

ARTICLES

Handcuffing the Cops? A Thirty-Year Perspective on *Miranda*'s Harmful Effects on Law Enforcement

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*After the Supreme Court's 1966 decision in *Miranda v. Arizona*, critics charged that it would "handcuff the cops." In this article, Professors Cassell and Fowles find this claim to be supported by FBI data on crime clearance rates. National crime clearance rates fell precipitously in the two years immediately after *Miranda* and have remained at lower levels in the decades since. Multiple regression analysis reveals that other possibly confounding factors—such as the rising crime rate and baby boom children reaching crime prone-years in the 1960s—do not account for much of the post-*Miranda* decline in clearance rates. Rather, the cause of the decline was most likely the Supreme Court's broad new restrictions on police questioning. The authors conclude that *Miranda* has in fact "handcuffed" the police and that society should begin to explore ways of loosening these shackles.*

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Helpful suggestions were provided by Ron Allen, Akhil Amar, Ian Ayres, Ron Boyce, Craig Bradley, George Brower, Lionel Frankel, William Gangi, Joe Grano, Sam Gross, Yale Kamisar, Richard Leo, Eric Rasmusen, Morgan Reynolds, Stephen Schulhofer, David Sklansky, Bill Stuntz, Lee Teitelbaum, George Thomas III, Richard Uviller, Lloyd Weinrieb, participants in presentations at the American Bar Association ("ABA") 1996 National Convention, Northwestern University, Stanford University, UCLA, and the University of Utah, and discussants on <crimprof@chicagokent.kentlaw.edu>, an electronic mail distribution list for criminal law professors. David Huth provided valuable assistance in running preliminary regression equations, and Katarzyna Celinska helped collect several data series and provided much other valuable statistical analysis. Research assistants Karen Korevaar and Bret Hayman were also vital to the success of this project. The article was supported by the University of Utah College of Law Research Fund and the University of Utah Research Committee. Although this article was a collaborative effort, Professor Cassell had initial responsibility for data collection and legal analysis, whereas Professor Fowles had initial responsibility for statistical analysis.

I believe the decision of the Court . . . entails harmful consequences for the country at large. How serious these consequences may prove to be only time can tell. . . . The social costs of crime are too great to call the new rules anything but a hazardous experimentation.

—Justice John Harlan, dissenting in *Miranda v. Arizona*¹

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¹. 384 U.S. 436, 504, 517 (1966) (Harlan, J., dissenting).

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INTRODUCTION

On June 13, 1966, the United States Supreme Court ignited a firestorm of controversy with *Miranda v. Arizona*, which became its most famous criminal law decision.² *Miranda* established new rules for interrogation of criminal suspects, requiring among other things that officers deliver the now-famous warnings to suspects and obtain a waiver of their constitutional rights to remain silent and speak to an attorney before beginning any custodial interrogation. Critics immediately predicted that the requirements would put “handcuffs on the police”³ and prevent the prosecution of countless dangerous criminals.⁴ Justice Harlan warned in his dissenting opinion that the decision would “entail[] harmful consequences for the country at large. How serious these consequences may prove to be only time can tell.”⁵ Justice White likewise predicted that “[i]n some unknown number of cases the Court’s rule will return a killer, a rapist or other criminal to the streets and to the environment which produced him, to repeat his crime whenever it pleases him.”⁶ After extensive hearings, the Senate Judiciary Committee agreed with the dissenting justices, stating that “crime will not be effectively abated so long as criminals who have voluntarily confessed their crimes are released on mere technicalities.”⁷ And, during the 1968 presidential campaign, then-candidate Richard Nixon charged that *Miranda* “had the effect of seriously ham stringing [sic] the peace forces in our society and strengthening the criminal forces.”⁸

². A 1974 ABA survey of lawyers, judges, and law professors found that *Miranda* was the third most notable decision of all time, trailing only *Marbury v. Madison* and *United States v. Nixon* and leading *Brown v. Board of Education*. See JETHRO K. LIEBERMAN, MILESTONES! 200 YEARS OF AMERICAN LAW: MILESTONES IN OUR LEGAL HISTORY at vii (1976).

³. *More Criminals to Go Free? Effect of High Court’s Ruling*, U.S. NEWS & WORLD REP., June 27, 1966, at 32, 33 (quoting Los Angeles Mayor Samuel W. Yorty).

⁴. See *id.* (including a statement by Fred E. Inbau, Professor of Criminal Law at Northwestern University, that law enforcement officials would choose not to prosecute a number of cases because of *Miranda*).

⁵. *Miranda*, 384 U.S. at 504 (Harlan, J., dissenting).

⁶. *Id.* at 542 (White, J., dissenting).

⁷. S. REP. NO. 90-1097, at 25 (1968), reprinted in 1968 U.S.C.C.A.N. 2112, 2123.

⁸. 114 CONG. REC. 12,936, 12,937 (1968) (Mr. Mundt reading into the record Richard M. Nixon, *Toward Freedom from Fear* (1968)); see also LIVA BAKER, *MIRANDA: CRIME, LAW AND POLITICS* 248 (1983) (citing Nixon campaign speeches attacking *Miranda*).

In spite of these predictions of substantial harm to law enforcement, few researchers ever attempted to confirm or refute the claims by gathering hard statistics on *Miranda*'s effects.⁹ In what has been aptly described as an "empirical desert,"¹⁰ we have little knowledge about what police interrogation looked like shortly after *Miranda*, much less what it looks like today. The lack of data, however, has not deterred *Miranda*'s academic supporters from asserting that the decision has had only a "negligible" effect on law enforcement¹¹ and that "the *Miranda* dissenters' fears [of harm to law enforcement] did not prove justified."¹² Typical of the view from academe is the claim in one of the nation's leading criminal procedure hornbooks that "little has changed since *Miranda* was decided."¹³ But careful examination of these claims reveals that they typically rest on selective citation of a few atypical studies or carefully culled anecdotal information. Has *Miranda* handcuffed the cops over the last thirty years? In truth, even the most knowledgeable defenders of *Miranda* have offered little beyond informed guesses about this fundamental issue. Yet they have been quite successful in spinning the straw of speculation into the gold of a generally accepted, empirically confirmed statement about *Miranda*'s effects.¹⁴

⁹. See Gerald Caplan, Book Review, 40 WAYNE L. REV. 279, 281 (1993) (reviewing JOSEPH D. GRANO, *CONFESSIONS, TRUTH, AND THE LAW* (1993)) ("We still lack sufficient data about *Miranda*'s impact on the administration of justice."); George C. Thomas III, *Is Miranda a Real-World Failure? A Plea for More (and Better) Empirical Evidence*, 43 UCLA L. REV. 821, 837 (1996) ("We need more empirical evidence [about *Miranda*]. What we have so far raises more questions than it answers.").

¹⁰. H. RICHARD UVILLER, *TEMPERED ZEAL: A COLUMBIA LAW PROFESSOR'S YEAR ON THE STREETS WITH THE NEW YORK CITY POLICE* 198 (1988).

¹¹. See, e.g., Welsh S. White, *Defending Miranda: A Reply to Professor Caplan*, 39 VAND. L. REV. 1, 20 (1986) (claiming a "widely shared perception that *Miranda*'s effect on law enforcement has been negligible"); Yale Kamisar, *30 Years Later: Miranda Does Not Look So Awesome Now*, LEGAL TIMES, June 10, 1996, at 22, 50 (reporting that post-*Miranda* studies indicated "that the impact of the *Miranda* rules on conviction rates had been quite negligible").

¹². Yale Kamisar, *Remembering the "Old World" of Criminal Procedure: A Reply to Professor Grano*, 23 U. MICH. J.L. REFORM 537, 585 (1990).

¹³. 1 WAYNE R. LAFAVE & JEROLD H. ISRAEL, *CRIMINAL PROCEDURE* 484 (1984 & Supp. 1991) (citation omitted); see also Paul G. Cassell, *Miranda's Social Costs: An Empirical Reassessment*, 90 NW. U. L. REV. 387, 389 & nn.2-5 (1996) (collecting similar statements).

¹⁴. See Richard A. Leo, *The Impact of Miranda Revisited*, 86 J. CRIM. L. & CRIMINOLOGY 621, 645-46 (1996) (noting the "conventional wisdom" on *Miranda*'s lack of harm but cautioning that the limited studies will not support it).

The question of *Miranda*'s practical effect has far more than academic significance. The Supreme Court views *Miranda* as a pragmatically grounded, prophylactic rule—"a carefully crafted balance designed to fully protect *both* the defendant's and society's interests."¹⁵ If the costs of the decision are greater than the Court has recognized, then the balance is upset and the Court would presumably need to rethink the doctrine's current contours. Even the strongest defenders of *Miranda* seem to agree that the number of criminals who go free "does matter" in assessing the ruling.¹⁶

Justice Harlan's dissent was surely right on one point: that "only time [could] tell" what the real effects of *Miranda* would be.¹⁷ The aim of this article is to draw on the more than thirty years of experience with *Miranda* to quantitatively assess its effect on law enforcement's ability to solve crimes.¹⁸ Although some have suggested that questions about *Miranda*'s effects "probably lie beyond the capacity of social science methodology to answer,"¹⁹ we are not so skeptical. Social science has powerful analytic tools that can and should be brought to bear on this question.

¹⁵. *Moran v. Burbine*, 475 U.S. 412, 433 n.4 (1986).

¹⁶. Stephen J. Schulhofer, *Miranda's Practical Effect: Substantial Benefits and Vanishingly Small Social Costs*, 90 NW. U. L. REV. 500, 505 (1996); see also Yale Kamisar, *The "Police Practice" Phases of the Criminal Process and the Three Phases of the Burger Court*, in *THE BURGER YEARS: RIGHTS AND WRONGS IN THE SUPREME COURT 1969-86*, at 143, 150 (Herman Schwartz ed., 1987) (noting that striking a balance between the defendant's and society's interests "is the way *Miranda*'s defenders—not its critics—have talked about the case").

¹⁷. *Miranda v. Arizona*, 384 U.S. 436, 504 (Harlan, J., dissenting).

¹⁸. We will not discuss other, perhaps less quantifiable, harms of *Miranda*. See, e.g., GRANO, *supra* note **Error! Bookmark not defined.**, at 199-222 (1993) (making a strong case for overruling *Miranda* as an "illegitimate" decision without reference to its costs in terms of damage to law enforcement); Gordon Van Kessel, *The Suspect As a Source of Testimonial Evidence: A Comparison of the English and American Approaches*, 38 HASTINGS L.J. 1, 129 (1986) (discussing other costs of *Miranda*, such as consumption of police and judicial resources and the undermining of public confidence in the criminal justice system).

¹⁹. Caplan, *supra* note **Error! Bookmark not defined.**, at 281.

A brief road map of our article may be in order. Part I reviews the limited evidence about *Miranda*'s effects. Part II then considers the only available long-term measure of *Miranda*'s effects: crime clearance rate data. The clearance rate is the rate at which police declare crimes solved. It is properly and generally regarded as a conservative measure of *Miranda*'s harmful effects. Yet contrary to the prevailing conventional wisdom, crime clearance rates fell precipitously immediately after *Miranda* and have remained at lower levels ever since. This suggests that the decision prevented police from solving a substantial number of crimes.

Part III presents a multiple regression time series analysis of crime clearance rates. After discussing the appropriateness of regression analysis for determining *Miranda*'s effects on clearance rates, we propose a model based on the crime rate, expenditures on law enforcement, the percentage of the population in the crime-prone younger years, and other relevant variables. Even controlling for these factors, police ability to solve crimes declined sharply after *Miranda*. The reductions in clearance rates are observed for the composite groupings of "violent" and "property" crimes, as well as for the individual crime categories of robbery, vehicle theft, larceny, and (in most models) burglary. Only the categories of murder, rape, and assault did not show significant clearance rate reductions due to *Miranda*. The findings recur across a variety of specifications of the regression equations with respect to such issues as the timing of the *Miranda* effect and the length of the time series.

Part IV defends the position that *Miranda* was an important cause of the 1966-1968 drop in clearance rates. This was the contemporaneous view of the Federal Bureau of Investigation ("FBI") and law enforcement officers on the streets. Moreover, *Miranda* was the Warren Court's most substantial restriction on police investigative techniques. Apart from *Miranda*, other possible causes are poor candidates for explaining the sharp decline in clearance rates over that three-year period, a conclusion that is consistent with other available data.

Part V then explains why a *Miranda*-induced drop in clearance rates should be regarded as a social cost. It first refutes the argument that clearance rates might have fallen because police officers were generally using unconstitutionally coercive questioning techniques before *Miranda* that disappeared after the decision. It then disproves Professor Kamisar's hypothesis that the post-*Miranda* clearance rate drop was merely a harmless reduction in purely "paper" clearances.

Finally, Part VI places our findings in the context of the ongoing debate about the wisdom of *Miranda*. It concludes that *Miranda* has in fact handcuffed the cops and that society should begin to explore other, less costly ways of regulating police interrogation.

I. OUR LIMITED KNOWLEDGE ABOUT *MIRANDA*

Immediately after *Miranda*, a handful of researchers attempted to measure the decision's effects. The studies generally suggested that fewer suspects gave confessions after the decision. In another publication, one of us collected the limited empirical evidence on *Miranda*'s social costs in terms of lost criminal cases.²⁰ Examining direct information on *Miranda*'s effects—the studies of confession rates “before-and-after” the decision—the earlier article concludes that *Miranda* significantly depressed the confession rate.²¹ For example, a 1967 research effort in Pittsburgh revealed that confession rates there fell from 49% before the decision to 30% after.²² Averaging this and other reliable before-and-after studies leads to the conclusion that confession rates fell by about 16% in the year or two after *Miranda*,²³ meaning that the decision results in a lost confession in about one out of every six criminal cases. Because confessions are needed to convict in about one out of every four criminal cases, *Miranda* results in the loss of criminal convictions in about 3.8% of all criminal cases (16% confession rate drop multiplied by the 24% confession necessity rate).²⁴

²⁰. See Cassell, *supra* note **Error! Bookmark not defined.**. For discussion of this assessment, compare the criticism of this analysis in Schulhofer, *supra* note **Error! Bookmark not defined.**, with its defense in Paul G. Cassell, *All Benefits, No Costs: The Grand Illusion of Miranda's Defenders*, 90 NW. U. L. REV. 1084 (1996).

²¹. The term “confession rate” is used generally to embrace not only confessions but also incriminating statements useful to the prosecution. See, e.g., Thomas, *supra* note **Error! Bookmark not defined.**, at 825.

²². See Cassell, *supra* note **Error! Bookmark not defined.**, at 396 (reporting results from and discussing Richard H. Seeburger & R. Stanton Wettick, Jr., *Miranda in Pittsburgh—A Statistical Study*, 29 U. PITT. L. REV. 1, 12 tbl.1 & 13 tbl.3 (1967)).

²³. See Cassell, *supra* note **Error! Bookmark not defined.**, at 417, 418 tbl.1. In other words, if the confession rate was 60% before *Miranda*, it was 44% (60% - 16%) after. *But cf.* Schulhofer, *supra* note **Error! Bookmark not defined.**, at 538, 539 tbl.1 (analyzing a somewhat different set of studies and concluding that the before-and-after data show confession rates fell at most 9.7% after *Miranda*).

²⁴. See Cassell, *supra* note **Error! Bookmark not defined.**, at 437-38. *But cf.* Schulhofer, *supra* note **Error! Bookmark not defined.**, at 545 & tbl.2 (concluding that convictions are lost in 1.1% of cases if the post-*Miranda* regime is compared to one in which no warnings are given (9.7%

This tentative estimate of *Miranda*'s harmful effects came solely from the before-and-after studies conducted directly after *Miranda*, which are not universally accepted as accurate measures. At least one commentator believes the studies to be "so crudely designed, so ineptly executed, and so thoroughly riddled with the most elementary methodological defects" as to be inadequate for careful statistical analysis.²⁵ Moreover, because the studies rely on data from the months immediately surrounding *Miranda*, they fail to capture *Miranda*'s long-term effects.²⁶ As a result, defenders of *Miranda* argue that the studies overstate the harm to law enforcement, because they do not capture later adjustments by police organizations to minimize the harms of *Miranda*.²⁷

On the other hand, critics of *Miranda* contend that the studies understate the harmful effects, because full implementation of and compliance with the decision was not immediate.²⁸

confession rate drop multiplied by 19% confession necessity rate)).

²⁵. Leo, *supra* note **Error! Bookmark not defined.**, at 676 n.243.

²⁶. See Cassell, *supra* note **Error! Bookmark not defined.**, at 450-51.

²⁷. See, e.g., Schulhofer, *supra* note **Error! Bookmark not defined.**, at 507-10.

²⁸. See Cassell, *supra* note **Error! Bookmark not defined.**, at 1088-89 (raising this possibility).

To gain a better, long-term perspective on *Miranda*'s effects, we need some solid statistical indicator that extends beyond 1967 and, indeed, into the 1990s. In theory, the ideal study would be to review confession rates since 1967 to see whether, despite initial declines after the decision, they have since "rebounded"—in other words, a before-and-after study of confession rates over several decades rather than over several months. Confession rates are the preferred measure of *Miranda*'s effects because *Miranda* directly affects the ability of police to obtain confessions. Unfortunately, statistics on confession rates have not been regularly collected on a nationwide basis either before or after *Miranda*. Instead, the only figures that exist were gathered by individual researchers for particular cities on a one-time basis. Although broad generalizations are hazardous, before-*Miranda* confession rates in this country were probably somewhere around 55-60%.²⁹ After *Miranda*, the few studies extant reveal lower confession rates. The most recent empirical study of confession rates in this country is the Cassell-Hayman 1994 study in Salt Lake County, Utah, reporting an overall confession rate of only 33%.³⁰ Richard Leo's 1993 study from Berkeley, California found an in-custody questioning success rate by detectives of 64%.³¹ This percentage, when adjusted to account for suspects not questioned and the greater efficacy of questioning by detectives of suspects in-custody, so as to be comparable to earlier studies, translates into an overall confession rate of about 39%.³² A

²⁹. See Paul G. Cassell & Bret S. Hayman, *Police Interrogation in the 1990's: An Empirical Study of the Effects of Miranda*, 43 UCLA L. REV. 839, 871 (1996); see also CHRISTOPHER SLOBOGIN, CRIMINAL PROCEDURE: REGULATION OF POLICE INVESTIGATION: LEGAL, HISTORICAL, EMPIRICAL AND COMPARATIVE MATERIALS 6 (Supp. 1995) (concluding that a 64% confession rate is "comparable to pre-*Miranda* confession rates"). But cf. George C. Thomas III, *Plain Talk About the Miranda Empirical Debate: A "Steady-State" Theory of Confessions*, 43 UCLA L. REV. 933, 935-36 (1996) (deriving a lower estimate with which to compare studies).

³⁰. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 869 tbl.4. For an interesting although ultimately unpersuasive argument that the Salt Lake County confession rate is actually higher, see Thomas, *supra* note **Error! Bookmark not defined.**, at 944-53.

³¹. See Richard A. Leo, *Police Interrogation in America: A Study of Violence, Civility and Social Change* 255-68 (1994) [hereinafter Leo, *Police Interrogation in America*] (unpublished Ph.D. dissertation, University of California (Berkeley)) (on file with the *Stanford Law Review*). Leo's informative dissertation has been published in part as Richard A. Leo, *Inside the Interrogation Room*, 86 J. CRIM. L. & CRIMINOLOGY 266 (1996) [hereinafter Leo, *Inside the Interrogation Room*], and Leo, *supra* note **Error! Bookmark not defined.**

³². See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 926-30 (discussing Leo, *Police Interrogation in America*, *supra* note **Error! Bookmark not defined.**, at 255-68). For criticism of the downward adjustment of Leo's figures, see Thomas, *supra* note **Error! Bookmark not defined.**, at 953-54.

