ESSAYS ON ECONOMICS OF LAW, CRIME, AND SOCIAL CAPITAL

by

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PREFACE

The 1960's was a decade of radical changes in the political and economic structure of the United States. Not only was this a time of significant economic growth but this was also accompanied with legislative changes and reform. These reforms were reflected within the War on Poverty which implemented programs geared towards health, education, opportunity, and employment. But imbedded in the War on Poverty was legislative changes that increased access to medical coverage, the voting booth, and fair treatment regardless of race and gender. These reforms aimed to strengthen families and increase access to economic and political institutions. These reforms also occurred during the same time that crime rates and divorce rates in the United States began to rise dramatically. The increase in crime and divorce rates may be coincidental or influenced by the same events. Whether or not these two are linked, many social policies over the last 50 years have been implemented to strengthen families and reduce crime. Considering the timing of these events, my dissertation investigates the links between social reform, crime, and divorce.

Crime as well as divorce, are both highly researched areas in economics. Previous studies on crime and divorce use Becker models as a baseline but empirically rely on plausibly-exogenous variation that impact divorce or crime. Most of the crime literature tries to find instrumental variables to account for the endogeneity between enforcement and crime or exogenous changes in policy to identify various changes in criminal or police behavior. The research on divorce focuses on changes in divorce laws. This research is centered on the

perceived information that individuals have when a marriage is formed or destroyed. Central to this literature are bargaining rights and how changes in bargaining rights will influence whether separation occurs.

My dissertation will focus on institutional changes and political events that provide plausibly-exogenous variation that impact crime and divorce. I use the wide variation in the grant making process to evaluate empirically the effects of the War on Poverty's legal service program. The first two chapters of my dissertation examine the impact of federally funded legal services under the War of Poverty on the likelihood of divorce and on crime. The third chapter explores how elections influence incumbent's mayor's response to crime and staffing of the police department.

The War on Poverty directly and indirectly impacted both crime and divorce. Research has been done on the War on Poverty as it relates to housing, health, and education. However, there has not been any research on how legal services affected community responsiveness to crime. Also, there has been no research on how subsidizing legal assistance reduces the cost of divorce. Chapter 1 & 2 use federally funded legal services under the War of Poverty to examine the effects subsiding legal assistance and their effect on building social capital as it relates to crime reporting and divorce. Lastly, to address the question on whether police reduce crime, Chapter 3 uses election cycles as a plausible exogenous shock in police hiring. By expanding the sample size I can test the robustness of past research on enforcement and crime.

Chapter 1 finds that Legal Services are associated with higher the property values in 1980. Central to this finding is legal service role in resolving disputes that resulted in riots. By

changing police behavior and community involvement, crime reporting rose dramatically in places that received legal services. The increase in reporting is accompanied with large increases in arrest and more police presence. Chapter 2 shows that increased access to lawyers resulted in higher divorce rates in the 1960s and 1970s. Counties that received legal service are associated with a 6 percent increase in divorces shortly after the program arrives. Chapter 3 finds that elections influence the size of the police department. Results show that mayoral election years are associated with a 2 percent increase in the number of police officers available. Secondly, result show the elasticity of police on violent crime is -0.74 and as large as -1.88 for cities with the population greater than 150,000 residents.

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MORE LAWYERS, MORE CRIME? AN EVALUATION OF THE OEO LEGAL SERVICE PROGRAM

> "*Legal Services Lawyers have won the confidence of angry young men and women and have channeled their grievances into democratic procedures. This capability and achievement mark a major victory for those concerned with maintaining law and order.*"
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> > – From the Office of Economic Opportunity, November 1969 Senate Hearing
>
> "*You can carry a machete through the streets of Newark and not get locked up*"
>
> > – Mr. Kowalewski, New York Times 1967

After decades of decline, reported crime in the United States began to rise in the early 1960s. The rise in violent crime, especially homicide, along with widespread urban riots pushed crime to the forefront of political debates (Loo & Grimes 2004). Concern over the decline of urban communities and eruptions of urban violence gave rise to discussion of policies and programs to address these issues. This dialogue sparked an effort for urban revitalization and efforts to alleviate poverty that resulted in various experimental programs implemented during the 1960s and 1970s.

The Neighborhood Legal Service Program (LSP) was established in 1965 to catalyze change in social and political interactions between the poor and the rest of American society.

The program was a public provision to provide subsidized legal services to the poor. This new model for providing legal aid developed and operated law firms within the communities where the poor were concentrated.[1] Unlike previous legal aid devoted for the poor, these neighborhood law firms were located in close proximity to the population it was intended to serve. The program provided these poor populations lawyers to handle civil cases dealing with housing, contracts, employment, welfare legislation and family disputes as well as assistance with criminal matters.

The goal of the LSP was to protect the legal rights of the poor. By subsidizing the cost of legal consultation for impoverished clients, the idea was to provide the poor with legal channels to remediate grievances, especially those resulting in riots (Gillette 1996).[2] Historically, the poor had limited access to legal institutions due to financial constraints and discrimination. The lack of legal recourse often resulted in the poor holding demonstrations to dissent against perceived injustices. These demonstrations during the 1960s often escalated into riots.

A mandate of the LSP was to facilitate better relationships between the poor and bureaucracies that interacted with the poor. Advocates of the program argued that LSP lawyers greatly improved the quality of life for the poor and reduced the likelihood of riots. In their view this was accomplished by changing how private businesses and public institutions interacted with the poor.[3] For example, advocates claim that LSP lawyers were influential in reducing police brutality, improving police response time, and securing the actual filing of police reports (Finman 1971).

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[1] Historically this was not the case, as legal services that may have been provided for poor by the National Legal Aid and Defender Association (NLADA) and other local bar affiliated organizations were located in city centers.
[2] It took two years for federally funded legal services to be fully operational due to opposition from local bar associations. The American Bar Association pledge full cooperation on February 8, 1965.
[3] The threat of litigation resulted in local government department and agencies consulting Legal Service lawyers before implementing new rules, procedures, or eligibility standards to avoid litigation or future problems. See Hollingsworth (1977)

However, the program met with great criticism. Opponents of the program criticized attorneys for inciting riots and emboldening criminals by providing them with legal counsel. Fifty years later, little is known about the actual impact of the program. This is in part due to lack of data on the users of the LSP, and to a greater extant, the lack of convincing measures of legal services themselves.

This chapter is the first to quantitatively evaluate the impact of the LSP on the quality of life of the poor. I use newly collected data on the communities receiving legal service grants between 1965 and 1975, and I focus on crime as a measurement of quality of life for several reasons. First, crime captures actions that negatively affect individual welfare, including threats to individual safety and personal property. Second, crime is one of the few measures of well-being consistently recorded over time at the city level for my period of interest. Lastly, crime was an outcome linked to the LSP by advocates and opponents as mentioned above. In addition to crime rates, I provide evidence of the impact of LSP on other measures of welfare, such as property values and community relations with local police.

My research design takes advantage of the differential timing of the LSP implementation in cities across the United States and uses a before and after design to analyze changes in outcomes after the establishment of the LSP. I use an event-study framework (Jacobson *et al.* 1993) which provides a statistical description of the evolution of pre-trends in outcomes as well as the dynamics of changes after the program began.

My results show that there is a short run *increase* in criminal offenses reported and offenses cleared by arrest after LSP grants are received. Cities that receive LSP grants are associated with a 7 percent increase in the number of crimes reported and 13 percent increase in

offenses cleared by arrest 3 years after a grant is received. After 4 years, reported crime and arrests decrease and evolve similarly to untreated cities.

These findings may reflect two different phenomena: an increase in actual crime (consistent with critics and the second quote above) or an increase in reported crime (consistent with advocates and first quote above). Although it is difficult to disentangle changes in crime versus changes in reporting, the event-study specification provides insight into the evolution of crime after legal services was established. The intertemporal response of crime and arrest after LSP is implemented displays an immediate increase in reported crime and arrest followed by a large decrease in reported crime. This hump-shape response is consistent with an increase in reporting followed by a decrease in actual crime. This is similar to Levitt's (1998) emphasis on changes in reporting behavior due to changes in likelihood that a crime will be solved. Second, there is an immediate increase in the staffing of police officers in cities that received federally funded legal services which has been shown to negatively relate with crime (Levitt 1996, 2002, McCrary 2002, 2013). Third, consistent with changes in police effort, I find that the increase in arrests is *twice as large* as the increase in reported crime (Mas 2006).

Additional evidence is also consistent with legal service programs increasing social capital and improving welfare. Using the 1970 survey of legal service programs, I show that cities that received LSP earlier report having more positive interactions between the poor and law enforcement agencies in 1970. Also, I provide evidence that cities receiving LSP had higher property values by the 1980 census. This final piece of evidence is consistent with LSP mitigating the rise in crime that reduced the quality of life inner city neighborhoods and contributing to improvements in the well-being of the poor.

I. Brief History Of Legal Services Under The War On Poverty

A. Empowerment of the Poor through Legal Services

The Federal Legal Service Program was motivated by an influential journal article by Jean and Edgar Cahn, which called for the "*civilian perspective*" to be incorporated in the War on Poverty (Cahn & Cahn, 1964). The Cahns' proposal was concerned with the monopoly power that could be generated by a large bureaucracy. The concluded the only way to protect the true interest of the poor was to provide them with legal representation. Giving the poor the ability to criticize, dissent, and compel responsiveness of local institutions would allow the poor to participate in helping themselves.

The Cahns' proposed that university-affiliated, neighborhood law firms be established to serve as intermediaries between the community and those administering social programs. The law firms would provide professionals to aid in developing and stimulating leadership through opportunity, orientation, and training. Each law firm would be staffed with lawyers, research assistants, and investigators with the goal of making public officials and private businesses more responsive to the needs of the poor. Legal representation would be available for divorce, eviction, welfare fraud, police brutality, installment buying, and destroying the momentum of a "militant community effort."[4] The Cahns proposed that neighborhood law firms provide legal advocacy and legal analyses in four arenas: traditional legal assistance, law reform, law advocacy, and community outreach.

B. The Expansion of Federally-Funded Legal Services

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[4] The deterrence of "militant community effort" refers to the availability of a lawyer to provide avenues for differences between the poor and various entities to use the political and judicial establishments to solve problems peacefully. Proponents of Federally Funded Legal Services often boast of their success with ending or resolving differences that resulted in riots.

Largely in response to the Cahn's ideas, the Office of Economic Opportunity (OEO) launched the Neighborhood Legal Service Program as part of the Community Action Program (CAP). Federal legal service grants went directly to the community organizations and excluded local and state authorities, allowing federal funds to be spent rapidly. The OEO delegated the choice of whether a local legal aid organization would run a Legal Service Project to local Community Action Agencies (CAA). [5] Implicitly, location and timing of these grants also were dependent on local political pressure and support from the local bar association. Often, differences between the CAA and local bar generated confusion about where and when LSPs were established. Once the National Bar Association fully backed the LSP there was great effort to fund as many places as possible. [6] This process, in which local bars and community action agencies influenced funding, resulted in wide variation in the timing of the establishment of the LSP in various cities (Johnson, 1974).

The first year of legal services under OEO resulted in over 155 grants being issued. The annual budget during the first year was 20 million, an amount that steadily increased each fiscal year. In the second year, the budget for federally funded legal services was double the budget of the legal aid societies affiliated with the National Legal Aid Defender Association. In 1967, the legal service program doubled in size issuing over 300 grants with a budget of over 40 million dollars. By the end of 1967, the federal legal service program was funding 250 projects and providing legal assistance in 48 states.

To gain a better understanding of how legal service funds were utilized, during 1968 fiscal year a total of 282,000 cases were accepted. Cases involving family problems – i.e.

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[5] The OEO was responsible for the antipoverty programs and one of the largest initiatives was the Community Action Programs (CAP). Community Action Programs are the bread and butter of the Anti-poverty movement.
[6] It took two years for federally funded legal services to be fully operational due to opposition from local bar associations. The American Bar Association pledged full cooperation on February 8, 1965.

divorce, nonsupport, and paternity – represented nearly 40 percent of the cases. Criminal and juvenile cases were responsible for over 18 percent of the cases.[7] Administrative cases, which include cases that challenge laws and policies for welfare recipients and low skilled workers, accounted for only 7 percent of the total cases but were very effective. A single administrative case potentially affected thousands of residents in a city or state. These cases usually involved challenging governmental agencies such as state and local welfare, social security, workman's compensation, and unemployment insurance. It must also be noted that neighborhood law firms could have had many indirect effects. The availability of legal assistance may result in changes in business practice, educational disciplinary responses, and police policies even without litigation or long after litigation is resolved.

C. Description of Federally Funded Legal Services from Legal Service Agency Survey

The Legal Service Agency Survey (LSAS) provides further information about the typical LSP.[8] The LSAS contains geographical information such as state, region, and county of grant recipient. Table 1.1 shows that the typical LSP in 1970 was staffed with five attorneys, one R.H. Smith Fellow, six clerical support personnel, and three law students.[9] These workers expected to serve over 1900 participants with a budget of slightly above $200,000. The majority of participants had income below the poverty line, but those with income above the poverty line were served as well. According to table 1.2, a typical LSP generally operated in a legal climate where law enforcement was perceived to have a hostile relationship with the community and

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[7]See Levitan (1969) for more information on the utilization of legal service grants.

[8] The Legal Service Agency Survey was conducted in 1970 and 1971 to evaluate the effectiveness of the Legal Service Projects (LSP) and to make recommendations on whether to expand, cut back, or eliminate some agencies. The survey included roughly 2/3 of all the LSP project operating at that time.

[9] R.H. Smith Fellowship is a prestigious fellowship to attract highly qualified attorneys to practice in legal aid for the poor.

minorities. However, according to survey responders, legal services were viewed favorably in the community and viewed as having competent staff and relevant activities.

D. Federally-Funded Legal Services and Crime

The OEO funded neighborhood law firms were responsible for providing legal assistance in civil cases for individuals who were unable to afford private attorneys. Legal representation in criminal matters was provided by the state, however, these services were not always deemed as high quality. [10] Legal services lawyers provided some form of legal assistance for alleged criminals when lawyers deemed that the state could not adequately represent clients with compelling cases. Also, LSP lawyers were often called upon to provide pamphlets or information concerning citizen's legal rights when interacting with the police. If LSP lawyers were successful at representing alleged criminals or improving the "quality" of criminals, crime could *increase* due to the lack of arrests and convictions. [11]

Another mandate of the legal service program was to build community relationships with economic institutions such as the police department. Within this mandate to LSP, public institutions were to be held responsible for services rendered on behalf of the poor. Conceptually, the availability of legal aid would serve as a deterrent for unfair or unjust treatment by police authorities (i.e., police brutality or not taking reports).[12] If police services were underutilized by the poor due to social and political structure or resources, LSP was supposed to correct the market inefficiency.

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[10] Gideon v Wainwright (1963) ruled that state courts are required to provide an attorney for criminal defendants who were unable to afford an attorney from a private law firm.

[11] This was often a discretionary decision with local political consequences. Lawyers were compelled to represent clients when they felt the state would not adequately provide a proper defense for someone the lawyer deemed innocent or wronged by the police.

[12] During a 1969 Senate hearing, advocates of LSP boast on the effectiveness of legal service lawyers to intervene in riots, decrease police brutality, reduce illegal police conduct, and increase the relationship between the police and the poor community.

The examination of the victimization reports and reported crime records reveals a distinct difference between the actual and observed crime rate. Boggess and Bound (1999) summarize differences between Victimization Reports and the Uniform Crime Report and surmises that reporting plays a large role in the discrepancy. According to Boggess and Bound, the large difference reflects reporting behavior of victims and witnesses as well as reporting behavior of the police. Myers (1980) finds that the actual crime rates between 1970 and 1974 were 1.5 to 3 times larger than reports. There are several reasons why this difference between actual and reported crime occurs. First, the pecuniary gains from reporting are likely small if the possibility of recovery is near zero. Also, the victim or observer of a crime is not likely to report if the criminal is of close relation or retaliation is possible. Additionally, political pressure to keep crime rates down, social economic status of victim, conviction rates, and various other reasons cause crime to go unreported.[13] If some criminal offenses are under-reported due to lack of institutional responsibility or the victim perception of lack of institutional responsibility, legal services could work to *increase* the number of crimes reported and the number offenses cleared by arrest.[14]

According to the mechanisms outlined above, the introduction of the LSP has two possible implications within a Becker type crime model. For potential criminals (supply-side), LSP increases the quality of criminals and as a result, decreases the marginal cost of committing a crime. Consequently, this will decrease the number of crimes cleared by arrest, increase the number of crimes committed, and increase the number of crimes reported. For non-criminals

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[13] Sieidman and Couzens (1974) discuss how the production of crime rates depends on the victim perspective, the police perspective, and also political pressure. Unobserved differences in any of these three areas can cause crime to be drastically under-reported across cities and vary over time. Also see Black (1970).
[14] Legal services lawyers have been seen as improving relationships between the community and the police. Many encounters of how the LSP have influenced behavior of local institutions are documented in congress subcommittee meetings between 1965 to 1974.

(demand for crime prevention), the LSP could improve police-community relationships which increases the reporting of crime. Better policing and more reporting would also lead to an increase in arrests which serves as a deterrent for committing future crimes.

Combining the two offsetting effects, the prediction that follows is that LSP could *increase* or *decrease* crime *after* legal service grants were received. Furthermore, both of these changes in crime would result in an *increase* in *reported* crime. However, what is important from a social capital perspective is whether actual crime increased or decreased due to the establishment of LSP. My analysis uses property values to shed light on this.

II. Data On Legal Service Grants and Crime

A. *Data on Federal Legal Service Grants*

Data on the recipients of federal legal service grants funded by the OEO were compiled from the National Archives Community Action Program (NACAP) files. NACAP provides information on the city, county, and state for which the funds were received and the targeted communities. Also, provided is the date the grant was issued, the amount of the grant, and a brief description for the intended purpose of the grant. I use this information to match legal service grants to city level observations on crime and I use the date of the first grant to identify when the legal services program started.[15]

B. *Uniform Crime Reporting from 1960 to 1985*

The Uniform Crime Reporting: Offenses Known and Clearance by Arrest (UCR) database provides information on offenses submitted by law enforcement agencies to the Federal Bureau of Investigation (FBI). The data on crime includes monthly information on the number of unfounded offenses, actual offenses, offenses cleared by arrest, and offenses cleared involving

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[15] NACAP files does not provide information for grants received in 1969. Data from Federal Outlays are used to supplement CAP data to provide grants in 1969.

individuals under the age of 18. The following offenses of interest are recorded in this database: murder and manslaughter, rape, robbery, assault, burglary, larceny, and motor vehicle thefts. Also recorded is the number of offenses cleared by arrest for each of these criminal offenses.

The UCR also provides counts for more detailed descriptions of each of the offenses listed. The number of actual offenses includes reports of crime received from victims and police who discover possible infractions. Unfounded offenses are reports which are concluded to be baseless or false. The number of offenses in a month is provided by city, county, and state agencies. The data compiled for the UCR is submitted voluntarily by city, county, and state enforcement agencies.[16] The FBI or state law agencies directly provide survey forms to local agencies and are collected on a monthly basis.

C. Data on Demographic Characteristics

City characteristics in this analysis are taken from the 1960, 1970, 1980, and 1990 Census City and County Books. The city level demographic information is constructed by linearly interpolating between the 1960, 1970, 1980, and 1990 census. To calculate the proportion of the population that are males between the ages of 15 to 24 and 25 to 39 for each city, I interpolated 1960 census county age profile to 1968 and used annual county age profiles from the Surveillance, Epidemiology, and End Results (SEER) from 1968-1985.

D. Analysis Sample

The analysis consists of city level observations with crime statistics and census demographic information from 1960 to 1985 for 606 cities.[17] Figure 1.1 is of map of cities that

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[16] Reporting in the UCR is major concern. A local agency may report or may be "covered by" a larger county or state agency. This can lead to double counting of offenses or population in the UCR.
[17] The fully reporting city sample only contains city observations that have demographic information for 1960, 1970, 1980, and 1990 census files.

received federal legal services grants over this sample period. All cities in this sample have a population of over 25,000 residents in every year. Table 1.3 reports summary statistics for these cities from the 1960 census. The average population in the sample is 98,515 residents with the median income of $5,871 dollars.[18] The final sample contains 208 cities that received legal service grants (treatment group) and 398 non grant cities (comparison group). Cities that received legal service grants have a larger proportion of residents who are non-white and also have lower median incomes. Unfunded cities have a more educated population but spend less of total expenditures on education. However, cities that received grants are similar to unfunded cities with regard to the proportion of residents who are men and between the ages of 15 to 24 and 25 to 39, a key determinant of crime (Blumstein and Rosenfeld, 2008).

III. Event-Study Methodology

The empirical strategy will take advantage of the variation in the location of LSPs. Although there are key cross-sectional differences between funded and unfunded cities, the identification strategy is dependent on how crime evolves before the establishment of the legal service program. Table 1.4 reports summary statistics for the log of criminal offenses available in the UCR between 1960 and 1964.[19] According to the table, cities that received legal services grants have much higher average crime rates before 1965. This is not surprising as demographic characteristics from table 1.3 are known predictors of crime. Important for my research design, however, is that crime *evolved* similarly in *treated* and *untreated* cities prior to 1965. This is consistent with changes in the percentage of the population in high crime age groups evolving *similarly* over time in treated and untreated places. My analysis will account for the cross-

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[18] Legal Service programs were located in larger cities, however, 103 of the 208 cities that received LSP grants had a population less than 100,000 residents in 1960 and 45 cities had a population less than 50,000. Every city with a population greater than 500,000 residents in 1960 received a legal service grant.
[19] Summary statistics are the average over 1960 to 1964 of offenses reported.

sectional differences by using city fixed effects to capture differences in cities that are unobservable but are constant over time. Untreated cities in this analysis will help estimate for how crime is *evolving* over time and provide a comparison group for how crime is expected to *evolve* after treatment. The untreated cities in this sample provides a plausible comparison group if demographic characteristic as well as city and year fixed effects capture the difference in how crime *evolves* in treated cities versus untreated cities before the establishment of legal service programs. A test of this assumption is embedded within the difference-in-difference approached used in this analysis. If crime evolves similarly in treated and untreated cities before the establishment legal service program, my analysis will capture any *trend break* in crime due to the introduction of legal services.

The empirical strategy will also take advantage of the variation in the timing of the establishment of LSPs. The key identifying assumption is that the timing of the establishment of LSPs is uncorrelated with other determinants of *changes* in crime. The first test of this assumption is a regression of 1960 demographic characteristics that are determinants of crime on the year LSPs was established. The LSP was also supposed to be affiliated with university law programs. So ex ante, one would predict that legal services would be in cities that have law schools.

Table 1.5 reports weighted and unweighted estimates from ordinary least squares (OLS) regressions for the year grants were received. I use an indicator variable equal to one if a city is located in a county that has a law school and another indicator variable for if the county has a medical school. The medical school indicator captures the effect of a university versus a stand-alone law school. This can distinguish whether timing of grants are affiliated with law schools or large universities. Columns 1 report estimates from an unweighted OLS regression and

column 2 are from a weighted OLS regression (weighted by 1960 county population).[20] In both columns, having a medical school or law school is associated with receiving legal services earlier. However, having a medical school in the county is not statistically significant. Law schools are weakly statistically significant in columns 2. According to table 1.5, demographic characteristics fail to predict when a city first received a grant.

A second test of the identifying assumption is to compare the timing of the LSP with the pre-program reported crime rates and pre-program growth in reported crime. Figure 1.2 plots the changes in log of total crime from 1960 to 1964 and the reported crime in 1964 against the year of LSP establishment. Both figures show that timing of the LSP is uncorrelated with reported crime or changes in reported crime in the pre-period.[21] These two tests provide statistical evidence that the timing of establishment of LSPs were not determined by pre-period crime rates or predictors of crime.

Legal service programs were rolled out over an 11 year interval.[22] I use the variation in the timing and location of funding within an event-study framework to test for causality. The event study framework lends itself well for testing the effects of an outcome before and after exposure to the treatment and provides another falsification test of how crime is evolving before treatment.[23] The pre-treatment effects test whether changes in the outcomes occur before the implementation of treatment. I estimate the effects of federally funded legal services using the following linear regression:

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

[21] The slope in panel A is -0.011 (0.0073) and panel B -0.0249 (.02082). The slope for panel A & B are from univariate regressions of the crime on the year LSPs were established.

[22] Legal Services operated under the OEO until October of 1974 when it became the Legal Service Corporation.

[23] For other papers using event study framework see Bailey (2012), Jacobson *et al* (1993), Kline (2010), and McCrary (2007).

