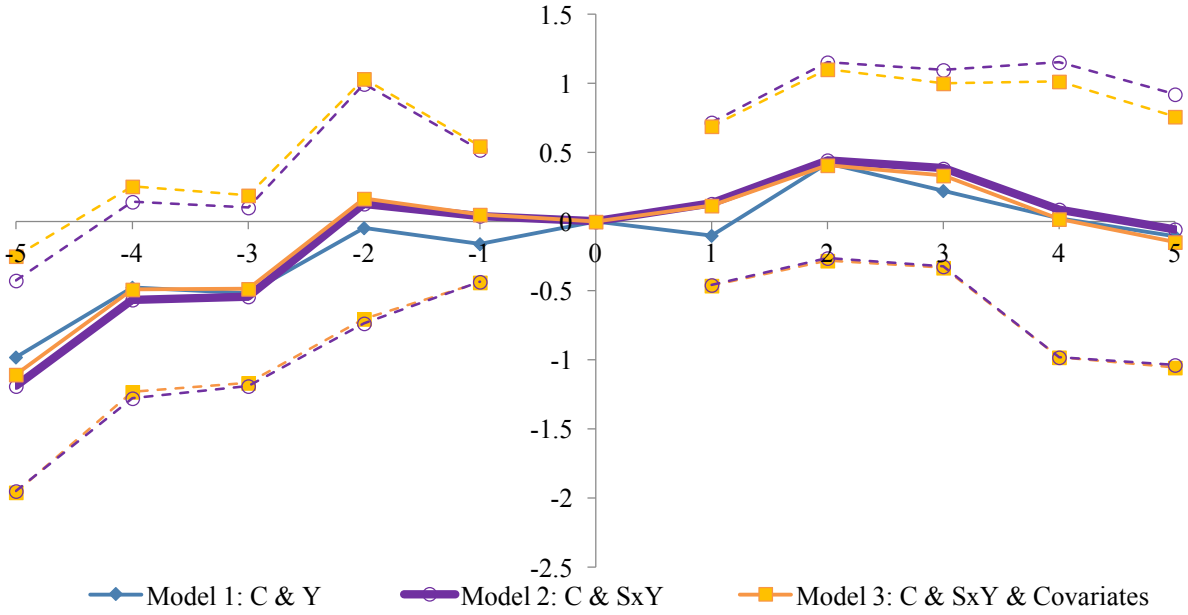

*Notes:* Regression coefficients and predicted values are from a univariate regression of the dependent variable crime on the year a city received their first discretionary Byrne grant. The slope is -0.0000229 (0.00244).