1977 study of six cities reported a “confession” rate of 40.3%.³³

Taken together, the limited data suggest that confession rates in the years after *Miranda* are lower than confession rates in the years before, implying that *Miranda* has in fact impeded law enforcement.³⁴ But this conclusion, too, has been criticized by defenders of *Miranda* because it rests on studies from individual cities that may not be generalizable across the country.³⁵ Responding to such criticisms is difficult, because it is quite true that existing data on confession rates are limited. In short, the existing empirical research has arguably not resolved, at least to the satisfaction of *Miranda*’s defenders, the question of whether *Miranda* has hindered law enforcement over the long haul. Only a national, long-term assessment of *Miranda*’s effects will respond to concerns and shed light on the impact of *Miranda* on police effectiveness.

II. DECLINING CLEARANCE RATES AFTER *MIRANDA*

A. *Clearance Rates As a Measure of Miranda’s Effects*

Since regularly collected, long-term data on confession rates are unavailable, we must search for a second-best alternative. The strongest candidate for such a statistic is the crime “clearance” rate, the rate at which police “clear” or solve crimes. Since at least 1950, the FBI has collected clearance rate figures from around the country and reported them annually in the *Uniform Crime Reports* (“*UCR*”).³⁶ Because of this extended range of data, clearance rates might permit a broad perspective on *Miranda*’s effects.

³³. See Gary D. LaFree, *Adversarial and Nonadversarial Justice: A Comparison of Guilty Pleas and Trials*, 23 *CRIMINOLOGY* 289 tbl.2 (1985).

³⁴. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 871-76.

³⁵. See Thomas, *supra* note **Error! Bookmark not defined.**, at 954-56 (raising this possibility).

³⁶. See, e.g., FEDERAL BUREAU OF INVESTIGATION, U.S. DEP’T OF JUSTICE, *UNIFORM CRIME REPORTS, CRIME IN THE UNITED STATES 1995* (1996) [hereinafter *UCR*-[year]]. Before 1958, the *UCR* was published twice a year. The first edition of each year was entitled “Semiannual Bulletin” and included data for the first six months of the year. The second edition, the “Annual Bulletin,” contained data for the whole year. Citations herein are to the Annual Bulletin for the given year, unless “Semiannual Bulletin” is specified. Crime clearance rates for 1950-1974 are helpfully collected in JAMES ALAN FOX, *FORECASTING CRIME DATA: AN ECONOMETRIC ANALYSIS* 83-86 tbl.A-1 (1978).

The clearance rate appears to be a reasonable if understated surrogate measure for the confession rate.³⁷ Sometimes police officers, lacking evidence to clear a case, will bring a suspect in, deliver *Miranda* warnings, interrogate, and—if no confession results—release him, leaving them with insufficient evidence to clear the case.³⁸ If *Miranda* prevented the confession, by discouraging the suspect from talking or otherwise, the crime may never be cleared. As the leading police interrogation manual explains, “[M]any criminal cases, even when investigated by the best qualified police departments, are capable of solution only by means of an admission or confession from the guilty individual or upon the basis of information obtained from the questioning of other criminal suspects.”³⁹ The most recent field research on police interrogations found that “virtually every detective . . . insisted that more crimes are solved by police interviews and interrogations than by any other investigative method.”⁴⁰ Confessions are also sometimes necessary to solve multiple crimes committed by the same perpetrator. For example, even if police can arrest and convict a robber for one robbery, without a confession they may not be able to clear four other robberies he also committed.⁴¹

³⁷. See GERALD N. ROSENBERG, *THE HOLLOW HOPE: CAN COURTS BRING ABOUT SOCIAL CHANGE?* 328 (1991) (“[O]ne of the ways in which police are often evaluated is through the ‘clearance’ rate One of the major ways this occurs is through confessions.”). For an explanation of how clearance rates understate the effect on confessions, see notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text.

³⁸. Obtaining statistics on the frequency of such interrogations is difficult. A 1967 study in Pittsburgh found that detectives held 73 of 74 suspects after interrogation. See Seeburger & Wettick, *supra* note **Error! Bookmark not defined.**, at 24. But the study appears to have examined only files of cases that had been cleared, *see id.* at 6, and thus likely missed all of the cases in which a suspect refused to confess and the confession was necessary for clearing the case. In New York City in 1967, only 4% of suspects were released after interrogation, and most of these had not talked. See VERA INST. OF JUSTICE, *TAPING POLICE INTERROGATIONS IN THE 20TH PRECINCT, N.Y.P.D.* 68 & n.27 (1967) [hereinafter *TAPING POLICE INTERROGATIONS*]. This study appears primarily to have involved cases in which suspects had been arrested and thus sheds little light on suspects not arrested.

³⁹. FRED E. INBAU, JOHN E. REID & JOSEPH P. BUCKLEY, *CRIMINAL INTERROGATION AND CONFESSIONS* at xiv (3d ed. 1986) (some letters capitalized in original).

⁴⁰. Leo, *Police Interrogation in America*, *supra* note **Error! Bookmark not defined.**, at 373.

⁴¹. See Michael Wald, Richard Ayres, David W. Hess, Mark Schantz, Charles H. Whitehead II, *Interrogations in New Haven: The Impact of Miranda*, 76 *YALE L.J.* 1519, 1595 (1967) (giving examples of such clearances). The issue of multiple clearances from a single arrest is discussed at greater length below. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text.

Clearance rates have been widely viewed as statistics that would reveal *Miranda*'s effects—especially by defenders of *Miranda*.⁴² For example, a widely cited passage in Professor Stephen Schulhofer's influential 1987 article praising *Miranda* claimed that, although some of the before-and-after studies suggested declining confession rates after *Miranda*, "within a year or two [clearance] rates were thought to be returning to pre-*Miranda* levels."⁴³ Many other supporters of *Miranda* have likewise viewed clearance rates as an appropriate indicator of *Miranda*'s benign effects.⁴⁴ Similarly, law enforcement officers at the time of *Miranda* believed that the decision would lower their clearance rates.⁴⁵ We have been unable to locate any article arguing that clearance rates would not capture at least some of the decision's effects.⁴⁶

⁴². See, e.g., Seeburger & Wettick, *supra* note **Error! Bookmark not defined.**, at 20 (discussing clearance rate as a measure of *Miranda*'s effects); see also James W. Witt, *Non-coercive Interrogation and the Administration of Criminal Justice: The Impact of Miranda on Police Effectuality*, 64 J. CRIM. L. & CRIMINOLOGY 320, 330-31 (1973) (same).

⁴³. Stephen J. Schulhofer, *Reconsidering Miranda*, 54 U. CHI. L. REV. 435, 456 (1987) [hereinafter Schulhofer, *Reconsidering Miranda*] (linking the supposedly rebounding clearance rates to the notion that *Miranda* "posed no barrier to effective law enforcement"); see also Stephen J. Schulhofer, *The Fifth Amendment at Justice: A Reply*, 54 U. CHI. L. REV. 950, 954 n.17 (1987) [hereinafter Schulhofer, *Fifth Amendment*] (arguing that apparently steady clearance rates coupled with other evidence refute the notion that *Miranda* has harmed law enforcement).

⁴⁴. See, e.g., SPECIAL COMM. ON CRIMINAL JUSTICE IN A FREE SOC'Y, AMERICAN BAR ASS'N, CRIMINAL JUSTICE IN CRISIS 63 n.53 (1988) (collecting evidence that *Miranda* has not harmed law enforcement, including clearance rates); White, *supra* note **Error! Bookmark not defined.**, at 18 n.93 & 19 n.99 (citing clearance and confession rates to argue *Miranda* did not harm law enforcement); cf. Leo, *supra* note **Error! Bookmark not defined.**, at 645 (suggesting that *Miranda* has not significantly affected crime clearance rates, although "in some instances" they may have dropped).

⁴⁵. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text (collecting law enforcement views on *Miranda* and clearance rates).

⁴⁶. Otis Stephens apparently suggests that there is "doubtful utility" to measuring *Miranda*'s effects through "clearance rates, conviction rates, and the incidence of confession." OTIS H. STEPHENS, JR., THE SUPREME COURT AND CONFESSIONS OF GUILT 181 n.55 (1973). However, he appears primarily concerned with "[d]ifferences in the record-keeping procedures" of individual departments, see *id.*, which is a concern that has little application to a review of aggregate, national data for clearance rates, see note **Error! Bookmark not defined.** *infra* and accompanying text. Professor Schulhofer has also recently suggested that, to measure *Miranda*'s effects, one should look to clearance totals (that is, the total number of cleared crimes), in addition to clearance rates (that is, cleared crimes divided by crimes reported to the police). See Stephen J. Schulhofer, *Bashing Miranda Is Unjustified—And Harmful*, 20 HARV. J.L. & PUB. POL'Y 347, 361-64 (1997)

While an apparent consensus exists that clearance rates at least partially gauge *Miranda*'s impact, one note of caution should be sounded. Police can record a crime as "cleared" when they have identified the perpetrator and placed him under arrest, even where the evidence is insufficient to convict, or even to indict. As a result, clearance rates fail to measure any of *Miranda*'s harmful effects that manifest themselves at stages of the criminal justice process after a crime has been cleared. It seems quite likely that, for some lost confessions, police may still be able to "clear" the crime but lack sufficient evidence to obtain a conviction.⁴⁷ Therefore, clearance rates are a quite conservative measure of *Miranda*'s harmful effects on the conviction of criminals.

In theory, one could begin to measure the understatement of *Miranda*'s harms by measuring the rate at which cleared cases are prosecuted. If confession rates fell after *Miranda*, prosecutors might charge fewer suspects because the lack of a confession made the prosecution more difficult.⁴⁸ Thus, a reduction in confessions could lower the charging rate in cleared cases. This possibility is, in practice, a moot point because of the practical problem of collecting adequate data. The FBI's data on charging decisions are woefully inadequate for statistical analysis, swinging wildly from year to year during the 1960s.⁴⁹ The FBI stopped reporting charging figures in the 1970s.

[hereinafter Schulhofer, *Bashing Miranda*]; Stephen J. Schulhofer, *Miranda and Clearance Rates*, 91 NW. U. L. REV. 278, 285 (1996) [hereinafter Schulhofer, *Clearance Rates*]. However, this suggestion is misguided. See Paul G. Cassell, *Miranda's "Negligible" Effect on Law Enforcement: Some Skeptical Observations*, 20 HARV. J.L. & PUB. POL'Y 327, 336-38 (1997) (explaining why clearance rates rather than clearance totals must be used to assess law enforcement effectiveness).

⁴⁷. See Cassell, *supra* note **Error! Bookmark not defined.**, at 398-99 (arguing that clearance rates understate *Miranda*'s adverse effect on law enforcement).

⁴⁸. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 908 tbl.15 (finding a statistically significant difference in prosecutorial charging decisions between suspects who were successfully interrogated and those who were not).

⁴⁹. Compare, e.g., UCR-1965, *supra* note 36, at 103 tbl.12 (50,980 persons charged with violent crimes, derived by summing four violent crime categories), and UCR-1966, *supra* note **Error! Bookmark not defined.**, at 104 tbl.16 (44,641 persons charged with violent crimes, derived by summing four violent crime categories), with UCR-1967, *supra* note **Error! Bookmark not defined.**, at 109 tbl.16 (50,515 persons charged with violent crimes, derived by summing four violent crime categories).

One other theoretical possibility for measuring *Miranda*'s impact would be to investigate conviction rates rather than clearance rates. Convictions have the advantage of resting on court adjudications of guilt or innocence.⁵⁰ However, conviction rates would probably miss many of *Miranda*'s effects.⁵¹ More significantly, conviction rate data in this country are notoriously bad.⁵² The basic problem is that police agencies, the source for FBI data, are poorly situated to report on ultimate court outcomes. Perhaps for this reason, the FBI stopped reporting conviction figures in 1978.⁵³

Through a process of elimination, then, the choice for a long-term evaluation of *Miranda* boils down to the understated measure of clearance rates.

B. *The National Clearance Rate Trend*

Did clearance rates go up or down after *Miranda*? The conventional academic wisdom is that *Miranda* had no noticeable effect on crime clearance rates.⁵⁴ That wisdom is perhaps most prominently embodied in Professor Schulhofer's 1987 article, *Reconsidering Miranda*, which said that clearance rates were "thought to be returning to pre-*Miranda* levels" shortly after the decision and that "[s]tudy after study confirmed this trend."⁵⁵ Although Schulhofer has since repudiated this position,⁵⁶ his 1987 article has been cited repeatedly as proof that *Miranda* has not hampered law enforcement. For example, Professor Yale Kamisar, perhaps *Miranda*'s leading academic supporter, has concluded that Schulhofer's article "effectively refutes [the] contention" that *Miranda* has harmed law enforcement.⁵⁷

⁵⁰. See Monica A. Walker, *Do We Need a Clear-Up Rate?*, 2 POLICING & SOC'Y 293, 304 (1992) (suggesting that in Britain conviction data are better than clearance data because they rest on known offenders as opposed to suspects).

⁵¹. See Cassell, *supra* note **Error! Bookmark not defined.**, at 396-98 (discussing why conviction rates cannot show how *Miranda* hinders the investigative process).

⁵². See 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 AM. ECON. REV., May 1987, Papers & Proceedings, at 99, 104 (finding conviction rate data "highly questionable").

⁵³. See Letter from Bennie F. Brewer, Chief, Programs Support Section, Criminal Justice Information Services Division, FBI, to Paul G. Cassell, Associate Professor of Law, University of Utah College of Law 1 (Feb. 7, 1995) [hereinafter Brewer Letter] (on file with the *Stanford Law Review*) (noting the discontinuation of the FBI conviction rate series).

⁵⁴. See Leo, *supra* note **Error! Bookmark not defined.**, at 645-46 (concluding that the

The support for these claims about lack of impact on clearance rates is vanishingly thin. Boiling down Schulhofer's secondary sources to their primary authorities, one discovers that he has referenced only two studies with clearance rate data.⁵⁸ Neither of these studies provide support for the thesis that clearance rates have returned to pre-*Miranda* levels.⁵⁹ Indeed, the few other statistical analyses of post-*Miranda* clearance rates that we have found suggest that clearance rates fell. In New York City, in February 1967, the Deputy Commissioner of the New York Police Department reported that clearance rates dropped about 10% in 1966.⁶⁰ He attributed the drop in part to "recent Supreme Court decisions that had limited the admissibility of confessions in court."⁶¹ In "Seaside City," California, crime clearance rates

proposition that *Miranda* "has exercised only a negligible effect on the ability of police to elicit confessions [and] solve crimes" has "become the conventional wisdom among scholars").

⁵⁵. Schulhofer, *Reconsidering Miranda*, *supra* note **Error! Bookmark not defined.**, at 456.

⁵⁶. See note **Error! Bookmark not defined.** *infra* and accompanying text.

⁵⁷. Kamisar, *supra* note **Error! Bookmark not defined.**, at 586 n.164; see also Janet E. Ainsworth, *In a Different Register: The Pragmatics of Powerlessness in Police Interrogation*, 103 YALE L.J. 259, 299 n.200 (1993) (reporting that most commentators believe "*Miranda* has had little negative effect on criminal prosecutions" (citing Schulhofer, *Reconsidering Miranda*, *supra* note **Error! Bookmark not defined.**, at 455-58)); Louis Michael Seidman, *Brown and Miranda*, 80 CAL. L. REV. 673, 743 (1992) (concluding that *Miranda* "did not result in a huge decline in the number of confessions" (citing Schulhofer, *Reconsidering Miranda*, *supra* note **Error! Bookmark not defined.**, at 455-61)); Marcy Strauss, *Reinterrogation*, 22 HASTINGS CONST. L.Q. 359, 379 (1995) (arguing that "the most recent data is fairly consistent" that shortly after *Miranda* "clearance . . . rates were thought to be returning to pre-*Miranda* levels" (quoting Schulhofer, *Reconsidering Miranda*, *supra* note **Error! Bookmark not defined.**, at 456)).

⁵⁸. See Schulhofer, *Fifth Amendment*, *supra* note **Error! Bookmark not defined.**, at 954 n.17; Schulhofer, *Reconsidering Miranda*, *supra* note **Error! Bookmark not defined.**, at 456 n.52.

⁵⁹. One of the studies found exactly the opposite of Schulhofer's claim. See NEAL A. MILNER, THE COURT AND LOCAL LAW ENFORCEMENT: THE IMPACT OF *MIRANDA* 217 (1971) (finding substantial decreases in clearance rates in early 1967 in three of four Wisconsin cities). The other study, the Pittsburgh study, offered mixed results, noting at one point that "there has been a decline in the clearance rate from the first half of 1966. One of several possible explanations for this is the imposition of the *Miranda* requirements on the Pittsburgh police." Seeburger & Wettick, *supra* note **Error! Bookmark not defined.**, at 24.

⁶⁰. See Bernard Weinraub, *Crime Reports up 72% Here in 1966; Actual Rise Is 6.5%*, N.Y. TIMES, Feb. 21, 1967, at A1.

⁶¹. *Id.* But cf. Daniel Nagin, *General Deterrence: A Review of the Empirical Evidence*, in PANEL ON RESEARCH ON DETERRENT AND INCAPACITATIVE EFFECTS, NATIONAL RESEARCH COUNCIL, DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL SANCTIONS ON CRIME RATES 95, 114-15 (Alfred Blumstein, Jacqueline Cohen & Daniel Nagin eds., 1978) (suggesting that more accurate recordkeeping policies might explain the decline in the New York clearance rate); TAPING POLICE INTERROGATIONS, *supra* note **Error! Bookmark not defined.**, at

dropped about 3% after *Miranda*.⁶²

The studies finding post-*Miranda* declines in clearance rates in various cities suggest that something is amiss with the conventional wisdom. But results from any particular city might be idiosyncratic. To gain a broader view of *Miranda*'s effects, we should examine evidence from across the country. Surprisingly, even thirty years after the decision, the national data from the FBI's *UCR* remain to be systematically examined. The data show that, in fact, crime clearance rates fell sharply all over the country immediately after *Miranda* and have remained at these lower levels ever since. In 1965, the year preceding *Miranda*, the *UCR* noted that the national clearance rate for the "grand total" crimes⁶³ was "virtually unchanged from 1964."⁶⁴ The next year in June, the Court handed down *Miranda*. At the end of 1966, the *UCR* (which usually describes police performance in decidedly upbeat terms) acknowledged a substantial drop in clearances.⁶⁵ Indeed, the *UCR* observed that the drop in clearance rates from 1965-1966 was equal to the entire drop in clearance rates from 1961-1965.⁶⁶ The 1966 drop in clearances was "universally reported by all population groups and by all geographic divisions."⁶⁷ The following year, 1967, the *UCR* continued to report widespread bad news.⁶⁸ Again a clearance rate drop was "universally reported by all population groups and by all geographic divisions."⁶⁹ In 1968, the *UCR* acknowledged another fall in clearance rates.⁷⁰ In 1969, the *UCR* reported that most clearance rates declined slightly,⁷¹ and in 1970 the *UCR* reported that clearance rates were unchanged.⁷² Clearance rates have remained roughly stable since 1970.⁷³

79-80 (questioning whether *Miranda* could have caused the reported changes in New York's clearance rate).

⁶². See Witt, *supra* note **Error! Bookmark not defined.**, at 330-31. Witt cautioned that clearance rates actually rose in 1966 and then fell in subsequent years. See *id.* at 331. Post-*Miranda* data also came from other jurisdictions, such as Omaha, Nebraska, where clearance rates rose slightly after the decision, see Cyril D. Robinson, *Police and Prosecutor Practices and Attitudes Relating to Interrogation As Revealed by Pre- and Post-Miranda Questionnaires: A Construct of Police Capacity to Comply*, 1968 DUKE L.J. 425, at 465 n.90 (citing statement of Detective Inspector Richard R. Andersen of the Omaha Police Department to the Senate Judiciary Subcommittee on Constitutional Amendments), and Austin, Texas, where clearance rates appeared to fall after *Miranda*, a drop that is attributed in part to the decision, see *Controlling Crime Through More Effective Law Enforcement: Hearings Before the Subcomm. on Criminal Laws and Procedures of the Senate Comm. on the Judiciary*, 90th Cong. 726 (1967) [hereinafter *Controlling Crime Hearings*] (statement of Austin Chief of Police R.A. Miles).