(1.1) $\quad Y_{i,t} = \gamma_i + \alpha_{t,s(i)} + \sum_{\tau=1}^{q} \pi_{-\tau} D_i 1(t - T^* = -\tau) + \sum_{\tau=1}^{p} \delta_\tau D_i 1(t - T^* = \tau) + X'_{i,t}\beta + \varepsilon_{i,t}$

where $Y_{i,t}$ is the annual log number of offenses per 100,000 residents in city i in year t *(t= 1960, 1961,…1985)*; γ_i is a set of city effects which control for unobservable city characteristics that are time invariant; α_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 such as the 1972 Supreme Court ruling capital punishment cruel and unusual. 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.

The row vector $X_{i,t}$, consists of covariates from the 1960, 1970, 1980, and 1990 census, which I have linearly interpolated between census years.[24] The covariates are proportion of the population non-white, the proportion male and between 15 to 24 years of age, the proportion male and between the age of 25 to 39, percentage of population with more than 12 years of education, and family median income. Because using the treatment may actually have an effect on the controls, I estimate the regression with and without the covariates from the census for robustness of the specification. D_i is an indicator variable equal to one if the city ever received federally funded legal services. $1(t - T^* = -\tau)$ is an indicator variable equal to one if the observation year is $-\tau$ years from the date that the legal services grant is received or $1(t - T^* = \tau)$ is equal to one if the observation year is τ years after the date legal services were first available. $1(t - T^* = 0)$ is omitted due to collinearity where T^* is the funding year for the legal services grant; q refers to the number of lags or years before legal services are funded, and p is the lead or years after legal services are funded. To ensure the coefficients are well estimated, event time for $\tau > 10$ and $\tau < -5$ are grouped into endpoints, $q = 6$ and $p = 11$. The endpoint

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[24] Census information is gathered in the County and City Data Book and is publicly available at the ICPSR website.

coefficients are not estimated using a balanced sample of cities and will also 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 sample, cities receive legal services grants between 1965 and 1975. A balanced event panel using UCR data on criminal offenses from 1960 to 1985 will focus on five years before and ten years after federally funded legal services are received. The coefficients of interest are $\pi_{-\tau}$, which are pre-treatment effects, and post-treatment effects δ_{τ}. These estimates describe the dynamics of reported crime in funded cities before and after legal services grants are received. If the econometric model captures the pre-legal services evolution of the dependent variable, the pre-treatment effects should be indistinguishable from zero. The treatment effect, δ_{τ}, is the average change in the difference in criminal offenses τ years after the city received the grant.

IV. Results

A. *Using the Timing of First Grants to Identify Impact*

Using the estimates from equation 1.1, I plot pre-treatment effects and post-treatment effects from a balanced panel. Figure 1.3 plots the estimates from three different specifications of equation 1.1. 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 also includes city and state-by-year effects with additional city characteristics interpolated from 1960 to 1985 using the 1960, 1970, 1980, and 1990 census data and is plotted with square markers. I present 95-percent confidence intervals for model 2 and 3 by dashed lines. The confidence intervals are constructed from heteroskedasticity robust standard errors clustered by city. The sample consists of cities with population greater than

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25,000 residents in every year but excludes New York City, Los Angeles, and Chicago. Presented are estimates where the natural log of crime is the dependent variable $Y_{i,t}$ for equation 1.1. All regressions are estimated using 1985 population as weights to correct heteroskedasticy related to city size in the error term.[25]

Figure 1.3 plots pre-treatment and post-treatment effects for federal legal services grants on log of total criminal offenses reported per 100,000 residents. Total crime is an unweighted aggregate of property crime and violent crime. Property crime includes burglary, larceny, and motor vehicle theft, whereas violent crime aggregates include murder, manslaughter, rape, assault, and robbery. According to all three models, the point estimates for $\pi_{-\tau}$ are near zero or slightly less than zero but statistically insignificant. After the first year of operations, changes in offenses reported are positive and statistically significant. The log of actual offenses reported increases drastically over the next 3 years and is statistically significant. Using model 3 estimates, total crime per 100,000 residents increases 7 percent 3 years after treatment. Three and four years after treatment, total crime reported begins to decrease and eventually becomes indistinguishable from zero. The results are consistent with buildup of services within a community. After the grant is received, neighborhood legal services will have to hire staff, build community support and rapport, while also accumulating exposure.

Estimates in figure 1.3 are produced using weighted least squares and exclude New York, Chicago, and Los Angeles. These cities are excluded to avoid giving them enormous weight. Also these cities lack a proper comparison within the treated or control samples, which is important because identification is dependent on both timing and location. Population weights are used to gain efficiency when error term has heteroskedasticity related to city size. However,

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[25] Weighted least squares is used to make error term homoscedastic. New York, Chicago, and Los Angeles are removed to avoid giving these cities enormous weight in addition to having cities to compare them with. The issue of weighting and very large cities will further be discuss in table 1.6 and 1.7.

weighted least squares (WLS) often lead to estimates that are less efficient than ordinary least squares (OLS) estimates (Solon, Haider, and Wooldridge, 2013). Tables 1.6 and 1.7 report treatment effects, standard errors, and t-ratios for equation 1.1 using WLS and OLS regressions with New York, Chicago, and Los Angeles included in the sample. Column 1 and 2 report weighted least squares estimates and columns 3 and 4 report ordinary least square estimates. Column 2 and 4 adds New York, Chicago, and Los Angeles to the sample. With regards to the pre-treatment effect in table 1.6, the weighted least square produces estimates that are smaller in absolute value and have smaller t ratios. This is consistent with WLS procedure capturing the pre-period trend in crime. According to table 1.7, post-treatment effects for WLS regression are larger than OLS estimates and have large t-ratios. For weighed least square estimates, the sample that excludes New York, Chicago, and Los Angeles produce larger post-treatment effects and more efficient estimates. For the remainder of this analysis, the sample will exclude New York, Chicago, and Los Angeles and use weighted least squares.[26]

B. Interpretation of the Impact of Federally Funded Legal Services

The availability of legal services is associated with a large increase in crime as predicted under the context of Becker's Crime Model. The hump shape response indicates that there is an increase in crime followed by a decrease in crime after legal services were available. However, the hump shape response does not distinguish between changes in actual crime or reported crime. On one hand, this result is consistent with LSP increasing the demand for police service and reducing the demand for crime. Advocates of the legal services program boasted about its effectiveness in reducing police brutality, minimizing or stopping riots, increasing victim response time, and ensuring filing of police reports. Improvements in these areas should enhance

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[26] Estimates including New York, Chicago, and Los Angeles as well as OLS regression are available upon request.

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the relationship between the poor and police. Ensuring police filed reports alone will increase the reported crime rate without changes in criminal behavior.

By protecting the poor's legal rights to adequate law enforcement protection and services, legal services should increase the likelihood that a crime is reported and also increase the likelihood that the report would be investigated. In general, these effects will not only lead to more reporting but also increase the probability of arrest given a criminal offense occurred. While the intertemporal response of reported crime provides evidence of changes in reporting behavior, I cannot dismiss the possibility of changes in criminal behavior. In part, the hump shape response is also consistent with an increase in actual crime followed by a decrease in crime. According to opponents, LSP lawyer decrease the probability of arrest and conviction. The combination of these effects could embolden criminals and result in more crimes being committed. Although not articulated by opponents, the decrease in crime could be a result of increased effort to clear offenses by arrest by the police. Despite the conflicting view points, it is clear that the establishment of legal services impacted the crime rate in treated cities.

If legal services lawyers are effectively advocating for the poor's right to better police protection, the impact could be displayed in two ways. First, police can change their behavior in response to legal services. An increase in the number of offenses cleared by arrest is synonymous with an increase in police effort.[27] Second, city officials could respond to the increase in the demand for law enforcement services by increasing the number of officers available. Dramatic increases in the number of offenses cleared by arrest give credence to changes in effort by local law enforcement. Contrarily, increasing the size of the police

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[27] McCrary (2007) and Mas (2006) use similar arguments for using offenses cleared by arrest as a measurement of job performance for police officers.

department is a local government response that is a long term investment. In part, additional police officers require additional public funds and additional training.

Figure 1.4 plots pre- and post-treatment effects for log of arrest per 100,000 residents. [28] I have valid arrest data from 1963 to 1985; therefore, I only have one year for the pre-treatment. As with reported crime, pre-treatment effects are zero and post-treatment effects are positive and statistically significant. Worth noting is the magnitude of arrest compared to reported crimes. In figure 1.3 total criminal offenses reported increased by 7 percent after 3 years and then by 5 years after treatment the effects were not distinguishable from zero. Here, total arrests increased by 16 percent after three years and the post-treatment effects remain high for the next couple of years before declining to zero. In this case, the response to legal services and the increase in reported crime resulted in a dramatic increase in arrests in treated cities. The percentage increase in arrests after legal services are available is double the percentage increase in crimes reported.

Another measure of police intensity is changes in the number of sworn police officers in a department. Crime reporting and arrest both are measures that depend on the commitment of the community and current law enforcement staff to fight crime. However, hiring of staff are long-term investment made by local and state officials. Figure 1.5 plots pre- and post-treatment effects for the log of sworn police officers per 100,000 residents. Before legal services are available the pre-treatment effects are zero. After legal services become available the log of sworn police officers increases over the next 10 years relative to untreated cities.

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[28] Mas (2006) also used clearance rates as a proxy for police performance. Here, by protecting the rights of the poor, should also be reflected in more effort and energy to doing better police work. Using clearance rates serves as a proxy.

The post-treatment effects, clearly shows a large immediate increase in the log of sworn police officers after legal services are established.[29] An increase of 2.2 percent in the first year is the largest increase over the next ten years. The increase in sworn police officers in the first year is an average increase of 8 additional police officers in treated cities. Using estimates of police elasticities from research on crime and police from Levitt (1996, 2002) and McCrary (2002, 2013), this imply that violent crime would be reduced by 6 to 14 crimes per 100,000 residents because of additional police officers. The reduction in property crime would be between 25 and 94 crimes per 100,000 residents.

C. *Event-study results by UCR crime category*

Figure 1.6 plots pre-treatment effects and post-treatment effects from model 3 for the effect of legal services on property and violent crimes reported. Results for property crime show that five years before federal legal services grants, funded cities are indistinguishable from unfunded cities. According to Model 3, the point estimates for $\pi_{-\tau}$ are zero or slightly less than zero but statistically insignificant. Three years after federally funded legal services are implemented, the number of crimes reported increased on average by 93 property crimes per 100,000. Three years after a city received a legal services grant, property crimes continued to grow at a steady pace. Similar to property crimes, the pre-treatment effects for reported violent crimes are not statistically significant. After the first year of operations, changes in violent offenses reported are positive and increasing over the next 3 years.

Figure 1.7 plots pre-treatment effects and post-treatment effects for log of criminal offenses reported per 100,000 residents for sub categories of property and violent crimes. Panel A displays the result for the log of murder per 100,000 residents. The estimates show a steady

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[29] Police employment data from the Annual Survey of Government provide results similar to the estimates displayed in figure 1.5.

increase in the log of murders reported before and after treatment but the estimates never becomes statistically significant nor does it display hump-shape response. After legal services begin, the log of rapes per 100,000 residents and the log of robberies per 100,000 residents reported are increasing and statistically significantly. Panel C and D plot treatment effects for assault, burglary, larceny, and motor vehicle theft. Pre-treatment effects for larceny are positive and statistically different from zero. Post-treatment effects are positive and statistically different from zero after a few years. Motor vehicle theft and burglaries post-treatments are positive, statistically different from zero, and follow the hump shape response of total crimes.

V. Discussion

A. *Legal Services Results and Implications*

Property and violent crimes, as well as total crime, responded in a similar fashion to the establishment of legal services. The post-treatment effects of murder and manslaughter are not statistically significant which support the notion that legal services increased reported crime and not actual crime. Murder and manslaughter are reported fairly accurately and proxies for changes in actual crime. The zero post-treatment effects for murder and manslaughter show that LSP was unlikely to increase actual crime. Also, crimes that are typically under-reported, such as rape, display the largest increase. This is consistent with an increase in reporting in places where legal services are established.

The notion that crime is decreasing while reporting is increasing is not unique in the crime literature.[30] Boggess and Bound (1997) showed that reported crime increased in the UCR

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[30] Levitt (1998) makes a similar argument about the effect of police on the reporting of crime. Levitt argues that an increase in police officers increases the likelihood that a crime is reported and reduces that amount of actual crime that occurs.

in the 1980's while crime decreased according to the National Crime Survey.[31] They concluded that overall criminal activity by virtue of the fact that the murder rate declined over the sample period, while reporting over the sample period increased. Consistent with their story, my results indicate that the impact of LSP on murder and manslaughter are statistically insignificant although the post-treatment estimates are positive.

However, other criminal categories that are reported somewhat accurately indicate that crime may actually be increasing due to legal services. Two categories that are likely to be reported somewhat accurately are robberies and motor vehicle theft (Bogges and Bound, 1997; Levitt, 1998). According to figure 1.7, the post-treatment effects are positive and statistically significant. If LSP are improving reporting of crimes, it is reasonable to assume that murders and motor vehicle theft would be unaffected. However, I find large effects for motor vehicle theft and positive effect on murder. It is quite possible that legal services are associated with increases in actual crime or associated with events that increased crime.

B. Using Rioting to Distinguish Changes in Reported Crime

It is reasonable to be concerned that the significant increase in reported crime after exposure to federally funded legal services is a direct consequence of riots that occur during this time period.[32] The inclusion of year fixed effects will capture national events that increase crime across cities. However, all riots are not triggered by national events. Therefore, year fixed effects and state by year fixed effects will not capture changes in local sentiments that may result in riots. If the increase in reported crime is purely a consequence of rioting, then legal services

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[31] National Crime Survey was implemented in 1972 to collect data on victimization. The data is administered by the Bureau of Justice Statistics and available at the ICPSR. The NCS provides information about crimes reported and not reported to the police as well as provide information about the victim and the offender. The increase in reporting overtime is also substantiated by Biderman and Lynch (1991).

[32] According to data from Collins and Margo (2007), 409 riots are recorded in 193 cities in the sample. Event-studies estimates show that riots are associated with higher level of crime after the first riot in a city occurs. These estimates are not presented but are available upon request.

were either established in locations where rioting would occur or in places where rioting was the most intense or severe. Although riots are considered spontaneous events (Collins & Margo 2007), it is likely that LSPs are established in places where the tension between institutions and the poor/minorities is high. As mentioned before, the program was considered an anti-rioting initiative by advocates and thus selection on the likelihood of riots is plausible and difficult to test.

To examine these concerns, I use riot data from Collins and Margo (2007). This data consists of detailed information of riots occurring between 1964 and 1971. Also included is a riot severity index which compares riots across cities in the sample. I use the riot intensity index to first test how rioting effect my estimates. Table 1.8 displays estimates for the log of total crime per 100,000 residents. Column 1 includes estimates of model 3 from figure 1.3. Column 2 displays estimates from model 3 including a variable for riot intensity in city j in year t. Estimating the effect of legal services on total criminal offenses reported accounting for rioting does not change the pre- and post-treatment effects. The coefficient on rioting is positive and statistically significant but small. There are various explanations why this occurs. One reason is that many of the riots in Collin and Margo's data are relatively small. Also, more intense riots may reflect changes in national sentiment and are captured by year fixed effects. Lastly, changes in crime could be a post-riot effect related to changes in demographics and economic activity due to rioting.

It is also reasonable to believe that rioting is an indication that actual crime is increasing. Changes in reported crime will arise because more crimes are being committed due to rioting or as a consequence of rioting. Cities where riots occurred should be associated with higher acts of crime being committed and reported. In non-rioting cities, changes in crime or reporting are not

driven by riots. To analyze the changes in actual crime due to LSP, I remove cities where riots occurred. If rioting is causing an increase in crime which is reflected in reporting, the non-rioting sample would just reflect changes in reporting due to LSP. Also, if actual crime is not increasing in non-rioting cities, the murder rate would not be affected by the presence of LSP.

Figure 1.8a plots pre and post-treatment effects for the log of total crimes per 100,000 residents for non-rioting sample. The non-rioting sample contains 413 cities with 86 cities receiving legal services grants. The result show that reported crime rises immediately to 6 percent after treatment and remains between 4 and 8 percent over the next 10 years. Figure 1.8b, 8c, and 8d plot pre and post-treatment effects for the log of murder, log of motor vehicle theft, and log of rape per 100,000 residents for the non-rioting sample. For non-rioting cities, legal services are not associated with increases in murder. More convincingly, there is a large increase in log of rapes reported in cities where riots are not occurring while post-treatment effects for motor vehicle thefts are close to zero and statistically insignificant. Rape is severely under-reported and also not a crime associated with rioting. The fact that the reporting of rapes increased in non-rioting places that receive legal services while murder did not change supports the argument that the availability of legal services increased reporting and decreased crime. Finally figure 1.9 plot pre and post-treatment effects for the log of sworn police officers per. According to figure 1.9, legal services are associated with a 2.8 percent increase in sworn police officers after one year. Similar to figure 1.4, the largest increase occurred one year after legal services is established. The large increase in number of police officers available is consistent with the historical account of legal services influence on the police. LSP lawyers often filed lawsuits against local police departments, demanding more police officers, diversity in police hiring, and more training with regards to race and poverty.

C. Other Measures of Social Capital

Evidence thus far suggests the establishment of legal services programs increase reported crime. These estimates imply that LSPs are associated with an increase in reported crime of roughly 5 to 6 percent. Legal services are also associated with an increase in the number of police officers in treated cities. All together these results are consistent with legal services reducing crime and increasing social capital by requiring citizens and public institutions to become more responsible for community development. However, they may also reflect an increase in crimes committed.

To examine this claim further I first examine changes in social capital by focusing on changes in the property value of homes. Reducing crime will make communities safer and consequently influence property values in treated cities. Conversely, an increase in crime would reduce them. I use the median property value from 1960, 1970, and 1980 census as the dependent variable. If legal services decrease crime, the increase in social capital should be reflected in the property value of homes. This is supported by Pope and Pope (2010), who show there is a negative relationship between crime and property values.

There is caution required in using the median property value as a measure of welfare. In part, is likely that users of legal services were not home owners and property value will not capture changes in the welfare of the poor. Additionally, changes in the property value in high crime areas may not impact the value of property in low crime areas or the median home owner. However, policing and crime is a city level statistic and can have spillover effects within a city. Policing high crime areas and minimizing the opportunity for crime to spread outside of high crime areas could be reflected in the median property value of all home owners. Therefore,

using median property value is a modest attempt to estimate the causal effect of LSP on the welfare of the poor and community as a whole.

To analyze the impact of LSPs on property values, I estimate the following difference-in-difference regression:

(1.2) $\quad Y_{i,t} = \gamma_i + \alpha_t + \delta LSP_{i,t} + X'_{i,t}\beta + \varepsilon_{i,t}.$

The dependent variable is the log of the median residential property value for all home owners in city i in year t from 1960, 1970, and 1980 Decennial Census. *LSP* is equal to $D_i 1(t - T^* > 0)$ which is an indicator variable that is equal to one if a legal services project is operating in city i before census year t. The row vector $X_{i,t}$, consists of covariates from the 1960, 1970, and the 1980 census. The covariates are the natural log of the following: the proportion of population non-white, percentage of population with more than 12 years of education, and the population per square mile. Also included is an indicator variable equal to one if a riot has occurred in city i before census year t.

Table 1.9 displays the results from equation 1.2 where the dependent variable is the log of the median residential property value for all home owners. According to column 1, property values are two percent higher in cities that receive a LSPs. Column 2 adds additional covariates, the log of median income and log of the percentage of the population with 12 or more years of education. Estimates in column 2 further support that the impact of LSP on property value is positive and the results are larger and statistically significant. According to column 3, cities that receive legal services earlier are associated with higher property values in 1970 and 1980 relative to cities that did not receive legal services or receive legal services later. Higher property values are consistent with LSPs increasing social capital and making these communities safer and better

off. The increase in social capital through changes in reporting, according to advocates, was driven by lawyers improving the relationship between the poor and institutions that interacted with the poor.

To aggregate the effect of LSP on property values, I use a same procedure used in Collins and Margo. Using the estimated effects in column 2, I predict the log-value of median property values in 1980 for each city. Using these predicted values, I calculate a counterfactual for property values in treated cities by subtracting the estimated value-added due to legal services. Using the number of owner occupied housing in each city as weights, I calculate the weighted average of property values in 1980 across cities to construct an average counterfactual value of homes. The weighted average of property values in the non-LSP counterfactual is $16,273. The weighted average of the actual property in 1980 is $17,370. The difference between the actual and counterfactual property values implies value added in property values of $1097 due to the presence of LSPs. The average number of owner occupied housing in 1980 across cities is 22,200. Using this number, legal services is associated with a 24 million dollar increase in property values by 1980.

Table 1.10 further examines the impact of LSPs on property values by characteristics of the LSP. Columns 1 through 4 uses the Legal Service Agency Survey (LSAS) which provides the number of attorneys, size of the budget, and the number of planned participants in 1970. Columns 5 and 8 examines the effects of LSP by grant size and pre-period crime rates. Table 1.10 presents estimates for equation 1.2 when only including the groups of LSPs that are below or above the median value for each particular characteristic. Column 1 and 2 reports treatment effects for LSPs with the number of attorneys per planned participants below and above the median number of attorneys per planned participants according to the LSAS. In both columns,

LSPs are associated with higher property values and riots are associated with lower property values. However, the below median groups are associated with higher property values relative to the above median group. A similar pattern exist when comparing LSPs below and above the median budget per planned participant. In columns 5 and 6, the treatment effect of LSPs on property values are both positive but not statistically different for the below and above median per capita first grant groups. This is not the case when comparing LSPs with pre-period crime rates below and above the median crime rate in 1960. Although not statistically significant, LSPs in the below median group are associated with lower property values in 1970 and 1980. However, legal service programs in locations with high pre-period crime rates are associated with higher property values after the establishment of legal services.

A second test of whether LSPs improved social capital in treated cities is to use the LSAS to evaluate the relationship between the poor and law enforcement. The LSAS also provides a description of the relationship between the poor and various government agencies. I use the variables that describe the law enforcement climate as dependent variables with the length of time LSP has been operating and 1960 census characteristics as independent variables. The law enforcement climate variables denote if the police are perceived to have a hostile or supportive relationship with the poor, minorities, or LSP. The variables that describe the climate are labeled 1 to 3, with 1 being not supportive or hostile and 3 being cooperative or supportive.

Table 1.11 displays the results from the LSAS regressions on the legal climate as it relates to law enforcement. Law enforcement agencies in places that received legal services earlier are correlated with having better relationships with poor communities as well as with minorities by 1970. Also, the interaction between LSPs and law enforcement agencies are considered more supportive in locations where LSPs are established earlier, however, this result

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is not statistically significant. These estimates in conjunction with the estimates from table 1.9, further supports the notion that LSP was a pro-social capital program with benefits that extended beyond the immediate recipients of services.

VI. Conclusion

In 1960 many legislative acts and federal programs were implemented to increase the quality of life of the poor, reduce poverty, and improve urban communities. This declaration of reform is reflected in the War on Poverty initiated in 1964 by President Lyndon B. Johnson. However, introducing social programs such as Head Start and Job Corps was deemed too small according to President Johnson. At Howard University's Commencement in 1965 he stated, "It is not enough just to open the gates of opportunity. All our citizens must have the ability to walk through those gates." The legal service program was established to equip the poor with the tools to do just this. Fifty years later however, research has failed to evaluate the impact of LSP in part because of the difficulty with quantifying its effects.

This paper examines Uniform Crime Report files from 1960 to 1985 to identify the impact of the introduction of the OEO Neighbor Legal Services Program on crime. The Becker Crime Model provides a theoretical framework about the implications of subsidizing lawyers in competitive market of crime, but, it is difficult to disentangle empirically the predicted increase in crime reporting from changes in actual crime. To understand the welfare implications of LSPs, my event-study approach allows use of the dynamic response to provide insight into these dynamics. The hump-shape response of reported crime provides suggestive evidence of changes in reporting. Also, the large increase in arrest and the immediate increase in the number of police officers available provide additional evidence of increased effort in crime prevention.

Lastly, non-rioting cities displayed a large increase in reported crime but no evidence of changes in murder and manslaughter due to the presence of LSPs.

