The INDEPENDENT INSTITUTE

EFFECTS OF CRIMINAL PROCEDURE ON CRIME RATES: Mapping Out the Consequences of The Exclusionary Rule

Independent Institute Working Paper #9

Paul Rubin and Raymond A. Atkins

October 1999

EFFECTS OF CRIMINAL PROCEDURE ON CRIME RATES:
Mapping Out the Consequences of The Exclusionary Rule

Raymond A. Atkins, Covington and Burling,
Paul H. Rubin, Emory University
October 23, 1998

Correspondence:

Paul H. Rubin
Department of Economics
Emory University
Atlanta, GA 30322-2240
Voice: 404-727-6365
Fax: 404-727-3082
Email: prubin@emory.edu

EFFECTS OF CRIMINAL PROCEDURE ON CRIME RATES: Mapping Out the Consequences of The Exclusionary Rule[1]

Abstract

In 1961, in its *Mapp v. Ohio* ruling, the Supreme Court declared that every state must exclude from criminal trials evidence obtained in violation of the Fourth Amendment, the "exclusionary rule." At the time the Court issued its ruling, twenty-four states allowed ill-gotten evidence in their criminal trials, and twenty-four excluded it. Commentators who have looked only at the cases in which evidence is actually excluded have found that there is little impact of the rule. The economic model of the search warrant process predicts an increase in crime rates after the Court forced states to adopt the exclusionary rule as police officers substitute away from searches towards alternatives they consider less effective. Our empirical analysis supports these theoretical predictions. A statistically and economically significant increase in crimes followed the Court's enactment of the exclusionary rule, ranging from 3 percent increases in larceny to 30 percent increases in assault. The major impact of this ruling was on smaller cities. In addition to the *Mapp v. Ohio* ruling, we also examined two other major rules imposed on the states by the Court. These are the rule granting indigent defendants the right to counsel, imposed in the *Gideon v. Wainwright* ruling of 1962, and the *Miranda v. Arizona* ruling of 1966, granting the right to remain silent and have an attorney present during questioning. While the effects are not as large as those of *Mapp v. Ohio,* these two rulings also increased crime rates in those states affected by the new rules.

---

[1] This paper has benefited from the suggestions and observations of the following people: Peter H. Aranson, Martin J. Bailey, Robert E. Carpenter, Paul G. Cassell, Christopher Curran, Andrew Dougherty, Isaac Ehrlich, Barry Hirsch, D. Bruce Johnsen, John Lott, Richard Posner, Hugh Spall, and Richard Murphy. This research was sponsored by a grant from the Donner Foundation. Data for the empirical section was made available, in part, by the Inter-University Consortium for Political and Social Research (ICPSR study numbers 8076 & 7716).

I. INTRODUCTION

Since the work of Becker,[2] economists have studied the impact of various aspects of the criminal justice system on crime rates. However, there has been little analysis of the effects of criminal procedure.[3] This is surprising because in the 1960s the Supreme Court revolutionized criminal procedure in the U.S. and its rulings were and remain controversial. The relevant decisions include the *Miranda v. Arizona*[4] ruling of 1966, giving all citizens the right to a lawyer when arrested; the *Gideon v. Wainwright*[5] ruling of 1963, giving all citizens the right to a lawyer at trial; and the *Mapp v. Ohio*[6] ruling of 1961, excluding from trial evidence obtained without a search warrant. Each of these changes was a radical transformation of the criminal justice system; consequently, there was great interest in observing the effects of these new procedural rules. At the time of these decisions, however, the empirical techniques and theoretical models available to analyze the criminal justice system were unable to predict or detect the effects of the Supreme Court's rulings. Although techniques have now developed, this gap in the literature persists.[7] Economists and other students of crime who embrace the rational

---

[2] Gary S. Becker, "*Crime and Punishment: An Economic Approach*," 76 J. POLITICAL ECONOMY 169 (1968).

[3] An exception is Isaac Ehrlich & George D. Brower, "*On the issue of Causality in the Economic Model of Crime and Law Enforcement: Some Theoretical Considerations and Experimental Evidence*," 77 AMERICAN ECON. REV.: PAPERS & PROCEEDINGS 99 (May 1987). This paper incorporated all changes in criminal procedure into a weighted proxy, which was introduced as an explanatory variable in a multiple-equation system to explain movement in national crime rates over three decades. The analysis supports the proposition that criminal procedure is a significant determinant of criminal behavior, but, as we show, a much more detailed analysis is possible. In contrast, economics journals are filled with extensive empirical analysis into the effect of changes in punishment, conviction rates, clearance rates, unionization, gun control, and various socio-economic factors on criminal behavior.

[4] 384 U.S. 436 (1966).

[5] 372 U.S. 335 (1963).

[6] 367 U.S. 643 (1961).

[7] Several others have examined empirically the effect of criminal procedure on police productivity and criminal behavior, although not on crime rates. The effect of *Miranda* has been investigated using modern statistical techniques by Paul G. Cassell: "*Miranda's Social Costs: An Empirical Reassessment*," 90 NORTHWESTERN UNIV. L. REV. 391 (1996); Paul G. Cassell, "*All Benefits, No Costs: The Grand Illusion of Miranda's Defenders*," 90 NORTHWESTERN UNIV. L. REV. 1084, 1090 (1996); and Paul G. Cassell and Richard Fowles, "*Handcuffing the Cops? A Thirty-Year Perspective on Miranda's Harmful Effects on Law Enforcement*," 50 STANFORD LAW REVIEW, 1055 (April, 1998.) Cassell found a dramatic decrease in confessions when *Miranda* was enacted. Professor Cassell's studies rely upon both regression analysis and investigation of crime statistics. Because *Miranda* was enacted by the Supreme Court, and there were no states that had voluntarily enacted a similar rule, the analysis is complicated by the possibility that a uniform shock to the criminal justice system occurred simultaneously with the Court's decision and is responsible for the changes. However, his analysis suggests that the radical transformation

models of criminal behavior have ignored this aspect of the system, instead focusing upon the detection and sentencing phases of the process.

Although recent changes in criminal procedure have been marginal, the earlier decisions of the Supreme Court that we examine were radical shifts in the criminal justice system. These rulings allow us to test the economic theories of criminal behavior in the context of changes in criminal procedure. Of the Supreme Court cases mentioned, the *Mapp v. Ohio* ruling of 1961 is best suited for empirical analysis for several reasons. First, when the Supreme Court decided *Mapp*, exactly half of the states had already enacted a similar rule. (See Table 1.) This creates a control group to be used in the statistical analysis. It is a perfect example of a "natural experiment." [8] Second, the exclusionary rule established in *Mapp* has been debated vigorously, both in the past, when the controversial decision was made, and in the present, with Congress recently considering altering the exclusionary rule. [9] Changing this rule was also part of the "Contract with America." [10] Finally, much of the importance of the other procedural rules stems from *Mapp*. For example, the possibility of excluding evidence from trial greatly augments the value and impact of the Fifth Amendment's right to counsel. [11] For these reasons, this study will focus primarily on the predicted and actual effect of the exclusionary rule on crime rates.

The rational choice model of criminal behavior predicts that if the *Mapp* ruling did affect the behavior of police—altering either the probability of conviction or detection—then citizens should respond by increasing their level of unlawful activity. Since the exclusionary rule increases the costs of police investigations, the police will respond by substituting away from those activities that require a warrant towards those that do not.

---

in criminal procedure had a large impact upon police productivity, as seen by the dramatic change in violent crime clearance rates.

[8] See Bruce D. Meyer, "Natural and Quasi-Experiments in Economics," 13 *Journal of Business and Economic Statistics* 151 (April).

[9] Jarett B. Decker, "*The 1995 Crime Bill: Is the GOP the Party of Liberty and Limited Government*?," 229 POLICY ANALYSIS 20 (June 1995).

[10] Ed Gillespie and Bob Schellhas, editors, *Contract with America*, Times Books, New York, 1994, pp. 52-53.

[11] For some non-obvious and interesting implications of alternative procedural rules, see William J. Stuntz, "*The Uneasy Relationship Between Criminal Procedure and Criminal Justice*," 107 YALE LAW JOURNAL 1 (October 1997). For example, Stuntz argues that it is cheaper to contest procedural than factual claims, so that the system will be biased towards disputing procedural issues and away from examining factual matters.

As this change in police behavior is understood by the criminals, they too alter their behavior. The theoretical impact on crime rates due to the exclusionary rule is therefore expected to be positive as the decisions by judges affect the police and, eventually, criminals.

This prediction is supported by the data. The evidence reveals a significant impact on crime rates following the involuntary adoption of an exclusionary rule as the penalty for an unlawful search and seizure. This finding is dramatically at odds with current academic and judicial beliefs regarding the impact of the exclusionary rule. This finding is similarly in conflict with numerous older studies of the *Mapp* ruling, studies that failed to detect any significant adverse effect. These studies, however, generally overlooked the effect of the exclusionary rule on the decisions of the investigator and the criminal, focusing instead on the decisions of the prosecutor and the trial judge. As a measure of the cost and effect of the exclusionary rule, the older studies examined the number of cases thrown out or lost at trial because of unlawfully obtained or tainted evidence. But this is not a proper measure; we expect the police to adapt to the new requirements and to adjust their investigative techniques. The courts would exclude evidence only in the rare instances where the police misjudged the new requirements. The dominant effect of the exclusionary rule should be for the police to substitute to alternative methods of investigation that they consider less effective.

Section II describes the early history of the exclusionary rule leading up to *Mapp v. Ohio* and examines the older studies of the *Mapp* ruling. Section III provides a formal model of the search warrant process. Our empirical work is presented in two sections, dictated by data availability. Section IV analyzes state crime data from 1958-1967; and Section V investigates 396 cities from 1948-1969. This section also includes an empirical analysis of related Supreme Court rulings. Section VI summarizes the empirical findings, and Section VII summarizes the paper and discusses policy implications.