⁶³. See note **Error! Bookmark not defined.** *infra* and accompanying text for a description of

A long-term perspective on clearance rates comes from plotting the FBI's annual figures. Figure 1 depicts the national crime clearance rate from 1950-1995 for violent crimes (i.e., nonnegligent homicide, forcible rape, aggravated assault, and robbery). As can be seen, violent crime clearance rates were fairly stable from 1950-1965, generally hovering above 60%. They even increased slightly from 1962-1965. Then, in the three years following *Miranda*, the rates fell dramatically—to 55% in 1966, to 51% in 1967, to 47% in 1968.⁷⁴ Violent crime clearance rates have hovered around 45% ever since. Because *Miranda* probably took effect over several years—while both police practices and suspect volubility adjusted to the new rules⁷⁵—simple

grand total crimes. We do not consider arson, which was only added to the Crime Index in the 1970s.

⁶⁴. UCR-1965, *supra* note **Error! Bookmark not defined.**, at 18 (citing 24.6% clearance for grand total crimes and 26.3% for the index crimes). For reasons cited in Appendix.A.1, this article shall address only the seven index crime categories. *See also* UCR-1964, *supra* note **Error! Bookmark not defined.**, at 2 (listing the seven Crime Index offenses and explaining that the FBI uses them “to determine the trend of crime in the United States”).

⁶⁵. *See* UCR-1966, *supra* note **Error! Bookmark not defined.**, at 27 (“Whereas police, nationally, cleared 26.3 percent of Crime Index offenses in 1965, in 1966 this dropped to 24.3 percent. The decrease was noted in every Crime Index offense with robbery solutions having the sharpest decline, down 14 percent.”).

⁶⁶. *See id.*

⁶⁷. *Id.*

⁶⁸. *See* UCR-1967, *supra* note **Error! Bookmark not defined.**, at 30 (“Whereas police nationally cleared 24.3 percent of these offenses in 1966, this dropped to 22.4 percent in 1967. The decrease was noted in every Crime Index offense with auto theft solutions having the sharpest decline”)

⁶⁹. *Id.*

⁷⁰. *See* UCR-1968, *supra* note **Error! Bookmark not defined.**, at 30 (“Whereas police nationally cleared 22.4 percent of these offenses in 1967, this dropped to 20.9 percent in 1968. The decrease was noted in every Crime Index offense.”).

⁷¹. *See* UCR-1969, *supra* note **Error! Bookmark not defined.**, at 28.

⁷². *See* UCR-1970, *supra* note **Error! Bookmark not defined.**, at 30.

⁷³. *See, e.g.*, UCR-1975, *supra* note **Error! Bookmark not defined.**, at 37; UCR-1980, *supra* note **Error! Bookmark not defined.**, at 180; UCR-1985, *supra* note **Error! Bookmark not defined.**, at 154.

⁷⁴. To be clear, the point on the graph marked “Last Pre- *Miranda* Year” is 1965, the last pre-*Miranda* data point. Because the FBI figures are reported annually, the 1966 number is the first to reflect *Miranda*'s effects. *But cf.* George C. Thomas III, *Telling Half-Truths*, LEGAL TIMES, Aug. 12, 1996, at 20, 24 (possibly misunderstanding this point in claiming that clearance rates had already fallen substantially before the decision).

⁷⁵. *See* notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text.

visual observation of the long-term trends suggests that *Miranda* substantially harmed police efforts to solve violent crimes. Moreover, contrary to the notion that clearance rates returned to pre-*Miranda* levels,⁷⁶ violent crime clearance rates in fact have been permanently depressed since the decision.

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A similar conclusion is suggested in Figure 2, which shows the annual crime clearance rate for the same period for the property crimes of burglary, vehicle theft, and larceny. The rate at which police cleared property crimes fluctuated somewhat from 1950-1960, declined from 1961-1965, fell at an accelerating rate from 1966-1968, and then generally stabilized. Here again, clearance rates dropped during the critical post-*Miranda* period, although somewhat less dramatically than violent crime clearance rates.

⁷⁶. See Schulhofer, *Reconsidering Miranda*, *supra* note **Error! Bookmark not defined.**, at 456.

III. REGRESSION ANALYSIS OF THE CLEARANCE RATE DECLINE

A. *Using Regression to Sort Through Competing Causes*

A more thorough analysis of the hypothesis that *Miranda* caused the declines in crime clearance rates must contend with other competing causes. If another factor—call it the “X factor”—was responsible for the fall in clearance rates, then *Miranda* would be absolved of responsibility. For example, in two new brief essays Professor Schulhofer now concedes that, contrary to his earlier suggestions, clearance rates did indeed fall after *Miranda*.⁷⁷ He maintains, however, that there is no basis for blaming the decline on *Miranda*; instead he blames other factors such as rising crime rates during the 1960s for the decline.⁷⁸

The standard technique for sorting through such competing possibilities is multiple regression analysis. In this section, we report our multiple regression equations for crime clearance rates, using an interrupted time series design.⁷⁹ Specifically, we analyze whether, controlling for other relevant factors, there was a detectable change in clearance rates at the time of *Miranda*. Before diving into the complexities of multiple regression equations, however, one important point must be emphasized. Although sophisticated econometric techniques are available for analyzing the data, simple visual observation has its place as well.⁸⁰ The graphs in the previous section demonstrate that there was a sharp, post-*Miranda* drop in clearance rates; that overall picture nicely fits the handcuffing-the-cops theory advanced by *Miranda*'s critics. Even standing alone, the trend lines are important evidence suggesting *Miranda* had an adverse effect. We wonder whether the many defenders of *Miranda* who concluded that the decision had no adverse effects on the basis of a posited stable post-*Miranda* clearance rate⁸¹ will now, consistent with their methodological approach, rethink their position and agree that *Miranda* was indeed harmful to police efforts.

⁷⁷. See Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 356; Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 279.

⁷⁸. See Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 364-72; Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 280-85.

⁷⁹. See generally DAVID McDOWALL, RICHARD MCCLEARY, ERROL E. MEIDINGER & RICHARD A. HAY, JR., *INTERRUPTED TIME SERIES ANALYSIS* (1980). Time series design permits “the analysis of the impact of interventions upon a single time series of data.” *Id.* at 5.

⁸⁰. See Gary A. Mauser & Richard A. Holmes, *An Evaluation of the 1977 Canadian Firearms*

We turn to time series analysis out of necessity, as the *Miranda* decision precludes the use of other common statistical techniques. The preferred methodology for assessing a social policy is a true experiment in which one jurisdiction at random is subjected to the new policy, while another “control” jurisdiction is not.⁸² Unfortunately, such research is not possible with *Miranda*. On June 13, 1966, the Supreme Court required all jurisdictions across the country to follow the prescribed interrogation procedures. Since then, police agencies have generally followed the *Miranda* requirements with little innovation.⁸³ Comparison of a control group with a subject group is thus not possible.⁸⁴

Legislation, 16 EVALUATION REV. 603, 604 (1992) (“Although unsophisticated, a visual inspection has the advantage of being able to assess trends across time, so that possible links . . . may be identified.”). See generally WILLIAM S. CLEVELAND, VISUALIZING DATA (1993) (discussing the importance of visualization to data analysis and the application of various types of visualization tools). For illustrations of this technique, see David McDowall, Colin Loftin & Brian Wiersema, *Using Quasi-Experiments to Evaluate Firearm Laws: Comment on Britt et al.’s Reassessment of the D.C. Gun Law*, 30 L. & SOC’Y REV. 381, 390 (1996), and Franklin E. Zimring, *Firearms and Federal Law: The Gun Control Act of 1968*, 4 J. LEGAL STUD. 133, 170-94 (1975).

⁸¹. See notes **Error! Bookmark not defined.-Error! Bookmark not defined.** *supra* and accompanying text.

⁸². See, e.g., Simon I. Singer & David McDowall, *Criminalizing Delinquency: The Deterrent Effects of the New York Juvenile Offender Law*, 22 L. & SOC’Y REV. 521, 527 (1988) (discussing the use of control groups for their case study).

⁸³. See Paul G. Cassell, *The Costs of the Miranda Mandate: A Lesson in the Dangers of Inflexible, “Prophylactic” Supreme Court Inventions*, 28 ARIZ. ST. L.J. 299, 311 (1996) (noting the reluctance of police agencies to explore alternatives to *Miranda*).

⁸⁴. It seems fair to number among *Miranda*’s costs the fact that the decision has prevented controlled experimentation in this area. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 922 (concluding that *Miranda* prevented research on important interrogation questions).

We also used time series analysis because another similar and commonly used statistical technique, cross-sectional analysis, is unavailable to us. In this technique, the impact of legal rules is analyzed by state-by-state “cross-sectional” analysis, where data from states following one legal regime are compared with data from states that do not. Professor Schulhofer suggests using this methodology to assess *Miranda*’s impact, claiming that “[p]rofessional econometric studies of the impact of legal rules almost invariably rely on analysis of cross-sectional, not aggregate, data.”⁸⁵ One of the studies he cites as proof of an “almost invariable” practice of cross-sectional analysis, however, is in fact a national time series analysis similar to ours.⁸⁶ The other studies he cites involve not a legal norm applied across the country, but rather legal rules that vary from jurisdiction to jurisdiction, permitting a true cross-sectional comparison.⁸⁷ It would make no sense to apply that methodology here. There simply are no “control” jurisdictions unaffected by *Miranda* to observe,⁸⁸ at least in this country.⁸⁹ Moreover, as a

⁸⁵. Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 291.

⁸⁶. *See id.* at 291 n.31 (erroneously citing as cross-sectional analysis a national time series study by Isaac Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 AM. ECON. REV. 397, 406 (1975) (providing a national time series analysis of “aggregate crime statistics relating to the United States” from 1933-1969)); *see also* Ehrlich & Brower, *supra* note **Error! Bookmark not defined.**, at 100, 104-06 (describing a national time series analysis on questions similar to those discussed here and reaching similar conclusions). In addition, virtually all of the data for Schulhofer’s analysis of clearance rates come from a book by Professor James Alan Fox that is in fact a national time series analysis. *See* Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 281 n.12 (noting that all graphical data “are drawn from” FOX, *supra* note **Error! Bookmark not defined.** (providing a national time series econometric equations attempting to forecast crime)).

⁸⁷. *See* Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 291 n.31 (citing Raymond Atkins & Paul H. Rubin, *The Impact of Changing Criminal Procedure on Crime Rates* (Oct. 28, 1995) (unpublished manuscript, on file with the *Stanford Law Review*) (reporting a cross-sectional analysis of states that used the exclusionary rule with those that did not before and after the Supreme Court’s decision in *Mapp v. Ohio*), and John R. Lott, Jr. & David B. Mustard, *Crime, Deterrence, and Right-to-Carry Concealed Handguns*, 26 J. LEGAL STUD. 1 (1997) (reporting a cross-sectional analysis of jurisdictions that permit concealed handguns with those that do not)).

⁸⁸. In theory, such research might be possible if some jurisdictions followed *Miranda* rules before the decision and some did not. Then it would be possible to compare changes in these differing groups. *See, e.g.*, Atkins & Rubin, *supra* note **Error! Bookmark not defined.**, at 2; *cf.* Abraham N. Tennenbaum, *The Influence of the Garner Decision on Police Use of Deadly Force*, 85 J. CRIM. L. & CRIMINOLOGY 241, 247-48, 255-56 (1994) (supplementing national interrupted time

practical matter, obtaining appropriate cross-sectional data for the variables in our equations appears to be impossible,⁹⁰ and such data as exist might be contaminated by other problems.⁹¹

series data with state comparison data). However, before *Escobedo v. Illinois*, 378 U.S. 478 (1964), no state had adopted anything approaching the full set of the *Miranda* rules. Even during the 1964-1966 period, only a few states moved in the direction of *Miranda*, and they did not impose all the *Miranda* rules. See *Miranda v. Arizona*, 384 U.S. 436, 519 n.17, 521 (Harlan, J., dissenting) (collecting cases and concluding that “no State . . . [has] chosen to go nearly so far on its own”); see also Cassell, *supra* note **Error! Bookmark not defined.**, at 493-96 (discussing efforts to warn suspects after *Escobedo* and concluding that the largest drop in confession rates came from *Miranda*’s novel imposition of waiver requirements).

⁸⁹. It might be possible to compare American confession rates with overseas confession rates. The available data on foreign confession rates support the conclusions reached in this article. See Cassell, *supra* note **Error! Bookmark not defined.**, at 418-22 (noting that post-*Miranda* confession rates are much lower in the United States than in Britain and Canada). *But cf.* Thomas, *supra* note **Error! Bookmark not defined.**, at 942-43 (not contesting the lower foreign rates but arguing that British and Canadian data are simply not comparable and therefore do not offer strong support in the *Miranda* impact debate).

⁹⁰. In 1966, for example, the FBI-reported clearance rate data only for the nation and various regional groupings (e.g., New England states, Middle Atlantic states, etc.). See UCR-1966, *supra* note **Error! Bookmark not defined.**, at 102 tbl.13. State-by-state clearance data appear to be unpublished for the relevant time period. See Brewer Letter, *supra* note **Error! Bookmark not defined.**, at 1 (indicating that state-by-state clearance rate data are available for purchase from 1979 to date). As a result, the only feasible cross-sectional analysis of clearance rates is one based on these rather artificial regional groupings, which seems likely to shed little light on *Miranda* questions. Moreover, even if clearance rate data were available, other data limitations would also preclude effective analysis. See James Alan Fox, *Reexamining Some Perils of Quantification in the Econometric Study of Crime: A Reply to Decker and Kohfeld*, 19 J. RES. CRIME & DELINQ. 122, 123 (1982) (noting that many crime researchers have used national aggregates because “annual data on most exogenous variables (e.g., demographic composition and economic indicators) are available only for a restricted range of units”).

⁹¹. See Nagin, *supra* note **Error! Bookmark not defined.**, at 113 (concluding that police departments that record fewer of the crimes reported to them will have a higher clearance rate, generating a spurious correlation in cross-sectional deterrence studies). Moving to smaller units would also inevitably introduce considerable “noise” into the data. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text (noting the advantages of national clearance rate data).

In any event, interrupted time series analysis is quite appropriate for assessing the effect of a legal reform.⁹² This statistical technique is commonly used for assessing the effects of legal changes.⁹³ Standard statistical texts suggest that this technique is well-suited for such issues, provided that care is used in analyzing the data and other factors not covered in the regressions are considered.⁹⁴

B. *A Model of Crime Clearance Rates*

The first step in developing any regression model is to identify relevant variables for the equations. For our dependent variable, we decided to use national clearance rates using country-wide FBI data. Although FBI data have been criticized on various grounds, they remain the only data available to resolve the question of *Miranda*'s effects.⁹⁵ In any event, the literature generally suggests that FBI data, at least for the last few decades, are of sufficient quality for time series analysis.⁹⁶

⁹². See Donald T. Campbell, *Reforms As Experiments*, 24 AM. PSYCHOLOGIST 409, 417 (1969) ("The interrupted time-series design . . . is available for those settings in which no control group is possible, in which the total governmental unit has received the experimental treatment, the social reform measure."); D.J. Pyle & D.F. Deadman, *Assessing the Impact of Legal Reform by Intervention Analysis*, 13 INT'L REV. L. & ECON. 193, 213 (1993) ("Legal reform should be an area in which intervention analysis is widely used."); H. Laurence Ross, Donald T. Campbell & Gene V. Glass, *Determining the Social Effects of a Legal Reform: The British "Breathalyzer" Crackdown of 1967*, 13 AM. BEHAV. SCIENTIST 493, 495 (1970) ("Legal change is a subject for which the Interrupted Time-Series design seems eminently suited.").

⁹³. See, e.g., H. Laurence Ross, Richard McCleary & Gary LaFree, *Can Mandatory Jail Laws Deter Drunk Driving? The Arizona Case*, 81 J. CRIM. L. & CRIMINOLOGY 156, 161 (1990); Singer & McDowall, *supra* note **ERROR! BOOKMARK NOT DEFINED.**, at 526-32; Zimring, *supra* note **ERROR! Bookmark not defined.**, at 171-94.

⁹⁴. See, e.g., DONALD T. CAMPBELL & JULIAN C. STANLEY, *EXPERIMENTAL AND QUASI-EXPERIMENTAL DESIGNS FOR RESEARCH* 37-42 (1966); THOMAS D. COOK & DONALD T. CAMPBELL, *QUASI-EXPERIMENTATION: DESIGN AND ANALYSIS ISSUES FOR FIELD SETTINGS* 207-32 (1979). For particular studies that suggest using interrupted time series analysis with care, see, for example, Chester L. Britt, Gary Kleck & David J. Bordua, *A Reassessment of the D.C. Gun Law: Some Cautionary Notes on the Use of Interrupted Time Series Designs for Policy Impact Assessment*, 30 L. & SOC'Y REV. 361, 362-64 (1996) (recognizing the strengths but also identifying some problems with interrupted time series research), and McDowall et al., *supra* note **ERROR! Bookmark not defined.**, at 381-90 (discussing the observations of Britt et al., *supra*).

⁹⁵. See Charles R. Tittle, *Crime Rates and Legal Sanctions*, 16 SOC. PROBS. 409, 411-12 (1969) ("The unreliability of crime statistics is well-known, but the lack of other sources of data precludes alternative approaches."). Victimization data has been used in preference to FBI-reported

FBI clearance rate data in particular have been criticized. To clear a crime, police need not actually obtain a conviction, but only determine to their own satisfaction that the crime has been solved.⁹⁷ Unsurprisingly, clearance rate statistics have been attacked on the grounds that police can manipulate them in an effort to look good,⁹⁸ or that police may simply disagree as to what crimes should be regarded as “cleared.”⁹⁹ A study in New York City, for

crime data in some other research, but that is not possible here because victimization data was not collected on any significant scale until the 1970s, well after *Miranda* was decided.

⁹⁶. See FOX, *supra* note **Error! Bookmark not defined.**, at 8 (“[A]lthough there are inherent problems in using UCR data for research purposes, these problems are minimized in analyses that employ aggregate time series data.”); Scott H. Decker & Carol W. Kohfeld, *Certainty, Severity, and the Probability of Crime: A Logistic Analysis*, 19 POL’Y STUD. J. 2, 6 (1990) (concluding that statistical analysis of “official crime statistics” can often be done “with considerable confidence”); Ehrlich & Brower, *supra* note **Error! Bookmark not defined.**, at 104 (finding “no apparent technical reasons” to avoid using FBI data for time series analysis); Walter R. Gove, Michael Hughes & Michael Geerken, *Are Uniform Crime Reports a Valid Indicator of the Index Crimes? An Affirmative Answer with Minor Qualifications*, 23 CRIMINOLOGY 451 (1985) (defending UCR data generally); James A. Inciardi, *The Uniform Crime Reports: Some Considerations on Their Shortcomings and Utility*, 6 REV. PUB. DATA USE 3, 14 (1978) (concluding that UCR clearance rate trend data can be useful); Wesley G. Skogan, *The Validity of Official Crime Statistics: An Empirical Investigation*, 55 SOC. SCI. Q. 25, 37-38 (1974) (finding official crime statistics to be distributionally accurate across cities); George D. Brower, *The Supreme Court and the Growth of Crime 170* (1985) (unpublished Ph.D. dissertation, State University of New York (Buffalo)) (on file with the *Stanford Law Review*) (concluding that, “given appropriate care in the selection and preparation by researchers of particular series,” FBI annual data “are appropriate for use in empirical analysis”).