Federally funded legal services attorneys are historically credited with advocating on behalf of the poor to stop police brutality, increase response times to victims, ensure that reports are filed and investigated, and change policing policies. However, individuals that opposed the program identified it as an anti-government program that emboldened criminals. My results are consistent with the establishment of legal services increasing the demand for law enforcement services. Also, that the legal services program is associated with places having better police/poor relationships as well as higher median property values. These results suggest that the LSP was impactful and this effect would have been primarily for individuals who could not articulate grievances before the program began. Crime is one of many areas in which this program could have influenced how public institutions interacted with the poor. The establishment of the legal services program will have implications on welfare receipts, divorce, evictions, as well as changes in debt repayment which can greatly change an individual and a community's economic outlook. This indicates that the LSP had far reaching implication on the poor and urban.

CHAPTER 2.

DID THE FEDERAL GOVERNMENT SUBSIDIZE DIVORCE? THE WAR ON POVERTY INFLUENCE ON DIVORCE (WITH MARTHA BAILEY)

> "*People may say that poverty prevents the poor from having the same rights to get a divorce as a person with money. Yet we must remember that obtaining a divorce is not a right, but a privilege. For most legal aid clients a separation is just as useful and practical as a divorce.*"

> -Frankel, Experiments in Serving the Indigent, in CONFERENCE PROCEEDINGS: NATIONAL CONFERENCE ON LAW AND POVERTY

> "*If you can help a man out of a hole, that is breaking the cycle of poverty. Also, if you can help a woman who was deserted ten years ago get a divorce now, I think that is breaking the cycle of poverty.*"

> - Anonymous lawyer, Finman 1971; OEO Legal Service Programs and the Pursuit of Social Change

> "*Our experience is that indigents already have a large family supported by the community, and to permit them a free divorce allows the man particularly to remarry, frequently a much younger woman, and procreate a fresh batch of public charges.*"

> - Carlin and Howard, 1965:4 14

A war against poverty was declared in 1964 by President Lyndon B. Johnson. This declaration of war initiated a series of experimental antipoverty programs funded by the federal government. The mission was to strengthen and empower the poor and their families. However, there has been much debate on if these antipoverty programs strengthened poor families or broke down family foundations and left the poor even more dependent on government assistance. One

of the more controversial programs launched in the WOP was the Neighborhood Legal Service Program (LSP).[33] The Neighborhood Legal Services Program was launched in 1965 by the Office of Economic Opportunity (OEO). The OEO was responsible for antipoverty programs that were financed by federal grants to local community organizations. These grants financed neighborhood law firms in poor communities to provide legal representation for individuals that could not afford a private attorney. These lawyers provided the poor free legal consultation to deal with housing, contracts, workmen compensation claims, welfare legislation, and criminal matters.

Legal Service lawyers also provided legal assistance with divorces and annulments. The use of federal dollars to assist in divorce proceedings generated great criticism of the legal service program. Neighborhood law firms were often called *divorce mills* by local judges and attorneys.[34] The conventional argument made against legal service programs was that federal funds were utilized to destroy families rather than strengthen them.[35] Many viewed the legal service program as turning the War on Poverty into a war against poor people (Katz, 1978). However, advocates believed that helping the poor obtain divorces brought stability to families and protected the economic interest of the poor especially poor women (Finman, 1971). This begs the question: did the increase in the supply of legal services lead to an increase in divorce rates?

The establishment of the legal service program also coincides with the dramatic increase in divorce rates that occurred in the 1960s and 1970s. There have been several empirical studies

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[33] Both Earl Johnson (1974) and Sar Levitan (1969) discuss difficulty of launching the legal service initiative. Previous legal services used with "pilot programs" of the Community Action Program were attacked by political leaders as well as local bar associations. The legal services was not launched in 1964 with many WOP programs but in 1965 after much debate about the structure, fees, and intended purpose.
[34] In cases of divorce, many judges made referrals for divorce clients to legal service attorneys. See Janowitz and Stumpf (1969).
[35] Previous charitable legal aid was discouraged to accept divorce clients. Using funds for divorce cases was not a social need and supersedes the need of the individual client. See Hannon (1969).

on the changes in the divorce rate in the United States during this time period. However, none have examined the impact of LSP's on marital outcomes and family structure. Most of the research on divorce in this period is centered on changes in divorce laws occurred in the early 1970s (Peters 1986, Allen 1992, Friedberg 1998, Wolfers 2006). Peters (1986) empirical results indicated that divorce rates were not influenced by changes in divorce laws. Center to this conclusion is that Coasian bargaining limits the impact of changes divorce laws. However, Allen and Friedberg found significant changes in divorce rates due to changes in unilateral divorce laws. Further, Wolfers' results showed that changes in divorce laws did not have a significant impact in the long-run. However, changes in divorce laws are associated with a large increase in divorce rates in the short-run due to "*pent-up demand*" for divorce in reform states. The debate is still open on how strong the impact of divorce laws has had on divorce and divorce proceedings (Solon & Lee 2010).

This chapter uses newly compiled data on *when* and w*here* legal service programs were established to examine the impact of federally funded legal services under the War of Poverty on divorce. We use the wide variation of the grant making process to evaluate the effects of introducing legal services in poor communities. We use the funding process as a source of variation because grants were received by local community based organizations and averted state or local governments. This community based approach allowed "virtually any type of proposal" to be funded under the mission of antipoverty (Johnson 1974). We will look at the implications for marriage and divorce rates using newly entered Vital Statistic data from 1960 to 1988. Because, federally funded legal service efforts were directed to those who are unable to afford attorneys at private law firms, we will examine the effects of federally funded legal services on those who are living in poverty.

To do this, we take advantage of the differential timing of the establishment of LSP in counties between 1965 and 1975. This allows us to use a *before* and *after* design to analyze changes in marital outcomes after legal services are available. We use an event study-framework (Jacobson *et al.* 1993) to analyze short and long run changes that describe the evolution of pre-trends in divorce rates as well as the evolution of changes in divorce rates due to legal services.

Our results show that there is a short run increase in the number of divorces per 100,000 residents after legal service grants are received. Counties that received a LSP grants are associated with a 6 percent increase in divorces three years after legal services becomes available. Four years after legal services is available, divorce rates remain high and eventually begin to decrease until divorce rates evolve similarly in treated and untreated counties. Considering income restriction and the targeted population of the program the average treatment effect 3 to 4 years after legal services are available can be as large as 124 divorces per 100,000 poor residents. The large treatment effect is consistent with the political debate surrounding the program as well as legal services lawyers' reflection on the volume of caseloads devoted towards family matters.

I.   **Legal Services And The War On Poverty**

The War on Poverty recruited professionals and bureaucrats to administer antipoverty programs on behalf of the poor. The declaration of war was supported and financed by legislative acts such as the Economic Opportunity Act (EOA) of 1964 which funded experimental antipoverty programs. The incorporation of the *"civilian perspective"* was motivated by Jean and Edgar Cahn in the *Yale Law Review*. The Cahn's proposed that a university affiliated, neighborhood law firm be established to serve as intermediaries between the community and antipoverty bureaucracies. Legal representation would be available for divorce,

eviction, welfare frauds, coerced confessions, arrest, police brutality, installment buying, and destroying the momentum of a militant community effort.

Following the Cahn's proposal the Neighborhood Legal Services Program was launched as part of the WOP in 1965. Neighborhood law firms were financed by grants from the Office of Economic Opportunity (OEO) and operated under the Community Action Program (CAP). The OEO was responsible for the antipoverty programs and one of the largest initiatives was the Community Action Programs (CAP). The community based approached created wide variation on how federally funded grants were not only used but received. The OEO grants averted local and state authorities and went directly to community organizations and programs allowing federal funds to be spent rapidly, almost randomly, with wide variation for intended purposes (Johnson, 1974; Gillette 1996).

The first year of the legal services program under OEO resulted in over 155 grants being issued with an annual budget over 20 million dollars. By 1967, the legal service program doubled in size issuing over 300 grants with annual budget over 40 million dollars. By the end of 1967, the federal legal service program was funding 250 projects and providing legal assistance in 48 states. To gather further understanding of how legal service funds were being utilized, during 1968 fiscal year a total of 282,000 cases were accepted. Cases involving family problems – i.e. divorce, nonsupport, paternity represent nearly 40% of the cases.

II.   Legal Services And Divorces

Neighborhood law firms provided representation, consultation, and referrals to the poor at a cost that is substantially lower than private law firms. Unlike previous charitable legal aid, these neighborhood law firms were in close proximity of the intended population it served. The LSP subsidized the cost of legal representation as well as reduced the transportation cost associated

with seeking legal advice. Although legal services were provided to handle family matters, it became a center issue of conflict between opponents and advocates of federally funded legal services. While most antipoverty programs were developed to strengthen communities and families, legal services were criticized for dissolving the family structure. Supporters of the program believed that the poor had the same rights to obtain a divorce as the rich. Legal Service lawyers not only provide free legal assistance in divorce proceedings but often were able to obtain waivers for court fees which further reduced the cost of obtaining a divorce. Advocates view these efforts as rewarding to the community as well as to poor women by providing stability and protection of women's economic interest.

Becker (1981) and Peters (1986) models for marriage and divorce develops a framework to analyze how legal service can influence divorces. Following these models, marriage is viewed as a long term contract between two individuals that produce a good (output, i.e. children, love, money). Decisions about marriage and divorce are made assuming ex post information allowing symmetric opportunities for husband and wife when a divorce occurs. Decisions about marriage are made given the present value of not entering marriage and the probability of divorce which is determined by a joint distribution of outside options. As the marriage continues information is revealed about possible alternatives to marriage. Given this information the couple decides whether to continue the marriage or divorce.

According to Peters, costless bargaining could redistribute the gains to marriage in such a way that divorce only occurs when the joint benefits exceed the joint costs. In our case, grants that established legal services would reduce the price of entering into a divorce and raised the value of any revealed outside option by lowering cost.[36] If the alternative to marriage is a

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[36] Legal Services does not directly affect any union. Legal services reduces the cost associated with divorce proceedings, therefore, making divorce a more likely option.

possible new spouse through re-marriage, then the reduction in cost of divorce makes the alternative more viable. Secondly, the ability to dissolve bad matches will reduce the cost of marriages and influence the marriage rate. Thirdly, the reduction in the price of legal services would lower the cost of bargaining at the time of divorce and also secure proper compensation which would make any outside alternative more appealing for at least one of the individuals involved.[37]

Figure 2.1 plots the divorce rate per 1,000 residents from 1960 to 1990. The bold line plot the average divorce rate in counties where legal service programs were established (treated) and the thin solid line represents counties that never received a legal service program (control). The dashed line represents the difference in the divorce rates between the treated and control counties. The Legal Service Program began in 1965 and was rolled-out over the next 10 years before becoming the Legal Service Corporation. According to figure 2.1, counties that were eventually treated had higher divorce rates until the mid-eighties. Much of the divergence in the two series occurs after 1967. It is clear from the dashed line that shortly after legal services is introduced the divorce rate in the treated counties grows at a faster pace. Equally interesting, the difference between the two series eventually shrinks and approach zero. By 1984, untreated counties have similar divorce rates as treated counties.

The short-run and long-run differences in divorce rates are similar to the stock-flow dynamics described by Wolfers (2006). Using the same logic treated and control counties have different short-run and long-run effects. Due to free legal representation, individuals who previously lacked information and financial resources can now proceed in obtaining a divorce. Therefore, introducing legal service creates a potentially treated population of married couples

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[37] Would expect potential Legal Service divorce clients to either a) already be in a failed marriage and seeking an attorney b) Already separated and now can afford a subsidized attorney c) someone looking to remarry but has yet to file for divorce.

who are already poor and the joint benefits of divorce exceeds the joint cost of divorce only because of free legal services. Assuming this population is fixed, we would expect an immediate increase in divorces after legal services become available. After these individual are treated we would expect the divorce rate to move towards a new equilibrium reflecting new norms, more information, and lower cost.

III. Data on Legal Service Grants And Divorce

A. Federally Funded Legal Service Grants

As stated early the legal service program was rolled out between 1965 and 1975. New data on the recipients of federal legal service grants funded by the OEO was compiled from the National Archives Community Action Program (NACAP) files. NACAP provides information on the city, county, and state for *where* the funds were received and the targeted communities. Also provided is the date *when* the first legal service grant was issued, the amount of the grant, and a brief description for the intended purpose of the grants. We use this information to match legal service grants to county level observations on marital outcomes. We also used the date that each county received its *first* legal service grant to identify the year each legal service program began operating.

From 1965 to 1975, roughly 300 communities received federal legal service grants. Figure 2.2 is a map of the rollout of federal legal service grants to U.S. counties over this period. The lightest shade of gray represents communities which were the first to receive federal funding for legal service in the initial years of the legal service programs in 1965 and 1966. The next darkest gray shade represents counties first receiving grants in 1967 and the darkest shade is communities receiving their first grants between 1968 and 1971. The communities highlighted in black received their first federal grant for legal services between 1972 and 1975. By 1967,

each of the lower 48 states had a county that received a legal service grant. However there is wide variation in the timing when the first grant is received within a state.

B. *Vital Statistics of the United States – Marriage and Divorce*

Vital Statistics information on marriage and divorces are compiled by the National Center of Health Statistics. The vital statistics provides the total number of marriages and divorces that occur in the United States by region, state, and county. Using data on marriages and divorces from the Vital Statistics we create a panel from 1960 to 1988 detailing the number of divorces and marriages in each county. To create the dependent variable, divorces per 100,000 residents, we interpolated the 1960 census county population to annual population data provided by the Surveillance, Epidemiology, and End Results (SEER) in 1968 and we use SEER's annual county information from 1968-1988. Our sample includes 3,076 counties with 271 counties receiving a legal service grant between 1965 and 1975.[38]

Table 2.1 report county characteristics from the 1960 census for all counties, counties awarded legal service grants, and counties without legal service grants. Counties that received legal service grants had an average population of 296,772 in 1960 compared to 28,314 in unfunded counties. The large difference in population between funded and unfunded counties is reflected by funded counties being in urbanized areas. Funded and unfunded counties are quite similar with regards to the proportion of the population that are under that age of 5 and population over the age of 65. These counties are also similar in the share of the population that is non-white. Funded counties are not only more likely to be in urban areas but they are slightly more educated. Funded counties have a higher proportion of residents with 12 years of education and fewer residents with less than 4 years of schooling. The counties funded with legal service grants were highly populated and also more affluent. This is more than likely due

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[38] Cook County, Los Angeles County, and New York City is removed from the sample

to the innate structure of the legal service programs. Legal service programs were design to be affiliated with law schools. More urbanized areas are likely to have one or more law schools and would have been more likely to apply for legal service grants and subsequently be funded.

IV.   Empirical Strategy

Our empirical strategy will use the variation in the timing of the establishment of LSPs to conduct an event-study. The key assumption for this approach is that the timing of the establishment of LSPs is uncorrelated with other determinants that influence divorce rates. To justify the assumption we conduct two tests; the first test is a regression of 1960 demographic characteristics on the year LSPs was established. The second test compares the timing of the LSP with the pre-program divorce rates and pre-program changes in divorce.

Table 2.2 reports weighted and unweighted estimates from ordinary least squares regression of the year a county first received a legal service grants on 1960 county characteristics and indicator variables for a nearby law school and medical school. The legal service program was proposed to be affiliated with a local university law program. Also, also local universities are likely to have resources devoted toward grant writing. In our case, a law school or university could influence the grant making process and affect the timing of when a county received a grant. Therefore, we use medical schools to proxy for local major universities. Column 1 and 3 are for all counties that received legal service grants, while column 2 and 4 are estimates for counties funded before 1970. Also, columns 1 and 2 are unweighted and columns 3 and 4 are weighted by 1960 county population. According to the table 2.2, the proportion of the population living in urban areas is significant in all columns except column 3. It is not surprising the urbanization and population density if correlated with the timing of War on Poverty grants, highly urbanized places often have more resources and non-profit organizations to apply for

funding. In column 3, the regression is weighted by 1960 population and includes all funded counties in our sample. All the covariates from the 1960 census and the indicator variables fail to predict when a county first received a legal service grant.

Figure 2.3 compares the timing of the establishment of the LSP with divorce rates in 1965 and the changes in the divorce rates between 1960 and 1965. Figure 2.3a shows no indication of relationship between divorce rates in 1965 and when a county received a legal service grants. Similarly, figure 2.3b shows that changes in divorce rates do not determined when and where OEO administrators established legal service programs. According to the literature, there were no precedents or guidelines for grant applications. Table 2.2 and figure 2.3 are consistent with the *wild funding* process that is documented in the history of the program.

Taking advantage of the variation in the timing of the establishment of the legal service program, we use the *date* the grant was issued to identify when a county *first* received legal services. Our event study analysis will provide a statistical evaluation of changes in divorce rates before legal services was established as well as describe how divorce rates evolved after legal services became available. The pre-treatment effect provides additional comparison of how the divorce rate was changing before the legal service program began. To do this we estimate the following linear regression:

(2.1) $\quad Y_{i,t} = \gamma_i + \alpha_{t,s(i)} + \sum_{\tau=1}^{q} \pi_{-\tau} D_i 1(t - T^* = -\tau) + \sum_{\tau=1}^{p} \delta_\tau D_i 1(t - T^* = \tau) + X'_{i,t}\beta + \varepsilon_{i,t}$

where $Y_{i,t}$ is the annual number of divorces per 100,000 residents in county i in year t *(t= 1960, 1961,...1988)*; γ_i is a set of county fixed effects which control for unobservable county characteristics; α_t is either a set of year effects or state-by-year fixed effects $(\alpha_{t,s(i)})$. The column vector $X'_{i,t}$, consists of covariates from the 1960 census interacted with a linear trend. Covariates include the percentage of population under age of 5, percentage of the population

over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, the percentage of the population with less than 4 years of education, percent of the population in households earning more than 10,000 dollars, percent of the population in households earning less than 3,000 dollars, and the percent of the population in urban areas. D_i is a dummy variable equal to one if the county ever received federally funded legal services. $1(t - T^* = -\tau)$ is an indicator variable equal to one if the observation year is $-\tau$ years from the date that the legal service grant is received or $1(t - T^* = \tau)$ is equal to one if the observation year is τ years after the date legal services were first available. $1(t - T^* = 0)$ is omitted due to collinearity where T^* is the funding year for the legal service grant; q refers to the number of lags or years before legal services are funded and p is the lead or years after legal services are funded. To ensure the coefficients are well estimated, event time for $\tau > 12$ and $\tau < -5$ are grouped into endpoints, $q = 6$ and $p = 13$.

In our sample, counties receive legal service grants between 1965 and 1975. A balanced event panel using Vital Statistics data on marriages and divorces from 1960 to 1988 will focus on five years before and twelve years after federally funded legal services are received. The year legal services is established in a county $(\tau = 0)$ is considered the event. The coefficients of interest are $\pi_{-\tau}$, which are pre-treatment effects, and post treatment effects δ_τ. These estimates describe the dynamics of marital outcomes in funded counties before and after legal service grants are received (T^*). Pre-treatment effects describe the change in the average difference in eventually treated county dependent variable τ years before the establishment of legal services relative to untreated counties. If the econometric model captures the pre-legal services evolution of the dependent variable, the pre-treatment effects should be statistically indistinguishable from

zero. The post-treatment effects δ_τ, is the average change in the difference in treated county marital outcomes τ years after the county received the grant.

V. Results from using the Timing of First Grants

Using the estimates from equation 2.1, we plot pre-treatment effects and post-treatment effects from a balanced panel. Figure 2.4 plots the estimates from three different specifications of the linear regressions. Model 1 is plotted in the solid line with no markers. It contains only county and year fixed effects. Model 2 is plotted with a solid line and square markers and includes county and state-by-year fixed effects. Model 3 also includes county and state-by-year fixed effects with additional county characteristics from 1960 interacted with a linear trend. We present 95 percent confidence intervals for model 3 by dashed lines. The confidence intervals are constructed from heteroskedastic-robust standard errors clustered by county. The sample does not include New York City, Los Angeles County, and Cook County. Presented are estimates where the annual number of divorces per 100,000 residents is the dependent variable $Y_{i,t}$ for equation 2.1. All regressions are estimated using 1988 population as weights.

The pre-treatment effects $\pi_{-\tau}$, in figure 2.4 are consistent with the narrative described in table 2.1 and figure 2.3. The pre-treatment effects in each model are close to zero and statistically insignificant. According to these estimates, the establishment of legal services was not influenced by divorce rates and family formation. More importantly divorce rates in eventually treated counties are evolving similarly to untreated counties before legal services program began. Because our econometric model captured the pre-period trend in divorces, a break in the trend can be contributed to the establishment of federally funded legal service program. This is displayed by the post-treatment effects δ_τ, which shows a statistically significant trend break after the establishment of legal services. After initial exposure to legal

services, the divorce rates increases dramatically and is statistically different from zero in all 3 models.

Table 2.3 report pre-treatment and post-treatment effects from equation 2.1 by grouping years before and after exposure to legal services. The groupings are indexed by g, where $g = -2$, -1 for $y \leq -5$ and $-5 \leq y \leq -1$ and $g = 1, 2, 3, 4, 5, 6$, and 7 for $y = 1$ or 2, $y = 3$ or 4, $y = 3$ or 4 and so on until $y \geq 12$ ($g = 0$ is omitted). As stated above the pre-treatment effects are close to zero in three columns. By adding state-by-year fixed effects in model 2, the pre-treatment effects are smaller in absolute value and closer to 0. Adding covariates in model 3 increases the absolute value of the estimates in the pre-period but they are still small and statistically indistinguishable from zero. The post-treatment estimates for year 1 to 2 are positive and statistically significant in column 2 and 3. Adding state-by-year fix effects increases the size of the post-period estimates in year 1 to 2 by 46 percent and adding covariates increase the magnitude of the coefficient by additional 5 percent. Estimates for 3 to 4 years after the availability of legal services are larger and similar in magnitude in all 3 columns and more than double the size of estimates for year 1 to 2. The first decline of the estimates occurs in year 5 to 6 and they continue to decline in model 1. While the decline in the post period estimates is robust across specification in year 5 to 6, the post-treatment effects in model 2 and 3 rebounds and peaks in years 7 to 8 and then gradually declines.

The long-run effect of divorces due to legal services is not supported by all three models. Post-treatment effects in all 3 columns are statistically insignificant after 9 to 10 years of exposure to free legal services. After 9 years, model 1 report negative coefficients while models 2 and 3 report coefficients that are positive but not statistically significant. The long-run implications with regards to divorce are consistent with figure 2.1, where the divorce rates in

treated and untreated counties converge around 1984. The preferred specification, model 3, do not report negative or zero post treatment effects but with a longer horizon we would expect the coefficients to become zero or even negative at some point.

According to our preferred specification, model 3, the divorce rate increases by more than 6 percent three to four years after a legal service grant is received. The post-treatment effects are positive and increase for the first 4 years and remain positive for the next 8 years in the second and third specification. The cumulative effect of having federally funding legal services available for seven to eight years is associated with divorce rates increasing by 27 percent according to model 3.

The statistically significant increase in divorce during the first 4 years of operation of the legal service program is robust across specification but differs from similar studies on divorce. Wolfers (2006) shows a large immediate increase in divorce due to pent-up demand for divorce prior to changes in divorce laws. However, the steady increase in divorce in our model over the first 4 years is consistent with buildup of services within a community. After the grant is received, neighborhood legal services have to hire staff, build community support and rapport, while also accumulating exposure. It is important to note that legal services are not causing divorces to occur but handling the demand for divorce due to lower cost and more information of viable alternatives for family support. Previously, poor individuals (more often women) were unable to obtain divorces because 1) previously available legal aide refused to handle divorce cases 2) they were unable to afford a private attorney or 3) they were unable to pay court cost which legal service lawyers often arranged for courts to waive.

Secondly, our post treatment effects are smaller in magnitude than Wolfers but the size of our treatment effect is larger. Wolfers studies changes in divorce due to changes in divorce laws

which impacted every married and future married couple in a state. However, legal services eligibility was determined by income, family size, and cost of living and thus limited availability of legal services to families and individuals in poverty. The estimates reported in table 2.3 are considered intention-to-treat estimates (ITT), the average effect of legal services over all the residents in a county. These estimates do not distinguish between users and non-users of the legal service program. If we assume that the increase in divorce is only driven by users of legal services and all users were considered living in poverty, then the Average Treatment Effect on the Treated (ATET) is derived by dividing the increase in the divorce rate by 17 percent (percentage of the population living in poverty in 1964).[39] Averaging the ITT over columns 1 through 3 for three and four years after exposure (21 per 100,000 residents), yields an ATET of 124 divorces per 100,000 poor residents. Although, this treatment effect appears to be large, it is consistent with the narrative concerning neighborhood legal services as divorce mills. This is also reflected in interviews of legal service lawyers in which many complained about the large number of caseloads that were for divorces, annulments, and separations.