**Figure 4. Estimates of the Effects of the First Byrne Grant on Sworn Police**



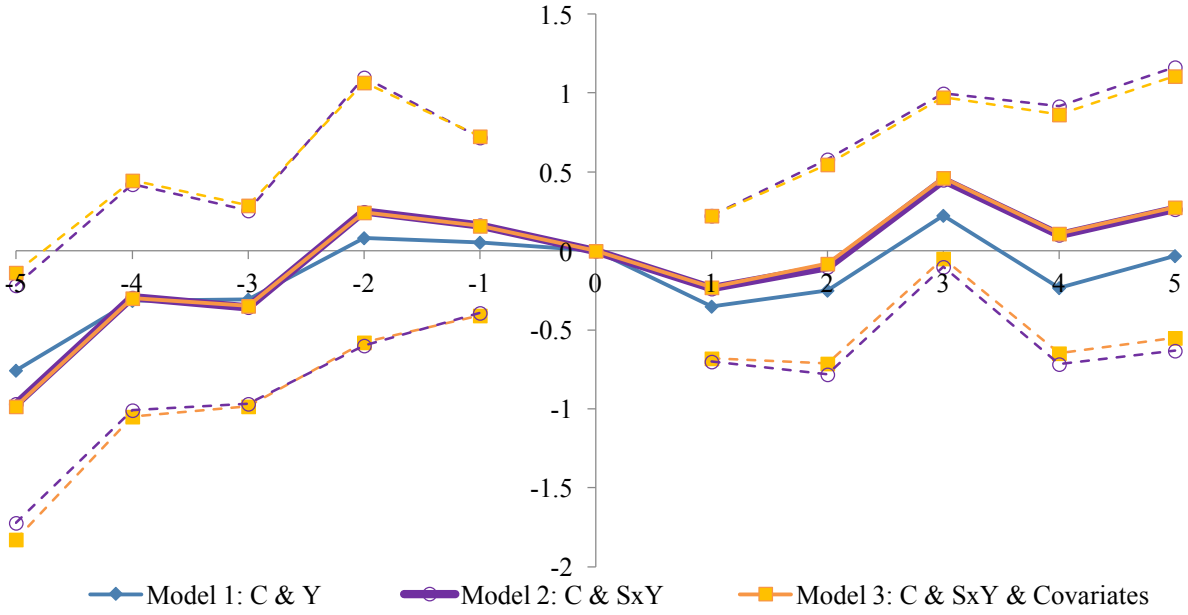

Notes for Figure 4: Police data comes from the Uniform Crime Report: Law Enforcement Killed or Assaulted Files. Model 1 includes City, C, and year, Y, effects, Model 2 adds state-by-year S-Y, effects, and Model 3 add covariates to Model 2. Heteroskedasticity-robust standard errors clustered by city are presented for models 2 &3. Each regression is weighted by city population in the year 2000. The horizontal axis represents event years (years before and after the first grant).

**Figure 5. Estimates of the Effects of the First Byrne Grant on Total Drug Arrest**



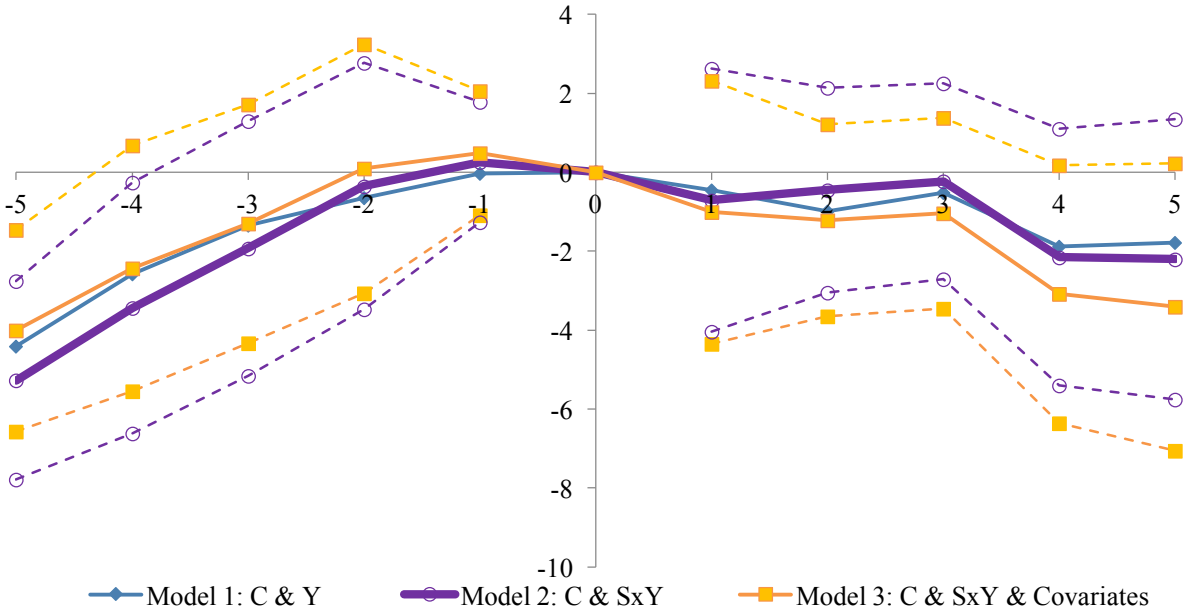

Notes for Figure 5: Crime Data comes from the Uniform Crime Report: Gender, Age, and Race Supplement. Model 1 includes City, C, and year, Y, effects, Model 2 adds state-by-year S-Y, effects, and Model 3 add covariates to Model 2. Heteroskedasticity-robust standard errors clustered by city are presented for models 2 &3. Each regression is weighted by city population in the year 2000. The horizontal axis represents event years (years before and after the first grant).