⁹⁷. See James Q. Wilson & Barbara Boland, *The Effect of the Police on Crime*, 12 L. & SOC’Y REV. 367, 368 (1978).

⁹⁸. See, e.g., JEROME H. SKOLNICK, *JUSTICE WITHOUT TRIAL: LAW ENFORCEMENT IN DEMOCRATIC SOCIETY* 167-81 (2d ed. 1975) (providing examples of clearance rate manipulations); Wald et al., *supra* note **Error! Bookmark not defined.**, at 1580 n.161 (noting manipulation of clearance rate data); Yale Kamisar, *How to Use, Abuse—and Fight Back With—Crime Statistics*, 25 OKLA. L. REV., 239, 252-53 (1972) (discussing problems with clearance rates); David Seidman & Michael Couzens, *Getting the Crime Rate Down: Political Pressure and Crime Reporting*, 8 L. & SOC’Y REV. 457, 476 (1974) (describing political modification of crime data).

⁹⁹. See *Controlling Crime Hearings*, *supra* note **Error! Bookmark not defined.** (statement of Robert W. Johnson, County Attorney, Anoka County, Minnesota) (“[C]leared cases can mean different things to different chiefs.”); cf. Keith Bottomley & Clive Coleman, *The Police*, in *INTERPRETING CRIME STATISTICS* 44, 54-55 (Monica A. Walker ed., Royal Statistical Soc’y Lecture Note Series No. 1, 1995) (criticizing British clearance statistics on the ground that clearances are interpreted differently from jurisdiction to jurisdiction); Lawrence W. Sherman, *Defining Arrest: Practical Consequences of Agency Differences (Part I)*, 16 CRIM. L. BULL. 376, 380 (1980) (“Clearance rates may vary substantially among departments for reasons having nothing to do with the objective probability of getting caught.” (quoting Wilson & Boland, *supra* note **Error! Bookmark not defined.**, at 368)).

example, found that the number of clearances per burglary varied widely from precinct to precinct, probably because individual police commanders defined clearances differently.¹⁰⁰ Another study found large variances in the way that clearances were recorded.¹⁰¹

Although such deviations might present a serious problem for analyses of police effectiveness in particular jurisdictions, our focus here is on a Supreme Court decision binding on police forces around the country. Accordingly, we use the aggregate national clearance rate, comprised of clearance reports from thousands of agencies.¹⁰² Even if a particular city reported rates in a questionable fashion, our results would be unaffected if any such manipulations did not change significantly in the several years surrounding *Miranda* or if any changes in the manipulations were relatively small in comparison to the total number of reports nationally—reasonable assumptions both.¹⁰³ We also report results for the aggregated categories of clearance rates for “violent crimes” and “property crimes,” for which inter-jurisdictional variations in categorizing crimes should be minimized.¹⁰⁴

¹⁰⁰. See PETER W. GREENWOOD, AN ANALYSIS OF THE APPREHENSION ACTIVITIES OF THE NEW YORK CITY POLICE DEPARTMENT 18 (1970).

¹⁰¹. See BERNARD GREENBERG, OLIVER S. YU & KAREN I. LANG, ENHANCEMENT OF THE INVESTIGATIVE FUNCTION 10 (1972).

¹⁰². Cf. Yehuda Grunfeld & Zvi Griliches, *Is Aggregation Necessarily Bad?*, 42 REV. ECON. & STAT. 1, 10 (1960) (“[A]ggregation is not necessarily bad if one is interested in the aggregates.”).

¹⁰³. See FOX, *supra* note **Error! Bookmark not defined.**, at 7 (concluding that the problem of data manipulation is “not overly troublesome” for time series analysis that “does not involve cross-sectional data, but rather a time series from the *same* population”); Charles R. Tittle & Alan R. Rowe, *Certainty of Arrest and Crime Rates: A Further Test of the Deterrence Hypothesis*, 52 SOC. FORCES 455, 456 (1974) (stating that although manipulations are possible, “there are good reasons to doubt that they greatly contaminate the data Moreover, such biases would seem to be distributed throughout the various police departments so that the validity of a study which examines internal variations in the entire body of data . . . would be unaffected.”).

¹⁰⁴. See Gene Swimmer, *The Relationship of Police and Crime: Some Methodological and Empirical Results*, 12 CRIMINOLOGY 293, 304 (1974) (claiming that the impact on the data by variability of reporting techniques is minimized by grouping the types of offenses in larger “property” and “violence” categories); see also Fox, *supra* note **Error! Bookmark not defined.**, at 124 n.4 (noting that most econometric studies employ aggregated crime indicators).

Another potential problem is that the FBI database varies slightly from year to year, depending on which cities report their figures to the FBI. But, again, this problem is relatively small for national data.¹⁰⁵ We defer discussion of other issues surrounding our data to the Appendix.¹⁰⁶

What factors influence clearance rates? The literature on clearance rates is surprisingly thin, particularly in assessing clearance rates over time.¹⁰⁷ A few studies analyze clearance rates in particular jurisdictions. These studies suggest that existing models of clearance rates are underdeveloped, as they rarely meet with much success in explaining clearance rates.¹⁰⁸ The available literature does, however, offer a few possible control variables.

¹⁰⁵. See FOX, *supra* note **Error! Bookmark not defined.**, at 127 n.11 (“Although the group of cities included in the FBI tabulations does change annually, the extent of error resulting from these fluctuations is minimal relative to the aggregate data.”). The problem of changing jurisdictions in the UCR, like the other problems just discussed, reinforces the desirability of focusing on *national* data. National data tends to average out these fluctuations, allowing a view of the “forest” rather than individual “trees.”

¹⁰⁶. See Appendix.

¹⁰⁷. See Peggy S. Sullivan, *Determinants of Crime and Clearance Rates for Seven Index Crimes* 28 (1985) (unpublished Ph.D. dissertation, Vanderbilt University) (on file with the *Stanford Law Review*) (“Social science research has generally not focused on the clearance rate . . . as a topic for study.”).

¹⁰⁸. See, e.g., Thomas F. Pogue, *Effect of Police Expenditures on Crime Rates: Some Evidence*, 3 PUB. FIN. Q. 14, 24 (1975) (finding that “clearance ratios are not well-explained by the variables included in the regression equation”); Sullivan, *supra* note **Error! Bookmark not defined.**, at 174 (concluding after extensive modeling that “[t]he models used to predict the clearance rates in general were very poorly specified”).

The factor most commonly cited as affecting the clearance rate is the crime rate. The standard argument is that as police officers have more crimes to solve, they will be able to solve a smaller percentage of them¹⁰⁹—variously called the “overload” theory,¹¹⁰ the “overtaxing” theory,¹¹¹ or the “system strain” theory.¹¹² Although the theory has intuitive appeal, the empirical support is mixed.¹¹³ Moreover, crime rates rose throughout the 1960s and later, a pattern that does not correspond to the sharp 1966-1968 decline in clearance rates.¹¹⁴ To control for the number of crimes, we collected FBI data on the estimated number of FBI “index” crimes committed across the country each year from 1960-1994. Because consistent FBI data are unavailable before 1960, we estimated an extension of the FBI data back

¹⁰⁹. See FOX, *supra* note **Error! Bookmark not defined.**, at 26 (advancing this hypothesis); Llad Phillips & Harold L. Votey, Jr., *An Economic Analysis of the Deterrent Effect of Law Enforcement on Criminal Activity*, 63 J. CRIM. L. CRIMINOLOGY & POLICE SCI. 330, 335 (1972) (same); cf. Dale O. Cloninger & Lester C. Sartorius, *Crime Rates, Clearance Rates and Enforcement Effort: The Case of Houston, Texas*, 38 AM. J. ECON. & SOC. 389, 396-97 (1979) (finding that the clearance rate in Houston, Texas, from 1960-1975 was inversely related to the crime rate); Pogue, *supra* note **Error! Bookmark not defined.**, at 21 (suggesting that the number of clearances might be positively related to the number of crimes, i.e., the number of potential clearances).

¹¹⁰. Michael Geerken & Walter R. Gove, *Deterrence, Overload, and Incapacitation: An Empirical Evaluation*, 56 SOC. FORCES 424, 429-31 (1977).

¹¹¹. Nagin, *supra* note **Error! Bookmark not defined.**, at 119.

¹¹². Charles H. Logan, *Arrest Rates and Deterrence*, 56 SOC. SCI. Q. 376, 384 (1975).

¹¹³. Compare Pogue, *supra* note **Error! Bookmark not defined.**, at 27 tbl.4 (finding an apparently statistically significant overload effect for rape, larceny, and auto theft but not for other crimes), and Sullivan, *supra* note **Error! Bookmark not defined.**, at 171 (finding support for overload hypothesis in cross-sectional study), with Richard R. Bennett, *The Effect of Police Personnel Levels on Crime Clearance Rates: A Cross-National Analysis*, 6 INT'L J. COMP. & APPLIED CRIM. JUST. 177, 186 (1982) (finding, in a cross-national study, that an increase in the number of crimes increases police ability to solve them); David F. Greenberg, Charles H. Logan & Ronald C. Kessler, *A Panel Model of Crime Rates and Arrest Rates*, 44 AM. SOC. REV. 843, 849 (1979) (finding no consistent, statistically significant relation between either instantaneous or lagged effect of crime rates on arrest rates); and Eric Rasmusen, *Stigma and Self-Fulfilling Expectations of Criminality*, 39 J.L. & ECON. 519, 520 (1996) (concluding that “[t]he overload theory . . . cannot explain the U.S. pattern” of crime).

¹¹⁴. See Cassell, *supra* note **Error! Bookmark not defined.**, at 307-08 (noting that, although crime rates rose throughout the 1960s and early 1970s, the drop in clearance rates occurred in a shorter time span); Cassell, *supra* note **Error! Bookmark not defined.**, at 334-35 (noting that clearance capacity fell from 1962-1965 while clearance rates rose and that a continued drop in clearance capacity in the 1970s was accompanied by steady clearance rates).

to 1950, as explained at greater length in the Appendix.¹¹⁵ This variable, converted to a crime rate by dividing the number of index crimes by the resident population for the country, is identified as *CRIME RATE*.

Apart from the crime rate, the factors most often cited as influencing clearance rates are law enforcement personnel and expenditures on law enforcement.¹¹⁶ With more personnel and resources available, the argument goes, more crimes should be cleared. Here again, despite the intuitive appeal of the hypothesis, the studies are mixed.¹¹⁷ To control for any influences these factors may have, we collected data on the number of law enforcement employees per capita ("*POLICE PERSONNEL*")¹¹⁸ and the dollars spent on police protection per capita by state and local governments, adjusted for inflation by the consumer price index ("*POLICE DOLLARS (REAL)*").

¹¹⁵. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text.

¹¹⁶. See, e.g., FOX, *supra* note **Error! Bookmark not defined.**, at 26 (hypothesizing that clearance rates depend on the size of police forces); Bennett, *supra* note **Error! Bookmark not defined.**, at 179 (hypothesizing that "personnel levels and police expenditures intuitively relate to the ability of the police to clear crimes").

¹¹⁷. See, e.g., Bennett, *supra* note **Error! Bookmark not defined.**, at 187 (reporting cross-national analysis suggesting that police variables have minimal effect on clearance rates); Pogue, *supra* note **Error! Bookmark not defined.**, at 24 (reporting cross-sectional analysis finding no support for effect of police expenditures or police manpower on clearance rates); Charles R. Wellford, *Crime and the Police: A Multivariate Analysis*, 12 CRIMINOLOGY 195, 205-06 (1974) (reporting that police variables did not account for much variation in clearance rates); Thomas F. Pogue, *The Crime Prevention Effects of Arrest and Imprisonment: Evidence from Multiple Cross-Section Analysis* 31-35 (1983) (unpublished manuscript, on file with the *Stanford Law Review*) (finding no statistically significant relationship between criminal justice expenditures and clearance rates); Sullivan, *supra* note **Error! Bookmark not defined.**, at 169 (reporting cross-sectional analysis suggesting that police variables have no effect on clearance rates for violent crime but positive effects for property crimes); cf. Dale O. Cloninger, *Enforcement Risk and Deterrence: A Re-examination*, 23 J. SOCIO-ECON. 273, 281 (1994) (finding that police presence is superior to clearance rates as a measure for deterrence studies).

¹¹⁸. Ideally, we should control for the number of police personnel on the streets. See Wilson & Boland, *supra* note **Error! Bookmark not defined.**, at 377 (noting that the correlation between the number of officers and the number of patrol units on the street is low). Unfortunately, national data on this subject are unavailable.

Professor Schulhofer has suggested that clearance rates would respond not simply to changes in law enforcement manpower and expenditures, but also to interactions between these variables and the overall number of crimes—what he calls the “capacity” of the system.¹¹⁹ To test this theory, we added to the equations *PERSONNEL CAPACITY*, which was defined as the rate of police employees per capita divided by the crime rate for index crimes, and *DOLLAR CAPACITY (REAL)*, which was defined as the number of inflation-adjusted dollars spent on police protection per capita divided by the crime rate for index crimes.¹²⁰

Criminal justice variables are not the only ones that might affect clearance rates. Other variables have been identified in the criminal justice literature as having some bearing on clearance rates or, more generally, on crime rates. Perhaps the most salient of these factors is the number of persons in the crime-prone younger years.¹²¹ Most crimes will be committed by persons who are in adolescence or early adulthood.¹²² Increases in the number of young persons, particularly in connection with the post-World War II “baby boom,” have been linked with changes in crime rates.¹²³ The age band commonly identified with this effect is fifteen to twenty-four,¹²⁴ which also corresponds to readily available census data. Since it is conceivable that more juveniles on the streets might similarly influence clearance rates, we included a variable to take into account this age band (“*JUVENILES*”).¹²⁵ A control variable of this type has been used in other criminal justice research.¹²⁶

¹¹⁹. See Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 291.

¹²⁰. Specifically, we took the previously defined variables *POLICE PERSONNEL* and *POLICE DOLLARS* and divided them each by *CRIMES* (the total number of index crimes committed in a given year). Because the numerator and denominators contain rates, the effect is to express the capacity variables in terms of personnel/crimes and dollars/crimes—e.g., $(\text{personnel} \div \text{population}) \div (\text{crimes} \div \text{population}) = \text{personnel} \div \text{crime}$. This is precisely as Schulhofer has suggested. See Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 365.

In an earlier, preliminary report of our findings, we generated the personnel capacity by dividing the rate of employees by the number of crimes and the dollar capacity by using nominal dollars. These differences, along with a data set that extended only to 1994, produced slightly different coefficients from those reported here. See Cassell, *supra* note **Error! Bookmark not defined.**, at 339 tbl.1 (reporting preliminary regression results).

¹²¹. Another commonly cited demographic variable is racial composition of the population. We have not controlled for this factor because, among other reasons, long-term racial changes are unlikely to explain short-term clearance rate fluctuations and because the empirical support for an association with clearance rates is thin. See Sullivan, *supra* note **Error! Bookmark not defined.**, at 30, 163-64 (concluding from the existing literature that the impact of minority groups on the

Changes in various socioeconomic variables are also plausible candidates for affecting clearance rates. Criminal justice literature identifies variances in the unemployment rate as a possible explanation for crime rate fluctuations.¹²⁷

clearance rate is “unknown” and then reporting her own cross-sectional analysis finding no influence of minority groups on clearance rates).

¹²². See generally Travis Hirschi & Michael Gottfredson, *Age and the Explanation of Crime*, 89 AM. J. SOC. 552 (1983); Darrell Steffensmeier, Cathy Streifel & Miles D. Harer, *Relative Cohort Size and Youth Crime in the United States, 1953-1984*, 52 AM. SOC. REV. 702 (1987).

¹²³. See, e.g., TASK FORCE ON ASSESSMENT, PRESIDENT’S COMM’N ON LAW ENFORCEMENT AND ADMIN. OF JUSTICE, TASK FORCE REPORT: CRIME AND ITS IMPACT—AN ASSESSMENT app. D (1967) [hereinafter CRIME AND ITS IMPACT]; Darrel Steffensmeier & Miles D. Harer, *Did Crime Rise or Fall During the Reagan Presidency? The Effects of an “Aging” U.S. Population on the Nation’s Crime Rate*, 28 J. RES. CRIME & DELINQ. 330, 343 (1991).

¹²⁴. See Kamisar, *supra* note **Error! Bookmark not defined.**, at 246 (identifying the 15-24 age band as having the most influence on crime rates); Steffensmeier & Harer, *supra* note **Error! Bookmark not defined.**, at 331 (same).

¹²⁵. An argument could be made for using only males because crimes are most often committed by males. However, such a refinement would likely make little difference to our results, as the ratios of males and females remain relatively constant in this age range.

¹²⁶. See, e.g., Isaac Ehrlich, *Participation in Illegitimate Activities: A Theoretical and Empirical Investigation*, 81 J. POL. ECON. 521, 544 (1973) (using percentage of males ages 14-24 as a variable); Tittle & Rowe, *supra* note **Error! Bookmark not defined.**, at 457 (controlling for population aged 15-24 in a deterrence study). But cf. D. Wayne Osgood, Patrick M. O’Malley, Jerald G. Bachman & Lloyd D. Johnston, *Time Trends and Age Trends in Arrests and Self-Reported Illegal Behavior*, 27 CRIMINOLOGY 389, 412 (1989) (cautioning against the use of broad age bands in some types of criminal justice research); Steffensmeier & Harer, *supra* note **Error! Bookmark not defined.**, at 336 (same).

¹²⁷. Compare M. HARVEY BRENNER, SUBCOMM. ON ECONOMIC GOALS AND INTERGOVERNMENTAL POLICY OF THE JOINT ECONOMIC COMM., 98TH CONG., ESTIMATING THE EFFECTS OF ECONOMIC CHANGE ON NATIONAL HEALTH AND SOCIAL WELL-BEING, S. PRt. 98-198, at 54-56 (Comm. Print 1984) [hereinafter EFFECTS OF ECONOMIC CHANGE] (reporting associations between unemployment and crime rates); M. Harvey Brenner, Book Review, 70 J. CRIM. L. & CRIMINOLOGY 273, 274 (1979) [hereinafter Brenner, Book Review] (reviewing FOX, *supra* note **Error! Bookmark not defined.**) (arguing that Fox improperly excluded unemployment from crime forecasting models); Wilson & Boland, *supra* note **Error! Bookmark not defined.**, at 374-75 (including unemployment rate as a control variable in a study of robbery rates); M. Harvey Brenner, *Economy, Society & Health* 6 (Oct. 16, 1992) (unpublished manuscript, presented at the Conference on Society & Health, on file with the *Stanford Law Review*) (same); and Pogue, *supra* note **Error! Bookmark not defined.**, at 41 (finding an association between unemployment and crime), with FOX, *supra* note **Error! Bookmark not defined.**, at 29 (reviewing literature and concluding that “[t]he absence of an impact of the unemployment rate on the rate of crime appears at this time to be unequivocal”); Ehrlich, *supra* note **Error! Bookmark not defined.**, at 555 (finding no relation between unemployment and crime rate but wondering about this conclusion); Kenneth C. Land & Marcus Felson, *A General Framework for Building Dynamic Macro Social Indicator Models:*

Similarly, changes in income levels and labor force participation might be associated with crime rates¹²⁸ and clearance rates, particularly since such factors might be viewed as a measure of the opportunity cost of committing a crime. Accordingly, we included variables for the labor force participation (“*LABOR FORCE PARTICIPATION*”), unemployment rate (“*UNEMPLOYMENT*”), disposable per capita real income (“*PER CAPITA INCOME (REAL)*”).

As a measure of changing social circumstances that might be related to crime, we also added a variable that measured live births to unmarried mothers,¹²⁹ converted to a per capita rate by dividing by resident population (“*BIRTHS TO UNMARRIED WOMEN*”).