VI.   Legal Services and Unilateral Divorce Laws

The availability of legal services increased the number of divorces that occurred by 6 percent after 4 years. For many counties this precedes the widespread changes in states divorce laws that occurred in the 1970s. The divorce laws of the 1960s required that both parties involved must mutually consent for a marriage contract to be dissolved or one party must prove that the other was "at fault" of violating their marriage contract. Finman (1971) documents legal service lawyers dis-approval of mutual consent or at fault divorce laws and the difficulty with divorce proceedings in these states. It is possible that the magnitude of the increase in divorce due to the presence of legal services is influenced by divorce law reform. Wolfers show that

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[39] Figure comes from "A Decade of Federal Antipoverty Programs," edited by Robert H. Haveman (1977) pg 4.

there is a short-run increase in divorce rates in reform states immediately after changes in divorce laws but no long run difference in divorce rates between reform and non-reform states. The short-run increase is due to pent-up demand for divorces before states reformed divorce laws. More interesting it is quite possible that courts in reform states are more lenient when dealing with "at fault" divorce proceedings prior to law reform.

Figure 2.5 plot estimates for equation 2.1 for reform states and non-reform states. The reform states consist of 28 states that changed to no-fault unilateral divorce law.[40] Similar to figure 2.4, the pre-existing trends for reform and non-reform divorce states are close to zero and not statistically significant before the establishment of legal services. The post treatment effects for both regimes are positive and both are statically significant 3 years after exposure to legal services. The key difference in the post treatment effects in reform state vs non-reform states are in the first 4 years. For reform states, the increase in divorce is larger and immediate. While in non-reform states the increase in divorce occurs over a longer horizon and at a slower pace. However, the increase in divorce occurs steadily and is robust across divorce law regimes.

VII.   Post-Treatment Effects and Baseline Characteristics of Grant Recipients

The Legal Service Agency Survey (LSAS) provides further information about the typical LSP in 1970.[41] The LSAS contains geographical information such as state, region, and county of grant recipient as well as information on staffing, budget, and planned participants. The LSAS also provides a description of the relationship between the poor and various government agencies as well as the perceived image of the LSP in poor communities. These variables are labeled 1 to 3, with 1 being not supportive or hostile and 3 being cooperative or supportive. According to

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[40] Twenty seven state switch to a no-fault regime between 1968 and 1977. Oklahoma change divorce laws in 1953. Divorce laws are from Leora Friedberg (1998) provided by Justin Wolfer (2006)

[41] The Legal Service Agency Survey was conducted in 1970 and 1971 to evaluate the effectiveness of the Legal Service Projects (LSP) and to make recommendations on whether to expand, cut back, or eliminate some agencies. The survey included roughly 2/3 of all the LSP project operating at that time.

table 2.4 the typical LSP in 1970 was staffed with five attorneys, one R.H. Smith Fellow, six clerical support personnel, and three law students.[42] The LSP was expected to serve over 1900 participants with a budget of slightly above $200,000. Table 2.5 examines the impact of the LSP on divorce by survey characteristics. Column 1 and 2 reports pre and post treatment effects for LSPs with the number of attorneys per planned participants below and above the median numbers of attorneys per planned per participants according to the LSAS. Both groups display positive post treatment effects, but only the LSP with the number of attorneys above the median have statistically significant effects in the initial 4 years of exposure. Also, the below median group has larger effects than the above median group except for year 1 to 2 and years 11 to 12. These post-treatment effects are similar as it relates to LSP's below and above the median budget per planned participant in column 3 and 4.

        According to historical literature of the legal service program, LSP lawyers often persuaded courts to waive the court fees in divorce proceedings. It is also documented that judges with knowledge of the legal service program often referred individuals to legal services in proceedings where family matters required a lawyer to handle divorces or separations. Given this historical account, it is reasonable to believe that places where the courts are viewed as being supported and fair with regard to poverty issues would also be more likely to waive fees and reduce the cost of divorce for the poor. Column 5 and 6 reports effects for the group of LSP where court attitudes towards the poor and LSP are viewed as negative and positive according to the LSAS. Both columns report positive and statistically significant post treatment effects and the cumulative post-treatment effects are similar despite the legal climate as it relates to the

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[42] R.H. Smith Fellowship is a prestigious fellowship to attract highly qualified attorneys to practice in legal aid for the poor.

courts attitude towards the poor. This implies that divorce laws more so than court attitudes influenced the impact of subsidized legal aid on divorces.

Legal services involvement in divorce and divorce proceedings were viewed as counterproductive by bureaucrats, community leaders, and local bar associated lawyers. Legal services were often labeled as divorce mills in effort to discredit the need for federal dollars devoted for legal assistance to the poor. These sentiments may also be reflected within the community in which legal services are located. Columns 7 and 8 report treatment effects for LSPs located in places where the poor viewed the them favorable vs unfavorably in 1970 according to the LSAS. Both columns report large and statistically significant post-treatment effects but effects are larger for the unfavorable group. The cumulative increase in divorce in the unfavorable group is nearly twice the cumulative effect in the favorable group. The correlation between large increase in divorce and unfavorable view in the LSAS is consistent with political debate concerning legal aid and family structure.

Table 2.6 investigates whether LSPs had larger effects in locations with above-median per capita grants, or above median divorce rates in 1960, or in different census regions. Columns 1 and 2 present estimates for the group of LSPs with first grants below and above the median per capita first grant in 1970 dollars. Both groups report positive and large effects and the cumulative effects of LSP on divorce are similar. However the groups differ in the short and long-run responses in divorce to legal services. The short-run impact for the above median per capita group is twice as large as the below median per capita group. In year 7 and 8, post-treatment effects are similar but afterwards the effects in the above median group dissipate and eventually become negative. Columns 3 through 6 report estimates removing one region at a time. Regardless of the region the post treatments are positive, large, and statistically significant.

The cumulative effect is the largest when the Northeast is removed and the smallest when the Midwest is removed but post-treatment effects are statistically indistinguishable from each other in all columns. However, this is not the case when distinguishing LSPs by pre-period divorce rates. The post-treatment effects are statistically significant and larger in treated counties with above median 1960 divorce rates. The estimates in the below median group is not only smaller but are never statistically significant and negative for years 1 and 2 as well as after years 7 and 8. According to figure three as well as column 7 and 8 of table 2.6, the effects of legal service appears to be driven by lenient divorce laws and pre-LSP divorce rates.

VIII. Legal Services Impact on Marriage and Public Assistance

According to the quote in the beginning:

"Our experience is that indigents already have a large family supported by the community, and to permit them a free divorce allows the man particularly to remarry, frequently a much younger woman, and procreate a fresh batch of public charges."

This hypothesis can be tested by examining changes in marriage, divorces per marriages, as well as changes in spending on public assistance. Figure 2.6 plot pre and post treatment effects for the LSP grants on the marriage rates, divorces per marriages, and cash public assistance benefits per capita. According to figure 2.6a, the marriage rates are not affected by establishment of LSP. This differs from conventional wisdom where a decrease in the cost of divorce should also decrease the cost of marriage because separation is cheaper if a bad match occurs. However, our results do not imply an increase in marriages as many marriage market models would predict.

In addition to marriage rate, we examine divorces per marriages. According to figure 2.6b divorces per 1,000 marriages is decreasing before the establishment of LSP but post treatment effects are positive and statistically significant 3 years after LSP is available. Divorces per marriage increase by 12 percent in year 3 and remain positive for the following 7 years.

Although the post treatment effects are positive, they are only statistically significant for years 3, 5, and 6. Stemming from figure 6a, a large number of divorces occurring with no changes in the marriage rate concurrently or in the future creates a larger pool of currently divorce individuals after free legal services are available.

Figure 2.6c plots pre and post-treatment effects for the relationship between the initial LSP grant and public assistance. To measure public assistance at the county level, we use cash public assistance benefits per capita calculated using data from the Bureau of Economic Analysis Regional Information System (RIES). All three model reports a dramatic increase of public assistance after LSP grants are received. According to figure 2.6c, cash public assistance per capita increase by 28 to 45 percent in treated counties six years after legal services were established depending on the model. However, the large increase in public assistance per capita cannot be totally attributed to increase in take-up by divorced individuals but also reflect an organized push to reduce stigma related to government assistance in treated counties. Legal services contribution to the take up of government assistance is an accumulation of two effects. First, LSPs mandate to fight on behalf of the poor for welfare claims. This resulted in the legal service lawyers utilizing time and expertise in class-action suits to change welfare laws and eligibility criteria (i.e. courts striking down man in house rule).[43] Secondly, increase in the take up of funds from divorcees who are now eligible for public assistance.

IX. Discussion

From our analysis it is clear that the divorce rate increases after legal services are available. The short and long run dynamics of introducing legal services reflect changes in the

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[43] *Shapiro v Thompson*, *Goldberg v Kelly*, *Wheeler v Montgomery* as well as *King v Smith* radically change how AFDC benefits were distributed to the poor. Legal Service Back-up centers were responsible for most of the research used in litigation. These back-up centers were often university affiliated and received legal service grants specifically for research and training purposes.

demand for divorce subject to the new price and the clearing of the market which leads to a new steady state. What is not clear from our analysis is the convergence of the divorce rates in control and treated counties in the eighties. Although we have large ATET, it's also reasonable to believe that the introduction of legal services did little to affect the divorce rates over time. There are several explanations for this. First, reducing the cost of divorce only dissolved bad matches sooner. This would create a large of amount of divorces shortly after legal services are available but these bad matches would not be around to separate later.[44] Column 2 of table 2.7 reports pre and post-treatment effects from model 3 using only treated counties. In this case we are only identifying on the variation in the timing of the first grant. Not only are the coefficients in column 2 large and positive but they are increasing in magnitude over time. Any convergence between the treated and untreated places is likely due to changes in divorce and social norms concerning divorce in untreated places.

According to figure 2.1 the pre-legal service divorce rates were higher in treated counties. Although the timing and selecting of legal service locations were independent of divorce rates it is clear that places with higher divorce rates received legal service programs. As stated earlier legal services were located in larger more urbanized locations and the difference in divorce rate may reflect differences in social norms related towards divorce in rural and urban places. As social norms evolve and converge over time, divorce may become more acceptable in rural places. The convergence of social norms concerning divorce would lead to a larger increase in divorces in later periods contributing to the smaller and negative long run coefficients in figure 2.3. Column 3 attempts to shed light on the spillover effect of reducing the stigma of divorce. Assuming neighboring counties are more likely to interact with someone who has been treated or

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[44] The number of caseloads devoted to divorce decreased overtime. This is speculated in part due to political reason as well as less demand for divorce by clients. Hollingsworth (1977) ; Besharov (1990)

even used legal service themselves, we would expect the divorce rates to increase faster in locations in close proximity to legal services.[45] Column 3 compares contiguous counties of locations with legal services to counties that are not neighboring treated counties. The post-treatment effects in column 3 are never statistically significant but are positive. The coefficients are also increasing over time and the long-run coefficients are actually larger than column 1. The large increase in contiguous counties in later periods is consistent with changing of social norms about divorce.

The changes in social norms concerning divorce is not just a reflection of legal services but changes in divorce laws during the seventies in a period called the "divorce revolution". As mentioned before the negative long run effects and convergence of divorce rates in our analysis of legal services is similar to other studies on changes in divorce laws. It is important to note that treated and non-treated counties in the same state would be subjected to the same divorce laws. Our inclusion of state-by-year fixed effects capture time-varying state level changes such as changes in divorce laws. These fixed effects minimize the impact of including divorce law changes in table 2.3. The difference in our results compared to results from the divorce law literature and predictions from marriage market models are concerning the short-run impact on divorce and the impact on marriage rates.

Marriage market models and empirical work on divorce laws both agree on an immediate and large impact on divorce rates. This result is highlighted by two effects 1) pent-up demand for divorce and 2) knowledge of law changes. The pent-up demand for divorce in reform states reflects an existence of a quantity of bad matches where at least one individual wanted a divorce and could not obtain the divorce given the requirement of mutual consent. Secondly, the law

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[45] Eligibility for legal aid was based on income not residency. Many efforts were made to reach rural communities in treated counties as well as the poor in contiguous counties.

was passed to deal exclusively with divorce and separation agreements. These laws were passed with great political and policy debates and were more likely to inform the public about when the law would change and be implemented. These two effects would create an immediate impact on divorce once the law has passed.

However, legal services often entered communities usually without knowledge from the general public and the poor. The legal service movement was mostly driven by lawyers and bureaucrats within the War on Poverty that was familiar with legal aid. The program was mostly thought of as an anti-riot policy which allowed the poor to use lawyers to settle local grievances through the courts instead of social demonstrations. Using services for divorce was an afterthought in the creation of legal services and remained a constant debate during the initial roll-out of the program. The lack of public knowledge about the program and services available stalled any immediate impact on divorce predicted by a marriage model. Lastly, we identify the start date of the program by the year the grant was issued. In actuality, there exist a gap between the issuing of the grant and the build-up of services which reflect set-up time and accumulation of exposure. These reasons as well as the poor's own skepticism about using the judicial system resulted in a build-up in usage of legal services and a gradual increase in divorce after treatment.

Comparing our results concerning marriage rates differ from conventional wisdom concerning the marriage market. Typical marriage market prediction concerning free divorces would imply more selection into marriage because of the ease to dissolve a marriage and bad matches would divorce and re-marry to find better matches. Both of these mechanisms would lead to an increase in the marriage rates after the introduction of legal services.[46] However, we find that marriage rates are unchanged by the availability of legal services. On one hand, divorce

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[46] Rasul (2003; 2006) found that changing from mutual consent to unilateral divorce regimes resulted in an immediate and permanent decrease in marriage rates.

laws would have more of effect on selection than the cost of divorce.   As mentioned earlier, legal service lawyers were not as successful with obtaining divorces under "at fault" divorce regimes. Therefore, divorce laws would play a major role with regards to selection into marriage and divorce. On the other hand, the marriage market predictions assume that marriage is a better alternative to being single. This assumption would predict that divorcees would remarry once a better match has been found.   However, increased labor market participation of women, government programs for single parent families, and the reduced stigma of divorce would reduce the returns to marriage prompting individuals not to remarry.

In summary, the introduction of legal services greatly impacted the divorce rates amongst the poor. Previously the poor had limited access to lawyers and often felt victimized by the judicial system. The introduction of legal service was established to provide new channels for the poor to remediate grievances with local business, landlords, and government bureaucracies. However, the influx of legal aid and the reduction in cost revealed that the poor also desired services devoted towards divorce and other family matters. The introduction of legal services in poor communities is associated with a 6 percent increase in the divorce a few years after legal services became available. This increase equates to an ATET of 124 divorces per 100,000 poor residents.   The large increase in the divorce rate is accompanied by a marriage rate that is unaffected by the legal services and a large increase in the use of public assistance.   The combination of these results leads to a few unanswered questions. First, what happened to the poor individuals that obtained divorces using legal services? Did this lead to an increase in labor market participation amongst poor women? What is the influence of cash public assistance on divorce and selection into marriage? These question and further examination of the legal service

program can shed light on how experimental programs from the Great Society influenced family

formation, crime, and other outcomes.

CHAPTER 3.

ELECTION CYCLES IN POLICE HIRING REVISITED

In a highly cited paper in the literature of crime and economics, Steven Levitt estimates a causal relationship between police and crime. To address the question on whether police reduce crime, Levitt uses election cycles as a plausible exogenous shock in police hiring. The plausible exogenous shock is an important aspect of the research design in order to deal with simultaneity bias inherent in the analysis of police on crime. The endogeneity problem is a consequence of the relationship between the stock of police and the number of crimes committed in Becker's classical model of economics and crime.[47] Larger cities are associated with higher crime rates as well as larger police forces. This relationship makes it harder to identify a deterrent effect of police on crime in a cross-section analysis. Also, the natural response to higher crime rates or an expected crime wave is to increase expenditures devoted towards policing. This response to crime plague estimates from panel data analysis which usually concludes that changes in police is in response to changes in crime or identifies an anticipatory effect of changes in police to future increases in crime.

Circumventing the endogeneity problem, Levitt exploits exogenous changes in demand for crime during election years. Levitt argues that the size of a city police force fluctuates due to

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[47] Historically, studies using police and police expenditures have provided evidence of police having a positive or zero effect on crime. Breaking the link between police and crime has been major focus of recent literature in economics of crime to identify a causal relationship between police and crime. See Cameron (1988) and Lim, Lee, and Cuvelier (2010).

the election. This fluctuation in the police force is driven purely by political incentives. Levitt states

> *"Given the importance of crime as a political issue, incumbents will have incentives to increase the police force in advance of elections, either in hopes of actually reducing crime, or simply to demonstrate that they are tough on crime."* - Levitt p 274

If this is true then elections can serve as an instrument for the quantity of police since election timing is unrelated to crime.

Levitt proceeds by producing OLS estimates using data on 59 cities with more than 250,000 residents.[48] The estimates identify a statistical significant relationship between the log of the change in sworn officers and mayoral and gubernatorial election years. Using this result, Levitt subsequently estimates the log sworn officers per capita on violent crime, property crime, and subcategories crime using OLS and 2SLS procedures. Levitt surmises that police reduces violent crime and has a small impact on reducing property crime. Levitt contributions are first, election cycles can serve as plausible exogenous variation in the size of the police force. Secondly, that the number of police serves as a strong deterrent to violent crime. Lastly, 2SLS estimates are more negative than OLS estimates which signs the bias produced by OLS estimates.

McCrary (2002) replicated Levitt regressions but corrects two issues in Levitt's original paper. The first and most important contribution is that McCrary addresses a weighting error in Levitt's estimating procedure for violent and property crime. According to McCrary, Levitt's estimates are a result of weighing procedure that was designed to give more weight to crimes with lower year to year variability but a programing error caused the opposite to occur. When correcting for this error, McCrary's results show that the relationship between police and violent

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[48] The sample Levitt analysis consist of cities where the population exceeds 250,000 residents between 1970 and 1992 and also elect the mayor directly.

crimes no longer holds when elections are used as instruments.[49] Also, McCrary result shows that the consistent relationship between the 2SLS and OLS estimates no longer holds.

Secondly, McCrary proceeds by using a new measure of mayoral elections to increase the precision of the first stage estimates. McCrary finds that 33 cities have different measures for election years in the World Almanac and the Municipal Yearbook compared to Levitt's measure. Despite the difference, McCrary's first stage estimates when using the new measure of elections are not statistically different from Levitt's estimates. However, the estimates for the impact of mayoral elections on the size of the police force are larger when the new measure of elections are used in the first stage.[50] Despite the new measure, McCrary believes that the relationship between police and crime would only provide more precise estimates if a better research design is used or if a larger sample could provide a stronger estimate.

My contribution to the literature will be to expand the sample to include more cities over a longer time horizon. Previous results could have been driven by the sample period in which crime is steadily increasing and the political response may be more important or by the composition of the sample which only included large cities. Using only larger cities may exclude cities in different regions that reduce the heterogeneity that may result from crime in the South versus the Midwest. Also, increasing the sample size will increase precision of the estimates. McCrary argues that the largest 2SLS t-ratio when weighted correctly is 1.5 and 1.4 when estimated separately. These t-ratios are not far from 1.645 which is significant at the 90 percent level. Having a larger sample will give further credence to McCrary's conclusion.

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[49] The weighting procedure is the following. Levitt first regress crime on police and produce OLS and 2SLS residuals. The residuals are used estimate standard deviations by crime categories. The correct weighting procedure uses $1/\hat{\sigma}_j^2$ for weight for each crime category j. For the pooled OLS and 2SLS estimates, Levitt uses $\hat{\sigma}_j$ mistakenly.

[50] Second stage estimates using McCrary new mayoral election year are similar in size to Levitt. In general McCrary elasticities of police on crime are larger for rape, assault, larceny, and motor vehicle theft. Despite the larger estimates, t-ratios are small and insignificant.

Secondly, I will use new mayoral election data provided by Ferreira and Gyourko (2009) to estimate the impact of electoral cycles on police size for a larger sample of cities. New mayoral data includes information on election timing and results in the United States from 1950 to 2007.[51] Regression analysis using new mayoral data produce first stage estimates that are larger and more precise for mayoral elections influence on the size of the police force.

My results show that election years are associated with a 2 percent increase in the number police officers available. Cities with the population larger than 250,000 residents have an average increase in the size of the police force of 2.3 percent during election years. Two-stage least squares estimate indicate a negative relationship between police and crime. The elasticity of police on violent crime is -0.74 and as large as -1.88 for cities with the population greater than 150,000 residents.

Although I do find a negative relationship between police and crime, it is unclear if new mayoral data and a larger sample settles the debate between Levitt's and McCrary. This is in part due to the different cities in my sample relative to their analysis. My sample only contains 37 of the 59 cities used in McCrary and Levitt analysis. Using these 37 cities, my analysis fails to identify a causal relationship between police and crime. Secondly, when using the election year variable provided by Levitt or McCrary to complete the sample, the negative relationship between police and crime is still indistinguishable from zero. However, a large negative effect of police on violent crime is found when the sample is expanded to include cities with the population greater than 150,000 residents. This relationship holds when Levitt's or McCrary's election measure is used to complete the sample. However, the relationship disappears when the sample years are expanded or if the city size restrictions are lowered even further.

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[51] Data provided by Ferreira and Gyourko (2009) contains 451 cities but the sample used in this analysis relies on when the election took place to identify the campaign year. This drastically reduces that sample because the month of the election is not consistently recorded for cities in the original data set.

I.   Data

A. *Data on Elections*

The measurement for mayoral elections comes from Ferreira and Gyourko (2009). The election data provided consists of mayoral elections in the United States for cities with 25,000 residents or more from 1950 to 2007. The data provides information on the city in which the election took place, the winner (mayor), the runner-up, total votes for the winner & runner-up, total votes, name of the winner and runner up, month the election took place, party of winner and runner-up, and type of election (general, run-off, special). New mayoral data was collected from a voluntary survey sent to each city. The voluntary nature of the survey often resulted in incomplete election details or missing election data for many cities. As a result, I have new mayoral data for 55 of 82 cities where the population exceeds 150,000 residents by 2005. Gubernatorial election data is from the Candidate and Constituency Statistics of Elections in the United States, downloaded from the Inter-University Consortium for Political and Social Research (ICPSR).

B. *Uniform Crime Reporting – Law Enforcement*

UCR Law Enforcement Officers Killed or Assaulted (LEOKA) contains monthly counts of law enforcement officers killed and assaulted as well as annual law enforcement employment. The UCR reports the number of civilian officers and sworn officers as of December 31st of the reporting year until 1971. For 1972 and afterwards the number of officers are reported as of October 31st of the reporting year. The UCR LEOKA is available publicly at ICPSR website for years after 1974. For the years 1970 to 1974 data are manually entered from hardcopies of UCR LEOKA reports.

C. *Uniform Crime Reporting – Offenses Known*

The Uniform Crime Reporting: Offenses Known and Clearance by Arrest (UCR), provides information on offenses submitted by law enforcement agencies to the Federal Bureau of Investigation (FBI). The data on crime includes monthly information on the number of unfounded offenses, actual offenses, offenses cleared by arrest, and offenses cleared involving individuals under the age of 18. The UCR Offenses Known and Cleared by Arrest are merged with the LEOKA data by year and the unique agency identifier code "ORI". The following offense of interest is recorded in this database: Property crime which includes burglary, larceny, and motor vehicle theft, and violent crime which refers to murder, manslaughter, rape, assault, and robbery. The UCR also provides counts for more detailed descriptions of each of the offenses listed. The number of actual offenses includes reports of crime received from victims and police who discover possible infractions. The number of offenses in a month is provided by city, county, and state agencies.   The criminal offenses compiled for the UCR is submitted voluntarily by city, county, and state enforcement agencies.[52]

D. *Data for Demographic Characteristics*

Covariates used in this analysis are from the 1960, 1970, 1980, 1990, and 2000 Census City and County Books, Bureau of Labor Statistics, Historical Data Base of Individual Government Finances, and the Surveillance, Epidemiology, and End Results (SEER).   The percent of female headed household are constructed by linearly interpolating between the 1960, 1970, 1980, 1990, and 2000 census. To calculate the proportion of the population that are males between the ages of 15 to 24 and the percentage of the population that is black, I used the annual county population profiles from SEERs.   Public Welfare Spending per capita and Education

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[52] Reporting in the UCR is major concern. How the UCR data was edited or manipulated was not outline in either Levitt or McCrary published work. To replicate their work as closely as possible, I removed outliers based on an algorithm used in Chalfin and McCrary (2013) which recoded missing values for crimes that were 6 times higher or 1/6 lower than the city's average number of crimes for each category.

spending per capita comes from Historical Data Base of Individual Governments which collect local government data from the Census of Governments and the Annual Survey of Governments. The state unemployment rate is downloaded from the Bureau of Labor Statistics website. The covariates are created and used to follow the empirical design used in Levitt (1996).