**Figure 6. Estimates of the Effects of the First Byrne Grant on White Drug Arrest**




Notes for Figure 6: Crime Data comes from the Uniform Crime Report: Gender, Age, and Race Supplement. Model 1 includes City, C, and year, Y, effects, Model 2 adds state-by-year S-Y, effects, and Model 3 add covariates to Model 2. Heteroskedasticity-robust standard errors clustered by city are presented for models 2 &3. Each regression is weighted by city population in the year 2000. The horizontal axis represents event years (years before and after the first grant).

**Figure 7. Estimates of the Effects of the First Byrne Grant on Black Drug Arrest**




Notes for Figure 7: Crime Data comes from the Uniform Crime Report: Gender, Age, and Race Supplement. Model 1 includes City, C, and year, Y, effects, Model 2 adds state-by-year S-Y, effects, and Model 3 add covariates to Model 2. Heteroskedasticity-robust standard errors clustered by city are presented for models 2 &3. Each regression is weighted by city population in the year 2000. The horizontal axis represents event years (years before and after the first grant).

**Table 1. Characteristics of Cities between 1980 and 1985**

| | All Cities ( N =407 ) | Received Grant between 1987-2000 ( N = 199 ) | Control Group ( N = 208 ) |
|---|---|---|---|
| A. Average Characteristics 1980 | | | |
| Population | 107,514 | 164,725 | 52,778 |
| Population Per Square Mile | 416 | 425 | 407 |
| Median Age | 30 | 29 | 30 |
| Median Income | 17,416 | 15,891 | 18,875 |
| Percent of the Population | | | |
| with 12 or more years of education | 69.7 | 68.0 | 71.3 |
| with female head of households | 16.3 | 18.6 | 14.1 |
| Black | 11.2 | 16.6 | 6.0 |
| B. Average Characteristics 1980-1985 | | | |
| Crime Rates (per 1,000 Residents) | | | |
| Total Crime | 77.7 | 87.8 | 67.9 |
| Personnel | | | |
| Sworn Police (per 1,000 Residents) | 1.7 | 1.9 | 1.6 |
| Drug Arrest Rates (per 1,000 Residents) | | | |
| Total | 3.5 | 3.9 | 3.2 |
| Sales | 0.7 | 0.8 | 0.6 |
| Possession | 2.9 | 3.1 | 2.7 |
| Drug Arrest Rates by Race | | | |
| Black (per 1,000 Black Residents) | 9.7 | 7.7 | 11.7 |
| White (per 1,000 White Residents) | 3.5 | 3.6 | 3.4 |
| Drug Sales Arrest Rates by Race | | | |
| Black (per 1,000 Black Residents) | 2.0 | 1.8 | 2.3 |
| White (per 1,000 White Residents) | 0.6 | 0.7 | 0.6 |
| Drug Possession Arrest Rates by Race | | | |
| Black (per 1,000 Black Residents) | 7.6 | 5.9 | 9.2 |
| White (per 1,000 White Residents) | 2.9 | 2.9 | 2.9 |

Notes for Table 1: Crime Data comes from the Uniform Crime Report: Gender, Age, and Race Supplement. The sample is restricted to cities where the population exceeds 25,000 residents at some point between 1980 and 2010. City demographic information comes from City and County Data Books.