It has also been suggested that increasing urbanization during the 1960s was an explanation for rising crime rates.¹³⁰ Conceivably, urbanization could have some bearing on clearance rates as well.¹³¹ For instance, clearance rates for index crimes are generally higher in smaller cities.¹³² A few studies of clearance rates, however, have found slim predictive power in urbanization.¹³³

To control for the possible effects of urbanization, we added a variable for the percent of the resident population residing in urban areas (“*URBANIZATION*”).

Including an Analysis of Changes in Crime Rates and Police Expenditures, 82 AM. J. SOC. 565, 566 (1976) (finding no relationship between crime and unemployment); and Swimmer, *supra* note **Error! Bookmark not defined.**, at 309 (reporting that unemployment rate had little explanatory power in criminal justice models).

¹²⁸. See *Effects of Economic Change*, *supra* note **Error! Bookmark not defined.**, at 54, 57 (noting that a decrease in per capita income had an inverse effect on most serious crimes and that, surprisingly, there was a positive correlation between labor market participation and arrest rates for the 16-17 age group); Swimmer, *supra* note **Error! Bookmark not defined.**, at 309 (finding that “median income relates significantly with fewer violent crimes but not fewer property crimes”).

¹²⁹. Cf. Sullivan, *supra* note **Error! Bookmark not defined.**, at 165 (finding a relationship between murder clearances and the percentage of families with single mothers).

¹³⁰. See Kamisar, *supra* note **Error! Bookmark not defined.**, at 247.

¹³¹. See Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 366-68 (noting that “[c]learance rates are consistently lower in the larger series” and suggesting that as a greater percentage of the population (and of the crimes committed) are in large cities, the national clearance rate will be affected).

¹³². See UCR-1995, *supra* note **Error! Bookmark not defined.**, at 199 tbl.25 (providing that clearance rates for index crimes were 16.9% for the nation’s seven largest cities (1,000,000+ residents), 21.3% for medium-sized cities (100,000-249,999 residents), and 25.9% for smaller cities (10,000-24,999 residents)).

¹³³. See Pogue, *supra* note **Error! Bookmark not defined.**, at 33 tbl.5 (finding urbanization

In response to a preliminary version of our equations, Professor Schulhofer suggested that we should have controlled for the distribution of crimes committed in large and small cities.¹³⁴ Because smaller cities have higher clearance rates, Schulhofer reasons that a shift in the distribution of crimes could bias our results.¹³⁵ To test this hypothesis, we controlled for the percentage of violent crimes committed in small cities, as reported in the *UCR* (“*CRIME IN SMALL CITIES*”).¹³⁶

As a final control, we added a standard time trend variable, identified as “*TREND OVER TIME*.” Although some cautions have been raised about such a variable,¹³⁷ we thought it might be useful to control for long-term, time-related trends apart from *Miranda*.

Finally, to capture the effects of the *Miranda* decision, we included a “dummy” variable in the equations (“*MIRANDA*”), which was assigned the value of 0 before *Miranda* and 1 after. Because we were working with yearly data and because *Miranda* was handed down on June 13, 1966 (roughly halfway through 1966), deciding what to do with the 1966 value of the *MIRANDA* variable was an issue. As a first approximation of *Miranda*’s effects, we assigned *MIRANDA* the value of 0 for years before 1965, 0.5 for 1966, and 1 for 1967 and following years. This is the simplest model, which ordinarily serves as a starting point for time series research.¹³⁸ We discuss alternative assumptions about when *Miranda* began to affect clearance rates at greater length below.¹³⁹

not statistically significant in explaining clearance rates and population density related only to robbery clearance rates); Sullivan, *supra* note **Error! Bookmark not defined.**, at 161-62 (finding urban variables “virtually insignificant” in explaining most clearance rates); *cf.* Pogue, *supra* note **Error! Bookmark not defined.**, at 27 tbl.4 (finding that population density reduced clearance rates to a statistically significant degree for overall crime rates, murder, and auto theft, but not for rape, robbery, aggravated assault, burglary, or larceny).

¹³⁴. See Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 366-67 (suggesting this control for violent crimes).

¹³⁵. See *id.* at 367.

¹³⁶. In theory, one should look at the percentage of all crimes committed in small cities, not just violent crimes. However, using the larger number of index crimes might tend to smooth out the fluctuations that could explain away the *Miranda* effect. Accordingly, to give maximum play to Schulhofer’s theory, we use—as he has suggested we should—the violent crime percentage.

¹³⁷. See, e.g., Charles R. Nelson & Heejoon Kang, *Pitfalls in the Use of Time As an Explanatory Variable in Regression*, 2 J. BUS. & ECON. STAT. 73 (1984).

¹³⁸. See McDowall et al., *supra* note **Error! Bookmark not defined.**, at 387-88 (defending the use of an abrupt, permanent change model and the law’s effective date in gun control time series research).

¹³⁹. See text accompanying notes **Error! Bookmark not defined.**-**Error! Bookmark not**

We thought that these variables would contain the most important influences on crime clearance rates over the last several decades. Although other variables could be included in the equations, there are certain statistical advantages to a parsimonious construction.¹⁴⁰ We discuss below whether “omitted” variables could have influenced our conclusions.¹⁴¹ Wherever reasonably possible, we have also chosen the *least* complicated statistical technique for analysis. Our intent in resorting to econometrics is to clarify the factors that affect clearance rates, not to obfuscate them.

defined. *infra.*

¹⁴⁰. See, e.g., Richard A. Hay, Jr. & Richard McCleary, *Box-Tiao Time Series Models for Impact Assessment: A Comment on the Recent Work of Deutsch and Alt*, 3 EVALUATION Q. 277, 304-05 (1979) (cautioning against “overmodeling”).

¹⁴¹. See text accompanying notes **Error! Bookmark not defined.****Error! Bookmark not defined.** *infra.*

Using standard ordinary least squares (“OLS”) regression techniques, it is possible to develop an equation to explain national crime clearance rates as follows:

$$\begin{aligned}
 \text{CLEARANCE RATE}_i &= \beta_{0i} + \beta_1(\text{MIRANDA})_i \\
 \text{(for violent, property,} &+ \beta_2(\text{CRIME RATE})_i \\
 \text{and individual} &+ \beta_3(\text{POLICE PERSONNEL})_i \\
 \text{index crimes)} &+ \beta_4(\text{POLICE DOLLARS (REAL)})_i \\
 &+ \beta_5(\text{PERSONNEL CAPACITY})_i \\
 &+ \beta_6(\text{DOLLAR CAPACITY (REAL)})_i \\
 &+ \beta_7(\text{JUVENILES})_i \\
 &+ \beta_8(\text{LABOR FORCE PARTICIPATION})_i \\
 &+ \beta_9(\text{UNEMPLOYMENT})_i \\
 &+ \beta_{10}(\text{PER CAPITA INCOME (REAL)})_i \\
 &+ \beta_{11}(\text{BIRTHS TO UNMARRIED WOMEN})_i \\
 &+ \beta_{12}(\text{URBANIZATION})_i \\
 &+ \beta_{13}(\text{CRIME IN SMALL CITIES})_i \\
 &+ \beta_{14}(\text{TREND OVER TIME})_i \\
 &+ g_i,
 \end{aligned}$$

where i runs from 1950-1995 and the independent variables are as described above. This is a “reduced form” equation, which assumes that there are no “simultaneity” problems (that is, that the dependent clearance rate variables do not affect any of the independent variables), an assumption we review below.¹⁴²

C. Crime Clearance Rates by Aggregate and Individual Crime Categories

¹⁴². See text accompanying notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra*.

Table I reports our findings for the aggregate categories of violent and property crimes and shows a statistically significant coefficient¹⁴³ associated with *MIRANDA* for both equations. For those not familiar with interpreting regression equations, the table shows a statistically significant effect on crime clearance rates from *MIRANDA*. The coefficient associated with the *MIRANDA* variable in the violent crime equation is -6.731, which suggests that crime clearance rates would be 6.7% higher without the shift captured in the *MIRANDA* variable. In 1995, for example, the violent crime clearance rate was 45.4%.¹⁴⁴ The regression equations suggest that the rate would have been 52.1% (45.4% + 6.7%) in the absence of *Miranda*. Similarly, for property crimes, the coefficient is -2.272, which indicates that the clearance rate for property crimes in 1995 would have been 19.9% (17.6% + 2.3%).¹⁴⁵

The numbers in Table I rest on the aggregate categories of “violent” and “property” crime. There is a danger, of course, that such aggregations may produce misleading results.¹⁴⁶ In particular, it is possible that one component part of the aggregate may have changed in response to *Miranda*, creating the spurious impression that the entire category had changed.

TABLE I. VIOLENT AND PROPERTY CRIME CLEARANCE RATES (1950-1995)
OLS Regressions on Clearance Rates for Cities (*t* statistics in parenthesis)

Variable	Violent Crimes	Property Crimes
<i>MIRANDA</i>	-6.731 (-3.936)**	-2.272 (-4.080)**
<i>CRIME RATE</i>	-0.226 (-1.096)	0.00580 (.086)
<i>POLICE PERSONNEL</i>	-9.940 (-1.540)	1.633 (0.777)
<i>POLICE DOLLARS (REAL)</i>	0.162 (0.662)	-0.0422 (-0.530)
<i>PERSONNEL CAPACITY</i>	2.509 (1.445)	0.572 (1.012)

¹⁴³. All statistical significance tests reported in this article are two-tailed, although an argument could be made for a one-tailed test. We use the standard 95% confidence level for interpreting our results. We also report which of our results are significant at the 90% level to assist those who would use the lower confidence interval.

¹⁴⁴. See UCR-1995, *supra* note **Error! Bookmark not defined.**, at 199 tbl.25.

¹⁴⁵. See *id.* (listing the clearance rate for property crimes as 17.6%).

¹⁴⁶. Cf. Lott & Mustard, *supra* note **Error! Bookmark not defined.**, at 7 (raising a similar concern in a gun control study).

<i>DOLLAR CAPACITY (REAL)</i>	-6.806 (-0.609)	0.602 (0.166)
<i>JUVENILES</i>	-0.388 (-0.691)	-0.0923 (-0.504)
<i>LABOR FORCE PARTICIPATION</i>	0.634 (1.561)	0.0453 (0.342)
<i>UNEMPLOYMENT</i>	0.970 (2.992)**	0.458 (4.340)**
<i>PER CAPITA INCOME (REAL)</i>	0.00451 (2.698)*	0.00288 (3.448)**
<i>BIRTHS TO UNMARRIED WOMEN</i>	0.0213 (0.099)	0.0926 (1.326)
<i>URBANIZATION</i>	2.509 (2.067)*	0.460 (1.166)
<i>CRIME IN SMALL CITIES</i>	0.166 (1.496)	-0.0366 (-1.013)
<i>TREND OVER TIME</i>	-1.250 (-3.041)**	-0.508 (-3.800)**
<i>INTERCEPT</i>	2267.9 (3.068)**	962.9 (3.999)**
<i>Adjusted R²</i>	.983	.980
<i>Root MSE</i>	1.114	0.362
<i>Durbin-Watson</i>	2.065	2.102

** significant at .01 level * significant at .05 level † significant at .10 level

This problem can be eliminated by simply disaggregating violent and property crimes and running separate regressions on the component crimes. Figure 3 depicts clearance rates for the violent crimes of nonnegligent homicide, rape, and aggravated assault.

The three crimes exhibit a long-term downward trend, but, unlike the other violent crimes, robbery has a sharp downward break in the 1966-1968 period. Robbery is depicted separately, because its clearance rate is so much

lower, in Figure 4.

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As can be seen, Figure 4 shows the sharp reduction in 1966-1968. This suggests that robbery clearances in particular may have been depressed by *Miranda*.¹⁴⁷ The regression results reported in Table II confirm this conclusion.

¹⁴⁷. In the disaggregated equations, we continue to use the crime rate for all index crimes as the independent variable, rather than the crime rate for the particular index crime. We thought this was the superior model because police overload is presumably a function of the entire police workload, rather than the workload from a particular crime.

TABLE II. TOTAL AND INDIVIDUAL VIOLENT CRIME
CLEARANCE RATES (1950-1995)
OLS Regressions on Clearance Rates for Cities (*t* statistics in parenthesis)

Variable	Violent	Murder	Rape	Robbery	Assault
MIRANDA	-6.731 (-3.936)**	-2.078 (-1.097)	0.374 (0.148)	-5.306 (-2.858)**	-0.629 (-0.356)
CRIME RATE	-0.226 (-1.096)	-0.546 (-2.384)*	0.0350 (0.115)	0.213 (1.017)	-0.286 (-1.341)
POLICE PERSONNEL	-9.940 (-1.540)	-3.734 (-0.522)	-4.551 (-0.477)	5.342 (0.814)	-7.053 (-1.057)
POLICE DOLLARS (REAL)	0.162 (0.662)	0.203 (0.750)	0.0491 (0.136)	-0.341 (-1.369)	-0.161 (0.639)
PERSONNEL CAPACITY	2.509 (1.445)	1.266 (0.658)	-1.153 (-0.449)	-1.195 (-0.677)	2.718 (1.514)
DOLLAR CAPACITY (REAL)	-6.806 (-0.609)	-12.28 (-0.992)	12.47 (0.755)	18.46 (1.624)	-10.370 (-0.898)
JUVENILES	-0.388 (-0.691)	1.057 (1.700) [†]	-1.757 (-2.115)*	-0.259 (-0.454)	-0.304 (-0.525)
LABOR FORCE PARTICIPATION	0.634 (1.561)	0.314 (0.699)	0.256 (0.427)	0.174 (0.422)	0.381 (0.909)
UNEMPLOYMENT	0.970 (2.992)**	0.569 (1.585)	-0.121 (-0.253)	0.785 (2.379)*	1.131 (3.372)**
PER CAPITA INCOME (REAL)	0.00451 (2.698)*	0.000573 (0.309)	-0.00100 (-0.487)	0.00329 (1.935) [†]	0.00362 (2.093)*
BIRTHS TO UNMARRIED WOMEN	0.0213 (0.099)	0.00953 (0.040)	0.0554 (0.175)	0.216 (0.994)	-0.064 (-0.293)
URBANIZATION	2.509 (2.067)*	1.245 (0.927)	-1.237 (-0.689)	-0.134 (-0.109)	2.964 (2.363)*
CRIME IN SMALL CITIES	0.166 (1.496)	-0.361 (-2.940)**	0.176 (1.076)	0.0451 (0.399)	0.0500 (0.435)
TREND OVER TIME	-1.250 (-3.041)**	-0.530 (-1.165)	-0.506 (-0.833)	-1.103 (-2.637)*	-1.230 (-2.895)**
INTERCEPT	2267.9 (3.068)**	1035.7 (1.265)	1158.1 (1.050)	2149.550 (2.858)**	2235.2 (2.924)**
Adjusted R ²	.983	.986	.978	.983	.981
Root MSE	1.114	1.234	1.647	1.133	1.151
Durbin-Watson	2.065	2.078	2.199	2.185	2.019

** significant at .01 level

* significant at .05 level

† significant at .10 level

The only individual violent crime for which the *MIRANDA* variable has a statistically significant effect is the crime of robbery.

Turning to property crimes (i.e., burglary, larceny, and vehicle theft), Figure 5 shows that all exhibit a downward trend following *Miranda*. Vehicle theft clearance shows a particularly sharp drop in the 1966-1968 period, while the drop in burglary and larceny clearances extends from 1961-1968.

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The regression results reported below reflect these visual observations. As can be seen in Table III, the *MIRANDA* variable has a statistically significant downward effect on clearance rates for all of the property.

TABLE III. TOTAL AND INDIVIDUAL PROPERTY CRIME
CLEARANCE RATES (1950-1995)
OLS Regressions on Clearance Rates for Cities (*t* statistics in parenthesis)

Variable	Property	Burglary	Larceny	Vehicle Theft
MIRANDA	-2.272 (-4.080)**	-2.549 (-2.840)**	-2.360 (-2.214)**	-4.148 (-2.424)**
CRIME RATE	0.00580 (.086)	-0.00915 (-.084)	-0.0627 (-.0728)	0.00181 (.012)
POLICE PERSONNEL	1.633 (0.777)	3.187 (0.941)	-0.312 (-.0116)	-2.328 (-.0509)
POLICE DOLLARS (REAL)	-0.0422 (-.0530)	0.0232 (0.181)	0.0387 (0.380)	-0.0772 (-.0445)
PERSONNEL CAPACITY	0.572 (1.012)	1.762 (1.934)†	1.320 (1.825)†	-0.787 (-.0640)
DOLLAR CAPACITY (REAL)	0.602 (0.166)	-4.042 (-.0689)	-6.525 (-1.401)	3.968 (0.501)
JUVENILES	-0.0923 (-.0504)	-0.610 (-2.072)*	0.0455 (0.195)	-1.078 (-2.709)*
LABOR FORCE PARTICIPATION	0.0453 (0.342)	0.294 (1.383)	0.0639 (0.378)	0.523 (1.820)†
UNEMPLOYMENT	0.458 (4.340)**	0.706 (4.150)**	0.561 (4.156)**	0.108 (0.472)
PER CAPITA INCOME (REAL)	0.00288 (3.448)**	0.00219 (2.491)*	0.00249 (3.579)**	0.00180 (1.522)
BIRTHS TO UNMARRIED WOMEN	0.0926 (1.326)	0.0809 (0.718)	0.00925 (0.103)	-0.0820 (-.0540)
URBANIZATION	0.460 (1.166)	2.190 (3.440)**	0.415 (0.822)	0.745 (0.867)
CRIME IN SMALL CITIES	-0.0366 (-1.013)	-0.0136 (-.0234)	-0.0621 (-1.343)	0.0640 (0.814)
TREND OVER TIME	-0.508 (-3.800)**	-1.204 (-5.580)**	-0.274 (-1.600)	-0.682 (-2.343)*
INTERCEPT	962.9 (2.000)**	2190.1 (5.645)**	504.6 (1.628)	1290.2 (2.462)*
Adjusted R ²	.980	.992	.789	.983
Root MSE	0.362	0.584	0.464	0.789
Durbin-Watson	2.102	1.944	2.078	1.687

** significant at .01 level

* significant at .05 level

† significant at .10 level

D. *Explaining the Pattern*

Our equations suggest a “*Miranda* effect”¹⁴⁸ on clearance rates for robbery, larceny, vehicle theft, and burglary, but not homicide, rape, or assault. What could explain this pattern?¹⁴⁹ No doubt, the reasons are complex, but we venture some tentative interpretations.

At first glance, notice that what might in part be loosely denominated as crimes of passion or aggression (i.e., murder, rape, and assault) were apparently unaffected by *Miranda*, while what are more often crimes of deliberation (i.e., robbery, larceny, vehicle theft, and possibly burglary) were affected. These categories, of course, are gross oversimplifications, as there are obviously coolly calculated murders and impulsive car thefts. But if the generalizations are more often than not correct, they might correspond with the empirical evidence suggesting that *Miranda* more substantially affects police success in dealing with repeat offenders and professional criminals.¹⁵⁰ It seems probable that professional criminals would most often commit crimes such as robbery, burglary, and vehicle theft.

¹⁴⁸. In Part IV below, we discuss in detail why we attribute our results to a *Miranda* effect rather than some other cause.

¹⁴⁹. Because our focus is on the effects of *Miranda*, we will not discuss other effects we found or failed to find in specific equations—e.g., the effects of police and economic variables on clearance rates. Unlike our *Miranda* effect, these other effects appear to be “fragile” in the sense that they depend on model specification to produce them. See notes **Error! Bookmark not defined.** **Error! Bookmark not defined.** *infra* and accompanying text (presenting extreme bounds analysis of the equations).