E. *Data for the Final Sample*

The final sample consists of 101 cities spanning observations from 1970 to 2000. Cities in the sample have populations that exceed 100,000 residents during the sample period and contain the month the election took place. Ferreira and Gyrouko mayoral data contains 135 cities that provide the month the election took place and 101 of these cities population exceeds 100,000 residents between 1970 and 2005. The new mayoral data does not provide election information for all cities with the population greater than 100,000 residents. Only the cities that responded to the Ferreira and Gyrouko survey and met the above restrictions are included in the sample.[53]

The month the election occurred is used to determine the campaign year. If an election occurred before July, the previous year is identified as the campaign year. If an election occurred during the month of July or afterwards, the current year is identified as the election year. This is a similar procedure of identifying the campaign year outlined by McCrary (2001). The goal is to identify the time when the political incentive to influence the police and crime is the highest. If the above statement by Levitt is correct, the incentive should exist before the election take place.

Table 3.1 provides a summary of city level characteristics for the final sample. The average population in the sample is 466,187 residents with 13 percent of the population black

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[53] New mayoral data does not provide detail election data for 22 cities that were used in Levitt (1997). Therefore, new election measure is only provided for 37 of the 59 cities in Levitt original sample.

and 17 percent between the ages of 15 to 24. There are 37 cities that belong to both Levitt and McCrary sample which included 59 cities with the population greater than 250,000 residents.[54] The sample also includes 77 cities where the population exceeds 150,000 residents over the sample period.

II.   Elections and Police

Estimating the effects of police on crime suffers from an identification problem which produces bias estimates. To combat this problem, elections years have been used to estimate a causal relationship between police and crime. Levitt highlights reasons why mayoral elections influence the size of the police department. First, crime is major political issues. Elections can be won or lost based on the public perception of how the city is handling crime. Secondly, unlike the economy, the mayor can actually influence the size of the police and resource devoted towards security and protection of residents.

Using new data on mayoral elections I will first estimate the effects of election year on the size of the police using the following regression:

(3.1) $\Delta \ln P_{it} = \gamma_i + \alpha_t + \theta_1 M_{it} + \theta_2 G_{it} + X_{it}\beta + \varepsilon_{i,t}$

where P_{it} is the annual number sworn officers per capita in city i in year t (t= 1970, 1971,...2000); γ_i is a set of city effects, α_t is a set of year effects. M_{it} is a mayoral election year indicator that is equal to one if the year is an election year for mayor. G_{it} is a gubernatorial election year indicator that is equal to one when there is an election for governor. X_{it} includes public welfare spending per capita, education spending per capita, state unemployment rate, percent of the population between the ages of 15 to 24, the percentage of the population that is black, the percentage of female-headed households, region fixed effects, and city size indicators.

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[54] Levitt (1997) and McCrary (2002) sample was selected for all cities that population exceeded 250,000 residents between 1970 and 1992. Summary Statistics for crime categories are similar to those published by Levitt. Replications of Levitt's and McCrary's estimates are similar and available upon request.

City size indicator refer to 7 groupings of city population; $y<50,000$, $50,000\leq y<75,000$, $75,000\leq y<100,000$, $100,000\leq y<250,000$, $250,000\leq y<500,000$, $500,000\leq y<1,000,000$, $y\geq1,000,000$.

Table 3.2 reports coefficients from estimating equation 3.1.[55] The first column only include year fixed effects, while column 2 and 3 add covariates, city-size indicators, and city fixed effects respectively. In all three columns there exist a positive and statistically significant relationship between campaign years and number of police officers available. Also, all three indicate a strong positive relationship between changes in the number of sworn police officers available and gubernatorial elections. Adding covariates and other fixed effects in columns 2 and 3 slightly increase the point estimates and standard errors remain relatively unchanged.

Columns 4 and 5 report estimates of equation 3.1 using Levitt's & McCrary's measure of election years. All three measures differ drastically from each other. Levitt's measure of election cycles is more closely align to the measure provided by Ferreira and Gyourko with 23 of the 37 cities providing the exact same measure of election years. McCrary measure matches the Ferreira and Gyourko for 14 of the 37 cities that belong to Levitt's original sample. According to column 4, Levitt's measure produces a statically significant relationship between police and elections that is half the size of column 3. Although, McCrary estimate differs greatly from Ferreira and Gyourko, the estimated impact of elections on changes in police is similar to column 3. Moreover, Levitt's election measure produces an estimate of elections influence on police similar to the published estimate while Ferreira and Gyourko as well as McCrary estimate produces a much larger effect.

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[55] Standard errors reported in table 3.2 are constructed from heteroskedasticity-robust standard error clustered by city.

Compared to Levitt published estimates, new election data increases the magnitude of the influence of election years on police hiring by over 90 percent. However, this is not a proper comparison considering the different samples. Table 3.3 attempts to rectify this discrepancy by using Levitt's and McCrary's measure of elections to complete the sample. Columns 1 uses Levitt's measure of elections for the 22 missing cities while column 2 uses McCrary's measure of election years. Columns 3 and 4 use Levitt's and McCrary's measure for all 59 cities to replicate Levitt's published estimates. All four columns show a statistically significant positive relationship between campaign years and changes in the number of police officers. Estimates are larger when Levitt's election years are used to fill in the missing cities but are smaller than columns 1 through 3 in table 3.2. The effect of election cycles on police hiring in column 1 is 56 percent larger than published estimates. Replication of Levitt's first stage regression produced smaller effects but is within one standard deviation of the published estimates.[56]

With respect to gubernatorial election, all columns report a positive statistical relationship between governor elections and changes in local police. Similar to mayoral elections, the relationship between gubernatorial elections and changes in police force was larger in table 3.2. It is reasonable to believe the governors pool resources to affect the greatest number of votes. Contributing state funds to increase police presence in larger cities would likely be covered by local media and influence voters opinion about incumbent governor or political party justifying the positive relationship.

The positive relationship between changes in the number of police and election exist due to political positioning. The incumbent mayor's goal is to obtain credit for reducing crime or to appear tough on crime. The incentive is re-election for themselves or another party member or future political endeavors. For this relationship to hold politically, police must be an effect

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[56] Crime data used in McCrary (2002) also provide similar first stage results using Levitt's and McCrary measure.

instrument in reducing crime. For this to be a successful campaign maneuver, crime must drop

preferably before the election. The presence of additional police reduces crime by serving as a

physical deterrent for crime. With additional police officers, police departments can focus on

high crime areas and increase response time to victim calls serving as a deterrent. Secondly,

more policing should lead to more arrest. Criminals are now in jail and unable to commit future

crimes.   The incapacitation effect as well as the physical deterrent should indirectly make

elections years negatively related to crime. The combination of these effects with the political

pressure from high stake elections would reflect in lower crime rates. To test this hypothesis, I

will follow Levitt's (1996) and run the following regression:

(3.2) $\qquad \Delta \ln C_{ijt} = \omega_i + \sigma_{tj} + \delta_{1j} M_{it} + \delta_{2j} G_{it} + X_{it} \rho_j + \varepsilon_{ijt}$

where C_{ijt} is represent the crime per capita in city i in year t (t= 1970, 1971,...2000) for crime

category j (murder, rape, robbery, assault, burglary, larceny, motor vehicle theft, violent crime,

property crime). Similar to equation 3.1, γ_i is a set of city effects, α_t is a set of year effects.

M_{it} is a mayoral election year indicator and G_{it} is a gubernatorial election year. X_{it} includes the

same covariates from equation 3.1. Also, equation 3.2 is estimated by using seemingly unrelated

least squares where I estimate the effects of elections on the 7 categories of crime

simultaneously. For estimating the effect of elections on violent crimes, δ_{1j} and δ_{2j} is restricted

to be equal for murder, rape, aggravated assault, and robbery. For property crimes δ_{1j} and δ_{2j} is

restricted to be the same for burglary, larceny, and motor vehicle theft.

Table 3.4 reports δ_{1j} and δ_{2j} for equation 3.2. In general, the estimates are mostly

negative but not statically significant. For Ferreira and Gyourko election measure, murder rate is

3 percent lower in mayoral election years than non-mayoral election years and violent crime is

1.1 percent lower. This relationship remains when Levitt's measure of elections years are used

to complete the sample but disappears when using McCrary election measure. In general the incomplete and complete samples produce estimates consistent with the hypothesis stated above. For both of these samples estimates of election years influence on violent and property crimes are negative or close to zero.

III. Police and Crime

Table 3.4 indicates a negative relationship between election years and crime, and table 3.2 estimates clearly identify a positive relationship between mayoral election cycles and changes in the number of police officers. Using these relationships, I will proceed with estimating the effect of police on crime by using two-stage least square, using mayoral and gubernatorial election years as an instrument. I will estimate the same equation from Levitt's (1996)

(3.3) $\quad \Delta \ln C_{ijt} = \lambda_i + \gamma_{tj} + \beta_{1j} \Delta \ln P_{ijt} + \beta_{2j} \Delta \ln P_{ijt-1} + X_{it}\eta_j + \varepsilon_{ijt}$

Once again, C_{ijt} is represents crime per capita in city i and year t for crime category j. Similar to equation 3.1, λ_i is a set of city effects, γ_{tj} is a set of year effects for crime category j. P_{ijt} is the number of sworn police officers, and X_{it} includes the same covariates from equation 3.1.

Table 3.5 reports $\beta_{1j} + \beta_{2j}$ for the seven crime categories using ordinary least squares and two stage least squares estimation. Each column represents estimates from equation 3.3 using different measures of election years as instruments. The first column reports OLS estimates, column 2 uses Ferreira and Gyourko measurement for election years as instruments, columns 3 and 4 use Levitt's and McCrary's election measurements respectively. Pooled estimates report $\beta_{1j} + \beta_{2j}$ for the violent and property crimes using OLS and 2SLS estimation. The pooled estimates restrict $\beta_{1j} + \beta_{2j}$ to be the same for all violent crimes and for all property crimes.

Ordinary least squares estimates in column 1 are negative except for assaults. The coefficient on murder, robbery, burglary, and motor vehicle theft is statistically significant in column 1. The ordinary least squares estimates imply a 10 percent increase in the number of police officers would decrease the murders by 6.5 percent, robberies by 3.5 percent, burglaries by 2.1 percent, and motor vehicle theft by 2.6 percent. Two stage least squares estimates in column 2 through 4 are negative for murder and burglary. The other five crime categories report both positive and negative effects of police on crime depending on the instrument used in the first-stage. In general, 2SLS coefficients are larger in absolute value but none are statically significant. Although pooled estimates of the effect of police on violent and property crime are consistently negative, the relationship between police and crime is not supported by 2SLS estimates.

Table 3.6 expands the sample to include every city in Levitt's original analysis. The sample is expanded by using Levitt's measure of election year in column 2 and McCrary measure in column 3 for the 22 missing cities. Column 1 reports OLS estimates from equation 3.3, while columns 2 and 3 report 2SLS estimates. Ordinary least squares estimates of the elasticity of police on crime are negative and larger than OLS estimates reported in table 3.5. According to the pooled estimates, a 10 percent increase in police reduces violent crime by 2.9 percent and property crime by 2.7 percent. Although OLS estimate are larger, 2SLS estimates are in general smaller than reported in table 3.5. Levitt measure of election timing results in larger pooled estimates for violent and property crime compared to McCrary measure. Overall, 2SLS estimates are similar to published estimates by McCrary (2002) when Levitt's election measure is used. In this case, using new mayoral data does little to settle the debate between Levitt and McCrary for cities with the population greater than 250,000 residents.

Although the new measure does not provide drastically different 2SLS estimates, new mayoral data can expand the sample to include more cities and possibly provide more precise estimates of the elasticity of police on crime. To do this, I relax the sample restriction of cities with the population greater than 250,000 and include cities where the population exceeds 150,000 residents anytime between 1970 and 2005. Mayoral data provided by Ferreira and Gyrouko include 18 cities that meet this criteria. Table 3.7 report estimates of equation 3.3 when the sample is increased to include these 18 cities. Similar to previous tables, column 1 and 2 report OLS and 2SLS estimates for the 55 cities (37 in both sample plus 18 additional cities) in Ferreira and Gyrouko sample with the population greater than 150,000. Column 3 and 4 increases the sample by including the 22 cities in Levitt's original sample that are not included in Ferreira and Gyrouko new data of mayoral elections.

Ordinary least squares estimates in column 1 are negative but only statistically significant for murder and robbery. The pooled OLS estimates in column 1 also are negative and statistically significant. Two stage least squares estimates in column 2 are negative but not statistically significant for the individual crime categories. However, the pooled estimate for violent crime in column 2 is large and statistically significant. Also, 2SLS estimate in column 2 are larger in absolute value than OLS estimates in column 1. The estimated effect of police on crime is more than twice the size reported by McCrary (2002). Further expanding the sample to include all the cities in Levitt's original analysis produces even larger pooled estimates for violent crimes. Once again, Levitt's measure of election timing produces larger pooled estimates than when McCrary's measure is used. Including the additional 22 cities increase the size of the coefficient in absolute value by 67 percent when Levitt's election measure is used. According to column 3, increasing the number of police by 10 percent reduces violent crime by 28.4 percent.

IV.   Discussion

The key difference in these estimates compared to Levitt's and McCrary is the sample of cities with the population greater than 250,000 residents and the mayoral election measurement. The Ferreira and Gyrouko sample in this analysis contains 37 of the 59 cities in Levitt's original sample. In attempt to address this problem, I used Levitt and McCrary measurement of election years for the cities missing from the sample.   Levitt does not provide the source of the election data but McCrary election variable was created from the World Almanac and the Municipal Yearbook. Combining the election measures to complete the sample produces estimates of the elasticity of police on crime that are not statistically significant for the 59 cities. However, large statistically significant effects are produce when the sample increases to 77 cities. The sample is increased by lowering the population restriction from 250,000 residents to 150,000 residents. It is important to note that not all cities that met the criteria were included in the analysis. Only cities that met the criteria and responded to the survey conducted by Ferreira and Gyrouko were included.[57]

Further expanding the sample to include cities with the population greater than 100,000 residents or increasing the sample period to 2005 reduces pooled 2SLS estimates and the elasticity of police on crime becomes indistinguishable from zero. Table 3.8 report OLS and 2SLS estimates for pooled estimates of police on violent and property crime.   Column 1 &2 report estimates for the 37 cities that belong to both samples for the period between 1970 and 2005. Columns 2 and 3 include cities with the population greater than 150,000 and also expand the sample period to 2005. Lastly, columns 5 and 6 include cities with the population greater than 100,000 and the sample period is 1970 to 1992. Once again, OLS estimate are negative and

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[57] Plano, TX; Springfield, MO; Fontana California, Chandler, AZ; and Colorado Springs, CO are missing are missing from the sample of cities with the population greater than 150,000 residents.

statistically significant in columns 1, 3, and 5 but 2SLS estimates are greatly reduced and statistically insignificant.

Although the strength and precision of 2SLS estimates do vary by sample size and sample period, it is clear that election cycles do influence police hiring. Moreover, McCrary is correct in fact that additional cities do aid in obtaining precise estimates of the causal effect of police on crime. It is difficult to ascertain if the precise estimates are a result of relaxing sample restrictions or a different measurement of election cycles. However, it clear that there is a negative relationship between crime and police.

The negative relationships between police and crime in theory existed since Becker and Ehrlich produces economic models of crime. However, literature in economics and crime has produced mixed results as it relates to negative elasticities of police on crime. In fact many papers have found the existence of positive relationship between police and crime (Cameron, 1998; Marvel and Moody, 1996). However, research literature using quasi-experimental research designs have found elasticities similar in size to table 3.7 (Di Tella & Schargrodsky, 2004; Klick and Taborrok, 2005; Evans & Owens, 2006). The quasi-experimental research design solves the problem of simultaneity bias but McCrary (2013) suggest that the simultaneity bias has smaller effect than previously perceived. According to McCrary, by addressing measurement error in police staffing, more precise estimates of police on crime can be estimated. Although elections years identify variation in police staffing, imprecise estimates of crime and police are still persistent when using a larger sample of cities over a longer horizon. Moreover, election years may not just influence the size of the police. Mayors and elected officials can also influence police effort, change policing strategies, and even influence the reporting of crime (Couzens & Seidman, 1974).

Figure 1.1. The Locations of Legal Services Projects, 1965-1975
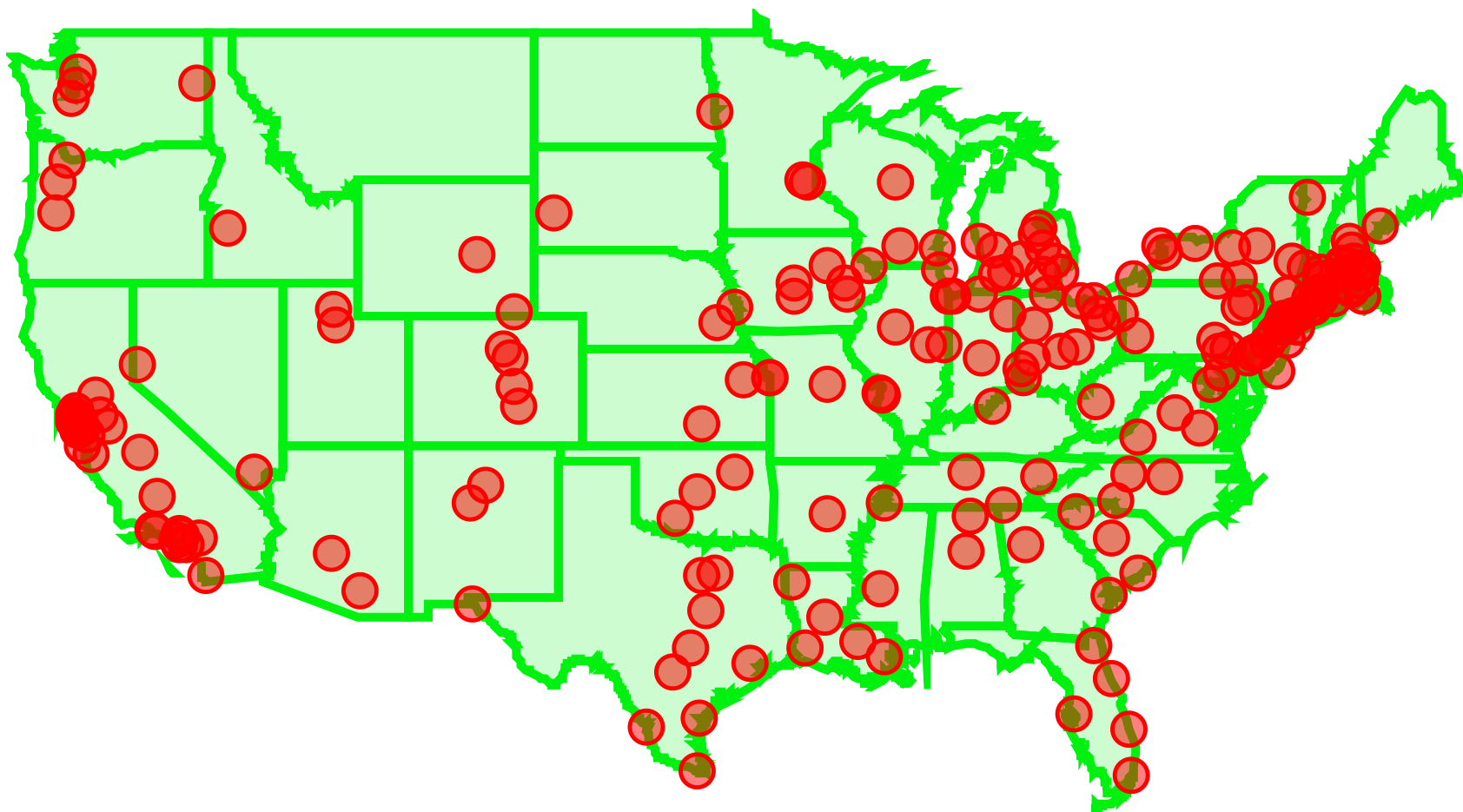
Source: The National Archives Community Action Program (NACAP) provided information on the recipients of legal service grants funded by the Office of Economic Opportunity between 1964 and 1975.

Figure 1.2. Crime Rates before the Legal Service Program Began

A. *Δ in Log of Total Crime 1960-1964*
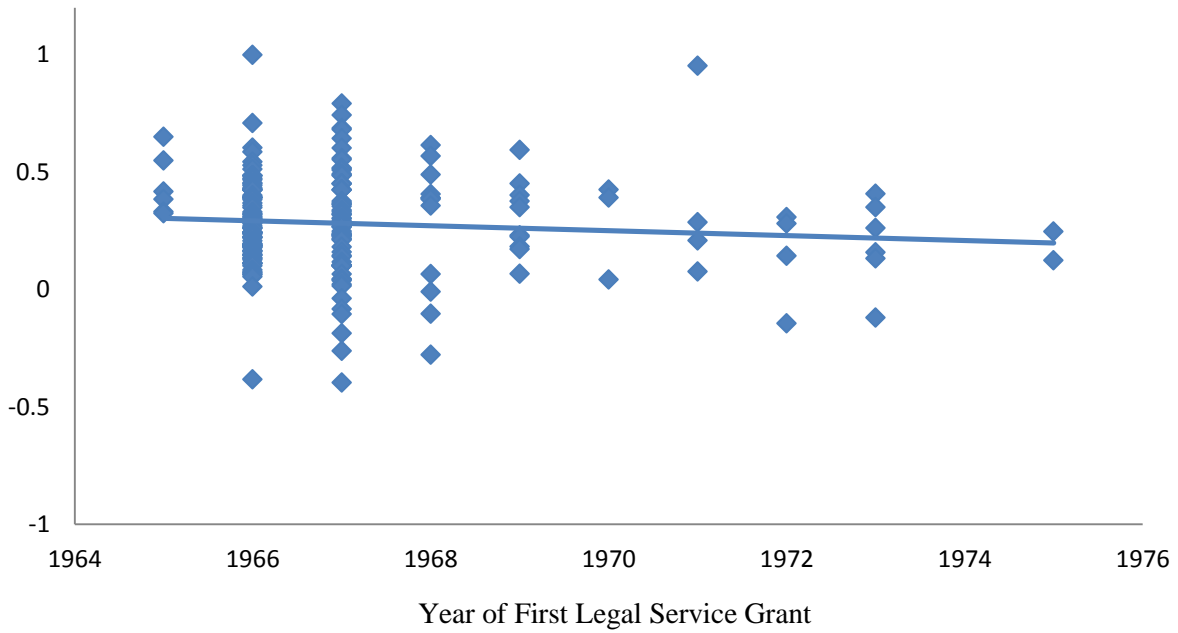
B. *Log of Total Crime per 100,000 Residents 1964*
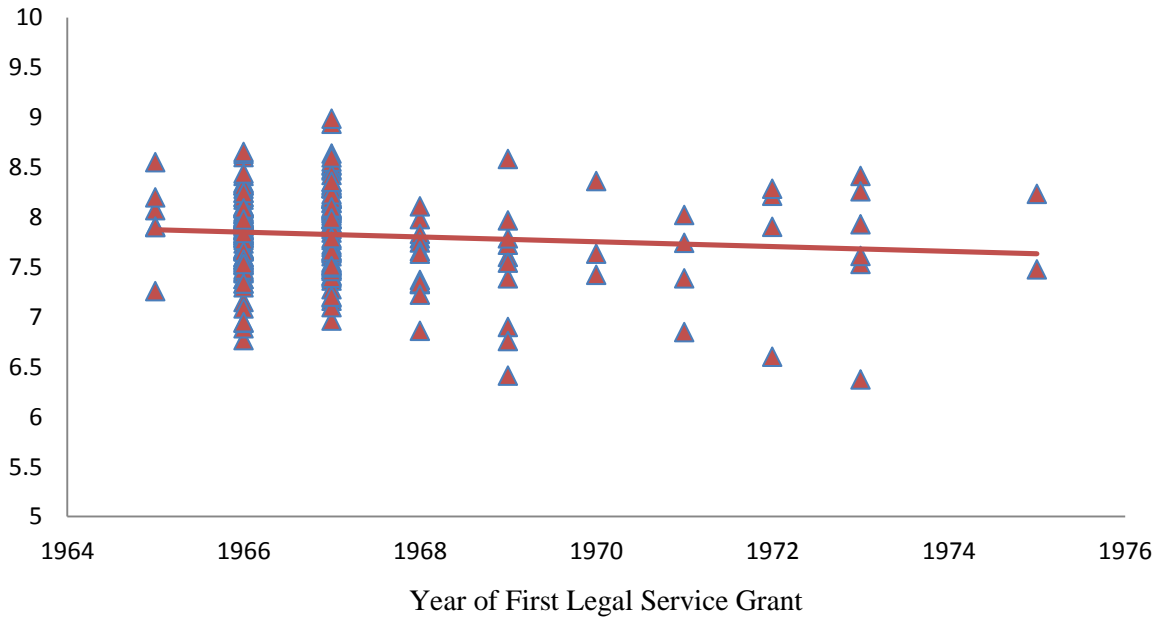
Notes: Panel A & B: Regression coefficients and predicted values are from univariate regressions of the dependent variable crime on the year LSPs were established. The slope in panel A is -0.011 (0.0073) and panel B -0.0249 (.02082).