II. BACKGROUND

A. The Exclusionary Rule: *Mapp v. Ohio*

The Fourth Amendment was established to protect the rights of U.S. citizens to be free from personal invasion by protecting them from the intrusive investigative techniques of the federal government.[12] The amendment, as interpreted by the Supreme Court, states that searches must be performed with a valid warrant.[13] The amendment does not indicate how it is to be enforced and is silent about the punishment for illegal searches. The details of enforcement were left for the judicial system to determine. Excluding evidence as a method of enforcing the Fourth Amendment was first considered by the Supreme Court in 1914. In *Weeks v. United States*,[14] the federal district court was presented at trial with private papers confiscated without the use of a warrant. In this landmark case, the Supreme Court established the United States as the only country that excluded valid evidence from trial in order to protect its citizens from the government.[15]

The *Weeks* decision, combined with several subsequent decisions, forced the federal district courts to exclude evidence gathered illegally by federal officers. But this ruling did not extend the protections of the Fourth Amendment and the exclusionary rule to state action. The Court considered this issue in *Wolf v. Colorado*[16] and determined that the Fourth Amendment was "implicit in 'the concept of ordered liberty' and as such enforceable against the states through the Due Process Clause."[17] Although the Court determined that all of the states were subject to the Fourth Amendment, it did not

---

[12] "The right of the people to be secure in their persons, houses, papers, and effects, against unreasonable searches and seizures, shall not be violated; and no Warrants shall issue, but upon probable cause, supported by Oath or affirmation, and particularly describing the place to be searched, and the persons or things to be seized." U.S. AMEND. IV.

[13] The Fourth Amendment does not in fact require a search warrant prior to all searches and seizures. Rather, the language suggests that "reasonable" searches and seizures are permitted with or without a search warrant issued with probable cause. The Supreme Court—in a series of decisions—declared the general, bright-line rule that a search without a search warrant is *per se* unreasonable. While there has been some movement by the Court back to a reasonableness standard, a warrant is still generally required.

[14] Weeks v. U.S., 232 U.S. 383 (1914).

[15] *See generally* Craig M. Bradley, "*The Emerging International Consensus as to Criminal Procedure*," 14 MICH. J. INT'L. L. 171 (1993). As was stated by Professor Wilkey, "one proof of the irrationality of the exclusionary rule is that no other civilized nation in the world has adopted it. " Wilkey, "*The Exclusionary Rule: Why Suppress Valid Evidence*?" 62 JUDICATURE 215, 216 (1978). However, several countries including Canada, Germany, Australia, and Italy have a discretionary exclusionary rule. *See* Robert Harve & Hamar Foster, "*When the Constable Blunders: A Comparison of the Law of Police Interrogation in Canada and the United States*," 19 SEATTLE U. L. REV. 497 (1996); Craig M. Bradley, "*The Exclusionary Rule in Germany*," 96 HARV. L. REV. 1032, 1032-35 (1983).

[16] 338 U.S. 25 (1949). To impose an exclusionary rule on all of the states, the Supreme Court needed to resolve two important constitutional questions. First, did the Due Process Clause of the Fourteenth Amendment dictate the inclusion of the Fourth Amendment, therefore allowing the Supreme Court to regulate the criminal proceedings of the individual states? And second, was the exclusionary rule a constitutionally mandated remedy for a breach of the Fourth Amendment by state officials?

[17] *Wolf*, 338 U.S. at 27-28.

command the exclusionary rule as the only acceptable method of enforcement. The Court recognized that the exclusionary rule was only one possible enforcement mechanism available to the states. Other remedies that the individual states might employ included civil and criminal liability placed on the officers who performed the searches and on the police departments permitting the searches. The Court concluded that as long as those individual states without an exclusionary rule had viable alternatives, it would be unnecessary to impose a universal exclusionary rule.

The *Wolf* ruling survived for twelve years. During this time, several states voluntarily moved to an exclusionary rule, supporting the argument that the states would self-select the most efficient rule that fit their particular circumstances. However, in 1961, the Supreme Court decided, in *Mapp v. Ohio,*[18] to overturn *Wolf* and to force the states to adopt an exclusionary rule. The Court argued that all other alternatives had failed and that the exclusionary rule was the only viable course. The Supreme Court argued further that judicial integrity itself mandated the universal enactment of the exclusionary rule because courts would, otherwise, be passively supporting the illegal behavior of the government by allowing the use of the ill-gotten gains at trial.

B. Previous Empirical Studies

In the 1960s and 1970s there were several attempts to measure the effect of *Mapp*.[19] The results of these empirical studies of the effect of *Mapp* were conflicting, making it difficult for the legal community to determine what impact, if any, *Mapp* actually had. After examining the studies as of 1976, Justice Blackmun reached a similar assessment: "The final conclusion is clear. No empirical researcher, proponent or opponent of the rule, has yet been able to establish with any assurance whether the rule has a deterrent effect."[20] This lack of a concrete empirical result, coupled with increasing political and judicial concerns, prompted further analysis into the cost of the exclusionary

---

[18] 367 U.S. 643 (1961).

[19] Note, "*Effect of Mapp v. Ohio on Police Search and Seizure Practices in Narcotics Cases*," 4 COLUMBIA J. OF LAW AND SOCIAL PROBLEM 87 (1968); Nagel, S. S, "*Testing the Effects of Excluding Illegally Seized Evidence*," WISCONSIN L. REV. 275-310 (1965); Oaks, D.H., "*Studying the Exclusionary Rule in Search and Seizure*," 37 UNIV. CHICAGO L. REV. 665, 665-757 (1970); Spiotto, J.E., "*Search and Seizure: An Empirical Study of the Exclusionary Rule and Its Alternatives*," 1 J. LEGAL STUD 243 (1972); Bradley Canon, "*The Exclusionary Rule: Have Critics Proven That It Doesn't Deter Police*," 62 JUDICATURE 398 (1979).

rule. But Canon's work marked the last attempt to understand what impact the *Mapp v. Ohio* ruling had on the criminal justice system in 1961. The host of studies performed in the late 70s examined the contemporary system, looking at the number of suspects released or the time spent on evidentiary issues to determine the cost of the exclusionary rule.

The studies of the late 70s and early 80s are ultimately responsible for the widely held belief that the exclusionary rule has little effect on crime rates in the United States. [21] These studies focused upon the number of cases lost at trial, and generally discovered that the percentage of cases lost due to an exclusionary issue was small. From this research, the judicial and political community presumed that there were few repercussions on the crime rate because few criminals were released. But this assumption overlooks the significant secondary effects of the exclusionary rule. If the exclusionary rule changes the behavior of police and alters the probability of apprehension, then the rational choice model predicts an increase in the number of crimes committed. Thus, the police may adhere to the exclusionary rule and commit fewer illegal searches, but an important secondary effect might be fewer crimes investigated, as the police weigh the benefits of investigating a crime against the costs involved. [22]

Although the more recent studi es focus on different costs of the exclusionary rule, their general findings demonstrate clearly that one of the costs of the exclusionary rule, the number of cases lost at trial, is not outrageous. The American Bar Association in 1988

---

[20] United States v. Janis, 428 U.S. 443, 450 n. 22 (1976).

[21] *E.g.*, *see* The Comptroller General, "*Impact of the Exclusionary Rule on Federal Criminal Prosecutions*," B-171019 (1979); The National Institute of Justice, "*The Effects of the Exclusionary Rule: A Study in California*," U.S. Department of Justice (1983); Canon, B. "*The Exclusionary Rule: have critics proven that it doesn't deter police*," 62(8) JUDICATURE 398 (1979); Canon, B. "*Is the Exclusionary Rule in Failing Health? Some New Data and a Plea Against a Precipitous Conclusion*," 62 KENTUCKY L. J. 681 (1974); Crocker, L. "*Can the Exclusionary Rule be Saved*?" 84 THE J. OF CRIM. L. & CRIMINOLOGY 310 (1993); Davies, T.Y. "*A Hard Look At What We Know (and still need to know) About The 'Costs' Of The Exclusionary Rule: The NIJ Study And Other Studies of 'Lost' Arrests*," 3 AMERICAN BAR FOUNDATION RESEARCH JOURNAL 611 (1983); Nardulli, P.F. "*The Social Cost of the Exclusionary Rule: An Empirical Assessment*," AMERICAN BAR FOUNDATION RESEARCH JOURNAL 585 (1983); Schlesinger, S.R. "*The Exclusionary Rule: Have Proponents Proven That It is a Deterrent to Police*," 62(8) JUDICATURE 404 (1979); Wilkey, M.R. "*The Exclusionary Rule: Why Suppress Valid Evidence*," 62(5) JUDICATURE (1978).

[22] Atkins, *supra* note 7.

summarized the results of these contemporary studies: [23]

- Between 0.2% and 0.8% of all adult felony cases are screened out by prosecutors because of illegal searches.
- Adding together data from all stages of the felony process, the cumulative loss from illegal searches ranges from 0.6% and 2.35%.
- In felony arrests for offenses other than drugs and guns, the number of illegal searches is lower.
- 2.3% of drug cases are screened out and the total cumulative loss ranges from 2.8% to 7.1%.

From these and other findings, the special committee wrote: "the conclusion that the exclusionary rule neither causes serious malfunction of the criminal justice system nor promotes crime is strongly supported by practically all of our . . . witnesses and by our telephone survey results."[24] This characterizes the mainstream academic opinion of the exclusionary rule—proponents agree that the exclusionary rule deters the police but consider a possible effect on criminals preposterous and irrelevant. Of course, these small numbers of excluded cases are quite consistent with a world in which the police adapt to the exclusionary rule and use alternative methods of obtaining evidence. Since police are being forced to use alternatives that are less preferred than search, we would expect the result to be higher crime rates, even if few or even no cases are ultimately lost at trial because of illegally obtained evidence.

We first provide a more formal theoretical model of this process. We then set forth an empirical analysis to test this theory.

III. AN ECONOMIC MODEL OF THE SEARCH WARRANT PROCESS[25]

In the search warrant process there are three major participants: the judge, the police, and the criminal. The basic analysis here assumes that each player is rational, choosing actions that maximize his or her objective function, and all players are

[23] ABA Special Committee on Criminal Justice in a Free Society, "Criminal Justice in Crisis," 21, 844 (1988).
[24] *Id*.
[25] This section is a shortened version of Raymond A. Atkins, " *The Warrant Process*," Unpublished

symmetrically informed . The goal is to focus on how the judge's choice of the probable cause standard effects criminals and the police.

A criminal chooses whether or not to commit a crime in the opportunity se t by weighing the subjective costs and benefits. Each crime has an associated benefit, distributed over a range [$\mathbf{b_l}$ **,** $\mathbf{b_h}$] according to a probability density function **f**(*) and cumulative density function **F**(*). The criminal has an opportunity cost of committing a crime, $\mathbf{O_p}$, and an expectation of the sentence, **S**, if caught. These costs and benefits of committing a crime are represented in monetary terms. Assuming risk neutrality, the criminal will maximize utility by maximizing expected wealth.