¹⁵⁰. See Cassell, *supra* note **Error! Bookmark not defined.**, at 464-66 (citing studies that show that repeat offenders may be less likely to confess than those without criminal records).

Still another explanation rests on the point that police may more often clear certain kinds of crimes through confessions. A study of the New York City Police Department around the time of *Miranda* compiled ratios of clearances to arrests across crime categories.¹⁵¹ The ratio of clearances to arrests is well in excess of one for some crimes—specifically burglary, grand larceny, grand larceny vehicle, and robbery.¹⁵² For burglary, for example, the ratio was 3.778, meaning that, for each burglary arrest, police cleared more than three burglaries. No doubt a substantial number of these additional clearances came from confessions.¹⁵³ On the other hand, for other crimes—specifically homicide, rape, and assault—the ratio was quite close to one. Presumably murderers and rapists rarely confess to more than one crime. This suggests that confessions may play a more important role for crimes such as burglary, vehicle theft, larceny, and robbery, and that clearance for these crimes are, therefore, more susceptible to changes in confession procedures.

¹⁵¹. See GREENWOOD, *supra* note **Error! Bookmark not defined.**, at 24 tbl.3.

¹⁵². See *id.* The following ratios of clearance rate to arrest rate can be derived from Greenwood's data: 1.045 for homicide; 1.063 for rape; 1.660 for robbery; 1.073 for assault; 3.778 for burglary; 2.564 for grand larceny; and 2.416 for grand larceny vehicle.

¹⁵³. For further discussion of this issue, see notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text.

Another possible partial explanation is that police may be able to shift resources to maintain high clearance rates for the most serious and less numerous crimes (e.g., murder and rape) at the expense of lower clearance rates for less serious and more numerous crimes (e.g. larceny and vehicle theft). Police agencies are frequently judged by their effectiveness in solving the most notorious crimes, especially murders.¹⁵⁴ As a result, maximum “detective-power” is allocated to solve homicides.¹⁵⁵ If *Miranda* affected clearances generally, one would expect police to respond. To the extent resources affect clearance rates,¹⁵⁶ police should be able to maintain high clearance rates for the most serious crimes by allocating more resources to solve them, but at the cost of lower clearance rates in crime categories less visible to the public.¹⁵⁷

¹⁵⁴. See DAVID SIMON, HOMICIDE: A YEAR ON THE KILLING STREETS 17-19 (1991) (giving an example of this for the Baltimore police).

¹⁵⁵. See Kamisar, *supra* note **Error! Bookmark not defined.**, at 252; *cf.* Joan Petersilia, Allan Abrahamse & James Q. Wilson, POLICE PERFORMANCE AND CASE ATTRITION 40 (Rand No. R-3515-NIJ 1987) (noting that police work “hardest on the most promising cases . . . and, to a degree, on the most important ones”).

¹⁵⁶. See note **Error! Bookmark not defined.** *supra* (collecting conflicting evidence on this issue); *cf.* Tables I & II (finding sporadic effect of police resources on clearances).

¹⁵⁷. See Sullivan, *supra* note **Error! Bookmark not defined.**, at 8 (suggesting this possibility).

If the shifting resources theory is correct, it places an interesting gloss on the only available before-and-after *Miranda* study that specifically analyzes individual crime categories. Researchers in Pittsburgh found that, after *Miranda*, the confession rate in homicide cases fell 27.3 percentage points, robbery cases 25.7 points, auto larceny 21.2 points, burglary and receiving goods 13.7 points, and forcible sex offenses 0.5 points.¹⁵⁸ Interestingly, the Pittsburgh study found no change in homicide clearances after *Miranda* and a huge drop in robbery clearance rates.¹⁵⁹ In the eighteen months before *Miranda*, Pittsburgh police cleared 94.3% (50 of 53) of all homicides; in the thirteen months after, they also cleared 94.4% (34 of 36) of all homicides—even though confessions for homicide fell sharply.¹⁶⁰ While it would have been feasible for the Pittsburgh police to devote the necessary resources to solve thirty-four homicides with fewer confessions, it would have been virtually impossible to deal with the vast decline in the number of robbery confessions. Before *Miranda*, the Pittsburgh police cleared 45.1% (970 of 2152) of all robberies; after *Miranda*, they cleared only 30.8% (556 of 1805).¹⁶¹ If the Pittsburgh numbers are indicative of what happened around the country, then there may have been a shift in police resources on a national basis toward homicide offenses at the expense of other less serious offenses (e.g., robbery, burglary, larceny, and vehicle theft). This could explain why our analysis did not find a *Miranda* effect for homicide.

¹⁵⁸. These figures are derived from subtracting the post- *Miranda* rates from the pre-*Miranda* rates listed in Seeburger & Wettick, *supra* note **Error! Bookmark not defined.**, at 11 tbl.1.

¹⁵⁹. *See id.* at 20 (finding overall clearances in Pittsburgh higher after *Miranda* than before).

¹⁶⁰. *See id.* at 21 tbl.9.

¹⁶¹. *See id.* This reading of the Pittsburgh study, however, does not explain why auto larceny and burglary clearances rose slightly, *see id.*, even in the face of declining confessions.

Another reason why no *Miranda* effect for homicide was found may be that we did not account for changing homicide patterns. Homicide, more than any other violent crime, exhibits a long-term decline in clearance rates. It is quite likely that at least some portion of this drop is attributable to the increase in the proportion of felony-type murders and the corresponding decline in murders within the family or as a result of what the FBI describes as “romantic triangles and lovers’ quarrels.”¹⁶² Presumably the family and romantic homicides are easier to solve, whereas the felony-type murders, often committed by strangers, are less so. We have not accounted for these changes because of the difficulty in obtaining data for the relevant time period.¹⁶³ It is possible that these changes have obscured any *Miranda* effect in the homicide regressions.

If changes in the patterns of homicides could have *obscured* a *Miranda*-induced drop in clearance rates, could changes in the patterns of other crimes have *caused* a drop in clearance rates that coincided with *Miranda*? It seems improbable that crime patterns would have changed suddenly enough to explain the kind of sharp 1966-1968 drop that we observed, for example, for robbery. In any event, there were two crimes for which patterns were relatively stable during the late 1960s: robbery and larceny.¹⁶⁴ Since we found a *Miranda* effect on both of these crimes, our *Miranda* effect does not appear to be an artifact of changing crime patterns.

E. *Specification of the Equations*

¹⁶². UCR-1993, *supra* note **Error! Bookmark not defined.**, at 285. A recent edition of the UCR contained the following data on homicides, estimating the percent of total murder victims attributable to each category:

CATEGORY	1965	1970	1975	1980	1985	1992
Family Victims	31%	25%	25%	16%	17%	12%
“Lovers’ Quarrels”	10.1%	7.1%	7.3%	5.4%	6.2%	4.9%
Total	16%	20%	22%	24%	20%	23%

See id. at 285 tbls.5.3-5.5.

¹⁶³. FBI data before 1965 on offender-victim relationships in homicide cases do not appear to be regularly published in a consistent format. *See* Letter from Bennie F. Brewer, Chief, Programs Support Section, Criminal Justice Information Services Division, FBI, to Paul G. Cassell, Professor of Law, University of Utah College of Law 1 (Jan. 28, 1997) (on file with the *Stanford Law Review*).

¹⁶⁴. *See* Lawrence E. Cohen & Marcus Felson, *Social Change and Crime Rate Trends: A Routine Activity Approach*, 44 AM. SOC. REV. 588, 600 tbl.5 (1979). The authors report offense trends for only four crimes: robbery, burglary, larceny, and murder. Although their analysis is based on data apparently collected as part of the UCR program, the data does not appear to be published in the annual crime reports and it is unclear whether the data is readily available.

In this section we consider various questions that might be asked about the specification of our regression equations.

1. *The timing of the Miranda effect.*

The basic equations just reported assume that *Miranda*'s impact was fully and immediately felt halfway through 1966. Yet not all of *Miranda*'s effects would have been felt immediately on and after June 13, 1966, the date the Court announced its decision. Instead, it is more likely that the decision took hold over several years.¹⁶⁵ Police compliance with *Miranda* was far from immediate and uniform throughout the country, as the requirements of the decision were "implemented slowly and imperfectly in many jurisdictions."¹⁶⁶

The four available empirical studies immediately following *Miranda* all found substantial deviations from the prescribed procedures. A study by student observers in New Haven in the summer of 1966 found that police failed to follow important parts of the *Miranda* regime, such as the requirements of obtaining an affirmative waiver of rights from a suspect or stopping questioning upon a suspect's request to see counsel.¹⁶⁷ While the observers reported improved compliance with *Miranda* over the summer, they observed full compliance in only a fraction of the interrogations.¹⁶⁸ Similarly, a 1966 study based on interviews of suspects interrogated in the District of Columbia concluded that police had "failed to observe the spirit and often the letter of *Miranda*."¹⁶⁹ Noncompliance with *Miranda* was also suggested by a field observation of D.C. police immediately after the decision.¹⁷⁰ A 1967 study by the Vera Institute in New York City, based in part on audiotapes of police interrogations, concluded that whether police complied with *Miranda* was "open to serious question."¹⁷¹ Finally, a 1969 study of interrogations in Denver reported general police compliance with *Miranda*, with some exceptions.¹⁷² Interviews with suspects suggested some possible police noncompliance, however.¹⁷³ These four studies are the only assessments of *Miranda* compliance shortly after the decision in which researchers either observed police interrogations or talked to persons who had been interrogated.¹⁷⁴ Interestingly, all report incomplete implementation.

¹⁶⁵. See MCDOWALL ET AL., *supra* note **Error! Bookmark not defined.**, at 75 (concluding that many social science impacts will be gradual).

¹⁶⁶. Schulhofer, *supra* note **Error! Bookmark not defined.**, at 513.

¹⁶⁷. See Wald et al., *supra* note **Error! Bookmark not defined.**, at 1550-56 (discussed in Cassell, *supra* note **Error! Bookmark not defined.**, at 407-08).

¹⁶⁸. See *id.* at 1550 tbl.3 (showing increasing compliance with *Miranda* from June through August, but not full compliance in every interrogation).

¹⁶⁹. Richard J. Medalie, Leonard Zeitz & Paul Alexander, *Custodial Police Interrogation in*

Of course, proving substantial noncompliance with *Miranda* in and around 1966 does not fully answer our question about the timing of *Miranda*'s effect. The remaining question is when, if at all, did police begin to substantially comply? Because of the dearth of police interrogation studies since 1970,¹⁷⁵ iron-clad evidence on police compliance with *Miranda* is unavailable. The prevailing view, however, is that police now follow *Miranda*'s requirements¹⁷⁶ and that they began doing so by around 1969. As one scholar who reviewed the available information concluded,

Our Nation's Capital: The Attempt to Implement Miranda, 66 MICH. L. REV. 1347, 1394 (1968). The study's data presentation is criticized in Leo, *supra* note **Error! Bookmark not defined.**, at 639.

¹⁷⁰. See Albert J. Reiss, Jr. & Donald J. Black, *Interrogation and the Criminal Process*, 374 ANNALS AM. ACAD. POL. & SOC. SCI. 47, 55 n.6 (1967) (reporting that few suspects received complete *Miranda* warnings in field interrogations). The proper interpretation of Reiss and Black's findings is unclear, however, because they do not specify which of the field interrogations involved custodial interrogation, the predicate for application of *Miranda*.

¹⁷¹. TAPING POLICE INTERROGATIONS, *supra* note **Error! Bookmark not defined.**, at 75-76.

¹⁷². See Lawrence S. Leiken, *Police Interrogation in Colorado: The Implementation of Miranda*, 47 DENV. L.J. 1, 10, 14, 47 (1970) (finding "strict compliance with [*Miranda*'s] formal requirements"). The study suggested that police sometimes ignored suspects' refusals to waive rights. See *id.* at 30.

¹⁷³. See *id.* at 30 (reporting allegations by suspects that police continued interrogations after requests for counsel).

¹⁷⁴. A study in Wisconsin also possibly followed quasi-observational methodology, but contains very little specific information on police practices. See Neal Milner, *Comparative Analysis of Patterns of Compliance with Supreme Court Decisions: "Miranda" and the Police in Four Communities*, 5 L. & SOC'Y REV. 119 (1970). A 1969-1970 survey of police officers in Tennessee and Georgia found that 36% (18 of 50) said that they would continue to question a suspect if the suspect first waived his right to presence of counsel and then later changed his mind by attempting to invoke his *Miranda* rights. See STEPHENS, *supra* note **Error! Bookmark not defined.**, at 196 tbl.10. However, this finding has limited significance because this scenario (of suspects initially waiving and subsequently invoking their *Miranda* rights) occurs infrequently. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 860 (finding that suspects waived and then asserted rights in only 3.9% of interrogations in the sample); Leo, *Inside the Interrogation Room*, *supra* note **Error! Bookmark not defined.**, at 275 tbl.2 (finding the percentage to be 1.1%). Other studies from around the time of *Miranda* contain only conclusory information about police compliance with the decision. See Cassell, *supra* note **Error! Bookmark not defined.**, at 1088 n.26 (collecting citations).

¹⁷⁵. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 848-49 (finding almost no studies on police interrogations since 1970); Thomas, *supra* note **Error! Bookmark not defined.**, at 822 (same).

¹⁷⁶. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 887-92 (collecting references and evidence to this effect).

implementation problems were largely a “transitional” feature, and “[b]y the end of the 1960s, *Miranda* advice had become an accepted part of police work.”¹⁷⁷ This conclusion suggests it is reasonable to assume that *Miranda* effects manifested themselves over several years.

Focusing on suspects rather than police also suggests that *Miranda*'s effects unfolded over several years. Presumably over time suspects gained experience, either directly or through the “grapevine,” in how to take advantage of the new interrogation rules. Precise empirical evidence on this point is hard to come by, but there are some suggestions that suspects would adjust to *Miranda*-style requirements over several years.¹⁷⁸

¹⁷⁷. Van Kessel, *supra* note **Error! Bookmark not defined.**, at 102-03; *see also* Leo, *supra* note **Error! Bookmark not defined.**, at 645 (concluding after comprehensive review of the studies that “in the years 1966-1969 after an initial adjustment period American police began to comply regularly with the letter of the new *Miranda* requirements”); Thomas, *supra* note **Error! Bookmark not defined.**, at 953 (suggesting that the “starting point” for *Miranda* research should be “after the controversy had subsided and the police had adjusted to its strictures . . . in the decade of the 1970s”).

¹⁷⁸. *See* David Dixon, Keith Bottomley, Clive Coleman, Martin Gill & David Wall, *Safeguarding the Rights of Suspects in Police Custody*, 1 POLICING & SOC'Y 115, 122 (1990) (finding a rapid increase in requests for legal advice by juveniles in British city from 1984-1987 under new interrogation regime, due in part to the spread of information). The claim is not that suspects responded to *Miranda* by changing the kinds of crime that they were committing. *Cf.* Philip J. Cook, *The Clearance Rate As a Measure of Criminal Justice System Effectiveness*, 2 CRIMINOLOGY REV. Y.B. 669 (1980) (discussing this problem in deterrence studies). Instead, the claim is that some suspects responded to *Miranda* by offering fewer incriminating statements, thereby making crimes harder to solve. *See* Cassell, *supra* note **Error! Bookmark not defined.**, at 450-51 (“There is some support for the hypothesis that the population has now, in effect, been ‘*Mirandized*.’”).

One last issue is whether confessions might have been declining *before* June 1966 because of other Warren Court decisions. In particular, in June 1964, the Supreme Court decided *Escobedo v. Illinois*,¹⁷⁹ suppressing a confession because the police had denied Escobedo's request to see a lawyer after the investigation had begun to focus on him. The ruling has been aptly described as having an "accordion-like quality," both "broad" and "confining,"¹⁸⁰ and it is clear that "the scope and meaning of the decision was a matter of strong and widespread disagreement."¹⁸¹ The limited available evidence suggests that *Escobedo* had little, if any, effect on confession rates.¹⁸² Nonetheless, it remains possible that *Escobedo* or its subsequent lower court interpretations¹⁸³ had some limited effect on confession rates before *Miranda*, an issue that can be investigated with various equations.

Although it seems likely that *Miranda*'s effects were not instantaneous, the available information does not establish precisely what impact structure is appropriate. Some care is required on this point, because in theory the selection of impact structure could make a difference to regression conclusions.¹⁸⁴

¹⁷⁹. 378 U.S. 478 (1964).

¹⁸⁰. YALE KAMISAR, WAYNE R. LAFAVE & JEROLD H. ISRAEL, *MODERN CRIMINAL PROCEDURE* 465 (8th ed. 1994).

¹⁸¹. *Id.*

¹⁸². The evidence is collected in Cassell, *supra* note **Error! Bookmark not defined.**, at 493-94. For example, the "clear consensus" of the attendees of the May 1996 annual meeting of the National Association of Attorneys General was that *Escobedo* had little effect on confession rates. See Sidney E. Zion, *Prosecutors Say Confession Rule Has Not Harmed Enforcement*, N.Y. TIMES, May 18, 1966, at 27.

¹⁸³. See, e.g., *United States ex rel. Russo v. New Jersey*, 351 F.2d 429 (3d Cir. 1965) (requiring police to advise suspects of a right to counsel), *vacated*, 384 U.S. 889 (1966); *People v. Dorado*, 398 P.2d 361 (Cal. 1965) (en banc) (requiring California police officers to warn suspects of their rights to counsel and to remain silent).

¹⁸⁴. See Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 291-92 (noting the potential effects of an inappropriate time period selection on regression analysis); see also Britt et al., *supra* note **Error! Bookmark not defined.**, at 368-70 (explaining that the specification of the model of the impact of a gun control law has significant influence on the validity of the conclusions); McDowall et al., *supra* note **Error! Bookmark not defined.**, at 385-86 (same).

We believe the best view is that *Miranda* took hold over the 1966-1968 period. However, to assess the competing possibilities, we tested a variety of different impact structures for the *MIRANDA* variable. We hasten to emphasize that we tested these alternative models only to rebut potential criticism of our model. There is considerable wisdom in those who caution against efforts to “fish” for an impact structure that will produce the desired outcome.¹⁸⁵ The attempt here, however, was not to obtain a result, but to test the strength of our conclusions by examining what difference alternative assumptions would have made.

We assumed that *Miranda*'s impact would be faithfully modeled in one of the following:

July 1966. As a baseline basic model, we used the *Miranda* impact structure discussed previously, which assumed that *Miranda* took effect almost immediately on July 1, 1966; that is, *MIRANDA* takes the value 0 before 1966, 0.5 in 1966, and 1 in 1967 and subsequent years. This is known as an abrupt permanent model.¹⁸⁶

1966-1968. *MIRANDA* takes the value of 0 before 1966, 0.333 in 1966, 0.666 in 1967, and 1 in 1968 and following years. Based on the available evidence of a short-term lag in police compliance with *Miranda*, this is probably the best model of *Miranda*'s effects. This is known as a gradual permanent model.¹⁸⁷

1966+. *MIRANDA* takes the value of 0 before 1966 and the value of 1 in 1966 and subsequent years.

1967+. *MIRANDA* takes the value of 0 before 1967 and the value of 1 in 1967 and subsequent years.

1964-1968. *MIRANDA* takes effect from 1964-1968, assuming the value of 0 before 1964, 0.100 in 1964, 0.333 in 1965, 0.666 in 1966, 0.900 in 1967, 1 in 1968 and subsequent years. This model captures any pre-*Miranda* effect from *Escobedo* or police “anticipation” of *Miranda*.¹⁸⁸

¹⁸⁵. See, e.g., McDowall et al., *supra* note **Error! Bookmark not defined.**, at 386.

¹⁸⁶. See SIR MAURICE KENDALL & J. KEITH ORD, *TIME SERIES* 222 (3rd ed. 1990).