Figure 1.3. Estimates of the Effects of Legal Service Grants on Log of Total Crime Per 100,000 Residents
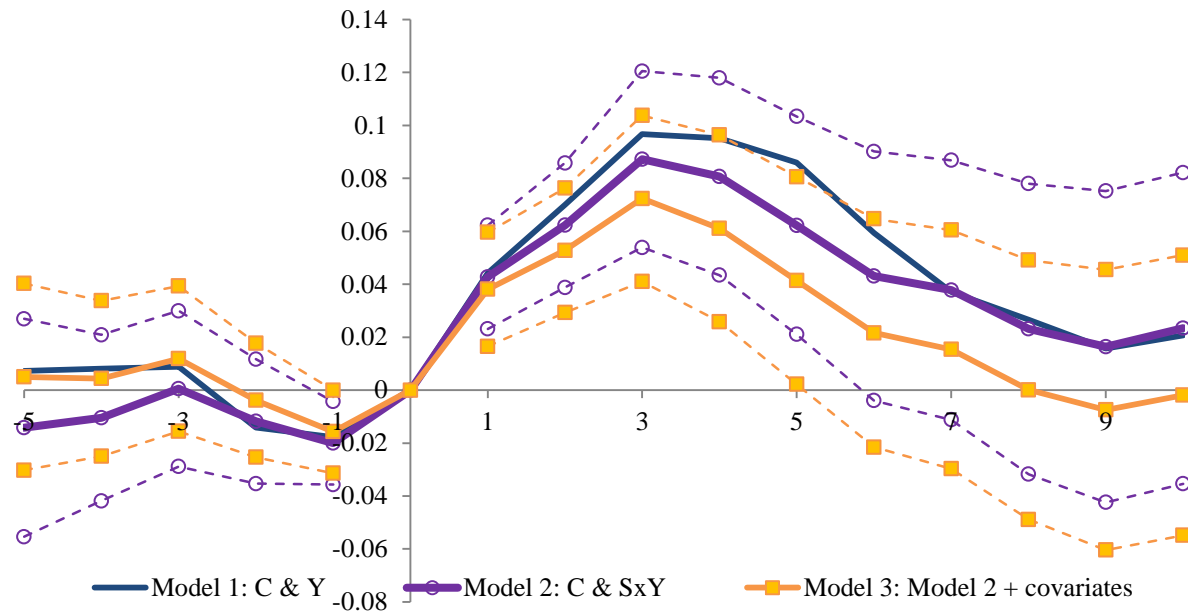
Figure 1.4. Estimates of the Effects of Legal Service Grants on Log of Total Arrest Per 100,000 Residents
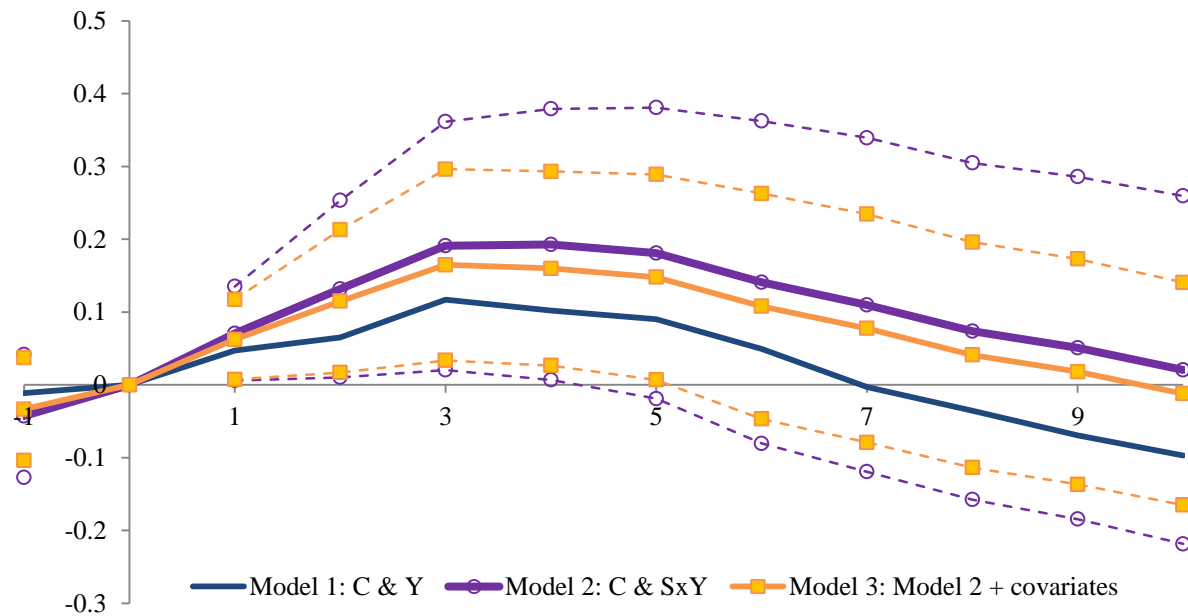
Notes: Panel A & B: Model 1 includes City, C, and year, Y, effect. Model 2 include city and state-by-year, S-Y, effects. Model 3 adds covariates from county and city data book, X, to model 2. Covariates include median household income, percentage of population under age of 5, percentage of the population over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, which are from the decennial census. Heteroskedasticity-robust standard errors clustered by city are presented for model 2 & 3. Each regression is weighted by 1985 population and excludes New York, Chicago, and Los Angeles. Panel B displays Model 3 for Log of Property crime and Log of Violent Crime Per 100,000 Residents.

Figure 1.5. Estimates of the Effects of Legal Service Grants on Log of Sworn Police Officers Per 100,000 Residents

Model 1: C & Y   Model 2: C & SxY   Model 3: Model 2 + covariates
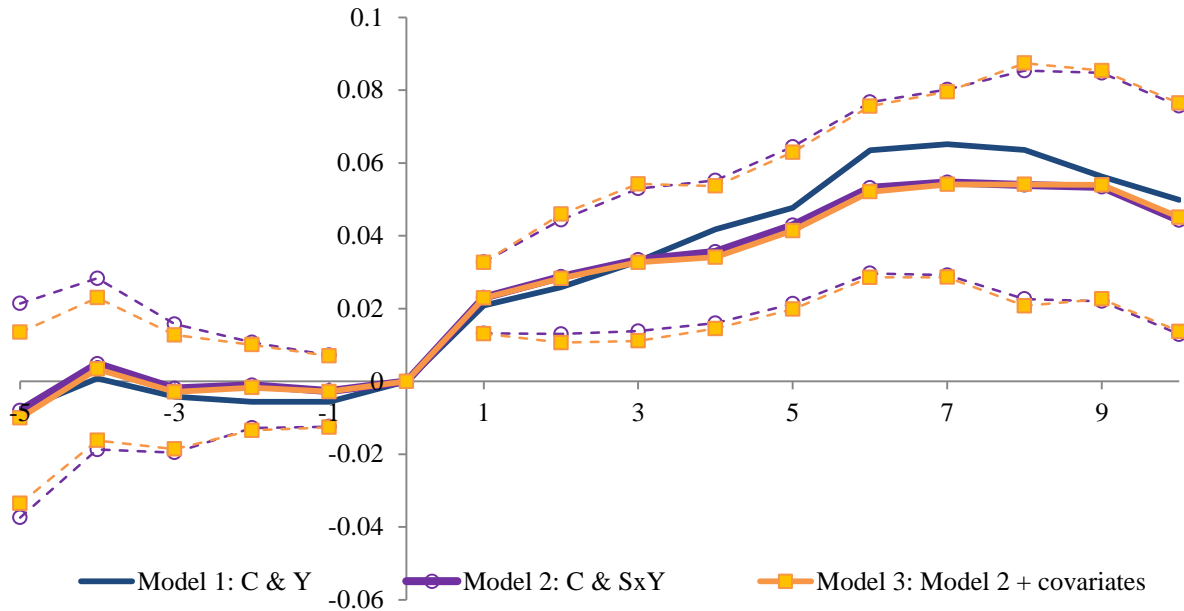
Figure 1.6. Estimate of the Effects of Legal Service Grants on Log Property and Violent Crime Per 100,000 Residents

Model 3: Property   Model 3: Violent

Notes: Model 1 includes City, C, and year, Y, effect. Model 2 include city and state-by-year, S-Y, effects. Model 3 adds covariates from county and city data book, X, to model 2. Covariates include median household income, percentage of population under age of 5, percentage of the population over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, which are from the decennial census. Heteroskedasticity-robust standard errors clustered by city are presented for model 2 & 3. Each regression is weighted by 1985 population and excludes New York, Chicago, and Los Angeles. Panel B displays Model 3 for Log of Property crime and Log of Violent Crime Per 100,000 Residents.

Figure 1.7. Estimates of the Effects of Legal Service Grants on Log Crime Per 100,000 Residents

A. Murder

B. Rapes and Robbery

C. Burglary and Motor Vehicle Theft

D. Assault and Larceny
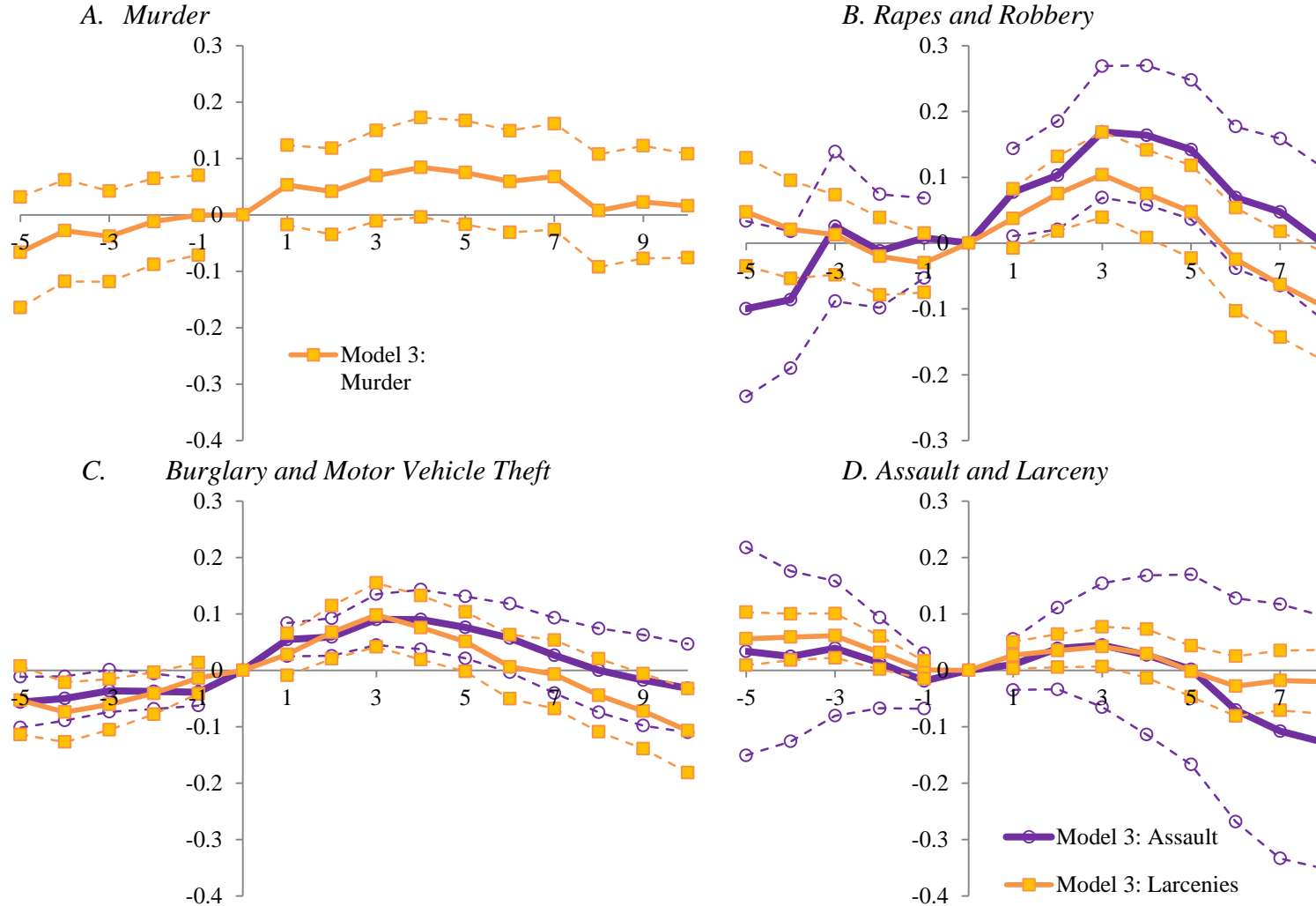
Notes: Panels presents estimates from model3 which include city, state-by-year, S-Y, effects, and adds covariates from county and city data book, X. Covariates include median household income, percentage of population under age of 5, percentage of the population over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, which are from the decennial census. Heteroskedasticity-robust standard errors clustered by city are presented for model 3. Each regression is weighted by 1985 population and excludes New York, Chicago, and Los Angeles.

Figure 1.8. Estimates of the Effects of Legal Service Grants on Log Crime Per 100,000 Residents for Non-Rioting Cities

A. Log of Total Crime

B. Log of Murder

C. Log of Motor Vehicle Theft

D. Log of Rape

Notes: Model 1 includes City, C, and year, Y, effect. Model 2 include city and state-by-year, S-Y, effects. Model 3 adds covariates from county and city data book, X, to model 2. Covariates include median household income, percentage of population under age of 5, percentage of the population over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, which are from the decennial census. Heteroskedasticity-robust standard errors clustered by city are presented for model 2 & 3. Each regression is weighted by 1985 population and excludes New York, Chicago, and Los Angeles. Panel B displays Model 3 for Log of Property crime and Log of Violent Crime Per 100,000 Residents

Figure 1.9. Estimates of the Effects of Legal Service Grants on Log of Sworn Police Officers Per 100,000 Residents

Notes: Model 1 includes City, C, and year, Y, effect. Model 2 include city and state-by-year, S-Y, effects. Model 3 adds covariates from county and city data book, X, to model 2. Covariates include median household income, percentage of population under age of 5, percentage of the population over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, which are from the decennial census. Heteroskedasticity-robust standard errors clustered by city are presented for model 2 & 3. Each regression is weighted by 1985 population and excludes New York, Chicago, and Los Angeles. Panel B displays Model 3 for Log of Property crime and Log of Violent Crime Per 100,000 Residents.

Table 1.1. Descriptive Statistics from Legal Service Agency Survey

Variable	Observations	Mean	Std. Dev	Min	Max
Staff					
R.H. Smith Fellows	189	0.86	1.71	0	10
VISTA Attorneys	189	0.25	0.82	0	7
Law Students	189	3.35	10.57	0	99
Professional Support Personnel	189	2.30	8.25	0	99
Clerical Support Personnel	189	6.13	6.12	1	48
Attorneys	189	5.66	6.30	0	36
Other Personnel	189	1.09	2.14	0	16
Planned Participants					
Age 0 - 5	189	16	123	0	1300
Age 6 - 15	189	50	186	0	1500
age 16 - 21	189	205	459	0	2500
age 22 -44	189	740	1,537	0	10000
age 45 - 54	189	234	579	0	4000
age 55- 64	189	129	381	0	2830
age 65+	189	80	249	0	2180
Above Poverty Level	189	240	626	0	4435
$10-$499 Below Poverty Level	189	351	875	0	7500
$500-1499 Below Poverty Level	189	352	1,038	0	11320
$1500 or More Below Poverty Level	189	300	775	0	6550
Mexican American	189	93	362	0	2853
Puerto Rican	189	80	296	0	2646
White	189	544	1,133	0	7185
Black	189	458	1,393	0	8662
Native	189	52	486	0	6540
Asian	189	10	95	0	1300
Other	189	11	97	0	1300
Participants per Year	189	1,974	3,499	0	20000
Total Budget per Year	189	203,509	233,460	14,856	1,558,209

Source: The data in the Legal Service Agency Survey (LSAS) 1970 survey was collected from the Auerbach Corporation for an evaluation of OEO legal service projects (LSP) in 1970 and 1971. Data publicly available at the ICPSR.

Table 1.2. Descriptive Statistics from Legal Service Agency Survey

Variable	Observations	Mean	Std. Dev	Min	Max
Overall Attorney Performance					
Quality-Individual Services	186	2.05	0.53	1	3
Quality-Staff Competence	183	2.25	0.43	1	3
OLS Goals Activity Quantity	186	1.92	0.66	1	3
OLS Goals Activity Quality	186	1.90	0.63	1	3
Attorney Performance Overall					
Understanding OLS Goals	185	2.26	0.49	1	3
Commitment to Poverty Community	185	2.28	0.49	1	3
Professional Development - Reading	185	2.36	0.51	1	3
Professional Development - Conferences	184	1.80	0.55	1	3
Overall Effectiveness	185	2.11	0.42	1	3
LSP image in the Community					
Relevance of Legal Service Program Activities	186	2.21	0.38	1	3
Assessment of LSP Results	186	2.33	0.37	1	3
LSP Staff Competence	186	2.42	0.38	1	3
Meeting of LSP Goals	186	2.24	0.40	1	3
Opinion Validity	186	2.22	0.32	1	3
Legal Climate - Law Enforcement					
Relation to Community	186	1.54	0.44	1	3
Relation with Minorities	185	1.44	0.45	1	3
Effect on LSP	186	1.93	0.38	1	3
Legal Climate - Courts Attitudes					
Attitude to Poverty Issues	186	1.87	0.39	1	3
Disposition of Cases	186	2.08	0.36	1	3
Attitude to LSP	186	2.10	0.40	1	3
Disposition of LSP Cases	186	2.18	0.39	1	3

Source: The data in the Legal Service Agency Survey (LSAS) 1970 survey was collected from the Auerbach Corporation for an evaluation of OEO legal service projects (LSP) in 1970 and 1971. Data publicly available at the ICPSR.

Table 1.3. 1960 Characteristics of Cities from 1960

A. 1960 City Characteristics	All Cities (N=606)	Received Grant from 1965-1975 (N=208)	Non-Grants Cities (N=398)
Means			
Population	98,515	190,585	50,397
Population per square mile	6,662	7,525	4,958
Median Income	6,004	5,868	6,273
Proportion of population in cities			
in Northeast	22.0	23.7	18.6
in Midwest	30.7	29.6	33.0
in South	29.9	28.7	32.2
in West	17.4	18.0	16.2
Proportion of residents			
men between 15 and 24 years of age	6.6	6.5	6.7
men between 25 and 39 years of age	10.0	10.0	10.0
Nonwhite	12.9	14.4	10.1
with 12 years of education	43.2	41.5	46.7

Source: Table displays weighted averages from the 1960 Decennial Census. Census data from 1962 County and City Data Book publicly available at the ICPSR.

Table 1.4. Summary Statistics between First Legal Service Grants and Offenses Reported

Criminal Offenses 1960-1964	All Cities (N=606)	Received Grant from 1965-1975 (N=208)	Non-Grants Cities (N=398)
Mean Per 100,000 Residents			
Violent Crimes	222	505	59
Murder	10	23	4
Rape	13	25	6
Assault	110	252	35
Robbery	82	205	17
Property Crime	2,829	5,891	1,230
Burglary	724	1,543	295
Larceny	1,773	3,585	827
Motor vehicle theft	333	763	108
Total	3,041	6,396	1,288
Growth from 1960 to1964			
Violent Crimes	0.590	0.644	0.562
Property Crime	0.294	0.267	0.308
Total	0.295	0.285	0.299

Source: Table averages are from the UCR from 1960 to 1964. Criminal offenses reported in the UCR are from the Uniform Crime Report Offenses Known and Cleared. UCR data are publicly available at the ICPSR.

Table 1.5. The Relationship between First Legal Service Grants and 1960 Census Demographics

	(1)	(2)
Dependent Variable:		
Year of first federal legal service grant		
Law School in County	-0.353	-0.664
	[0.307]	[0.345]
Medical School in County	-0.209	-0.0757
	[0.285]	[0.286]
median income	1.848	1.346
	[1.606]	[1.608]
population per square mile	-0.363	-0.164
	[0.234]	[0.180]
Proportion of residents		
with 12 years of education	0.369	0.702
	[0.834]	[0.671]
non-white	-0.260	-0.168
	[0.193]	[0.210]
men between the age of 15 and 24 years of age	1.072	0.562
	[0.757]	[0.766]
men between the age of 25 and 39 years of age	-0.189	0.0160
	[1.967]	[2.148]
Weighted		X
State fixed effects	X	X
Observations	208	208
R-squared	0.468	0.494

Note: Each column reports estimates from a separate linear regression. Heteroskedasticity-robust standard errors are corrected for clustering with state and presented in brackets. Independent Variables are from the 1960 Decennial Census. Columns 1 & 2 use 1960 population as weights.

Table 1.6. Event Study Pre-Treatment Effects for Log of Total Crime by Sample and Weights

	(1)	(2)	(3)	(4)
DV: Total Crime per 100,000 Residents				
Years Before Treatment				
-5	-0.00211	0.0202	-0.0241	-0.0230
	[0.0208]	[0.0217]	[0.0192]	[0.0192]
	0.11	*0.96*	*1.27*	*1.21*
-4	0.00188	0.0177	-0.0197	-0.0187
	[0.0163]	[0.0185]	[0.0174]	[0.0172]
	0.12	*0.98*	*1.16*	*1.10*
-3	0.00846	0.00852	-0.0149	-0.0149
	[0.0156]	[0.0170]	[0.0161]	[0.0159]
	0.56	*0.50*	*0.93*	*0.99*
-2	-0.00495	-0.0126	-0.0186	-0.0194
	[0.0128]	[0.0127]	[0.0135]	[0.0134]
	0.41	*1.05*	*1.43*	*1.49*
-1	-0.0170	-0.0354	-0.0152	-0.0176
	[0.00814]	[0.00903]	[0.00969]	[0.00979]
	2.13	*3.93*	*1.69*	*1.96*
Include NYC, CHI, LA		X		X
Weighted Least Squares	X	X		
Observations	15,756	15,834	15,756	15,834

Notes: Table display least-squares estimates obtained from estimating equation 1.1. All columns corresponds to model 3 and includes state-by-year, S-Y, effects and add 1960, 1970, and 1980 city linearly interpolated. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. T-ratios are presented under standard errors. In columns 1 & 2, regressions is weighted by 1985 population and columns 1 & 3 excludes New York, Chicago, and Los Angeles.

Table 1.7. Event Study Post-Treatment Effects for Log of Total Crime by Sample and Weights

	(1)	(2)	(3)	(4)
DV: Total Crime per 100,000 Residents				
Years After Treatment				
1	0.0336	0.0299	0.0238	0.0241
	[0.0106]	[0.00994]	[0.00941]	[0.00921]
	3.36	*3.32*	*2.64*	*2.68*
2	0.0530	0.0472	0.0390	0.0393
	[0.0122]	[0.0111]	[0.0134]	[0.0132]
	4.42	*4.29*	*3.00*	*3.02*
3	0.0737	0.0577	0.0545	0.0538
	[0.0159]	[0.0154]	[0.0153]	[0.0152]
	4.91	*3.85*	*3.63*	*3.59*
4	0.0647	0.0445	0.0468	0.0462
	[0.0177]	[0.0180]	[0.0167]	[0.0166]
	3.81	*2.47*	*2.93*	*2.89*
5	0.0456	0.0213	0.0275	0.0267
	[0.0179]	[0.0191]	[0.0181]	[0.0181]
	2.68	*1.12*	*1.53*	*1.48*
6	0.0197	-0.0111	0.00965	0.00791
	[0.0202]	[0.0212]	[0.0200]	[0.0200]
	0.99	*0.53*	*0.48*	*0.40*
7	0.0109	-0.0212	0.00433	0.00246
	[0.0230]	[0.0234]	[0.0213]	[0.0213]
	0.47	*0.92*	*0.21*	*0.12*
8	-0.00637	-0.0416	-0.00977	-0.0114
	[0.0256]	[0.0255]	[0.0230]	[0.0230]
	0.25	**1.66**	**0.42**	**0.50**
9	-0.0210	-0.0658	-0.0256	-0.0284
	[0.0270]	[0.0282]	[0.0236]	[0.0237]
	0.78	**2.35**	**1.11**	**1.23**
10	-0.0193	-0.0696	-0.0226	-0.0256
	[0.0258]	[0.0290]	[0.0239]	[0.0241]
	0.77	**2.40**	**0.98**	**1.07**
Include NYC, CHI, LA		X		X
Weighted Least Squares	X	X		
Observations	15,756	15,834	15,756	15,834

Notes: Table display post-treatment effects obtained from estimating equation 1.1. In columns 1 & 2, regressions is weighted by 1985 population and columns 1 & 3 excludes New York, Chicago, and Los Angeles.