When a cr ime is committed, evidence **E** is generated, which is sufficient to convict the criminal with probability **Pc**. But the police cannot directly observe this evidence until they search the suspect. An important distinction between a search and a sample carries through the entire model. A *search* occurs when the police get a warrant and search for evidence. A *sample* occurs when the police draw a signal about a suspect guilt without conducting a search. What is observable from a sample without search is a *signal* of guilt. One signal, $\mathbf{S_h}$, is more likely to be seen when the suspect is guilty; the other, $\mathbf{S_l}$, when innocent. These signals may be interpreted as an alibi, the criminal's arrest record, witness testimony, or any other indication of guilt or innocence that can be observed without searching the suspect. The signals, $\mathbf{S_i}$**,** of the evidence level have the following distribution:

		Suspect Guilty	Suspect Not Guilty	
Signal **S**	High	u	1- u	1> u > 1/2
	Low	1-u	u	

Each period, the police draw an additional signal, which are all independently distributed (with a constant cost **c**), or go to the judge and get a warrant (with cost **s**). If the police obtain the warrant, they search the suspect and observe the incriminating evidence. The police suffer a cost, **w**, when they search an innocent man, and they suffer a cost **K**(b) when they fail to catch a criminal. We assume K(b) is increasing in **b**, the benefits from the criminal act. This assumption reflects the idea that the general public and the higher governmental officials who control the police budget impose a cost on the police for not capturing a criminal. Given these costs and benefits of starting an investigation, the police must determine an optimal stopping rule for this sequential sampling problem that minimizes the net cost of investigating a crime.

Solving for the expected number of samples and the probability that a criminal will be caught becomes problematic as the number of suspects increases. To simplify the analysis, we assume that there are two suspects for each crime, one of whom is the criminal.[26] The police must then decide whether or not to start an investigation and, if an investigation is started, which suspect will be searched first. But since the courts set the level of evidence necessary for a search, the police must first meet the probable cause requirement as determined by the warrant-issuing judge. This requirement does not preclude the police from sampling further, if the expected cost of continued sampling is less than the cost of searching the current suspect.

In each period the police begin with a belief about each suspect's guilt, and an expectation of future signals. An optimal stopping rule chosen by the police will establish a critical belief about a suspect's guilt where the expected cost of continuing to sample is just equal to the benefit from additional information. Wald[27] proposed one method to determine the boundaries of the optimal stopping rule when the sampler may continue sampling indefinitely. His method, called the sequential probability ratio test, was created to address the possibility of multiple exclusive hypotheses. In this case, there are two hypotheses: H1, suspect one is guilty, and H2, suspect two is guilty.

Let $\mathbf{S_i}$ represent an observation that has probability $f_1(S_i)$ under H_1 and $f_2(S_i)$ under H_2. Further, let **r** be the police's belief that hypothesis one is true. Then a critical region can be defined $\{\gamma, \delta\}$ such that if $\rho > \delta$ then accept H_1; if $\rho < \gamma$ then accept H_2; and if $\gamma < \rho < \delta$ then continue sampling. At each stage, an additional observation is drawn that

[26] This assumption could be replaced to allow for the possibility that the criminal is not included within the suspect group. A simple model incorporating this idea would postulate a fixed probability P that the criminal is a suspect. But this would muddy the analysis without changing any of the results.
[27] Abraham Wald, *Sequential Analysis*, New York: John Wiley (1947).

updates the police's beliefs according to Bayes rule, allowing each period's belief to be written as a function of the initial belief, ρ, and the series of observations $\mathbf{S_i}$

such that
$$\rho_i = \frac{r\prod_{j=1}^{i} f_1(Sj)}{r\prod_{j=1}^{i} f_1(Sj) + (1-r)\prod_{j=1}^{i} f_2(Sj)}$$

and sampling is stopped if ρ_i is outside the critical range.

In order to calculate the expected number of samples, given a critical region (γ,λ), it is convenient to re-define the signals received, such that $S_i=1$ if the signal that period is high, and $S_i = -1$ if the signal is low. The sum of signals, $\sum S_j$, is a sufficient statistic for the police's belief about suspect 1's guilt, and the critical region is redefined to determine the bounds on this statistic. The probability that the police choose to search suspect one, based on its samples of the suspects, is equal to the probability that $\sum S_j$ reaches the upper bound **a** before it reaches the lower bound **-b**. Let $\pi_w(-b,a) = P_w(\sum S_j$ reaches -b before it reaches a | H_w is true) and let $E_w[n|-b,a]$ equal the expected number of samples before a decision is reached given that H_w is true. Then without approximation, these variables can be solved[28] as[29]

$$\pi_w(-b,a) = \frac{\left(\frac{1-u}{u}\right)^{a+b} - \left(\frac{1-u}{u}\right)^{b}}{\left(\frac{1-u}{u}\right)^{a+b} - 1} \quad \text{and} \quad E_w[n|-b,a] = \frac{a-(a+b)p_w(-b,a)}{(2u-1)} \quad \text{for } u \neq 1/2.$$

These two equations will determine the expected number of samples, the expected cost of an investigation, and the probability of a criminal being caught.

---

[28] David Blackwell and M.A. Girshick, *Theory of Games and Statistical Decisions*, New York: John Wiley (1954).

[29] The solution to this Markov process is fully explained in Shelton M. Ross, *Introduction to Probability Models*, New York: Academic Press (1980) p. 163. If P(I) is the probability that a is reached before b, where I is the sum of signals, and the probability that I increases by one is u, then P(I)= u*P(I+1) + (1-u)*P(I-1). Using the fact that P(a)=1 and P(b)=0, this Markov process with absorbing barriers can be determined by solving the system of equations generated. The probability of reaching a before b is then specified for all values of I. Set I=0 and the Blackwell equations are the result.

The expected cost of the optimal search procedure will determine whether or not the police start an investigation. Because the sampling costs are constant, if it is optimal to collect the first signal it will always be optimal to collect the next signal, until the decision criterion is reached. Because the loss function has not changed, the police face the same decision in the second period, but with better information. Once the police start to sample, the model predicts a perfect clearance rate for the police. [30] The only way the criminal can escape detection is if the police decide it is too costly to search for him or her . If the court's warrant requirement is **p**—where **p** is the level that the sum of signals must reach before a warrant is issued—then the expected cost of an investigation, before sampling begins, is given by:

$$E[C] = \rho\ \{(s + w) \times \pi_1 (\text{-}p, p) + c \times E_1[\ n \mid \text{-}p, p]\} +$$
$$(1\text{-}\rho)\ \{(s+w) \times [1 - \pi_2 (\text{-}p, p)] + c \times E_2[\ n \mid \text{-}p, p]\} + s$$

The police will begin an investigation if E[C] is less that K(b). If **b*** is the critical benefit from a crime such that E[C] = K(b*), then for all crimes with **b** greater that **b***, the police will investigate, and for all crimes with **b** below **b***, they will not.

Due to the opportunity costs of committing a crime, the citizen will not commit any crime for which $\mathbf{b} < \mathbf{O_p}$, regardless of the probability of conviction and the sentence received. The remaining expected cost of a crime is a consequence of the sentence received if caught, the expectation of which will determine whether a citizen commits the crime. In this model, the police have a perfect clearance rate for all crimes they investigate and the police's decision to investigate depends critically upon whether the crime involves a benefit to the offender (assumed to be correlated with the police's objective function) above a critical value **b***. Accordingly, the probability that a criminal will be caught depends on the benefits of the crime committed. Crimes with benefits exceeding **b*** are

[30] One complication of the model, which would replace the perfect clearance rate with a much more realistic less-than-perfect clearance rate, would allow for the true evidence to depreciate with time. In that model, during each period the evidence would have a fixed probability of disappearing, as drugs may be flushed away, memories fade, and physical evidence is contaminated. Any time taken by the police in sampling the suspects will increase the probability that the criminal will escape conviction. But this simple twist introduces serious mathematical complications into the model. Since the police will also consider the depreciation of evidence, the expectation of depreciation over time needs to be calculated . This, however, will depend upon the distribution of N, the number of samples before a decision is reached, which is unknown.

certain to be detected accurately (probability of one); crimes with benefits below **b*** certain not to be detected (probability zero). The crime rate is defined as $\phi(b^*,O_{p,},Pc,S) = F(b^*) - F(O_p) + F(b_h) - F(O_p + Pc^*S)$, or the proportion of criminal opportunities that are taken.

It is now possible to consider the potential impact of the Supreme Court's *Mapp v. Ohio* ruling on police investigation decisions and the eventual crime rates. The ruling may be interpreted as increasing the probable cause requirement imposed upon the police by the judicial system. This will increase the bounds for the police's optimal stopping rule and will increase the expected cost of an investigation. Therefore, this economic model of the search warrant process predicts that the Supreme Courts decision to heighten the warrant requirement will result in a decrease in the number of investigations undertaken. As the police adapt to the heightened judicial requirements, so to will the criminals respond to the change in police investigations . Thus, contrary to the assumptions underlie the previous empirical research of the *Mapp* ruling, imposing an exclusionary rule on the states may have increased the crime rates, without revealing a significant number of cases dismissed at trial. Employing two panel data sets, we explore the predictions of this economic model and the *Mapp v. Ohio* ruling.

IV. EMPIRICAL ANALYSIS—FIFTY STATES, 1958-67 DATA

A. The Data

The crime data was gathered from the Uniform Crime Reports, which are compiled by the FBI, and was used in many of the earlier studies, primarily because it is the only complete crime data set available from before 1970. [31] This data has several well recognized problems. First, it consists only of reported crimes, meaning both the victim and the police must report the crime to the FBI. Second, the data is not really uniform, as the number of cities reporting to the FBI increased over the 1957-1967 period. And finally, the data is compiled by a political organization with its own agenda. Nonetheless, since the exclusionary rule was enacted in 1961 and the Uniform Crime Reports are the only state crime data available, [32] we rely on these crime figures for our empirical analysis.

---

[31] U.S. DEPARTMENT OF JUSTICE, FEDERAL BUREAU OF INVESTIGATION, "*Crime in the United States: Uniform Crime Reports*."
[32] Except for homicide data compiled in the Vital Statistics Report.