**Table 2. The Relationship between the First Grants and 1960 City Demographics**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Year Received First Grant | | 0/1 Receive Byrne Grant | |
| Population per Square Mile | -1.596 | -1.612* | -0.0488 | -0.0445 |
| | [0.976] | [0.969] | [0.0462] | [0.0468] |
| Median Age | -1.397 | -1.520 | -0.677** | -0.630** |
| | [4.726] | [4.799] | [0.276] | [0.282] |
| Median Income | -2.673 | -3.054 | -0.134 | -0.0803 |
| | [4.272] | [4.386] | [0.217] | [0.225] |
| Log of the Proportion of Residents | | | | |
| 12 or more years of schooling | -4.229 | -4.323 | 0.526*** | 0.524** |
| | [4.419] | [4.470] | [0.202] | [0.203] |
| Female head of household | -10.52** | -10.51** | 0.687*** | 0.660*** |
| | [4.306] | [4.345] | [0.187] | [0.184] |
| Black | -0.446 | -0.444 | 0.0905*** | 0.0916*** |
| | [0.599] | [0.601] | [0.0266] | [0.0262] |
| Change in Crime Rates | | -1.868 | | 0.347** |
| | | [2.940] | | [0.150] |
| Observations | 199 | 199 | 407 | 407 |
| R-squared | 0.453 | 0.456 | 0.430 | 0.441 |

*Note:* Each column reports estimates from a separate weighted least squares regressions. The dependent variable in columns 1 & 2 is the year a city first receives a grant. The dependent variable in columns 3 & 4 is an indicator equal to 1 if a city receives a grant between 1987 and 2004. Heteroskedasticity-robust standard errors are corrected for clustering with state and presented in brackets. City demographic variables are from the 1980 Decennial Census. All columns use 1980 population as weights.

**Table 3: Difference in Difference Estimates of Byrne Grants on Drug Possession Arrests**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | *Drug Possession Arrest Per 1,000 Residents* | | | | | |
| | *Total Arrests* | | *White Arrests* | | *Black Arrests* | |
| | | Treated Only | | Treated Only | | Treated Only |
| Years -5 to -1 | -0.232 | -0.219 | -0.0677 | 0.0362 | -0.645 | -0.749 |
| | [0.226] | [0.228] | [0.203] | [0.224] | [0.669] | [0.681] |
| Years 1 to 2 | -0.0634 | -0.0779 | -0.0692 | 0.0346 | -0.537 | -0.838 |
| | [0.247] | [0.289] | [0.238] | [0.244] | [0.788] | [0.861] |
| Years 3 to 4 | -0.254 | -0.0818 | 0.0305 | 0.179 | -1.264 | -1.408 |
| | [0.323] | [0.363] | [0.320] | [0.325] | [1.026] | [1.164] |
| Years 5 to 6 | -0.429 | -0.0513 | 0.0167 | 0.307 | -2.087 | -1.756 |
| | [0.376] | [0.438] | [0.396] | [0.397] | [1.400] | [1.558] |
| Observations | 11,732 | 5,709 | 11,400 | 5,544 | 11,400 | 5,544 |
| R-squared | 0.531 | 0.627 | 0.430 | 0.523 | 0.279 | 0.511 |
| Number of Cities | 407 | 199 | 407 | 199 | 407 | 199 |

*Notes*: Table display least-squares estimates obtained from estimating Equation 2 by grouping years before and after treatment. All columns include city, C, and state-by-year, S-Y, effects. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. All columns use 2000 population as weights. Columns 1, 3, and 5 refers to the entire sample while columns 2, 4, and 6 only include cities that receive a Byrne grant between 1987 and 2004.