Table 1.8. Event Study Estimates for Log of Total Crime with Riot Intensity

	(1)	(2)
	DV: Total Crime per 100,000 Residents	
Years Before Treatment		
-5	0.00502	0.00558
	[0.0180]	[0.0181]
-4	0.00439	0.00487
	[0.0153]	[0.0153]
-3	0.0119	0.0119
	[0.0145]	[0.0146]
-2	-0.00381	-0.00397
	[0.0117]	[0.0118]
-1	-0.0157	-0.0150
	[0.00837]	[0.00858]
Years after Treatment		
1	0.0381	0.0328
	[0.0111]	[0.0118]
2	0.0528	0.0515
	[0.0125]	[0.0125]
3	0.0724	0.0719
	[0.0166]	[0.0168]
4	0.0611	0.0611
	[0.0188]	[0.0189]
5	0.0414	0.0414
	[0.0201]	[0.0202]
6	0.0216	0.0217
	[0.0224]	[0.0224]
7	0.0154	0.0155
	[0.0234]	[0.0234]
8	0.000119	0.000154
	[0.0257]	[0.0257]
9	-0.00746	-0.00747
	[0.0278]	[0.0278]
10	-0.00191	-0.00192
	[0.0272]	[0.0272]
Riot Intensity		0.00202
		[0.000623]
Observations	15,756	15,756
R-squared	0.887	0.887
Number of cities	606	606
F test: Pre Treatment	1.634	1.604
Prob > F	0.149	0.157

Notes: Table display least-squares estimates obtained from estimating equation 1.1. Column 1 corresponds to model 3 which in include state by year fixed effects, S-Y, and adds 1960, 1970, and 1980 city linearly interpolated. Column 2 includes a Riot intensity variable is based on data from Carter and Margo (2007). Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. Each regression is weighted by 1985 population and excludes New York, Chicago, and Los Angeles.

Table 1.9. The Relationship between Legal Services and Property Values

	(1)	(2)	(3)
	DV: Log of Median Residential Property Value for All Home Owners		
Legal Service Program	0.0195	0.0346	
	[0.0182]	[0.0169]	
Riot	-0.0887	-0.0574	-0.0581
	[0.0236]	[0.0199]	[0.0193]
Time Since LSP Established			0.00275
			[0.00134]
The Natural Log of			
Median Income		1.289	1.296
		[0.0995]	[0.0988]
% of pop with 12 years of education		-0.601	-0.622
		[0.104]	[0.103]
% of pop Nonwhite	-0.00380	0.00258	0.00239
	[0.00266]	[0.00245]	[0.00240]
Population per square mile	0.112	0.106	0.108
	[0.0414]	[0.0291]	[0.0284]
Observations	1,818	1,818	1,818
R-squared	0.488	0.656	0.657
Number of Cities	606	606	606

Notes: Table display least-squares estimates obtained from estimating equation 1.2. The dependent variable is the log of the median residential property value for all home owners provided in the City and County Data Books from 1962, 1972, and 1983. Covariates are also from the City and County Data Books. Riot indicator variable is based on data from Carter and Margo (2007). All regressions include year fixed effects. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. Each regression is weighted by 1985 population and excludes New York, Chicago, and Los Angeles.

Table 1.10. The Relationship between Legal Services and Property Values by Grant Characteristics

VARIABLES	(1) Below	(2) Above	(3) Below	(4) Above	(9) Below	(10) Above	(11) Below	(12) Above
	Number of Attorneys		Total Budget		Per Capita First Grant		1960 Crime Rate	
	Below	Above	Below	Above	Below	Above	Below	Above
Legal Service Program	0.0656	0.0221	0.0640	0.0289	0.0352	0.0456	-0.0123	0.0674
	[0.0247]	[0.0233]	[0.0243]	[0.0225]	[0.0187]	[0.0198]	[0.0211]	[0.0185]
Riot	-0.0390	-0.0586	-0.0375	-0.0605	-0.0378	-0.0639	-0.0618	-0.0363
	[0.0231]	[0.0199]	[0.0229]	[0.0198]	[0.0206]	[0.0194]	[0.0197]	[0.0219]
Observations	1,401	1,401	1,401	1,401	1,506	1,506	1,506	1,506
R-squared	0.713	0.656	0.703	0.669	0.661	0.688	0.650	0.695
Number of Cities	467	467	467	467	502	502	502	502

Notes: Table display least-squares estimates obtained from estimating equation 1.2. The dependent variable is the log of the median residential property value for all home owners provided in the City and County Data Books. The covariates are not reported but are the natural log of median income, percentage of the population with more than 12 years of education, the percentage of the population non-white, and the population per square mile. All regressions include year fixed effects. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. Each regression is weighted by 1985 population and excludes New York, Chicago, and Los Angeles. Columns 1 & 2 report estimates for treated counties with the number of attorneys per planned participants below/above the median; Columns 3 &4 refers to treated counties with the total budget per planned participant below/above the median; Columns 5 & 6 refers to treated counties with real per capita first grants below/above the median, & Columns 7 & 8 refers to treated counties with 1960 crime rates below/above the median 1960 crime rates in treated counties

Table 1.11. Law Enforcement Legal Climate from the Legal Service Agency Survey

VARIABLES	(1) Effect on LSP	(2) Relation to Community	(3) Relation with Minorities
Time Since LSP Established	0.0392	0.0516	0.0509
	[0.0203]	[0.0233]	[0.0235]
Proportion of Residents			
with 12 years of education	-0.00835	-0.00468	0.00278
	[0.00789]	[0.00904]	[0.00911]
with fewer than 4 years of education	-0.0194	-0.0228	-0.0216
	[0.0136]	[0.0156]	[0.0157]
65 or older	0.0104	0.0667	0.0586
	[0.0257]	[0.0294]	[0.0297]
Under 5 years of age	0.0337	0.0737	0.0877
	[0.0387]	[0.0443]	[0.0448]
Nonwhite	-0.000347	-1.28e-06	-0.00701
	[0.00486]	[0.00557]	[0.00562]
in urban areas	-0.00297	-0.00152	-0.00409
	[0.00216]	[0.00247]	[0.00250]
of households with income under $3,000	0.0139	0.0173	0.0114
	[0.0108]	[0.0124]	[0.0125]
of households with income greater than $10,000	0.0188	0.0236	0.0236
	[0.00969]	[0.0111]	[0.0112]
Observations	186	186	185
R-squared	0.334	0.358	0.374

Source: The data in the Legal Service Agency Survey (LSAS) 1970 survey was collected from the Auerbach Corporation for an evaluation of OEO legal service projects (LSP) in 1970 and 1971. Each column reports estimates from a separate linear regression. All three regressions use state fixed effects. Heteroskedasticity-robust standard errors are corrected for clustering with state and presented in brackets.

Figure 2.1. Divorce and Marriage Rates, 1960-1988

Figure 2.2. The Date of the First Federal Grants for Legal Services, 1965-1975

Figure 2.3. Divorce Rates before the Legal Service Program Began
     A. *Δ in Log of Total Divorce 1960-1964*

Year of First Legal Service Grant
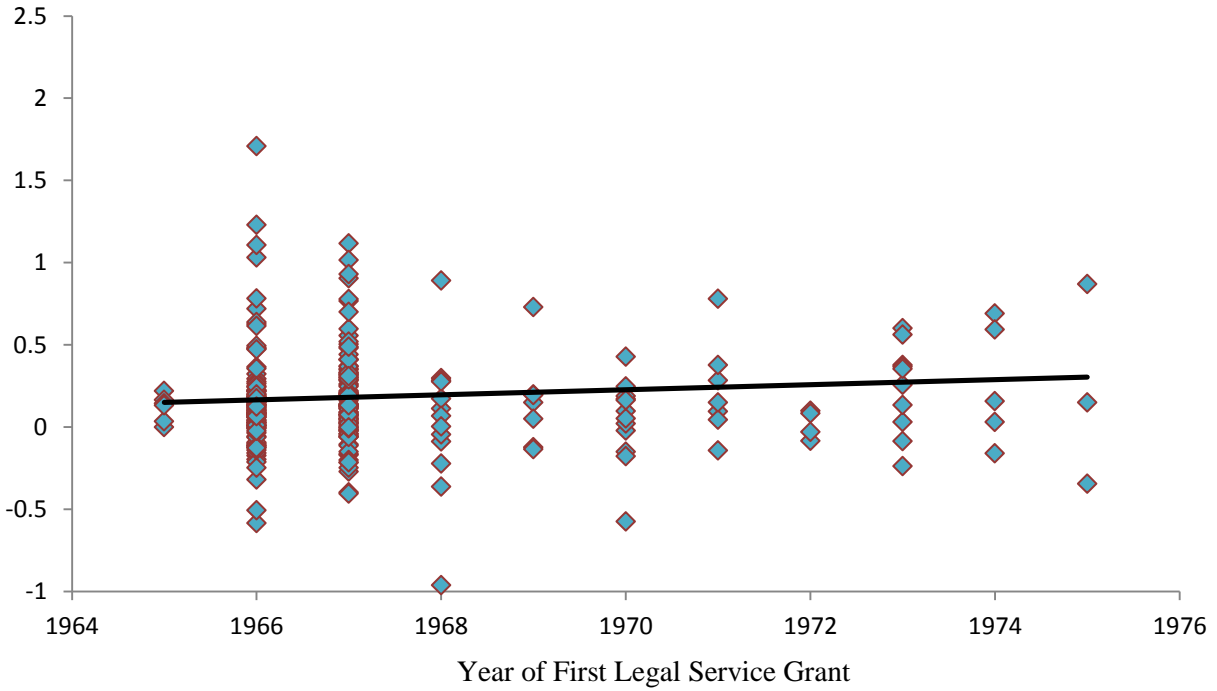
    B. *Log of Divorce per 100,000 Residents 1964*
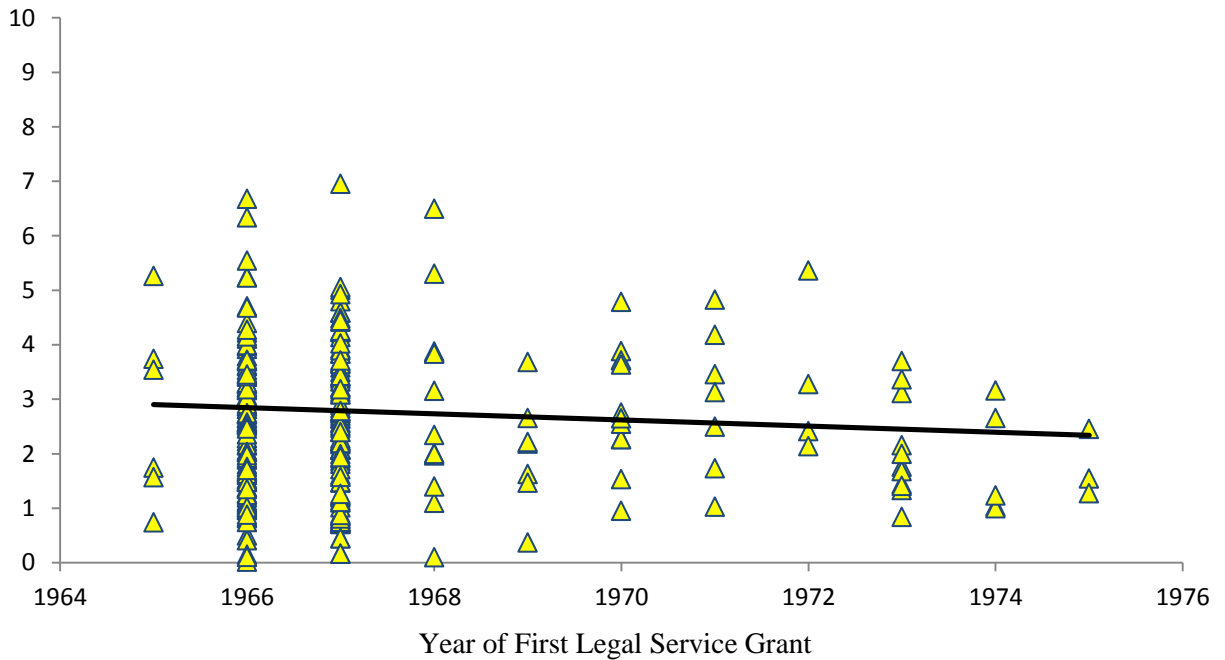
Year of First Legal Service Grant

Notes: Panel A & B: Regression coefficients and predicted values are from univariate regressions of the dependent variable change in divorces and the log of divorces per 100,000 residents on the year LSPs were established. The slope in panel A is 0.18 (0.013) and panel B 0.06 (.081).

Figure 2.4. Estimates of the Effects of Legal Service Grants on Divorces Per 100,000 Residents
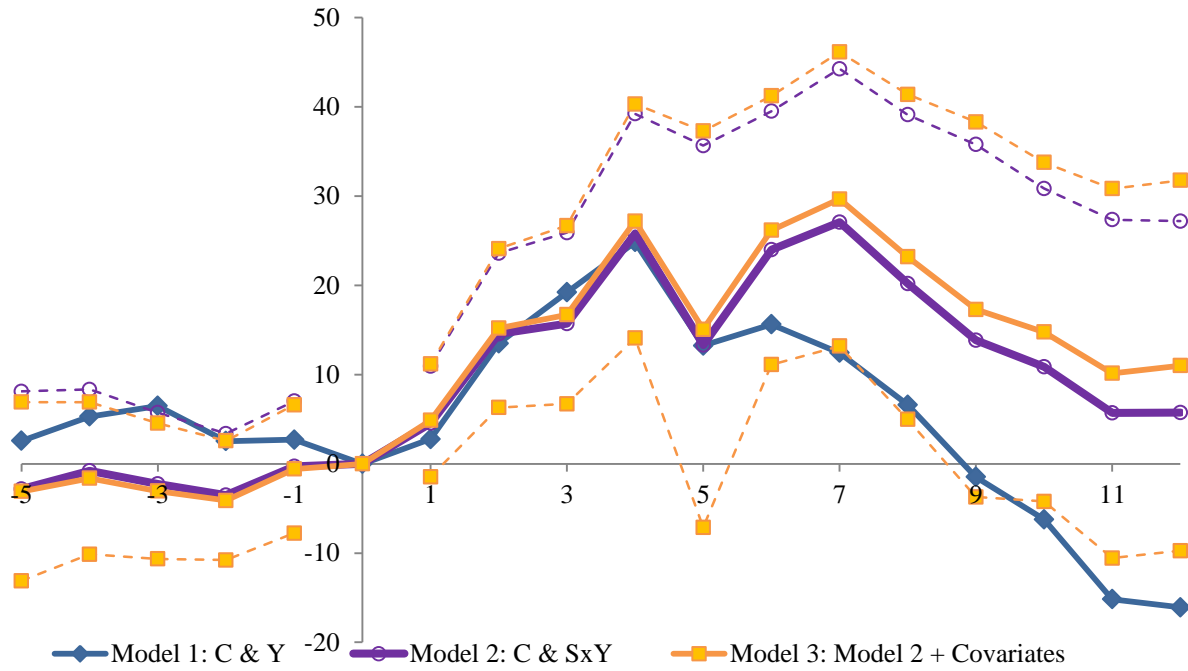
Figure 2.5. Estimates of the Effects of Legal Service Grants by Divorce Law

Notes: Model 1 includes County, C, and year, Y, effect. Model 2 include county and state-by-year, S-Y, effects. Model 3 which adds the interaction of 1960 county characteristics with linear time trend to model 2. Covariates include the percentage of population under age of 5, percentage of the population over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, the percentage of the population with less than 4 years of education, percent of the population in households earning more than 10,000 dollars, percent of the population in households earning less than 3,000 dollars, and the percent of the county urban. Heteroskedasticity-robust standard errors clustered by county are presented for model 3. Weighted regressions are weighted by 1988 county population. . Cook County, Los Angeles County, and New York City are excluded from the sample.

Figure 2.6. Estimates of the Effects of Legal Service Grants on Marriages Per 100,000 Residents

Model 1: C & Y   Model 2: C & SxY   Model 3: Model 2 + Covariates
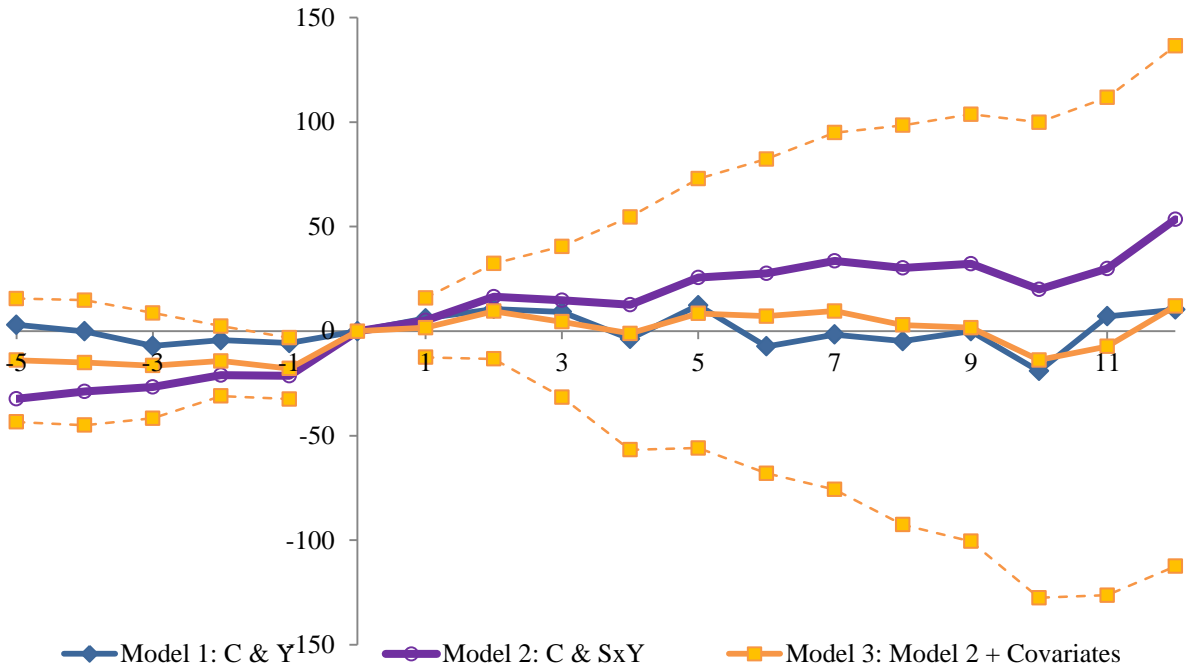
Figure 2.7. Estimates of the Effects of Legal Service Grants on Divorces Per 1,000 Marriages

Model 1: C & Y   Model 2: C & SxY   Model 3: Model 2 + Covariates

Notes: Model 1 includes County, C, and year, Y, effect. Model 2 include county and state-by-year, S-Y, effects. Model 3 which adds the interaction of 1960 county characteristics with linear time trend to model 2. Covariates include the percentage of population under age of 5, percentage of the population over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, the percentage of the population with less than 4 years of education, percent of the population in households earning more than 10,000 dollars, percent of the population in households earning less than 3,000 dollars, and the percent of the county urban. Heteroskedasticity-robust standard errors clustered by county are presented for model 3. Weighted regressions are weighted by 1988 county population. . Cook County, Los Angeles County, and New York City are excluded from the sample.

Figure 2.8. Estimates of the Effects of Legal Service Grants on Cash Public Assistance Per Capita

Notes: Model 1 includes County, C, and year, Y, effect. Model 2 include county and state-by-year, S-Y, effects. Model 3 which adds the interaction of 1960 county characteristics with linear time trend to model 2. Covariates include the percentage of population under age of 5, percentage of the population over the age of 64, percentage of population nonwhite, and the percentage of population with 12 or more years of education, the percentage of the population with less than 4 years of education, percent of the population in households earning more than 10,000 dollars, percent of the population in households earning less than 3,000 dollars, and the percent of the county urban. Heteroskedasticity-robust standard errors clustered by county are presented for model 3. Weighted regressions are weighted by 1988 county population. . Cook County, Los Angeles County, and New York City are excluded from the sample.

Table 2.1. 1960 Characteristics of Counties Receiving Legal Service Grants from 1965-1975

A. County Characteristics from 1960 Census	All Counties	Received Grant from 1965-1975	Non Grant Counties
	(N=3073)	(N=271)	(N=2802)
Mean Population	51,901	296,772	28,314
Proportion of population in counties			
in Northeast	23.1	33.3	12.8
in Midwest	29.1	26.9	31.4
in South	34.5	22.2	46.8
in West	13.3	17.5	9.0
in urban areas	66.4	85.9	46.7
Proportion of residents			
Under 5 years of age	11.5	11.5	11.4
65 or older	9.2	8.8	9.7
Nonwhite	10.9	11.1	10.7
with 12 years of education	42.4	44.1	40.7
with fewer than 4 years of education	8.6	7.0	10.2
of households with income under $3,000	22.6	15.8	29.6
of households with income greater than $10,000	14.1	17.9	10.2
Vital Statistics			
divorces per 1,000 residents	3.8	4.3	3.7
marriages per 1,000 residents	12.0	10.6	12.1
Change from 1960 to 1965			
divorces per 1,000 residents	0.354	0.114	0.377
marriages per 1,000 residents	0.129	0.071	0.134

Note: Table displays weighted averages using 1960 county population as weights. Sources: 1960 County and City Databooks (Haines 2005) and Vital Statistics of the United States Volume III Marriage and Divorce.

Table 2.2. The Relationship between First Legal Service Grants and 1960 Census Demographics

	(1)	(2)	(3)	(4)
Dependent Variable:				
Year of first federal legal service grant				
Law School in County	-0.436	0.0416	-0.698	-0.00703
	[0.356]	[0.131]	[0.378]	[0.156]
Medical School in County	-0.0769	-0.323	-0.366	-0.141
	[0.385]	[0.152]	[0.387]	[0.209]
Proportion of residents in				
with 12 years of education	-0.00476	0.0100	-0.0104	0.0152
	[0.0399]	[0.0104]	[0.0449]	[0.0131]
with fewer than 4 years of education	-0.0840	-0.00745	-0.105	0.0417
	[0.0850]	[0.0227]	[0.105]	[0.0312]
of households with income greater than $10,000	0.0220	-0.00132	0.0452	0.0204
	[0.0570]	[0.0151]	[0.0578]	[0.0181]
of households with income under $3,000	-0.0172	0.00132	0.0136	-0.00868
	[0.0480]	[0.0145]	[0.0467]	[0.0213]
65 or older	-0.0994	-0.0368	0.0309	0.0148
	[0.116]	[0.0319]	[0.140]	[0.0487]
Under 5 years of age	-0.208	-0.0112	0.0211	0.0656
	[0.205]	[0.0532]	[0.200]	[0.0734]
Nonwhite	-0.0305	-0.00916	-0.0175	-0.0123
	[0.0250]	[0.00719]	[0.0277]	[0.0138]
in urban areas	-0.0385	-0.0107	-0.0254	-0.0161
	[0.0115]	[0.00320]	[0.0159]	[0.00517]
Funded before 1970		X		X
Weighted			X	X
State fixed effects	X	X	X	X
Observations	271	219	271	219
R-squared	0.360	0.470	0.346	0.481

Note: Each column reports estimates from a separate linear regression. Heteroskedasticity-robust standard errors are corrected for clustering with state and presented in brackets. Independent Variables are from the 1960 Decennial Census. Columns 3 & 4 use 1960 population as weights.