A more significant problem is the absence of information on the severity of punishment for each offense type. This data is not available, at the state level, for the years from 1958-67. If the level of punishment did not change significantly across states, then the fixed effect model with group dummy variables will capture all of this effect, and the regressions will compute unbiased estimates. Similarly, if the severity of punishment changed in a uniform way each year (i.e., in the same way for all the states), then employing period effects in the fixed effect specification will produce unbiased estimates. But if the criminal sentences otherwise changed, or if the level of punishment was affected by the *Mapp* ruling, then our regressions may be misspecified and the estimates biased. We cannot test this with the national data set, but the consistency of results between alternative data sets indicates that it is unlikely to be a problem. Moreover, if penalties do change in response to *Mapp*, it is likely that they will increase to adjust for the increased cost of solving crimes. If so, then any effect we find of the rule will tend to understate its true magnitude.[33]

B. Model Specifications

The primary model specification for this data set will be a fixed effect model with period and group effects. This model permits the constant term to vary for each of the 48 states, so that many of the differences in crime rates between the states are captured in the constant terms. While we included the various explanatory variables, x_{it}, to capture broad movements in crime, it remains possible that we have failed to include some vital explanatory variable that had a broad and uniform effect on the crime rates of all the states. The introduction of the time-specific dummy variables addresses this concern. The principal model takes the form:[34]

$$\text{Log(CRIME)}_{it} = a_i + b' x_{it} + g\ \text{MAPP} + e_{it} \qquad (1)$$

One additional specification is examined. In the model, the variable Log(POLEXP) is

---

[33] We have access to Isaac Ehrlich's 1960 data, which does include data on probabilities and severity of punishment. This data is available from the Inter-University Consortium for Political and Social Research at www.icpsr.umich.edu. We have rerun Ehrlich's regressions with an additional variable for whether illegal evidence was admissible in the state in 1960. We find that admissibility was not significant in explaining crime rates, holding the probability of capture and conviction constant, but did significantly lower the probability of capture and conviction. (Detailed results available upon request from the authors.)

[34] CRIME includes the crime rates for murder, assault, robbery, burglary, larceny, and auto theft. Thus,

more than likely endogenous and a function of CRIME. Therefore, we examine another specification that takes the form:

$$\text{Log(CRIME)}_{it} = a_i + b' x_{it} + g\ \text{MAPP}_{it} + \lambda\ \text{Log(POLEXP)}_{it} + e_{it} \qquad (3)$$

$$\text{Log(POLEXP)}_{it} = \alpha_i + \beta\ \text{Log(CRIME)}_{it} + \chi\ \text{INCOME}_{it} + \delta\ \text{MAPP} + d_{it}$$

$$\text{s.t. COV}(e_{it}, d_{it}) = 0 \quad \forall\, i,t$$

The MAPP variable is a dummy variable that will capture the effect of the Supreme Court's *Mapp v Ohio* decision. This dummy variable equals 1 if the year is 1962 or later and the state had not previously adopted the exclusionary rule. The explanatory variables in x_{it} are used to hold constant other factors that could account for changes in crime rates. The criteria for using the data was principally the availability of the state data, and secondarily variables found relevant in previous criminal empirical and theoretical works.

Gary Becker developed the economic analysis of criminal behavior, applying the theory of choice under uncertainty to a criminal's decisions. [35] Becker's rational choice model predicts that citizens will weigh the expected costs of committing a crime against the expected benefits, and will participate in all crimes with a positive expected outcome. Empirical analysis that attempts to estimate a supply function for criminal behavior must, therefore, find adequate proxies for the expected costs and benefits of committing crimes.

In this study, the variables gathered and introduced as explanatory variables for a state's crime rates are similar to those used by Isaac Ehrlich, whose pioneering empirical work verified that regression analysis and the rational choice model are useful tools in the analysis of criminal behavior. [36] Generally, the explanatory variables attempt to capture the expected costs of committing the crime and the expected benefits from the crime. The expected costs include the opportunity costs of lost wages, reputation, social standing, and future employment. These variables have long been recognized as being as important as the more obvious explicit cost of committing a crime: the sentence received, adjusted for

---

for each specification developed, we will perform six separate regressions.

[35] Gary S. Becker, "*Crime and Punishment: An Economic Approach*," 76 J. POLITICAL ECONOMY 169 (1968).

[36] Isaac Ehrlich, "*Participation in Illegitimate Activities: An Economic Analysis*," 81 J. OF POLITICAL ECONOMY 521 (1973).

the probability of being caught and convicted.[37] The data gathered to analyze the impact of *Mapp v. Ohio* on crime rates includes several of the standard proxies for these theoretical explanatory variables. The variables (defined in Table 2) include state police expenditure, unemployment rates, personal incomes, education levels, percentage of the population living within an urban setting, population age, and racial distributions. These variables are proxies for the probability of being caught, the opportunity cost of committing a crime, and the benefits from committing a crime.

C. <u>Empirical Results</u>

We begin our analysis with the state data gathered from 1958-67 to test the impact of exogenous changes in criminal procedure on state crime levels. With this data set, only the impact of *Mapp v. Ohio* is investigated.[38] Crime rates were broken down by the type of offense, which allows the analysis to consider the effect of *Mapp* upon different types of criminal activity. (Table 3 provides the FBI definition of the six crime types.) Table 4 shows the results using a fixed effect model with group and period effects, which, we believe, is the best specification. Eight variables were included to explain the changes in crime rates for the decade surrounding the introduction of the exclusionary rule in 1961. Becker's rational choice model predicts the following signs for each variable: Log(PolExp), negative; Per18-20, positive; Employ, negative; PerWhite, negative; UpperEd, negative; Urban, no prediction; Rincome, positive.

In addition to the eight variables shown, 48 dummy variables were introduced to sweep the fixed state effects from the model. These dummy variables capture all of the interstate differences in crime due to constant differences between the states. For example, it is likely that the average temperature may account for some of the differences in murder rates between Vermont and Alabama. This difference is swept from the analysis by the group dummy variable, along with all other fixed state effects, such as the statutory required punishment or the level of corruption in the police precincts.

To supplement these group effects, the specification for Table 4 includes period

---

[37] See for example Jonathan M. Karpoff and John R. Lott, Jr., "*The Reputational Penalty Firms Bear from Commiting Criminal Fraud*," 36 JOURNAL OF LAW AND ECONOMICS 757 (October 1993).

[38] In the next section, we use the larger city data set to investigate the impact of *Miranda v. Arizona*, *Gideon v. Wainwright*, and *Wolf v. Colorado*.

effect dummy variables. This specification introduces 10 additional dummy variables, one for each year of the sample. These period effects capture any national trends in crime rates. The remaining differences in crime rates are explained by relevant characteristics of the state that change over the sample period and the MAPP dummy variable. Due to the period effects, the MAPP dummy variable captures any differences in crime rates between those states that used an exclusionary rule remedy and those that did not. These results show that in jurisdictions forced by the Supreme Court's ruling to exclude evidence, assaults increased by 16.5%, robbery by 7.3%, burglary by 6.0%, larceny by 3.8%, and auto theft by 4.3%. There was also a small but statistically insignificant increase in homicide rates.[39]

The results from the fixed effect model with group and period effects assumed that police expenditures are exogenously determined. This, of course, may not be true. If police expenditures are a function of crime rates, and thus endogenous to the model, our estimated OLS coefficients are inconsistent.[40] To account for any potential simultaneous-equation bias, we re-computed our estimates using a simultaneous-equation specification. The results are presented in Table 5. The significance of MAPP dropped slightly for some of the crime types, but the analysis and results are not dramatically different than the results from the single-equation specification.

The empirical findings from this analysis support the main prediction of the economic model of the search warrant process: that forcing states to adopt an exclusionary rule will have a detrimental impact on crime rates. We estimate with this data set that the exact magnitude of that effect, from 1961 to 1967, ranged from 3% for larceny crimes to 16% for assault.

---

[39] To test the robustness of these results, several specifications were examined. These included the ordinary least squares results with no fixed or period effects, a fixed effect model with only group effects, a random effect model with only group effects, and a random effect model with both group and period effects. The random effects model produced nearly identical estimates as its fixed effect counterpart . OLS without group effect detected substantially larger effects from the *Mapp v. Ohio* ruling, but this may be due to endogenity bias that is corrected by the fixed effect model. Specification tests rejected the non-logarithmic specification in favor of the logarithmic specification.

[40] WILLIAM GREENE, *Econometric Analysis*, 2nd Ed. p. 579 (1993)

V. EMPIRICAL ANALYSIS—396 CITIES, 1948-1969 DATA

A. The Data

This data was gathered by Herbert Jacob and was made available by the Inter-University Consortium for Political and Social Research (www.icpsr.umich.edu).[41] The crime data from 396 cities was gathered. (The list of cities is available from the authors.) The variables (defined in Table 6) available for some[42] of the years in this data set are:

- Crime rates for murder, rape, robbery, assault, burglary, theft, auto theft, violent crimes, and property crimes;
- Number of police officers & police expenditures;
- Population estimates;
- Median family incomes ;
- Percentage of adult population with 5th grade education;
- Percentage of population non-white;
- Civilian labor force & number of persons employed over 16 years old ;
- Number of persons aged 15-24 years old; and
- Percentage of families with income of less than $3,000 per year.

This larger, city data set offers two advantages over the state panel data set: (1) the smaller unit of analysis and nearly 9,000 observations permit for more detailed investigation of the impact of *Mapp v. Ohio*; and (2) the varying sizes of the different cities allows us to examine the effect of *Mapp* on large and small cities. In addition , we explore two collateral questions with this data: (1) did the effect of the *Mapp* ruling diminish over time; and (2) did the *Wolf*, *Miranda* and *Gideon* rulings of the Supreme Court have effects on criminal behavior?

B. The Model Specifications

The principal specification for this data set is the fixed effect model with group and period effects and will take the form:

$$\text{Log(CRIME)}_{it} = a_i + t_t + b'x_{it} + g\,\text{MAPP} + e_{it} \qquad (7)$$

---

[41] ICPSR Study 8076 (Part I: Baseline).

[42] Some of the socioeconomic variables like median income, age, education, etc ., are only provided for the census years. Estimates were constructed based on a linear logarithmic function. Observations where crime rates, arrest rates, or police expenditures were missing are dropped from the analysis.

The explanatory variables will include two new dummy variables: MappBurb, MappCity. MappBurb and MappCity attempt to determine if the Court's 1961 ruling had a different effect on highly populated cities than on smaller cities. A second specification we examine takes the form:

$$\text{Log(CRIME)}_{it} = a_i + t_t + b'x_{it} + g'\ t_t \times \text{MAPP} + e_{it} \qquad (8)$$

This specification permits the effect of MAPP to change over time. Examination of the vector of estimates, g', will reveal the short-run and long-run impact of the Supreme Court's *Mapp* ruling.