**Table 4: Difference in Difference Estimates of Byrne Grants on Drug Sale Arrests**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent Variable | *Drug Sales Arrest Per 1,000 Residents* | | | | | |
| | *Total Arrests* | | *White Arrests* | | *Black Arrests* | |
| | | Treated Only | | Treated Only | | Treated Only |
| Years -5 to -1 | -0.102 | -0.102 | -0.112 | -0.122 | -0.0216 | 0.227 |
| | [0.137] | [0.166] | [0.126] | [0.162] | [0.336] | [0.429] |
| Years 1 to 2 | 0.416*** | 0.416*** | 0.136 | 0.178 | 1.161 | 1.148 |
| | [0.141] | [0.150] | [0.120] | [0.111] | [0.773] | [0.703] |
| Years 3 to 4 | 0.455*** | 0.543*** | 0.317* | 0.383** | 0.130 | 0.0902 |
| | [0.170] | [0.185] | [0.166] | [0.172] | [0.505] | [0.455] |
| Years 5 to 6 | 0.329* | 0.505** | 0.527** | 0.642** | -0.0818 | 0.0294 |
| | [0.184] | [0.219] | [0.211] | [0.261] | [0.536] | [0.545] |
| Observations | 9,868 | 5,023 | 9,524 | 4,853 | 9,524 | 4,853 |
| R-squared | 0.584 | 0.661 | 0.412 | 0.488 | 0.319 | 0.483 |
| Number of Cities | 407 | 199 | 407 | 199 | 407 | 199 |

*Notes*: Table display least-squares estimates obtained from estimating Equation 2 by grouping years before and after treatment. All columns include city, C, and state-by-year, S-Y, effects. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. All columns use 2000 population as weights. Columns 1, 3, and 5 refers to the entire sample while columns 2, 4, and 6 only include cities that receive a Byrne grant between 1987 and 2004.

**Table 5: Difference in Difference Estimates of Byrne Grants on White Drug Arrests**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Dependent Variable | *White Drug Sale Arrest Per 1,000 White Residents* | | | | | | | | |
| | *1980 Crime Rates* | | *1980 % Black Population* | | *City Size* | | | *Year of First Grant* | |
| | Below Median | Above Median | Below Median | Above Median | 50K to 100K | 100K to 250K | 250K & Greater | Before 1995 | After 1994 |
| Years -5 to -1 | -0.0661 | -0.0610 | 0.00166 | -0.126 | -0.0390 | -0.0972 | -0.290 | -0.159 | 0.00631 |
| | [0.135] | [0.170] | [0.127] | [0.157] | [0.107] | [0.109] | [0.260] | [0.166] | [0.0644] |
| Years 1 to 2 | 0.161 | 0.127 | 0.282 | 0.0665 | 0.0369 | 0.0256 | 0.212 | 0.183 | 0.0526 |
| | [0.190] | [0.192] | [0.192] | [0.166] | [0.143] | [0.0594] | [0.271] | [0.161] | [0.0527] |
| Years 3 to 4 | 0.115 | 0.510 | 0.175 | 0.471* | 0.294 | 0.00967 | 0.432 | 0.398 | -0.00120 |
| | [0.177] | [0.319] | [0.198] | [0.274] | [0.282] | [0.0990] | [0.376] | [0.233] | [0.0648] |
| Years 5 to 6 | 0.00843 | 0.892** | 0.151 | 0.756** | 0.453 | 0.0175 | 0.517 | 0.630 | -0.0181 |
| | [0.170] | [0.380] | [0.159] | [0.343] | [0.305] | [0.151] | [0.428] | [0.291] | [0.0816] |
| Observations | 7,010 | 7,185 | 7,061 | 7,134 | 7,392 | 5,989 | 5,485 | 7,635 | 6,560 |
| R-squared | 0.530 | 0.372 | 0.406 | 0.425 | 0.234 | 0.387 | 0.549 | 0.440 | 0.359 |
| Number of Cities | 307 | 308 | 307 | 308 | 325 | 261 | 237 | 326 | 289 |

*Notes*: Table display least-squares estimates obtained from estimating Equation 3 by grouping years before and after treatment. The dependent variable is white drug arrest per 1,000 white residents. All columns include city, C, and state-by-year, S-Y, effects. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. All columns use 2000 population as weights. Columns 1 & 2 report estimates for treated cities with 1980 crime rates below/above the median; Columns 3 & 4 refers to treated counties with the 1980 proportion of black residents below/above the median; Columns 5 through 7 refers to treated cities with the population between 50,000 & 100,000, 100,000 & 500,000, and greater than 500,000 respectively. All columns used untreated cities as the comparison group.