Table 2.3. Event Study Estimates for Divorce Per 100,000 Residents

A. DV: Divorces per 1,000 Residents		
(1)	(2)	(3)
Years -5 to -1		
3.269	-2.285	-2.791
[4.327]	[3.583]	[3.356]
Years 1 to 2		
7.673	9.535	9.998
[3.537]	[3.499]	[3.425]
Years 3 to 4		
21.11	20.26	21.42
[7.182]	[5.818]	[5.659]
Years 5 to 6		
13.39	17.40	19.32
[9.118]	[8.700]	[8.546]
Years 7 to 8		
8.630	22.51	25.25
[9.898]	[8.886]	[8.471]
Years 9 to 10		
-4.384	12.04	15.63
[11.14]	[10.20]	[9.657]
Years 11 to 12		
-16.13	5.377	10.09
[11.36]	[10.70]	[10.21]
Observations	89,117	89,117	89,117
R-squared	0.247	0.309	0.313
Number of Counties	3,073	3,073	3,073

Notes: Table display least-squares estimates obtained from estimating equation 2.1 by grouping years before and after exposure. Column 1 corresponds to model 1 and includes City and Year fixed effects & column 2 corresponds to model 2 and includes state-by-year, S-Y, effects. Column 3corresponds to model 3 and adds covariates from the 1960 Census interacted with a linear trend to model 2. Heteroskedasticity-robust standard errors clustered by city are presented beneath each estimate in brackets. Each regression is weighted by 1988 population and excludes Cook County, Los Angeles County, and New York City.

Table 2.4. Descriptive Statistics from Legal Service Agency Survey

Variable	Observations	Mean	Std. Dev	Min	Max
Staff					
R.H. Smith Fellows	189	0.86	1.71	0	10
VISTA Attorneys	189	0.25	0.82	0	7
Law Students	189	3.35	10.57	0	99
Professional Support Personnel	189	2.30	8.25	0	99
Clerical Support Personnel	189	6.13	6.12	1	48
Attorneys	189	5.66	6.30	0	36
Other Personnel	189	1.09	2.14	0	16
Planned Participants					
Above Poverty Level	189	240	626	0	4435
$10-$499 Below Poverty Level	189	351	875	0	7500
$500-1499 Below Poverty Level	189	352	1,038	0	11320
$1500 or More Below Poverty Level	189	300	775	0	6550
Participants per Year	189	1,974	3,499	0	20000
Total Budget per Year	189	203,509	233,460	14,856	1,558,209
LSP image in the Community					
Relevance of Legal Service Program Activities	186	2.21	0.38	1	3
Assessment of LSP Results	186	2.33	0.37	1	3
LSP Staff Competence	186	2.42	0.38	1	3
Meeting of LSP Goals	186	2.24	0.40	1	3
Legal Climate - Courts Attitudes					
Attitude to Poverty Issues	186	1.87	0.39	1	3
Disposition of Cases	186	2.08	0.36	1	3
Attitude to LSP	186	2.10	0.40	1	3
Disposition of LSP Cases	186	2.18	0.39	1	3

Source: The data in the Legal Service Agency Survey (LSAS) 1970 survey was collected from the Auerbach Corporation for an evaluation of OEO legal service projects (LSP) in 1970 and 1971. Data publicly available at the ICPSR.

Table 2.5. Event Study Estimates by LSAS Outcomes

	A. DV: Divorces per 100,000 Residents							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of Attorneys		Total Budget		Court Attitudes		Community Perspective	
Characteristics Defining Stratification	*Below Median*	*Above Median*	*Below Median*	*Above Median*	*Below Median*	*Above Median*	*Below Median*	*Above Median*
Years -5 to -1	-0.694	-1.911	-5.423	0.520	-7.160	3.168	-3.276	0.719
	[5.827]	[5.204]	[5.462]	[5.103]	[4.669]	[5.591]	[6.499]	[3.800]
Years 1 to 2	17.27	9.326	14.72	10.47	14.28	10.21	15.30	9.976
	[12.43]	[3.767]	[11.96]	[3.676]	[6.909]	[4.078]	[6.180]	[4.153]
Years 3 to 4	14.05	31.33	14.85	31.24	24.76	28.99	31.43	24.84
	[8.914]	[7.648]	[8.519]	[7.661]	[8.855]	[7.215]	[8.861]	[6.456]
Years 5 to 6	43.38	34.62	44.05	33.10	19.13	47.45	43.05	27.43
	[10.72]	[10.37]	[10.03]	[10.45]	[10.22]	[10.09]	[11.90]	[8.168]
Years 7 to 8	44.39	40.00	41.73	39.72	39.23	41.43	48.06	29.23
	[13.31]	[11.61]	[13.02]	[11.74]	[13.15]	[11.68]	[14.16]	[8.176]
Years 9 to 10	48.36	30.41	45.92	29.45	43.04	26.85	41.07	22.96
	[19.31]	[11.84]	[18.51]	[11.94]	[14.93]	[12.40]	[15.69]	[9.114]
Years 11 to 12	18.92	27.85	22.29	26.12	33.77	18.74	44.24	2.739
	[21.83]	[12.74]	[20.31]	[13.03]	[14.55]	[14.93]	[15.99]	[11.30]
Observations	82,563	84,738	82,563	84,738	83,114	84,187	83,607	83,694
R-squared	0.218	0.277	0.222	0.273	0.232	0.265	0.257	0.241
Number of Counties	2,847	2,922	2,847	2,922	2,866	2,903	2,883	2,886

Notes: Table display least-squares estimates obtained from estimating equation 2.1 by grouping years before and after exposure. The dependent variable is Divorces per 100,000 residents. All Columns corresponds to model 3 and includes City and State-by-Year fixed effects as well as covariates from the 1960 Census interacted with a linear trend. Each column refers to Legal Service Agency Survey conducted in 1970. Survey provides scores to describe the relationship between the poor, government agencies, and legal services. Columns 1 & 2 report estimates for treated counties with the number of attorneys per planned participants below/above the median; Columns 3 &4 refers to treated counties with the total budget per planned participant below/above the median; Columns 5 & 6 refers to treated counties residing in locations where court attitudes toward the poor were reported to be below/above the median; & Columns 7 & 8 refer to treated counties residing in locations where the poor community view the legal service program performance below/above the median.

Table 2.6. Event Study Estimates by Demographic Characteristics and First Grant

		A. DV: Divorces per 100,000 Residents							
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
								1960 Divorce	
Characteristics Defining Stratification		Per Capita First Grant		Dropping One Region at a Time				Rate	
		Below	*Above*					*Below*	*Above*
		Median	*Median*	*Northeast*	*Midwest*	*South*	*West*	*Median*	*Median*
Years -5 to -1		-1.900	-1.675	-0.961	-4.781	-5.955	-0.822	-6.337	0.390
		[4.327]	[3.530]	[4.207]	[3.727]	[3.835]	[3.720]	[3.266]	[4.926]
Years 1 to 2		7.213	16.67	15.18	7.351	11.01	9.323	-1.822	17.40
		[4.278]	[4.102]	[4.245]	[3.651]	[4.087]	[4.156]	[3.432]	[5.016]
Years 3 to 4		18.17	30.10	26.50	20.54	22.16	22.36	8.637	29.38
		[6.880]	[6.769]	[6.913]	[7.011]	[6.052]	[6.733]	[6.168]	[8.007]
Years 5 to 6		15.49	26.52	27.88	14.83	19.86	20.71	-2.497	32.49
		[10.95]	[8.137]	[10.65]	[10.75]	[7.519]	[10.08]	[7.463]	[12.60]
Years 7 to 8		26.00	24.70	32.61	21.63	23.78	29.02	-0.540	40.65
		[10.71]	[7.686]	[10.53]	[10.27]	[8.020]	[9.807]	[8.593]	[12.01]
Years 9 to 10		19.14	5.204	21.12	14.72	16.01	20.04	-13.19	31.54
		[12.39]	[7.883]	[12.01]	[11.09]	[10.09]	[11.19]	[11.59]	[13.12]
Years 11 to 12		17.14	-9.046	15.62	9.354	7.712	11.69	-17.55	24.30
		[12.91]	[9.300]	[12.75]	[12.36]	[9.102]	[11.65]	[11.64]	[14.08]
Observations		85,260	85,260	82,969	58,609	48,517	77,256	85,260	85,260
R-squared		0.289	0.234	0.406	0.297	0.275	0.287	0.255	0.271
Number of Counties		2,940	2,940	2,861	2,021	1,673	2,664	2,940	2,940

Notes: Table display least-squares estimates obtained from estimating equation 2.1 by grouping years before and after exposure. The dependent variable is Divorces per 100,000 residents. All Columns corresponds to model 3 and includes City and State-by-Year fixed effects as well as covariates from the 1960 Census interacted with a linear trend. Columns 1 & 2 report estimates for treated counties with real per capita first grants below/above the median; Columns 3-6 are from separate regressions, each dropping on region at a time from the estimating equation 2.1. Columns 7 & 8 refers to treated counties with 1960 divorce rates below/above the median 1960 divorce rates in treated counties

Table 2.7. Event Study Estimates for Treated and Contiguous Counties

	DV: Divorces per 100,000 Residents		
	(1)	(2)	(3)
	Original	Treated	Contiguous
Years -5 to -1	-2.791	-14.56	-2.705
	[3.356]	[7.955]	[3.095]
Years 1 to 2	9.998	13.62	4.172
	[3.425]	[6.752]	[2.860]
Years 3 to 4	21.42	30.71	6.403
	[5.659]	[13.61]	[4.895]
Years 5 to 6	19.32	38.31	9.878
	[8.546]	[20.19]	[6.904]
Years 7 to 8	25.25	59.78	17.38
	[8.471]	[23.30]	[11.28]
Years 9 to 10	15.63	70.87	17.14
	[9.657]	[25.88]	[11.32]
Years 11 to 12	10.09	80.36	19.49
	[10.21]	[28.69]	[13.63]
Observations	89,117	7,975	81,142
R-squared	0.313	0.821	0.197
Number of Counties	3,073	275	2,798

Notes: Table display least-squares estimates obtained from estimating equation 2.1 by grouping years before and after exposure. Column 1 corresponds to model 3 estimates in table 2.3. Column 2 report estimates of equation 2.1 by dropping counties that did not receive a legal service grant between 1965 & 1975. Column 3 does not include counties that received legal service grants, estimates are from estimating a DiD specification of equation 2.1 where the treated counties are contiguous to legal service counties.

Table 3.1. Summary Statistics

Descriptive Statistics	All Cities (N=101)	Sample (N=37)	Levitt Sample (N=59)	Expanded Sample (N=77)
Mean Population	466,187	588,576	714,525	579,497
Violent Crime	1,001	1,241	1,196	1,115
Murder	17	21	21	19
Rape	59	77	70	65
Assault	481	568	538	526
Robbery	444	574	567	504
Property Crime	7,463	8,301	7,886	7,688
Burglary	2,188	2,503	2,347	2,284
Larceny	4,349	4,671	4,455	4,397
Motor Vehicle Theft	926	1,127	1,084	1,007
Sworn officers	218	227	240	223
Percent Black	13.5	15.0	16.3	14.8
Percent Female-Headed Households	19.3	21.1	21.1	20.0
Percent age 15-24	17.4	17.5	17.5	17.6
Public Welfare Spending Per Capita	283.4	262.6	296.3	287.1
Education Spending Per Capita	316.6	292.8	341.9	329.1
State Unemployment Rate	6.7	6.7	6.7	6.8

Source: Table averages are from the UCR from 1970 to 2005 and are presented per 100,000 residents. Criminal offenses reported in the UCR are from the Uniform Crime Report Offenses Known and Cleared. UCR data are publicly available at the ICPSR. Demographic characteristics are from the from the 1970, 1980, 1990, 2000 Census City and County Books, Bureau of Labor Statistics, Historical Data Base of Individual Government Finances, and the Surveillance, Epidemiology, and End Results (SEER).

Table 3.2. First Stage Regressions for Election Cycles and Police

Δln sworn officers	(1)	(2)	(3)	(4)	(5)	(6) Published
campaign	0.021	0.022	0.023	0.010	0.022	0.012
	[0.00462]	[0.00486]	[0.00504]	[0.00521]	[0.00502]	[0.004]
governor	0.027	0.026	0.025	0.032	0.025	0.024
	[0.0144]	[0.0145]	[0.0145]	[0.0150]	[0.0145]	[0.007]
Covariates	No	Yes	Yes	Yes	Yes	Yes
Year Fix Effects	Yes	Yes	Yes	Yes	Yes	Yes
City-Size Indicator	No	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	No	No	Yes	Yes	Yes	Yes
Ferreira & Gyrouko	X	X	X			
Levitt				X		X
McCrary					X	
Number of Cities	37	37	37	37	37	59
Observations	796	760	760	760	760	1276
R-squared	0.109	0.115	0.140	0.119	0.137	0.11

Note: The dependent variable in each column is Δln sworn police officer per capita. Columns 1-5 include the 37 cities that are in Ferreira & Gyrouko and Levitt sample. Column 6 report published estimates from Levitt (1997). Columns 4 and 5 report coefficients for separate regressions of equation 3.1, using election data provided by Levitt and McCrary respectively. Year fixed effects are included in all columns.

Table 3.3. First Stage Regressions for Election Cycles and Police – Complete Sample

Δln sworn officers	(1)	(2)	(3)	(4)	(5) Published
campaign	0.0188	0.0123	0.00960	0.0115	0.012
	[0.00449]	[0.00409]	[0.00459]	[0.00406]	[0.004]
governor	0.0178	0.0172	0.0190	0.0171	0.024
	[0.00747]	[0.00752]	[0.00756]	[0.00751]	[0.007]
Covariates	Yes	Yes	Yes	Yes	Yes
Year Fix Effects	Yes	Yes	Yes	Yes	Yes
City-Size Indicator	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes	Yes
Ferreira & Gyrouko	X	X			
Levitt	X		X		X
McCrary		X		X	
Number of Cities	59	59	59	59	59
Observations	1,207	1,207	1,207	1,207	1276
R-squared	0.107	0.098	0.095	0.097	0.11

Note: The dependent variable in each column is Δln sworn police officer per capita. Columns 1-4 include the 59 cities that are in Levitt's original sample. The sample is completed by using mayoral data provide by Levitt and McCrary for columns 1 and 2. Columns 3 and 4 uses Levitt's and McCrary election measure for all cities in the sample to replicate publish work.

Table 3.4. The Relationship Between Election Cycles and Crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
								Violent Crime	Property Crime
	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor Vehicle Theft		
Ferreira & Gyrouko									
campaign	-0.0302	-0.00300	-0.00767	-0.00376	0.00112	-0.00971	0.0194	-0.0112	0.00359
	[0.0132]	[0.0132]	[0.0132]	[0.0132]	[0.0132]	[0.0132]	[0.0132]	[0.00670]	[0.00771]
governor	-0.0105	-0.00412	0.0145	0.0395	0.00903	0.0213	-0.0179	0.00985	0.00414
	[0.0298]	[0.0298]	[0.0298]	[0.0298]	[0.0298]	[0.0298]	[0.0298]	[0.0149]	[0.0172]
Levitt									
campaign	-0.0142	-4.03e-05	-0.00942	0.0217	-0.000603	-0.00268	-0.00850	-0.000486	-0.00393
	[0.0138]	[0.0138]	[0.0138]	[0.0138]	[0.0138]	[0.0138]	[0.0138]	[0.00699]	[0.00804]
governor	-0.0186	-0.00460	0.0111	0.0442	0.00904	0.0191	-0.0170	0.00802	0.00372
	[0.0299]	[0.0299]	[0.0299]	[0.0299]	[0.0299]	[0.0299]	[0.0299]	[0.0150]	[0.0173]
McCrary									
campaign	-0.0139	-0.0147	0.00207	-0.0136	0.00765	-0.00773	0.0105	-0.0100	0.00348
	[0.0132]	[0.0132]	[0.0132]	[0.0132]	[0.0132]	[0.0132]	[0.0132]	[0.00665]	[0.00766]
governor	-0.0126	-0.00194	0.0130	0.0414	0.00783	0.0212	-0.0168	0.00995	0.00407
	[0.0298]	[0.0298]	[0.0298]	[0.0298]	[0.0298]	[0.0298]	[0.0298]	[0.0149]	[0.0172]
Ferreira & Gyrouko and Levitt									
campaign	-0.0362	0.00249	-0.0248	-0.0123	-0.00683	-0.00976	0.00324	-0.0177	-0.00445
	[0.0118]	[0.0118]	[0.0118]	[0.0118]	[0.0118]	[0.0118]	[0.0118]	[0.00596]	[0.00686]
governor	-0.000531	0.0255	0.00586	0.0345	0.00305	0.0148	-0.000295	0.0163	0.00584
	[0.0220]	[0.0220]	[0.0220]	[0.0220]	[0.0220]	[0.0220]	[0.0220]	[0.0111]	[0.0128]
Ferreira & Gyrouko and McCrary									
campaign	-0.0191	0.00243	-0.00693	-0.00222	0.000752	-0.00712	0.00541	-0.00647	-0.000320
	[0.0116]	[0.0116]	[0.0116]	[0.0116]	[0.0116]	[0.0116]	[0.0116]	[0.00587]	[0.00676]
governor	0.00216	0.0255	0.00790	0.0356	0.00367	0.0154	-0.000388	0.0178	0.00624
	[0.0220]	[0.0220]	[0.0220]	[0.0220]	[0.0220]	[0.0220]	[0.0220]	[0.0111]	[0.0128]

Note: All columns use year and crime specific city fixed effects as well as city-size indicators, and city/state demographic variables. For columns 1-7, estimates are from non-weighted joint regression of the seven crime categories Δln in number of crimes per capita. Columns 8 & 9 restrict coefficients to be the same for all property crimes and violent crimes.

Table 3.5. Second Stage Estimates for the Effect of Police on Crime

Sample 1970 - 1992	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS
Separate Estimates for Seven Crime Categories				
Violent Crimes				
Murder	-0.65	-3.48	-2.78	-3.54
	0.23	2.30	2.84	3.43
Rape	-0.12	0.14	0.45	0.03
	0.14	1.43	1.61	0.52
Robbery	-0.36	-0.77	-0.47	0.03
	0.13	1.16	1.49	0.52
Assault	0.28	-0.43	0.69	-2.09
	0.20	1.75	2.29	2.69
Property Crimes				
Burglary	-0.21	-0.63	-0.34	-0.46
	0.10	0.85	1.07	1.27
Larceny	-0.15	-0.41	0.21	0.11
	0.08	0.69	0.95	1.14
Motor Vehicle Theft	-0.26	1.17	-0.06	1.20
	0.13	1.31	0.89	1.95
Pooled Estimates				
Violent Crimes	-0.21	-0.74	-0.24	-0.80
	0.08	0.68	0.86	1.04
Property Crimes	-0.19	-0.22	-0.04	0.12
	0.06	0.45	0.57	0.68
Election Measure		F&G	Levitt	McCrary
Number of Cities	37	37	37	37
Observations	4,695	4,695	4,695	4,695

Note: Table 3.5 present estimates from the regression of Δln in number of crimes per capita on Δln sworn police officer per capita. All columns use year and crime specific city fixed effects as well as city-size indicators, and city/state demographic variables. Pooled estimates are from joint regressions of the seven crime categories on police. Columns 1 report OLS estimates. Columns 2 uses Ferreira and Gyrouko election data for instruments while columns 3 and 4 uses Levitt's and McCrary election measure. All columns are from a weighted least squares regression. The weighting procedure uses the OLS & 2SLS residuals by crime categories as weights. The goal of the procedure was to give more weight to crime categories with less year to year variation.

Table 3.6. Second Stage Estimates for the Effect of Police on Crime – Complete Sample

Sample 1970 - 1992	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS
Separate Estimates for Seven Crime Categories				Published McCrary
Violent Crimes				
Murder	-0.93	-2.92	-2.46	-3.03
	0.22	2.22	2.48	2.03
Rape	-0.17	0.73	1.34	0.74
	0.12	1.23	1.45	1.19
Robbery	-0.43	-1.42	-0.85	-1.39
	0.11	1.13	1.24	1.00
Assault	0.07	-0.18	0.45	-0.58
	0.16	1.29	1.69	1.16
Property Crimes				
Burglary	-0.36	-1.47	-0.96	-0.55
	0.08	0.84	0.91	0.67
Larceny	-0.18	-0.49	-0.05	0.53
	0.07	0.64	0.78	0.58
Motor Vehicle Theft	-0.36	0.34	-0.05	-0.44
	0.12	1.12	0.79	0.98
Pooled Estimates				
Violent Crimes	-0.29	-0.65	-0.07	-0.79
	0.07	0.66	0.67	0.61
Property Crimes	-0.27	-0.67	-0.37	-0.38
	0.05	0.42	0.48	0.34
Election Measure		F&G Levitt	F&G McCrary	Levitt
Number of Cities	59	59	59	59
Observations	6,545	6,545	6,545	6,545

Note: Table 3.6 present estimates from the regression of Δln in number of crimes per capita on Δln sworn police officer per capita. All columns use year and crime specific city fixed effects as well as city-size indicators, and city/state demographic variables. Pooled estimates are from joint regressions of the seven crime categories on police. Columns 1 report OLS estimates. Columns 2 uses Ferreira and Gyrouko and Levitt election data for instruments while columns 3 uses Ferreira and Gyroukos and McCrary election measure. All columns are from a weighted least squares regression.

Table 3.7. Second Stage Estimates for the Effect of Police on Crime, Extended Sample

Sample 1970 - 1992	(1) OLS	(2) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Separate Estimates for Seven Crime Categories					Published McCrary
Violent Crimes					
Murder	-0.35	-0.96	-4.47	-3.39	-3.03
	0.14	2.32	3.65	3.77	2.03
Rape	-0.05	-2.71	-2.70	-2.91	0.74
	0.08	1.95	2.36	2.62	1.19
Robbery	-0.15	-2.91	-4.65	-4.56	-1.39
	0.06	1.76	2.84	3.23	1.00
Assault	0.01	-0.04	-0.27	0.22	-0.58
	0.08	0.56	1.54	2.16	1.16
Property Crimes					
Burglary	-0.05	-0.39	-1.18	-0.50	-0.55
	0.04	0.73	1.09	1.05	0.67
Larceny	-0.04	-0.64	-1.08	-1.10	0.53
	0.03	0.68	0.95	1.14	0.58
Motor Vehicle Theft	-0.08	1.03	0.99	0.44	-0.44
	0.06	1.13	1.31	1.39	0.98
Pooled Estimates					
Violent Crimes	-0.10	-1.88	-2.84	-2.61	-0.79
	0.04	0.82	1.19	1.27	0.61
Property Crimes	-0.05	-0.28	-0.67	-0.58	-0.38
	0.02	0.43	0.51	0.61	0.34
Election Measure		F&G	F&G Levitt	F&G McCrary	Levitt
Number of Cities	55	55	77	77	59
Observations	4,695	4,695	4,695	4,695	6,545

Note: Table 3.7 present estimates from the regression of $\Delta\ln$ in number of crimes per capita on $\Delta\ln$ sworn police officer per capita. All columns use year and crime specific city fixed effects as well as city-size indicators, and city/state demographic variables. Pooled estimates are from joint regressions of the seven crime categories on police. Columns 1 report OLS estimates. Columns 2 uses Ferreira and Gyrouko election data for instruments. Columns 3 uses Ferreira and Gyrouko and Levitt election data for instruments while columns 4 uses Ferreira and Gyroukos and McCrary election measure. All columns are from a weighted least squares regression.

Table 3.8. Second Stage Estimates for the Effect of Police on Crime – Extended Sample and Sample Years

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Ferreira & Gyrouko						
All Violent Crimes	-0.18	-0.34	-0.10	-0.74	-0.10	-0.65
	0.07	0.70	0.04	0.72	0.04	0.70
All Property Crimes	-0.24	-0.22	-0.07	-0.55	-0.04	-0.61
	0.05	0.47	0.02	0.47	0.02	0.44
Ferreira & Gyrouko and Levitt						
All Violent Crimes					-0.12	-1.17
					0.04	0.82
All Property Crimes					-0.05	-0.51
					0.02	0.44
Ferreira & Gyrouko and McCrary						
All Violent Crimes					-0.12	-1.00
					0.04	0.86
All Property Crimes					-0.05	-0.50
					0.02	0.50
Sample Years 1970-2005	X	X	X	X		
Population Greater than 100,000					X	X
Population Greater than 150,000			X	X		

Note: Table 3.8 present estimates from the joint regression of Δln in number of crimes per capita on Δln sworn police officer per capita. All columns use year and crime specific city fixed effects as well as city-size indicators, and city/state demographic variables. All columns are from a weighted least squares regression.

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