C. Empirical Results: *Mapp*

The general results from the first data set carry through to this larger data set, with one interesting new observation: the exclusionary rule had a dramatically different effect on large urban cities than on smaller cities. These results are somewhat counterintuitive and unexpected. The effect on smaller cities was larger for almost every offense type. The only exception was assault; the *Mapp* ruling appeared to have an equally positive impact on assault rates in the smaller as in larger cities. One might expect that the smaller cities with lower crime rates would not be as significantly burdened by the heightened warrant requirement. But the data clearly says otherwise. The results are presented in Table 7. Included in Table 7 is the test statistic used to determine if the difference in the MAPP coefficients (MappBurb v. MappCity) is statistically significant. For all crime types other than assault, the *Mapp* ruling had a significantly lower effect in urban cities than in smaller suburban cities. The regression results show a tremendous impact in smaller cities, an effect that would be masked in the aggregated state data results due to the dominance of the larger urban cities.[43]

In addition to exploring the possibility that *Mapp* has a different effect on smaller cities than on urban cities, we use this larger disaggregated data set to explore the long-term versus short-term effect of *Mapp*. Two alternative hypotheses are explored. It is

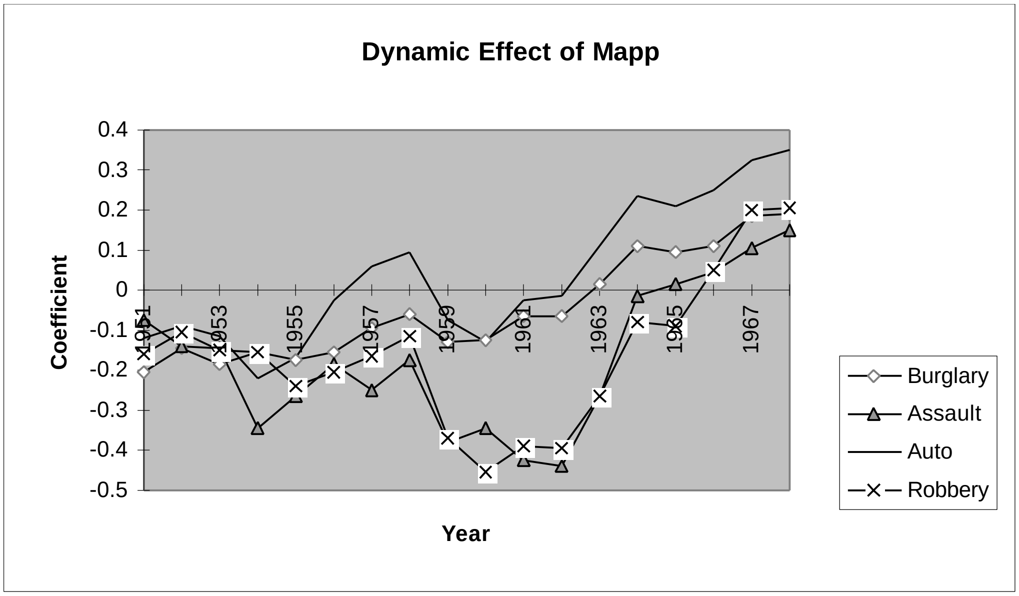
---

[43] As with the state data set, multiple specifications were examined with the city data set. These included a multiple equation specification, random effects model with period and group effects, and fixed and random effects with only group effects, all computed with the single equation and multiple equation specification. The results from Table 6 are representative of the results from all of these different specifications; the Mapp ruling was found to increase crime rates in those states that did not already exclude tainted evidence, and the effect was more pronounced in smaller cities.

possible that *Mapp* was merely a one-time shock to the criminal justice system, the effect of which would at first be dramatic but would quickly vanish as police, prosecutors and judges adjust to the new (but only marginally different) requirements. An alternative hypothesis might be that *Mapp* would initially have only a small impact, but this impact would grow over time as (1) federal courts begin to seriously review state officers' actions, and (2) the Supreme Court continues to heighten and refine the exclusionary rule requirements. For example, following the *Mapp* ruling, the Court declared that in addition to excluding unlawfully obtained evidence, state courts must exclude evidence tainted by that unlawfully obtained evidence, in other words evidence obtained as a direct result of the information revealed by the unlawful search and seizure. [44] The differential effect of *Mapp* on cities that did not have an exclusionary rule compared to those that did might also grow over time because in cities that already had an exclusionary rule police and criminals would have already adapted to this rule. In cities where the rule was newly imposed, it would have taken time for the police to adapt, and criminals would have not changed their behavior until they observed the effects of the changes in police behavior.

To examine the year-by-year effect of the *Mapp* ruling, several new dummy variables were introduced into the regression analysis. Each year was given its own MAPP dummy variable. For example, Mapp57 equals 1 if the state would be affected by the 1961 *Mapp* ruling and the year was 1957, and zero otherwise. These MAPPYEAR variables from 1951-1968 were introduced (four years were dropped in order to utilize a fixed effect model) and coefficients for those variables were estimated using the fixed effect model with group and period effects. Focusing only on those offense types for which a positive increases was detected, the dynamic effect of the *Mapp* ruling is revealed below:

[44] Wong Sun v. United States, 371 U.S. 471 (1964).



This graph illustrates the year-by-year impact of *Mapp*. Not surprisingly, from 1951-1962, those states that did not exclude tainted evidence had lower crime rates. Following 1962, crime rates began to rise quickly as compared to the crime rates of those cities that already excluded unlawfully obtained evidence, an effect that continued to increase over time.

D. <u>Empirical Results: *Wolf*, *Miranda and Gideon*</u>

Since a large number of cities are included in this data set, we are able to investigate the impact of three other controversial Supreme Court rulings in criminal procedure on crime rates: *Wolf v. Colorado*[45] in 1949; *Gideon v. Wainwright*[46] in 1963; and *Miranda v. Arizona*[47] in 1966. In *Wolf*, the Supreme Court incorporated the Fourth Amendment, thereby imposing its commands on state as well as federal actors. By truncating the sample to years 1948-1952, we test to see if the incorporation of the Fourth Amendment, without imposing any constitutional remedy, had any effect on crime rates. This specification will take the form:

$$\text{Log(CRIME)}_{it} = a_i + t_t + b'x_{it} + g\ \text{WOLF} + e_{it}. \qquad (9)$$

This permits us to determine if *Wolf* had a different impact on those states that already excluded unlawfully obtained evidence than on those that admitted such evidence. WOLF

[45] 338 U.S. 25 (1949).
[46] 372 U.S. 335 (1963).
[47] 384 U.S. 436 (1966).

is a dummy variable that equals one after 1949 if the state did not exclude evidence obtained unlawfully. Similarly, we test the impact of *Gideon* by including an additional explanatory variable into the fixed effect model with group and period effects. The new dummy variable, GIDEON, equals one if the year is 1963 or later and the state did not provide indigent defendants with counsel as of 1963, and zero otherwise.[48] To test for the impact of *Miranda v. Arizona*, we must employ a fixed effect model without period effects, to avoid multicolinearity. *Miranda*, unlike *Gideon*, *Mapp*, and *Wolf*, was a completely new rule developed by the Supreme Court with no comparable rule in any state, so that we do not have a control group of unaffected states.

In *Wolf*, the Supreme Court held that the directives of the Fourth Amendment bound the states. But the Court went on to declare that states could choose the method of enforcing Fourth Amendment rights. It seemed settled, following *Wolf*, that state court judges need not worry about continuing federal supervision and review of their decisions. To detect if *Wolf* had an impact on state judges, police, and criminals, we truncate our sample to look only at the years 1948-1952. The MAPP dummy variable is replaced with a WOLF dummy variable, which equals one after 1949 if the state did not exclude evidence and zero otherwise. The results are presented in Table 9. The results from the fixed effect model with group and period effects reveal that *Wolf v. Colorado* may have had a small (but statistically significant) negative impact on crime. Crime rates fell in those states free from the uncertainty regarding federal oversight of their decisions to use unlawfully obtained evidence at trial. We suggest that following *Wolf*, state courts felt more comfortable using unlawfully obtained evidence at trial, and the apparent sanctioning of that behavior by the Supreme Court resulted in slightly lower crime rates.

The effect of *Gideon v. Wainwright*, the landmark decision that gave every indigent defendant the right to trial, may have increased crime rates in those 15 states that

---

[48] See Table 8 for a list of those states that provided indigent defendants counsel at trial. These tabulation are not as reliable as the *Mapp* split, because there is considerable uncertainty regarding which states did not, in fact, provide counsel to indigent defendants. While many states did not require trial judges to provide counsel to indigent defendants, letters from prosecuting attorneys and attorney generals indicate that it was the practice in many states to do so. *See* Yale Kamisar, *The Right to Counsel and the Fourteenth Amendment: A Dialogue on "The Most Pervasive Right" of an Accused*, 30 UNIV. CHICAGO L. REV. 1, 67 (1962). For the *Gideon* dummy variable, we presume the Supreme Court's ruling will have some effect on those states where indigent defendants had no *guaranteed* right to counsel.

chose not to provide counsel to these defendants. The regression results, provided in Tables 10 and 11, suggest that robbery and assaults dramatically increased following the *Gideon* ruling, with a larger and more significant increase in suburban cities than in urban cities. One puzzling phenomenon was observed—a decrease in burglaries in smaller cities. One might question why *Gideon* would have had such a dramatic impact on crime rates. The answer is simple—in some states, *Gideon* was applied retroactively. This meant that criminals currently incarcerated without having counsel present were eligible for a new trial. In Florida, the state that convicted Clarence Earl Gideon for breaking and entering, 1,976 prisoners were released outright and another 500 were back in court by January 1, 1964.[49] This mass release of indigent men (who could not afford an attorney) with a disposition for committing crimes could alone be responsible for the observed increase in crime following the *Gideon* ruling.

The analysis of *Miranda* tends to support the conclusion that this surprising Supreme Court decision seriously hampered police investigation techniques. Professor Cassell previously examined the impact of *Miranda* on national clearance rates and discovered a dramatic decrease in confessions after 1966.[50] The results of the regressions are provided in Tables 12 and 13. The fixed effect model, without period effects, estimates that by hampering police investigations, the Supreme Court may have increased total crime rates by 11% with its *Miranda* ruling.

VI. SUMMARY OF EMPIRICAL ANALYSIS

When the Supreme Court decided to enact a universal exclusionary rule, it did not explicitly discuss the impact that rule would have upon the crime rates in the affected states. Although the Court may have understood that an effect was possible, the Justices needed to decide if one method of enforcement was required as the sole remedy capable of protecting our Fourth Amendment rights. To decide this, it may have been helpful to understand the effect of the rule on society, but this may not have altered their decision. The Court argued that the exclusionary rule would better protect individuals from unauthorized intrusion by government officials and probably would have concluded that

[49] ANTHONY LEWIS, *Gideon's Trumpet*, p. 215 (1964).
[50] *See* Cassell, *supra* note 7.

the benefits to society outweighed the costs from higher crime rates. This is especially likely since the Fourth Amendment implicitly places an individual's privacy interest above society's interest in being free from criminal behavior.