¹⁸⁷. See *id.*

¹⁸⁸. An anticipation effect would occur if police changed their procedures before receiving an expected legal mandate. Other time series analyses have considered the possibility of such an anticipation or “announcement” effect preceding the actual imposition of a legal change. See, e.g., Glenn L. Pierce & William J. Bowers, *The Bartley-Fox Gun Law's Short-Term Impact on Crime in Boston*, 455 ANNALS AM. ACAD. POL. & SOC. SCI. 120, 124 (1981) (finding an announcement

1964-1969. *MIRANDA* takes effect from 1964-1969, assuming the value of 0 before 1964, 0.166 in 1964, 0.333 in 1965, 0.500 in 1966, 0.666 in 1967, 0.832 in 1968, and 1 in 1969 and subsequent years. This model spreads out the *Miranda* effect over a five-year period.

As can be seen in Table IV, the sign of the *MIRANDA* variable is generally not sensitive to these alternative specifications of *Miranda*'s impact.¹⁸⁹

TABLE IV. ALTERNATIVE STRUCTURES FOR THE START AND PHASE-IN OF THE *MIRANDA* VARIABLE (1950-1995)
COEFFICIENTS OF THE *MIRANDA* VARIABLE
OLS Regressions on Clearance Rates for Cities
(*t* statistics in parenthesis)

Crime	July 1966	1966-1968	1966+	1967+	1964-1968	1964-1969
Violent	-6.731 (-2.026)**	-9.057 (-4.622)**	-3.987 (-2.545)*	-5.213 (-2.561)**	-7.978 (-2.652)**	-8.810 (-2.115)**
Murder	-2.078 (1.097)	-3.084 (-1.354)	-1.021 (-0.648)	-1.822 (-1.162)	-2.143 (-0.902)	-1.714 (-0.577)
Rape	0.374 (0.148)	-0.879 (-0.287)	0.601 (0.290)	-0.097 (-0.046)	0.0678 (0.021)	0.673 (0.172)
Robbery	-5.306 (-2.858)**	-6.383 (-3.013)**	-3.971 (-2.707)**	-3.267 (-2.125)*	-5.601 (-2.478)*	-5.121 (-1.749)†
Assault	-0.629 (-0.256)	-2.842 (-1.260)	0.149 (0.067)	-1.010 (-0.602)	-0.477 (-0.216)	-0.912 (-0.222)
Property	-2.272 (-1.080)**	-3.030 (-1.752)**	-1.522 (-2.050)**	-1.579 (-2.174)**	-2.748 (-2.804)**	-3.104 (-2.406)**
Burglary	-2.549 (2.840)**	-3.337 (-3.129)**	-2.253 (-3.117)**	-1.217 (-1.508)	-3.180 (-2.841)**	-3.649 (-2.580)*
Larceny	-2.360 (-3.314)**	-3.319 (-4.092)**	-1.580 (-2.548)*	-1.643 (-2.644)*	-3.173 (-3.678)**	-3.990 (-3.751)**
Vehicle Theft	-4.148 (-2.424)**	-5.231 (-2.600)*	-2.462 (-2.272)*	-5.231 (-2.600)**	-2.462 (-2.272)*	-4.758 (-2.246)*

** significant at .01 level * significant at .05 level † significant at .10 level

effect during the month prior to the actual effective date of a gun-control law). In the *Miranda* context, while it is possible that law enforcement agencies “anticipated” the *Miranda* decision by adjusting some of their procedures, see Milner, *supra* note **Error! Bookmark not defined.**, at 128 (noting that police may have “adjusted their education and interrogation procedures in anticipation of such a decision”), it does not appear that they anticipated the most harmful features of *Miranda*, specifically the waiver and questioning cut-off rules, by complying early. See Cassell, *supra* note **Error! Bookmark not defined.**, at 494-96.

¹⁸⁹ We also ran a more complex polynomial distributed lag over a several-year period. The results were generally consistent with the results reported in Table IV.

Violent crime, property crime, larceny, and vehicle theft remain statistically significant across all assumptions, and robbery and burglary are significant across virtually all assumptions. All of these categories are statistically significant in the expected best (1966-1968) model. The only thing that fluctuates across assumptions is the value of the *MIRANDA* coefficient. Generally the coefficients are larger as the impact is extended over a longer period of time, which is consistent with the theory that *Miranda*'s harmful effects took several years to fully develop.¹⁹⁰ In short, our findings are not dependent on assumptions concerning the timing and phase-in of the *Miranda* effect.

To test the strength of the competing hypothesis of *Miranda*'s defenders, that "police officers adjusted to *Miranda* over time and that any negative impacts quickly dissipated,"¹⁹¹ we also decided to model several alternative impact structures. If *Miranda* did indeed affect clearance rates, even if only briefly, and its effects truly disappeared quickly, then that impact would be faithfully modeled in one of the following:

Gradual temporary. As a baseline basic model of the theory offered by *Miranda*'s defenders, we tested the assumption that *Miranda*'s impact would have been felt partly in 1966, completely in 1967, dissipated halfway in 1968, and disappeared in 1969; that is, *MIRANDA* takes the value 0 before 1966, 0.5 in 1966, 1 in 1967, 0.5 in 1968, and 0 in 1969 and subsequent years. This is a gradual temporary model.¹⁹²

Extended gradual temporary. This model also assumed a gradual temporary effect, but further assumed that *Miranda*'s (and *Escobedo*'s) effects were felt over a more extended period of time. Here *MIRANDA* assumed the value of 0 before 1964, 0.25 in 1964, 0.5 in 1965, 0.75 in 1966, 1 in 1967, 0.75 in 1968, 0.5 in 1969, 0.25 in 1970, and 0 in subsequent years. This is an extended gradual temporary model.

¹⁹⁰. Cf. McDowall et al., *supra* note **Error! Bookmark not defined.**, at 385-86 (observing that incorrect placement of the effective date of a legal change can result in underestimating the magnitude of the effect of the change).

¹⁹¹. Schulhofer, *supra* note **Error! Bookmark not defined.**, at 507.

¹⁹². See KENDALL & ORD, *supra* note **Error! Bookmark not defined.**, at 222.

Abrupt temporary. This model assumed that *Miranda*'s effects were immediately felt, but then faded away. Here *MIRANDA* assumes the value of 0 before 1966, 1 in 1966, 0.75 in 1967, 0.5 in 1968, 0.25 in 1969, and 0 in 1970 and following years. This is an abrupt temporary model.¹⁹³

As can be seen in Table V, these models do not fit the data well.

None of the coefficients are statistically significant at the conventional 95% confidence level, suggesting that the theory that negative impacts were only temporary and then quickly dissipated does not fit the facts.¹⁹⁴

¹⁹³. *See id.*

¹⁹⁴. *Cf.* Britt et al., *supra* note **Error! Bookmark not defined.**, at 369 (arguing that hypothesized effects of a legal change should be tested and, if the model does not fit, the hypothesis should be rejected).

TABLE V. STRUCTURES FOR THE PHASE-IN OF THE *MIRANDA* VARIABLE:
 TEMPORARY MODELS (1950-1995)
 COEFFICIENTS OF THE *MIRANDA* VARIABLE
 OLS Regressions on Clearance Rates for Cities
 (*t* statistics in parenthesis)

Crime	Gradual Temporary	Extended Gradual,	Abrupt Temporary
Violent	-2.201 (-1.411)	-2.182 (-0.951)	-1.342 (-0.870)
Murder	-1.074 (-0.730)	-0.934 (-0.436)	-0.662 (-0.462)
Rape	0.426 (0.219)	1.173 (0.418)	0.374 (0.844)
Robbery	-2.481 (-1.703) [†]	-1.374 (-0.627)	-2.367 (-0.105)
Assault	1.219 (0.999)	1.787 (0.919)	0.879 (0.679)
Property	-0.757 (-1.477)	-0.882 (-1.175)	-0.626 (-1.245)
Burglary	-0.896 (-1.183)	-1.171 (-1.064)	-1.314 (-1.841) [†]
Larceny	0.201 (0.118)	-0.304 (-0.123)	0.307 (0.186)
Vehicle Theft	-1.596 (-1.519)	-1.485 (-0.959)	-0.906 (-0.964)

** significant at .01 level

* significant at .05 level

† significant at .10 level

2. *The length of the time series.*

One caution that has been raised about the use of interrupted time series is the possibility that specification of the beginning point or ending point of the data could affect the results.¹⁹⁵ Ordinarily, one should use all available data to analyze an issue.¹⁹⁶ It can be instructive, however, to examine whether conclusions change over different time periods because, it has been argued, “[i]f the results of a time series regression analysis are a faithful representation of underlying causal processes, the values of the estimated coefficients will be independent of the specific time period chosen for the analysis.”¹⁹⁷

¹⁹⁵. See *id.* at 370-71; David Cantor & Lawrence E. Cohen, *Comparing Measures of Homicide Trends: Methodological and Substantive Differences in the Vital Statistics and Uniform Crime Report Time Series* (1933-1975), 9 SOC. SCI. RES. 121, 135 (1980); McDowall et al., *supra* note

In our equations, two time periods are plausible candidates for exclusion from the analysis. First, we could run our equations from 1960 forward, dropping the 1950s. The quality of our data improves significantly in 1960, as the FBI has reported a consistent crime rate series ever since. The series has to be estimated if we want to include pre-1960 data.¹⁹⁸ The FBI also revised some of its crime definitions in 1958, meaning that earlier data might not be strictly comparable.¹⁹⁹ Running the equations from 1960 forward also eliminates potential problems in our “national” data resulting from the admission of Hawaii and Alaska to the Union in 1959.²⁰⁰ To answer these concerns, we reran the regressions from 1960-1995. Second, Professor Schulhofer has suggested that extending the analysis into the 1980s and 1990s introduces “post-1980 data that could suppress the significance of variables that were causally important during the critical 1960-1970 time period.”²⁰¹ Why this might be so is unexplained. Nonetheless, we have considered this possibility as well, by also running the regressions from 1950-1980.

Table VI reports the results of these respecifications, using the basic impact structure, over alternative lengths of data. As can be seen, our results are insensitive to both respecifications. The *MIRANDA* variable remains statistically significant across both data sets for violent crimes, robbery, property crimes, larceny, and vehicle theft. The only difference is that the *MIRANDA* variable is not statistically significant at the standard 95% confidence level for burglary using only 1950-1980 data.

Error! Bookmark not defined., at 389-90.

¹⁹⁶. See, e.g., McDowall et al., *supra* note **Error! Bookmark not defined.**, at 389.

¹⁹⁷. William J. Bowers & Glenn L. Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 YALE L.J. 187, 197 (1975). *But see* Isaac Ehrlich, *Deterrence: Evidence and Inference*, 85 YALE L.J. 209, 214-17 (1975) (disputing this conclusion).

¹⁹⁸. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text.

¹⁹⁹. See CRIME AND ITS IMPACT, *supra* note **Error! Bookmark not defined.**, at 20 (“[F]igures prior to 1958 . . . must be viewed as neither fully comparable with nor nearly so reliable as later figures.”); UCR-1958, *supra* note **Error! Bookmark not defined.**, at 2-3 (noting recommended changes in the UCR program). For present purposes, the most significant change seems to be limiting the figures for rape to forcible rape. See UCR-1958, *supra* note **Error! Bookmark not defined.**, at 2. It has also been reported that the definition of larceny changed in 1960. See Cohen & Felson, *supra* note **Error! Bookmark not defined.**, at 601.

²⁰⁰. See note **Error! Bookmark not defined.** *infra* and accompanying text (noting this problem).

²⁰¹. Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 292.

TABLE VI. ALTERNATIVE LENGTH OF SERIES
 COEFFICIENTS OF THE *MIRANDA* VARIABLE
 OLS Regressions on Clearance Rates for Cities
 (*t* statistics in parenthesis)

Crime	1950-1995	1960-1995	1950-1980
Violent	-6.731 (-2.026)**	-7.232 (-2.682)**	-5.224 (-2.842)*
Murder	-2.078 (-1.097)	-2.644 (-1.328)	-0.973 (-0.584)
Rape	0.374 (0.148)	-1.886 (-0.766)	1.213 (0.345)
Robbery	-5.306 (-2.858)**	-7.237 (-4.410)**	-6.614 (-3.346)**
Assault	-0.629 (-0.356)	-0.672 (-0.302)	0.844 (0.355)
Property	-2.272 (-1.080)**	-1.984 (-2.010)**	-2.205 (-2.687)*
Burglary	-2.549 (-2.840)**	-1.996 (-3.056)**	-2.616 (-1.888)†
Larceny	-2.360 (-3.314)**	-2.012 (-2.297)*	-1.923 (-2.144)*
Vehicle Theft	-4.148 (-2.424)**	-5.371 (-5.425)**	-5.179 (-2.620)*

** significant at .01 level * significant at .05 level † significant at .10 level

3. *The appropriateness of a linear model.*

In a brief essay anticipating the findings of our study, Professor Schulhofer has suggested that our linear regression equations might not accurately measure the impact of an increasing crime rate on the criminal justice system. He argues that the tide of rising crime during the 1960s might be analogized to “mounting pressure [that] finally took its toll—the dam finally broke.”²⁰² To account for such possibilities, Schulhofer argues that the regression equations must therefore take into account “reserve capacities, lagged responses and the like.”²⁰³

²⁰² . *Id.*

²⁰³ . *Id.* at 293.

We are skeptical about such an explanation for falling clearance rates. There is nothing in our equations that would suggest that a linear model is inappropriate, as all of the summary statistics suggest that linear modeling provides a good fit.²⁰⁴ In contrast, a nonlinear, quantum change model is unlikely to be a good predictor of aggregate *national* crime statistics, which rest on the activities of thousands of criminal justice agencies across the country. Even if one such agency should reach some kind of “breaking point,” it is not clear why all other agencies would reach exactly the same point at the same time. Criminal justice research has often used linear models to explain relationships between such variables as crime and clearance rates, especially when modeling national data.²⁰⁵ Finally, and perhaps most important, the positing of an unspecified quantum change model of non- *Miranda*-induced changes in clearance rates is almost equivalent to adopting, as an article of faith, that *Miranda* had no effect. Testing such a claim is virtually impossible, as it requires a set of assumptions about the nature of the quantum shift that are unlikely to be susceptible to observation and empirical review. For example, Schulhofer suggests that we model a “reserve” capacity of police forces.²⁰⁶ But, based on the objectively available data, there was no such capacity. In 1965, for example, roughly 75% of all index crimes were not cleared²⁰⁷ and were presumably keeping law enforcement officers busy. What sort of “reserve” capacity should we add for 1965? Why should we assume that it would disappear in 1966, rather than 1967 or 1968 or 1969, etc.? Why don't the various impact structures we modeled account for any

²⁰⁴. One measure of fit is provided by the Durbin-Watson statistic, which is a measure of autocorrelation. In a nutshell, autocorrelation is a violation of an assumption that the error terms in the regression model will exhibit no discernable pattern. Incorrect model specification (for example, fitting a linear function to a nonlinear function) can produce autocorrelation. See DAMODAR N. GUJARATI, *BASIC ECONOMETRICS* 353-59 (2d ed. 1988) (illustrating this point). In some of our equations, the Durbin-Watson statistic was “inconclusive” on the presence of autocorrelation. However, in none of our equations did the Durbin-Watson statistic conclusively indicate autocorrelation. Furthermore, in a number of our equations, notably the robbery equations, the Durbin-Watson statistic conclusively rejected the presence of autocorrelation, suggesting that our linear model is appropriate.

²⁰⁵. See, e.g., FOX, *supra* note **Error! Bookmark not defined.**; see also Peter Passell & John B. Taylor, *The Deterrent Effect of Capital Punishment: Another View*, 67 AM. ECON. REV. 445, 448 (1977) (criticizing nonlinear criminal justice models and noting that “forms are usually chosen which are linear in the parameters to make it easier to interpret the statistical properties of the estimators”).

²⁰⁶. See Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 293.

²⁰⁷. See UCR-1965, *supra* note **Error! Bookmark not defined.**, at 18.

such “reserves”? Moving from observable phenomena (such as the number of crimes and number of officers) to unobservable phenomena (such as “reserve capacities”) promises to make the *Miranda* debate turn on one’s initial predispositions rather than on actual empirical findings. One could simply characterize our *Miranda* effect as a “quantum shift in reserve capacity effect,” but we doubt that any but the most faithful adherents to the conventional wisdom about *Miranda* will be persuaded.

4. *The issue of simultaneity.*

All of our equations have been of the reduced form model, that is, we have specified only a single causal equation. This requires the assumption that crime clearance rates were explained by various variables, but not vice versa.

It is possible, of course, that crime clearance rates might influence some of the explanatory variables. In particular, a drop in the clearance rate might cause crime rates to rise, because the lower the chance of apprehension, the greater the expected rewards to crime.²⁰⁸ Such a possibility is known as “simultaneity.” Our model of crime clearance rates, resting as it does on the reduced form model, does not capture these possible interactions. There is a considerable literature suggesting that failure to consider simultaneity can lead to problems in predicting crime rates and the deterrent effects of police.²⁰⁹

However, the issue posed here is a slightly different one: whether failure to control for possible simultaneity affects our conclusions about the *MIRANDA* variable.

²⁰⁸. See generally Gary S. Becker, *Crime and Punishment: An Economic Approach*, 78 J. POL. ECON. 169 (1968); cf. Atkins & Rubin, *supra* note **Error! Bookmark not defined.**, at 20 (finding the creation of the exclusionary rule had an effect on crime rates starting in 1961).

²⁰⁹. For good introductions to the problem, see Nagin, *supra* note **Error! Bookmark not defined.**, at 117-29, and Swimmer, *supra* note **Error! Bookmark not defined.**, at 293-303.