**Table 6: Difference in Difference Estimates of Byrne Grants on Black Drug Arrests**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Dependent Variable | *Black Drug Sale Arrest Per 1,000 Black Residents* | | | | | | | | |
| | *1980 Crime Rates* | | *1980 % Black Population* | | | *City Size* | | *Year of First Grant* | |
| | Below Median | Above Median | Below Median | Above Median | 50K to 100K | 100K to 250K | 250K & Greater | Before 1995 | After 1994 |
| Years -5 to -1 | 0.161 | -0.643 | -0.0977 | -0.306 | 0.486 | -0.235 | -1.080* | -0.341 | 0.0148 |
| | [0.362] | [0.537] | [0.478] | [0.470] | [0.707] | [0.543] | [0.594] | [0.432] | [0.466] |
| Years 1 to 2 | 0.690 | 1.820 | 0.845* | 1.757 | 0.523 | 0.256 | 2.706 | 1.616 | -0.0407 |
| | [0.489] | [1.396] | [0.462] | [1.233] | [0.643] | [0.460] | [1.926] | [1.110] | [0.391] |
| Years 3 to 4 | 0.488 | 0.178 | 0.285 | 0.744 | 0.554 | 0.172 | 0.866 | 0.244 | -0.528 |
| | [0.403] | [0.878] | [0.540] | [0.752] | [0.845] | [0.550] | [1.254] | [0.706] | [0.615] |
| Years 5 to 6 | -0.308 | 0.369 | 0.179 | 0.547 | 0.403 | 0.0400 | 0.413 | -0.212 | -0.753 |
| | [0.524] | [0.913] | [0.638] | [0.797] | [0.872] | [0.758] | [1.209] | [0.758] | [0.750] |
| Observations | 7,010 | 7,185 | 7,061 | 7,134 | 7,392 | 5,989 | 5,485 | 7,635 | 6,560 |
| R-squared | 0.291 | 0.281 | 0.255 | 0.301 | 0.234 | 0.237 | 0.337 | 0.354 | 0.238 |
| Number of Cities | 307 | 308 | 307 | 308 | 325 | 261 | 237 | 326 | 289 |

*Notes*: Table display least-squares estimates obtained from estimating Equation 3 by grouping years before and after treatment. The dependent variable is black drug arrest per 1,000 black residents. All columns include city, C, and state-by-year, S-Y, effects. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. All columns use 2000 population as weights. Columns 1 & 2 report estimates for treated cities with 1980 crime rates below/above the median; Columns 3 & 4 refers to treated counties with the 1980 proportion of black residents below/above the median; Columns 5 through 7 refers to treated cities with the population between 50,000 & 100,000, 100,000 & 500,000, and greater than 500,000 respectively. All columns used untreated cities as the comparison group.