The results of our regression analysis, using a variety of data and econometric techniques, supports our theoretical predictions—namely that forcing police to look-listen-and-wait before searching suspected criminals will have a dramatic impact on police investigation techniques and eventually on crime rates. Both the state data and the city data support this conclusion. And while the effect of *Mapp* on murder rates is ambiguous, the effect on other offense types is not. The imposition of the exclusionary rule by the Supreme Court on states unwilling to self-select that remedy has proven to have a predictably adverse impact on crime rates.

The data also indicates that two other controversial Supreme Court rulings, *Gideon* and *Miranda*, caused detectable increases in crimes rates. The effect of *Gideon* on assault and robbery crimes may have been due to the release of thousands of indigent criminals, rather than to the effect of providing counsel at trial. The *Miranda* ruling, which further limited the investigation techniques of police, is correlated with an 11% increase in total crimes and a nearly 33% increase in violent crimes.

More surprisingly, the *Wolf v. Colorado* decision appeared to have an impact on crime as well. While not as controversial a decision (although it was overturned by the *Mapp* ruling), the *Wolf* decision did sanction the use of unlawfully obtained evidence at trial by state courts. If prior to *Wolf*, the state courts were concerned over the general movement to apply all of the bill of rights to the states, and the long-standing federal remedy of the exclusionary rule for Fourth Amendment violations, they may have hesitated to use tainted evidence, fearing to involve the federal courts. Following *Wolf*, the Court may have dispelled this concern, spurring additional use of tainted evidence and thus lowering crime rates. The limited empirical analysis that we could perform supports this conclusion.

The analysis of the larger city data set permitted deeper inquiry to differentiate the impact of *Mapp* on urban and suburban cities. Contrary to our expectation, the data unambiguously illustrates that smaller cities bore the brunt of the Supreme Court's decision. Because every study that examined the impact of *Mapp* focused only on larger cities, they missed the real effect of the new ruling. In may be that larger cities have greater flexibility and funding to adopt to the new procedures, or had more rigorously imposed the alternative civil sanctions. By only studying crime rates in larger cities, the older studies missed the most dramatic impact of the *Mapp* ruling.

For proponents of the exclusionary rule, the analysis of the dynamic impact of the *Mapp* ruling is disheartening. Proponents have argued that any short-term impact of the exclusionary rule would quickly dissipate as police adopted to the rule and began to discover other ways to catch criminals. But the data fails to support this seemingly plausible theory. Rather, the long-term impact of the *Mapp* ruling increased steadily as the decade passed. While police have adopted new police investigation techniques (such as finger-printing and ballistics analysis) these new techniques, if anything, make losing tangible property due to the exclusionary rule even more costly. Not until more recently did the Court revoke Fourth Amendment habeous petitions,[51] enact the good-faith exception,[52] and shift the general Fourth Amendment law away from a warrant requirement and back towards a reasonableness standard.[53] Our empirical analysis suggests that such steps to relax the warrant requirements will lower crime rates.

VII. CONCLUSION

This analysis cannot be used to show that the exclusionary rule should be replaced, or that the "good faith" doctrine should be used, or that the Supreme Court was incorrect in its decision. Rather, this is a positive analysis of a radical change in criminal procedure conducted to test currently held beliefs. At no time have we attempted to quantify the other costs of the exclusionary rule, which include the time spent on motions to suppress, the number of guilty suspects that escape conviction, and the decrease in police productivity as they alter their allocation of resources. More importantly, we do not examine the benefits of the exclusionary rule, nor do we posit or analyze any alternative procedure to control police misconduct. The goal of this research was to test the

---

[51] Stone v. Powell, 428 U.S. 465 (1976).
[52] United States v. Leon, 468 U.S. 897 (1984).
[53] Wilson v. Arkansas, 115 S.Ct. 1914 (1995) (declaring that the knock and announce requirement is not a rigid constitutional requirement, but rather a component of the Fourth Amendment reasonableness

application of the prevalent economic theories of criminal behavior in criminal procedure, which should alter any subsequent normative analysis of criminal procedure as its effect is incorporated in future analysis.

We accomplished this goal—as dramatic and predictable results from changing criminal procedure were observed. The findings of the fixed effect model with group and period effects are particularly strong. These findings show a positive and significant effect of the Supreme Court's alteration of criminal procedure on the crime rates of those states affected. The Court's decision to force states to provide indigent defendants counsel increased the number of assaults and burglaries. The novel ruling that created *Miranda* rights under the Fifth Amendment increased total crimes by 11%, with violent crimes increasing by 33% following this change in criminal procedure. But the most dependable results are the increases in crime rates associated with the Supreme Court's decision to force states to exclude unlawfully obtained evidence at trial. Looking at aggregated state data, this decision increased larceny by 3.8 %, auto theft by 4 %, burglary by 6%, robbery by 7.3% and assault by 16.5%. But these results mask gigantic impacts in smaller cities—where the imposition of the exclusionary rule increased violent crimes by 31% and property crimes by 21%. This compares with a 15% increase in violent crimes in urban cities and only a 3% increase in property crimes in urban cities. These increases in crime rates are a weighty cost attached to each of the Supreme Court's decisions to change criminal procedure. Society may decide the benefits of our new protections are worth these costs, but an informed debate requires that these costs be known and considered.

inquiry); California v. Acevedo, 500 U.S. 565 (1991) (J. Scalia concurring).

TABLES

Table 1: State Exclusionary Rules, 1961

	Admissible	Excludable
	Arizona	Alabama
	Arkansas	California
	Colorado	Delaware
	Connecticut	Florida
	Georgia	Idaho
	Iowa	Indiana
	Kansas	Kentucky
	Louisiana	Maryland
	Maine	Michigan
	Massachusetts	Mississippi
	Minnesota	Missouri
	Nebraska	Montana
	Nevada	North Carolina
	New Hampshire	Oklahoma
	New Jersey	Oregon
	New Mexico	Rhode Island
	New York	South Dakota
	North Dakota	Tennessee
	Ohio	Texas
	Pennsylvania	Washington
	South Carolina	West Virginia
	Utah	Wisconsin
	Vermont	Wyoming
	Virginia	Illinois .
Total	24	24

Table 2: Variable Descriptions—State Data (1958-67)

Variable (short name)	**Description**
LnPolExp	The per capita police expenditures, expressed in constant dollars. **Source**: Compendium of State Government Finances & Criminal Justice Expenditures and Employment
Per18-20	Percentage of the population between age 18 and 44. **Source**: Current Population Reports
Employ	Percentage of the population employed. **Source**: Social Security Bulletin (Annual Statistical Supp.)
PerWhite	Percentage of the population that is white, extrapolated from census data. **Source**: Statistical Abstract of the United States
UpperEd	Percentage of the population that are in upper schools divided by Per18-20. **Source**: Statistical Abstract of the United States
Urban	Percentage of the population living in the metropolitan area. **Source**: Statistical Abstract of the United States
Rincome	Real per capita income. **Source**: Survey of Current Business
Ln(Crime)	Log of per capita crime rates. (These are the dependent variables) **Source**: Vital Statistics of the United States (homicide data) Uniform Crime Reports (all other crime types)
MAPP	The exclusionary rule dummy variable. This variable is ONE if the State had an exclusionary rule enacted, and ZERO otherwise. After the *Mapp v. Ohio* ruling in 1961, the *Mapp* variable equaled ONE for all States. (Year>61—Mapp=1).

Table 3: Definitions of Crime Classifications

1. MURDER (CRIMINAL HOMICIDE): MURDER AND NON-NEGLIGENT MANSLAUGHTER—All willful felonious homicides as distinguished from deaths caused by negligence. Excludes: Attempts to kill, assaults to kill, suicides, accidental deaths, and justifiable homicides. Justifiable homicide are limited to: (a) the killing of a person by a peace officer; and (b) the killing by a private citizen of a person in the act of committing a felony.

2. ROBBERY: Stealing or taking anything of value from the care, custody, or control of a person by force or violence or by putting that person in fear, such as strong-arm, robbery, stickups, armed robbery, assault to rob, and attempts to rob.

3. ASSAULT (AGGRAVATED ASSAULT): Assault with intent to kill or for the purpose of inflicting severe bodily injury by shooting, cutting, stabbing, maiming, poisoning or scalding by the use of acids, explosives, or other means. Excludes: Simple assault, assault and battery, fighting, etc.

4. BURGLARY (BREAKING OR ENTERING): Burglary, House-breaking, safe-cracking, or any breaking or unlawful entry of a structure with the intent to commit a felony or a theft. Includes attempts.

5. LARCENY THEFT (EXCEPT AUTO THEFT): Fifty dollars and over in value; Thefts of bicycles, automobile accessories, shoplifting, pocket-picking, or any stealing of property or article of value that is not taken by force and violence or fraud. Excludes Embezzlement, con games, forgery, worthless checks, etc.

6. AUTO THEFT: Stealing or driving away and abandoning a motor vehicle. Excludes taking for temporary or unauthorized use by those having lawful access to the vehicle.

Source: U.S. DEPARTMENT OF JUSTICE, FEDERAL BUREAU OF INVESTIGATION, *Crime in the United States: Uniform Crime Reports*, p. 61 (1970). Provided by Isaac Ehrlich, ISPCR study 7716.

Table 4: State Data (1958-67)—Fixed Effect Model (Group and Period Variables)

	Murder	Assault	Robbery	Burglary	Larceny	Auto
MAPP	0.68923E-03	0.16561	0.073510	0.060396	0.038629	0.043159
(t-ratio)	(0.015)	(3.203)	(1.743)	(3.193)	(1.907)	(1.660)
LnPolExp	0.21054	0.064962	0.059493	0.078749	0.025133	-0.027239
(t-ratio)	(2.483)	(0.686)	(0.770)	(2.272)	(0.677)	(-0.572)
Per18-20	-0.90589	1.1938	-8.3707	8.0978	-7.6580	-12.003
(t-ratio)	(-0.072)	(0.085)	(-0.732)	(1.579)	(-1.394)	(-1.703)
Employ	-0.83571	0.41451	0.65662	-0.78887	0.67771	1.1806
(t-ratio)	(-0.687)	(0.305)	(0.593)	(-1.589)	(1.274)	(1.730)
PerWhite	0.49864	-0.17289	0.17934	-0.16091	0.13664	0.19965
(t-ratio)	(0.791)	(-0.245)	(0.312)	(-0.623)	(0.494)	(0.563)
UpperEd	0.034259	-0.067563	0.050231	0.16035	-0.28511	-0.32076
(t-ratio)	(0.099)	(-0.174)	(0.159)	(1.131)	(-1.878)	(-1.647)
Urban	-0.023527	-0.010073	-0.021405	-0.020127	0.026246	-0.020144
(t-ratio)	(-1.767)	(-0.672)	(-1.750)	(-3.669)	(4.467)	(-2.671)
Rincome	0.26035E-3	0.35789E-04	0.45650E-03	0.11804E-03	0.14584E-03	0.39905E-03
(t-ratio)	(3.440)	(0.422)	(6.601)	(3.805)	(4.389)	(9.358)
R^2	0.9058	0.92	0.94	0.95	0.96	0.943
N=480						

Table 5(a): State Data (1958-67)
Fixed Effect (Group & Period)
with Multiple Equations

	Murder	Assault
	Dependent Variable = Ln (Crime)	
	Coefficient (t-ratio)	Coefficient (t-ratio)
LnPolExp	1.3402	1.2450
	(2.095)	(1.740)
MAPP	0.020257	0.18652
	(0.350)	(2.993)
Per18-20	-28.872	-25.493
	(-1.412)	(-1.128)
Employ	-0.57085	-0.31263
	(-0.380)	(-0.193)
PerWhite	1.3299	0.86560
	(1.422)	(0.841)
UpperEd	-0.58696	-0.61581
	(-1.167)	(-1.115)
Urban	-0.042864	-0.018505
	(-2.736)	(-1.078)
R^2	0.8574	0.8878

	Murder	Assault
	Dependent Variable = LnPolExp	
	Coefficient (t-ratio)	Coefficient (t-ratio)
Ln(Crime)	0.67222	-0.74092
	(3.859)	(-3.666)
MAPP	0.022267	0.098572
	(0.562)	(1.764)
RIncome	-0.1614E-3	0.2140E-3
	(-2.046)	(2.465)
PopEst	-0.6234E-4	-0.2194E-3
	(-1.705)	(-8.400)
R^2	0.7950	0.6822

Table 5(b): State Data (1958-67)
Fixed Effect Model (Group & Period)
with Multiple Equations

	Robbery	Burglary
	Dependent Variable = Ln (Crime)	
	Coefficient (t-ratio)	Coefficient (t-ratio)
LnPolExp	2.4801	0.56260
	(2.633)	(2.043)
MAPP	0.10657	0.068014
	(1.299)	(2.836)
Per18-20	-61.862	-3.9920
	(-2.080)	(-0.459)
Employ	0.95886	-0.64533
	(0.449)	(-1.034)
PerWhite	1.7532	0.19165
	(1.295)	(0.484)
UpperEd	-1.1172	-0.11099
	(-1.537)	(-0.522)
Urban	-0.06088	-0.02967
	(-2.694)	(-4.491)
R^2	0.7833	0.9238

	Robbery	Burglary
	Dependent Variable = LnPolExp	
	Coefficient (t-ratio)	Coefficient (t-ratio)
Ln(Crime)	0.42558	1.0111
	(2.580)	(2.809)
MAPP	-0.011304	-0.055895
	(-0.345)	(-1.485)
RIncome	-0.1378E-3	-0.1347E-3
	(-1.620)	(-1.692)
PopEst	-0.1914E-3	-0.1994E-3
	(-11.555)	(-11.539)
R^2	0.8564	0.8568

Table 5(c): State Data (1958-67)

Fixed Effect (Group & Period)
with Multiple Equations

	Larceny	Auto
Dependent Variable = Ln (Crime)		
	Coefficient (t-ratio)	Coefficient (t-ratio)
LnPolExp	0.96211	2.5391
	(2.473)	(2.832)
MAPP	0.052127	0.077872
	(1.539)	(0.997)
Per18-20	-28.385	-66.792
	(-2.311)	(-2.359)
Employ	0.65244	1.1660
	(0.740)	(0.574)
PerWhite	0.78659	1.8778
	(1.406)	(1.457)
UpperEd	-0.73168	-1.4971
	(-2.438)	(-2.164)
Urban	0.012756	-0.056932
	(1.367)	(-2.647)
R^2	0.8926	0.5056

	Larceny	Auto
Dependent Variable = LnPolExp		
	Coefficient (t-ratio)	Coefficient (t-ratio)
Ln(Crime)	0.034322	0.42996
	(0.170)	(1.377)
MAPP	-0.4026E-2	-0.027083
	(-0.139)	(-0.767)
RIncome	0.3560E-4	-0.1187E-3
	(0.645)	(-0.948)
PopEst	-0.1823E-3	-0.1879E-3
	(-13.023)	(-11.920)
R^2	0.8932	0.8733

Table 6: Variable Descriptions—TEN CITY & 396 CITY DATA

Variable (short name)	Description
Ln(Crime)	The natural log of the per-capita crime ra te. **Source**: Uniform Crime Reports (1948-78)
LnPolExp	The natural log of the per-capita police expenditures **Source**: US Bureau of Census, City Gov. Finances
PoorEst	Percentage of families with income less than $3000. **Source**: Census data for 1950, 1960, 1970
PerEmploy	Percentage of the cities population employed and over 16 years old. **Source**: Census data for 1950, 1960, 1970
MedInc	Median family income divided by the CPI. **Source**: City County Data Book
EduEst	Percentage of the population 25 years and older with less than a fifth grade education. **Source**: City County Data Book
NonWhite	Percent of non-white population. **Source**: Census data for 1950, 1960, & 1970
Per14-25	Percentage the population aged 15-24. **Source**: Unknown
PopEst	Intercensus Population Estimate.
MAPP	A dummy variable equaling one if the state did not exclude evidence as of 1961 & the year > 1961.
MappCity	A dummy variable equaling one if MAPP = 1 and the city is classified as a central city by Jacob.
MappBurb	A dummy variable equaling one if MAPP = 1 and the city is classified as a suburban city by Jacob.
WOLF	A dummy variable equaling one if the state did not exclude evidence as of 1949 & the year > 1949.
EXCLUDE	A dummy variable that equal s one the year a state voluntarily adopts the exclusionary rule . For all but four states, this dummy variable is always zero.

Table 7: 396 City Data (1948-69)—FIXED EFFECT MODEL (GROUP & PERIOD): MAPP

	Murder	**Assault**	**Robbery**	**Burglary**	**Larceny**	**Auto**
MappCity	-0.079604	0.14071	0.067453	0.066802	-0.036706	0.15597
(t-ratio)	(-1.166)	(3.371)	(2.555)	(3.951)	(-2.594)	(8.050)
MappBurb	-0.82032	0.16885	0.27442	0.22463	0.15116	0.48792
(t-ratio)	(-6.916)	(2.366)	(6.046)	(7.705)	(6.189)	(14.615)
EXCLUDE	0.10808	-0.019267	-0.24782	-0.054485	-0.053487	0.37457E-2
(t-ratio)	(1.027)	(-0.300)	(-6.056)	(-2.071)	(-2.401)	(0.124)
LnPolExp	0.028011	0.073117	0.061372	0.14832	0.19091	0.019775
(t-ratio)	(0.307)	(1.316)	(1.729)	(6.509)	(9.968)	(0.758)
PoorEst	-0.011383	-0.93612E-2	0.086455	0.21537E-01	0.82611E-2	0.014287
(t-ratio)	(-0.470)	(-0.588)	(9.078)	(3.508)	(1.640)	(2.040)
PerEmploy	0.60914	-1.0249	-0.62631	-0.15887	0.18416	0.62421
(t-ratio)	(0.968)	(-2.678)	(-2.566)	(-1.014)	(1.403)	(3.475)
MedInc	-0.011718	0.15590E-3	0.10380E-3	0.11877E-3	0.13270E-3	0.15607E-3
(t-ratio)	(-3.433)	(7.628)	(7.962)	(14.158)	(18.887)	(16.260)
EduEst	-0.086359	0.038347	0.10001E-4	-0.045057	-0.028406	-0.015072
(t-ratio)	(-4.431)	(3.267)	(0.001)	(-9.309)	(-7.009)	(-2.719)
NonWhite	0.052507	0.011005	0.038010	0.02514	0.015906	0.038217
(t-ratio)	(8.779)	(3.001)	(16.218)	(16.674)	(12.558)	(22.131)
Per14-25	-0.90489	10.413	8.0700	5.2111	2.6766	3.8912
(t-ratio)	(-0.523)	(10.034)	(12.151)	(12.209)	(7.474)	(7.966)
Constant	-1.8907	-2.9186	-2.7356	0.83783	2.1137	-1.3339
(t-ratio)	(-3.054)	-7.802)	(-11.447)	(5.457)	(16.399)	(-7.592)
R^2	0.2958	0.7215	0.8449	0.8327	0.8698	0.8126
N	5518	5723	5607	5633	5601	5632
T-Statistic Burb=City	6.046	-0.384	-4.416	-5.244	-7.453	-9.632

Table 8: Right to Counsel at Trial, 1962

Granted	Not Automatically Granted
Arizona	Alabama
Arkansas	Colorado
California	Connecticut
Georgia	Delaware
Idaho	Florida
Illinois	Maine
Indiana	Maryland
Iowa	Michigan
Kansas	Mississippi
Kentucky	North Carolina
Louisiana	Pennsylvania
Massachusetts	Rhode Island
Minnesota	South Carolina
Missouri	Vermont
Montana	Virginia
Nebraska	
Nevada	
New Hampshire	
New Jersey	
New Mexico	
New York	
North Dakota	
Ohio	
Oklahoma	
Oregon	
South Dakota	
Tennessee	
Texas	
Utah	
Washington	
West Virginia	
Wisconsin	
Wyoming	