**Table 7: Difference in Difference Estimates of Byrne Grants on Other Crime Categories**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Dependent Variable | *Per 1,000 Residents* | | | | *Violent Crime Arrest Per 1,000 Residents* | | | |
| | *Violent Crime* | | *Total Crime* | | *White Arrests* | | *Black Arrests* | |
| | | Treated Only | | Treated Only | | Treated Only | | Treated Only |
| Years -5 to -1 | -0.233 | -0.177 | -1.588 | -2.382* | -0.218 | -0.352 | -0.0723 | -0.596 |
| | [0.339] | [0.508] | [1.296] | [1.234] | [0.152] | [0.226] | [0.671] | [0.751] |
| Years 1 to 2 | 0.0449 | -0.00585 | 0.799 | 0.993 | 0.240* | 0.328* | 0.175 | 0.182 |
| | [0.340] | [0.431] | [0.866] | [1.091] | [0.139] | [0.174] | [0.530] | [0.644] |
| Years 3 to 4 | 0.258 | 0.359 | 0.354 | 2.051 | 0.345** | 0.614** | -0.0489 | 0.0717 |
| | [0.513] | [0.699] | [1.577] | [1.874] | [0.169] | [0.246] | [0.750] | [0.886] |
| Years 5 to 6 | -0.476 | -0.362 | -0.650 | 1.587 | 0.482** | 0.841*** | -1.130 | -0.675 |
| | [0.713] | [1.028] | [1.906] | [2.375] | [0.210] | [0.321] | [0.977] | [1.130] |
| Observations | 11,633 | 5,698 | 11,786 | 5,741 | 11,554 | 5,626 | 11,554 | 5,626 |
| R-squared | 0.298 | 0.527 | 0.705 | 0.767 | 0.526 | 0.592 | 0.276 | 0.555 |
| Number of Cities | 407 | 199 | 407 | 199 | 407 | 199 | 407 | 199 |

*Notes*: Table display least-squares estimates obtained from estimating Equation 2 by grouping years before and after treatment. All columns include city, C, and state-by-year, S-Y, effects. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. All columns use 2000 population as weights. Columns 1, 3, 5 and 7 refers to the entire sample while columns 2, 4,6 and 8 only include cities that receive a Byrne grant between 1987 and 2004.

**Table 8: OLS Estimates of Byrne Grants on Drug Arrests**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | | | *Per 1,000 White Residents* | | | *Per 1,000 Black Residents* | | |
| | | | Treated Only | | | Treated Only | | | Treated Only |
| | | | | A: Drug Possession | | | | | |
| Byrne Grants per Capita (in $100) | 0.705 | 0.679 | 0.478 | 0.0121 | 0.0202 | -0.125 | 1.959* | 1.487 | 1.326 |
| (in 2000 dollars) | [0.433] | [0.434] | [0.463] | [0.370] | [0.367] | [0.429] | [1.057] | [1.122] | [1.218] |
| Observations | 11,732 | 11,732 | 5,709 | 11,400 | 11,400 | 5,544 | 11,400 | 11,400 | 5,544 |
| R-squared | 0.515 | 0.526 | 0.625 | 0.416 | 0.426 | 0.521 | 0.243 | 0.274 | 0.508 |
| Number of Cities | 407 | 407 | 199 | 407 | 407 | 199 | 407 | 407 | 199 |
| | | | | B: Drug Sales Arrest | | | | | |
| Byrne Grants per Capita (in $100) | 0.424*** | 0.405*** | 0.252** | 0.220 | 0.217 | 0.140 | 1.008** | 0.808* | 0.572 |
| (in 2000 dollars) | [0.157] | [0.152] | [0.114] | [0.185] | [0.180] | [0.173] | [0.452] | [0.427] | [0.470] |
| State-By-Year Fixed Effects | X | X | X | X | X | X | X | X | X |
| Covariates | | X | X | | X | X | | X | X |
| Observations | 9,868 | 9,868 | 5,023 | 9,524 | 9,524 | 4,853 | 9,524 | 9,524 | 4,853 |
| R-squared | 0.572 | 0.579 | 0.655 | 0.395 | 0.401 | 0.474 | 0.275 | 0.317 | 0.481 |
| Number of Cities | 407 | 407 | 199 | 407 | 407 | 199 | 407 | 407 | 199 |

*Notes*: Table display weighted least-squares estimates from Equation 4. The dependent variable is drug arrest per 1,000 residents in columns 1 through 3, white drug arrest per 1,000 white residents in columns 4 through 6, and black drug arrest per 1,000 black residents in columns 7 through 9. Column 1, 4, and 7 refers to Model 2 includes City, C, and state-by-year, S-Y, effects, column 2, 5, and 8 refers to Model 3 adds covariates, and column 3, 6, and 9 corresponds to Model 3 but limits the samples to treated cities only. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. Each regression is weighted by city population in the year 2000.