Table 9: 396 City Data (1948-1952)—FIXED EFFECT MODEL (GROUP & PERIOD): WOLF

	Murder	**Assault**	**Robbery**	**Burglary**	**Larceny**	**Auto**
WOLF	0.030247	-0.018950	-0.077954	-0.065445	-0.053883	-0.080928
(t-ratio)	(0.200)	(-0.207)	(-1.483)	(-2.005)	(-2.301)	(-2.193)
PolExp	-4.0452	4.3010	-2.1796	3.5763	0.98636	7.6422
(t-ratio)	(-0.248)	(0.438)	(-0.386)	(1.019)	(0.398)	(1.926)
PoorEst	-60.397	3.5293	31.956	70.556	-9.4345	-15.576
(t-ratio)	(-0.585)	(0.059)	(0.893)	(3.171)	(-0.568)	(-0.619)
PerEmploy	-5.4979	0.92487	0.89683	2.0579	1.9713	0.46847
(t-ratio)	(-1.087)	(0.311)	(0.511)	(1.888)	(2.558)	(0.380)
MedInc	0.58731E-3	-0.16859E-3	0.96753E-4	0.18528E-3	0.18753E-4	0.64169E-5
(t-ratio)	(1.257)	(-0.574)	(0.594)	(1.836)	(0.266)	(0.056)
EduEst	0.24668	-0.11561	-0.017142	-0.13370	-0.019364	-0.11044
(t-ratio)	(1.564)	(-1.191)	(-0.311)	(-3.932)	(-0.787)	(-2.873)
NonWhite	-0.060248	0.11385	0.056926	0.036000	0.037492	0.061392
(t-ratio)	(-0.960)	(2.660)	(2.629)	(2.678)	(3.625)	(4.014)
Per14-25	16.640	-1.8333	5.9161	3.8669	0.62788	9.4158
(t-ratio)	(0.970)	(-0.183)	(0.996)	(1.046)	(0.238)	(2.252)
R^2	0.5172	0.7969	0.8689	0.834	0.9253	0.8258
N	1029	1055	1027	1028	1010	1029

Table 10: *Gideon*—396 CITY DATA (GROUP & PERIOD FIXED EFFECTS)

	Murder	Assault	Robbery	Burglary	Larceny	Auto
MappCity	-0.088720	0.15767	0.095637	0.071239	-0.028489	0.15424
(t-ratio)	(-1.316)	(3.831)	(3.666)	(4.266)	(-2.037)	(8.062)
MappBurb	-0.84389	0.16170	0.32781	0.23434	0.15884	0.49089
(t-ratio)	(-7.141)	(2.279)	(7.253)	(8.076)	(6.532)	(14.779)
BGideon	0.061780	0.38350	0.20326	-0.085170	0.044756	0.041239
(t-ratio)	(0.341)	(3.603)	(2.959)	(-1.931)	(1.212)	(0.817)
CGideon	-0.038173	0.0042736	0.14919	-0.0052652	0.0048526	0.034063
(t-ratio)	(-0.480)	(0.089)	(4.849)	(-0.267)	(0.293)	(1.506)
LogPolExp	0.31617E-01	0.79912E-01	0.57563E-01	0.14588	0.19080	0.19884E-01
(t-ratio)	(0.346)	(1.439)	(1.620)	(6.398)	(9.951)	(0.762)
PoorEst	-0.012037	-0.011335	0.085247	0.022314	0.0082282	0.013634
(t-ratio)	(-0.497)	(-0.712)	(8.937)	(3.630)	(1.631)	(1.945)
Employ	0.59787	-1.0513	-0.60929	-0.15717	0.17991	0.63160
(t-ratio)	(0.949)	(-2.748)	(-2.493)	(-1.002)	(1.369)	(3.514)
MedInc	-0.11082E-3	0.14496E-3	0.81195E-4	0.11707E-3	0.12768E-3	0.15535E-3
(t-ratio)	(-3.303)	(7.245)	(6.326)	(14.182)	(18.476)	(16.454)
EduEst	-0.087020	0.037584	0.037963	-0.045578	-0.028708	-0.013689
(t-ratio)	(-4.407)	(3.164)	(0.497)	(-9.287)	(-6.985)	(-2.437)
NonWhite	0.052484	0.010975	0.037974	0.025167	0.015906	0.038186
(t-ratio)	(8.773)	(2.996)	(16.191)	(16.689)	(12.552)	(22.116)
Per14-25	-0.84948	10.226	7.7555	5.2355	2.6349	3.8455
(t-ratio)	(-0.490)	(9.854)	(11.651)	(12.243)	(7.339)	(7.859)
Constant	-1.8989	-2.7807	-2.6380	0.83200	2.1485	-1.3374
(t-ratio)	(-3.056)	(-7.412)	(-10.982)	(5.394)	(16.583)	(-7.579)
R^2	0.2958	0.7722	0.8448	0.8328	0.8697	0.8128
N	5518	5723	5607	5633	5601	5632

Table 11: *Gideon* and *Mapp*—396 CITY DATA (GROUP & PERIOD FIXED EFFECTS)

	Total Crime	**Violent Crimes**	**Property Crimes**
MappCity	0.049462	0.17724	0.040728
(t-ratio)	(4.274)	(7.170)	(3.530)
MappBurb	0.22951	0.35926	0.22649
(t-ratio)	(11.332)	(8.298)	(11.305)
Bgideon	0.020466	0.15213	0.011762
(t-ratio)	(0.660)	(2.291)	(0.387)
Cgideon	0.88744E-02	0.85945E-01	-0.15761E-02
(t-ratio)	(0.648)	(2.947)	(-0.115)
LogPolExp	0.14490	0.43204E-01	0.14042
(t-ratio)	(9.068)	(1.269)	(8.892)
PoorEst	0.21018E-01	0.41479E-01	0.17577E-01
(t-ratio)	(5.163)	(4.648)	(4.245)
Employ	0.17205	-0.71020	0.20857
(t-ratio)	(1.594)	(-3.077)	(1.926)
MedInc	0.12973E-03	0.14187E-03	0.13097E-03
(t-ratio)	(22.460)	(11.489)	(22.991)
EduEst	-0.26375E-01	0.47780E-01	-0.32971E-01
(t-ratio)	(-7.792)	(6.602)	(-9.736)
NonWhite	0.23362E-01	0.25359E-01	0.22641E-01
(t-ratio)	(22.675)	(11.585)	(21.679)
Per14-25	3.3826	8.5481	3.2074
(t-ratio)	(11.341)	(13.435)	(10.847)
Constant	2.2499	-2.3978	2.2255
(t-ratio)	(20.764)	(-10.383)	(20.850)
R^2	0.9008	0.8714	0.8961
N	5470	5504	5601

Table 12: 396 City Data (Group Fixed Effect Only): Miranda

	Murder	**Assault**	**Robbery**	**Burglary**	**Larceny**	**Auto**
MIRANDA	-0.26209	0.22537	0.40254	0.16592	0.59272E-01	0.18969
(t-ratio)	(-3.543)	(5.202)	(14.767)	(9.358)	(3.967)	(9.358)
MappCity	-0.039125	0.15437	0.087587	0.064848	-0.021246	0.10972
(t-ratio)	(-0.606)	(3.927)	(3.554)	(4.041)	(-1.569)	(5.976)
MappBurb	-0.78221	0.18766	0.31859	0.21873	0.16115	0.45007
(t-ratio)	(-6.754)	(2.696)	(7.305)	(7.680)	(6.715)	(13.822)
BGideon	-0.026346	0.35831	0.16695	-0.12381	0.021408	0.012141
(t-ratio)	(-0.147)	(3.378)	(2.493)	(-2.832)	(0.581)	(0.243)
CGideon	-0.33153E-2	0.047725	0.16232	-0.59781E-2	0.88350E-2	0.014561
(t-ratio)	(-0.043)	(1.011)	(5.482)	(-0.309)	(0.542)	(0.659)
LogPolExp	0.021747	-0.43040E-2	-0.021114	0.10396	0.19907	-0.26461E-2
(t-ratio)	(0.245)	(-0.079)	(-0.622)	(4.691)	(10.633)	(-0.104)
PoorEst	0.079971	-0.26085E-2	0.045029	0.96729E-2	0.013276	0.019086
(t-ratio)	(3.571)	(-0.194)	(5.310)	(1.750)	(2.851)	(3.020)
Employ	0.042394	-1.3485	-0.63207	-0.095636	0.19948	0.74061
(t-ratio)	(0.068)	(-3.558)	(-2.667)	(-0.618)	(1.530)	(4.186)
MedInc	-0.16042E-3	0.15729E-3	0.10043E-3	0.13308E-3	0.13247E-3	0.18304E-3
(t-ratio)	(-5.683)	(9.226)	(9.374)	(19.048)	(22.470)	(22.915)
EduEst	-0.071784	0.021995	-0.011916	-0.050853	-0.026928	-0.021797
(t-ratio)	(-3.644)	(1.848)	(-1.589)	(-10.394)	(-6.520)	(-3.897)
NonWhite	0.062018	0.010713	0.036177	0.025826	0.017319	0.036583
(t-ratio)	(10.264)	(2.928)	(15.718)	(17.187)	(13.654)	(21.299)
Per14-25	2.4016	11.500	7.1561	4.5418	2.3634	2.9673
(t-ratio)	(1.430)	(11.300)	(11.176)	(10.869)	(6.689)	(6.211)
R^2	0.2971	0.7200	0.8499	0.8329	0.8684	0.8134
N	5518	5723	5607	5633	5601	5632

Table 13: *Miranda*—396 City Data (Fixed Effect, Group Effect Only)

	Total Crime	Violent Crimes	Property Crime
MIRANDA	0.11800	0.33162	0.10561
(t-ratio)	(9.249)	(12.462)	(8.574)
MappCity	0.047054	0.15055	0.038306
(t-ratio)	(4.228)	(6.499)	(3.429)
MappBurb	0.22201	0.34388	0.21827
(t-ratio)	(11.165)	(8.288)	(11.035)
BGideon	-0.53923E-02	0.13982	-0.016922
(t-ratio)	(-0.175)	(2.175)	(-0.557)
CGideon	0.011526	0.093219	-0.44155E-03
(t-ratio)	(0.860)	(3.341)	(-0.033)
LogPolExp	0.12743	-0.023215	0.13326
(t-ratio)	(8.198)	(-0.718)	(8.634)
PoorEst	0.016050	0.011803	0.016017
(t-ratio)	(4.154)	(1.464)	(4.172)
Employ	0.21582	-0.74092	0.26374
(t-ratio)	(2.027)	(-3.339)	(2.453)
MedInc	0.13964E-3	0.16770E-3	0.14021E-3
(t-ratio)	(28.509)	(16.436)	(28.853)
EduEst	-0.029064	0.031169	-0.034275
(t-ratio)	(-8.568)	(4.416)	(-10.064)
NonWhite	0.023555	0.019286	0.023761
(t-ratio)	(22.640)	(8.904)	(22.726)
Per14-25	2.9307	8.2010	2.7077
(t-ratio)	(10.115)	(13.613)	(9.298)
R^2	0.9001	0.8724	0.8949
N	5470	5504	5601