Our conclusions are unlikely to have been substantially affected for several reasons. First, it is possible that there is no simultaneity—that is, clearance rates might not affect crime rates. While we do not necessarily subscribe to this counterintuitive theory, it is interesting that the literature on police effects on crime is conflicting.²¹⁰ Second, even if simultaneity is an

²¹⁰ For an illustrative sample of this voluminous literature, compare, for example, MORGAN O. REYNOLDS, CRIME AND PUNISHMENT IN AMERICA 23 (National Center for Policy Analysis, Policy Report No. 193, June 1995) (finding a deterrent effect); Cloninger & Sartorius, *supra* note **Error! Bookmark not defined.**, at 398-99 (providing step regression equations finding that rates for some crimes were responsive to clearance rates); Decker & Kohfeld, *supra* note **Error! Bookmark not defined.**, at 15-18 (finding increased clearance rates deter robbery, but not burglary or larceny); Ehrlich & Brower, *supra* note **Error! Bookmark not defined.**, at 104-06 (reporting simultaneous models with results generally consistent with a deterrent effect); Steven D. Levitt, *Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?*, 35 ECON. INQUIRY (forthcoming 1998) (finding a general deterrent effect from increased arrest rates); Lott & Mustard, *supra* note **Error! Bookmark not defined.**, at 57 (finding that higher conviction rates resulted in lower crime rates); Stephen J. Markman & Paul G. Cassell, *Protecting the Innocent: A Response to the Bedau-Radelet Study*, 41 STAN. L. REV. 121, 154-56 (1988) (collecting evidence of a deterrent effect from capital punishment); Wilson & Boland, *supra* note **Error! Bookmark not defined.**, at 367-68, 375-76 (collecting evidence that increased police resources reduce crime and reporting result that aggressive police patrols reduce robbery rates); Ann Dryden Witte, *Estimating the Economic Model of Crime with Individual Data*, 94 Q.J. ECON. 57, 74-78 (1980) (finding a deterrent effect of higher conviction rates for certain crimes in individual data); and Kenneth I. Wolpin, *A Time Series-Cross Section Analysis of International Variation in Crime and Punishment*, 62 REV. ECON. & STAT. 417, 421 (1980) (finding increased clearance rates associated with decreased level of robbery), with, for example, DAVID H. BAYLEY, POLICE FOR THE FUTURE 7 (1994) (“In all probability, the relationship does not run from clearances to crime but from crime to clearances.”); David F. Greenberg & Ronald C. Kessler, *The Effect of Arrests on Crime: A Multivariate Panel Analysis*, 60 SOC. FORCES 771, 784 (1982) (using multiwave panel models to deal with simultaneity issues and finding “no consistent evidence for the proposition that higher arrest clearance rates result in substantially lower index crime rates”); Greenberg et al., *supra* note **Error! Bookmark not defined.**, at 849 (using panel models to deal with simultaneity issues and concluding that “[a]ggregate criminal activity for the F.B.I. index offenses is not substantially influenced by marginal variations in arrest clearance rates within the range found in our sample of American cities”); Herbert Jacob & Michael J. Rich, *The Effects of the Police on Crime: A Second Look*, 15 L. & SOC’Y. REV. 109, 110, 120 (1980-1981) (collecting evidence that police effect on crime is mixed and reporting the result that in some cities police activities increase robbery rates); Thomas F. Pogue, *Offender Expectations and Identification of Crime Supply Functions*, 10 EVALUATION REV. 455, 478-79 (1986) (finding little effect of arrest rates on crime); and Pogue, *supra* note **Error! Bookmark not defined.**, at 40 (finding clearance rates are a minor source of cross-sectional variation in crime rates). Also, see generally John J. Donohue III & Peter Siegelman, *Is the United States at the Optimal Rate of Crime?* (ABF Working Paper Preliminary Draft, Feb. 13,

issue, results consistent with those reported here might still be found. Simultaneity can bias results not only upward, but also downward, or leave them unchanged. We do not think that simultaneity would explain away the kinds of dramatic shifts in crime clearance rates that we are studying here. The only sophisticated simultaneous equations study of the effect of the Supreme Court on crime rates reaches results similar to ours.²¹¹ In addition, our results are insensitive to the variables that we include or exclude in the equations.²¹² Third, we do not believe that simultaneity threatens our general findings. Reduced form equations can still generate policy-relevant conclusions even if unable to resolve specific relationships among variables.²¹³

We acknowledge that, in theory, simultaneous equations for our crime clearance rate model would be the most desirable way to proceed. In practice, however, the specification of such models is often highly dependent on underlying assumptions. Indeed, some commentators have suggested that it is simply impossible to effectively model simultaneous effects.²¹⁴ This is a quagmire that we hope, at least for present purposes, to avoid.²¹⁵

5. *Extreme bounds analysis.*

1995) (reviewing the literature on the police effect on crime and concluding that the literature “does not speak with an authoritative voice”).

²¹¹. See Ehrlich & Brower, *supra* note **Error! Bookmark not defined.**, at 104-06.

²¹². See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *infra* and accompanying text.

²¹³. Cf. Philip J. Cook, *The Clearance Rate As a Measure of Criminal Justice System Effectiveness*, in 2 CRIMINOLOGY REVIEW YEARBOOK 669, 676 (Egon Bittner & Sheldon L. Messinger eds., 1980) (stating, in a deterrence study, that even though the reduced form equation could not distinguish between two hypotheses, it is still relevant for policy analysis); Sullivan, *supra* note **Error! Bookmark not defined.**, at 171 (suggesting that certain models “can be used to predict even if they cannot be used to establish the causal relationship”).

²¹⁴. See, e.g., Cook, *supra* note **Error! Bookmark not defined.**, at 676; Nagin, *supra* note **Error! Bookmark not defined.**, at 129 (hoping that information for dealing with simultaneity issues in deterrence will develop).

²¹⁵. With the capable assistance of Katarzyna Celinska, we have run some preliminary simultaneous equations. For example, we have created a three-stage model in which, in addition to the clearance rates equation in this paper, equations also exist for crime rates affected by clearance rates and for police personnel levels affected by lagged crime rates. All the models we have tried so far continue to show the same strong *Miranda* effect found in our reduced form model. We hope to be able to publish a paper on this issue in the future.

While OLS regression is a widely used statistical technique to quantify the relationship between sets of variables, regression estimates can hinge on model specification (that is, which variables are included or excluded in the model). Model specification can create the serious practical problem of “fragility” if results depend on the inclusion or exclusion of certain variables in an equation.²¹⁶ For example, our *Miranda* result would be fragile if it disappeared when certain other explanatory variables were dropped from our equation.

Standard statistical significance tests cannot detect fragility. Yet conventional reporting of regression results often presents only the “best-fitting” results or those results that conform to a researcher’s prior beliefs. Since the methodology for searching for the model is often not published along with the final model, the reader may think the reported statistics represent a straightforward estimation process. Selective reporting of conventional inferential statistics based on these types of searches may be misleading, since the sampling properties of the resultant estimators—and thus those of the reported statistics—are not well-known.

²¹⁶. For more technical descriptions of the problem in various contexts, see Richard Fowles & Peter D. Loeb, *Speeding, Coordination, and the 55-MPH Limit: Comment*, 79 AM. ECON. REV. 916, 920 (1989); Richard Fowles & Mary Merva, *Wage Inequality and Criminal Activity: An Extreme Bounds Analysis for the United States, 1975-1990*, 34 CRIMINOLOGY 163, 168 (1996); Edward E. Leamer, *Let's Take the Con Out of Econometrics*, 73 AM. ECON. REV. 31, 41-42 (1983).

A theoretically straightforward solution to these problems is to examine multiple combinations of the variables and see if the results are affected. The practical econometric technique that allows for the consideration of an infinite array of models is known as extreme bounds analysis (“EBA”).²¹⁷ EBA computes the maximum and minimum coefficients that could be obtained using maximum likelihood estimation over all possible linear combinations of explanatory variables. The highest and lowest estimates are called the upper and lower bounds. If a negative or positive coefficient could be generated from a regression depending on the variables included, the bounds straddle zero; these bounds are known as “fragile” or “loose” bounds. If all possible combinations of the variables yield an unambiguous sign on the coefficient of interest, the upper and lower bounds do not straddle zero; these bounds are known as “nonfragile” or “tight” bounds. An illustration may clarify this. We have reported various equations showing that the *MIRANDA* variable has a negative sign, suggesting that *Miranda* harmed clearance rates. If the inclusion of certain variables is necessary to generate that negative sign, then the effect would be fragile. On the other hand, if all models generated a consistent negative effect, then the result would be nonfragile.

To test the extreme bounds of the *Miranda* result here, we divided the explanatory variables into two subsets: focus and doubtful.²¹⁸ The focus set was comprised of criminologically important variables that are always considered. These variables include *MIRANDA*, *CRIME RATE*, *POLICE PERSONNEL*, *POLICE DOLLARS (REAL)*, *PERSONNEL CAPACITY*, *DOLLAR CAPACITY (REAL)*, and a constant term. The doubtful set was comprised of variables that may or may not be included in every regression: labor force participation, unemployment, per capital income (real), births to unmarried mothers, urbanization, juvenile population, crime in small cities, and trend over time.²¹⁹

Table VII presents EBA results for the focus coefficients for each of the clearance rate dependent variables, using the standard July 1966 start for the *MIRANDA* variable.

²¹⁷. For technical explanations and examples of EBA, see Richard Fowles, *Micro EBA: Extreme Bounds Analysis on Microcomputers*, 42 AM. STATISTICIAN 274 (1985); Edward E. Leamer, *Sets of Posterior Means with Bounded Variance Priors*, 50 ECONOMETRICA 725 (1982).

²¹⁸. For another example of this approach, see Fowles & Merva, *supra* note **Error! Bookmark not defined.**, at 168.

²¹⁹. We included in our “doubtful” set all variables for which we had data over the relevant time period.

TABLE VII. EXTREMEBOUNDS ANALYSIS FOR FOCUS VARIABLE
Variable Parameters from OLS Regressions on Clearance Rates for Cities
(1950-1995)

	Violent	Murder	Rape	Robbery	Assault	Property	Burglary	Larceny	Vehicle
MIRANDA	-3.763	5.293	2.394	-1.663	3.885	-0.767	0.944 [†]	-0.085	-1.755
CRIME RATE	0.387	0.323	0.419	0.689	0.525	0.221	0.555	0.240	0.356
POLICE	0.302	1.100	0.471	0.148	0.538	0.111	0.254	0.110	0.307
POLICE	0.346	18.192	3.239	12.021	9.472	5.035	14.825	5.060	5.000
PERSONNEL	22.674	22.840	22.062	12.707	21.048	4.770	0.082	5.286	14.204
POLICE	0.577	0.838	0.744	0.279	0.578	0.171	0.342	0.216	0.355
DOLLARS	0.465	1.065	0.447	0.841	0.845	0.272	0.740	0.252	0.518
PERSONNEL	5.567	6.889	5.536	3.882	6.458	2.140	5.006	2.093	2.788
CAPACITY	2.105	7.282	2.222	4.455	4.126	1.166	2.055	1.205	2.718
DOLLAR	13.077	41.546	33.230	42.666	37.387	13.077	31.247	13.215	26.732

[†] See note **Error! Bookmark not defined.** *infra* (discussing this result).

The results demonstrate that the *Miranda* effect is robust with respect to model specification. As shown in the dark shaded boxes, the bounds do not straddle zero for violent crimes, robbery, property crimes, larceny, and vehicle theft. In other words, regardless of the model chosen, that is, the variables included in these equations, *MIRANDA* always had a negative effect on clearance rates. For burglary, too, the EBA analysis suggested that a negative effect was more likely.²²⁰ For all of the other criminal justice system variables, the results were fragile for every crime category—that is, one could generate either a positive or negative effect by manipulating the variables included in the equation. That the *MIRANDA* variable alone was able to produce nonfragile results suggests that our finding is not a mere artifact of our models. Moreover, the nonfragile results in are surprising, since EBA is designed to expose the fundamental fragility of regression modeling in the presence of collinear data.²²¹ Evidence of a *Miranda* effect is accordingly not only statistically significant but also robust with respect to model specification.

²²⁰. Here we need to make a technical argument. While the extreme bounds on burglary do straddle zero (ranging from +0.999 to -4.413), the bounds that are “favored” by the data do not. In Bayesian estimation, the posterior mean is pulled between the prior location (zero) and the maximum likelihood estimate (the data-favored location). Dogmatic priors are characterized by high prior precision that the prior location is correct. For example, by dropping a variable from a regression equation, the researcher is expressing an opinion that the associated parameter is zero with

F. *Summary of the Range of the Miranda Effect*

Having considered various models for the *Miranda* effect, it might be useful to set out a short summary of our findings and the range of the possible effect. Table VIII displays the pertinent information.

TABLE VIII. SUMMARY OF THE RANGE AND SIZE OF THE EFFECT OF THE
MIRANDA VARIABLE
UNIFORM CRIME REPORT DATA

Crime	1995 Clearance Rate	Range of <i>Miranda</i> Effect	Percentage Increase Without <i>Miranda</i>	1995 Additional Cleared Crimes Without <i>Miranda</i>
Violent	43.5%	3.7-8.9%	8.5-20.4%	56,000-136,000
Murder	63.2%	0%*	0%	0
Rape	50.8%	0%*	0%	0
Robbery	24.2%	1.6-7.2%	6.6-29.7%	8,000-36,000
Assault	54.4%	0%*	0%	0
Property	17.7%	0.7-2.9%	3.9-16.3%	72,000-299,000
Burglary	12.8%	0.8-3.7% [†]	6.2-28.9%	17,000-82,000
Larceny	20.1%	0.1-2.4%	0.4-11.9%	6,000-163,000
Vehicle	13.2%	1.7-6.0%	12.8-45.4%	23,000-78,000

* No robust, statistically significant *Miranda* effect found.

[†] For reasons treating the burglary finding as robust, see note **Error! Bookmark not defined.**, *supra*.

probability one. Diffuse priors place zero precision on the prior mean so the data are completely favored. With completely diffuse priors, the posterior mean and the maximum likelihood estimate are the same. Posterior bounds for burglary associated with low prior precision range from -0.875 to -3.752, a nonfragile result.

²²¹. See Fowles & Merva, *supra* note **Error! Bookmark not defined.**, at 167-68.

In the first column, we set out the 1995 clearance rate for the various crime categories—for example, a 24.2% clearance rate for robbery.²²² Then we set out the range of the *Miranda* effect found in the extreme bounds analysis just discussed. For example, depending on the model specification, *Miranda* reduced robbery clearances somewhere from 1.6-7.2%.²²³ To provide some context to these figures, we next set out the percentage increase in clearances without the *Miranda* effect. For example, given that only 24.2% of robberies were cleared in 1995, increasing the clearance rate by 1.6-7.2% would mean that 10.1-22.2% more robberies would be cleared.²²⁴ Because there may be some interest in the absolute number of crimes affected,²²⁵ in the last column we estimate how many more crimes would have been cleared in 1995 in the absence of the *Miranda* effect.²²⁶ Our equations suggest, for instance, that between 8000 and 36,000 more robberies would have been solved in 1995 in the absence of the *Miranda* effect. It is worth emphasizing again that these estimates are quite conservative because they capture only *Miranda*'s impact on crime clearances, missing some of the effects on prosecutions and convictions at later points in the criminal justice system.²²⁷ They are also conservative because they rely on regression analysis of national crime data, which can only detect significant and simultaneous changes in aggregate police performance across the country.

²²². We caution against drawing firm conclusions from the aggregate category of “violent” crimes for the reasons noted at note **Error! Bookmark not defined.** *supra* and accompanying text.

²²³. See Table VI.

²²⁴. $24.2\% + (1.7-7.2\%)/24.2\%$.

²²⁵. See note **Error! Bookmark not defined.** *infra* and accompanying text (noting the Supreme Court's interest in absolute numbers of crimes affected by the exclusionary rule).

²²⁶. This number is derived by multiplying the range of the *Miranda* effect by the number of offenses known in 1995. We have used offenses known to all law enforcement agencies rather than offenses known to city law enforcement agencies to provide a nationwide estimate. See note **Error! Bookmark not defined.** *infra* and accompanying text (noting the general parallel between nationwide and city data).

²²⁷. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

IV. *MIRANDA* AS THE CAUSE OF THE CLEARANCE RATE DECLINE

So far we have presented evidence that, controlling for several major criminal justice and socioeconomic variables, crime clearance rates for robbery, larceny, vehicle theft, and burglary fell significantly after *Miranda*. The question immediately arises whether *Miranda* caused those drops. Regression analysis can never establish causality. Instead, the causal conclusion can only come from combining the information provided by the regression equations with theory and other information to determine whether a causal interpretation is a reasonable one.²²⁸ The potential obstacle to concluding that *Miranda* was the cause of the 1966-1968 drop in clearance rates is the problem of alternate causality—that is, perhaps some other change in society, unaccounted for in our regression equations, was responsible for the reduction in clearance rates.

In interrupted time series analysis, especially without control groups, it is important to consider such alternative causes.²²⁹ We have made an aggressive effort to identify possible “omitted” variables that might influence our conclusions²³⁰ and discuss the various possibilities of such an omission in this section.²³¹ Before turning to these other, thus-far-unconsidered candidates for the “X factor,” we should first discuss briefly what we are looking for. To be a strong candidate for the X factor, the variable must change sharply in the critical 1966-1968 period when clearance rates fell—but not elsewhere. For example, Figure 4 shows that robbery clearances plunged from 37.6% in 1965 to 26.9% in 1968 and have remained relatively stable since. Our regression equations suggest that about half of this drop is explained by a structural shift at the time of *Miranda*, controlling for such criminal justice variables as crime rates and police personnel and such socioeconomic variables as unemployment and income. Now that we have used regression analysis to rule out the influences of those factors, we next look to the relevant theoretical, anecdotal, and logical explanations that could reveal what was the cause of the shift in the clearance rate: *Miranda* or something else.

²²⁸. See EDWARD R. TUFTE, DATA ANALYSIS FOR POLITICS AND POLICY 139 (1974) (“Whether or not there really is a causal relationship . . . depends on having a theory, consistent with the data, that links the variables.”).

²²⁹. See Britt et al., *supra* note **Error! Bookmark not defined.**, at 362-63 (expounding this point); cf. McDowall et al., *supra* note **Error! Bookmark not defined.**, at 382-84 (explaining that controls are useful but not required for time series inferences).

²³⁰. One of the present authors (Cassell) has presented a preliminary version of this paper to and sought comments from audiences at the ABA’s Annual National Conference, *see* *Miranda*

A. Law Enforcement Assessment of the Drop

In assessing plausible causal factors for the decline in clearance rates, it is useful to first examine the FBI's contemporaneous explanations. The FBI would have ample incentive to provide explanations for apparently declining police performance. During the critical 1966-1968 period, the *UCR* authors listed the following explanatory causes of falling clearance rates: "court decisions which have resulted in restrictions on police investigative and enforcement practices, sharply increasing police workloads not limited to crime increases, an almost static ratio of police to population not commensurate with the sharp rise in crime, and constant increasing criminal mobility."²³² The regression equation controls for two of the factors cited: the increase in police workloads and the static ratio of police strength. The last factor cited—increased mobility of those committing crimes—may have some long-term explanatory power,²³³ but seems an unlikely explanation for relatively sudden shifts in crime clearance rates.²³⁴ Increasing mobility could only affect clearances over the long haul. That leaves the first factor—"court decisions which have resulted in restrictions on police investigative and enforcement practices"—as the logical candidate for explaining the post-*Miranda* drop in clearance rates.

Decision's Legitimacy, Effects on Law Enforcement Debated at ABA Meeting, 59 CRIM. L. REP. (BNA) 1465, 1465-66 (1996), at Chicago-Kent, Loyola, Northwestern, San Diego, Stanford, UCLA, and Utah law schools, at the Goldwater Institute, the Heritage Foundation, and elsewhere. In addition, he has sought suggestions in widely available fora. See Cassell, *supra* note **Error! Bookmark not defined.**, at 1091 n.33 (seeking suggestions as to "other variables that should be included" in the regression analysis); Paul Cassell, *Harmful Effects of Miranda on Law Enforcement* (June 19, 1996) <crimprof@chicagokent.kentlaw.edu> (reciting regressions and asking, "Am I overlooking anything?"). We remain open, of course, to receiving other suggestions.

²³¹. See Ross et al., *supra* note **Error! Bookmark not defined.**, at 504 ("[W]hen a set of hypotheses cannot be ruled out mechanically through design, the researcher bears the burden of seeking out the reasonable hypotheses included therein and ruling them out or allowing for them individually.").

²³². UCR-1967, *supra* note **Error! Bookmark not defined.**, at 30.

²³³. Cf. Sullivan, *supra* note **Error! Bookmark not defined.**, at 161 (finding that travel time to work was negatively associated with clearance rates for auto theft).

²³⁴. See 2 BUREAU OF THE CENSUS, U.S. DEP'T OF COMMERCE, HISTORICAL STATISTICS OF THE UNITED STATES: COLONIAL TIMES TO 1970, at 717 ser.Q:175-86 (1975) (reporting that the percentage of families owning automobiles increased slowly from 1960-1970).

Confirming the FBI's assessment were the assessments from those who would know firsthand: law enforcement officers who questioned suspects both before and after *Miranda*'s constraints were imposed. Perhaps the best interviews of officers on the streets were done by Otis Stephens and his colleagues in Knoxville, Tennessee, and Macon, Georgia in 1969 and 1970.²³⁵

Virtually all of the officers surveyed believed that Supreme Court decisions had adversely affected their work, and most attributed this negative influence first and foremost to *Miranda*.²³⁶ Similarly, in New Haven, Connecticut, Yale students interviewed most of the detectives involved in the interrogations they observed during the summer of 1966 and an additional twenty-five detectives.²³⁷ They reported that "the detectives unanimously believe [*Miranda*] will unjustifiably [help the suspect]." ²³⁸ They also reported that "[t]he detectives continually told us that the decision would hurt their clearance rate and they would therefore look inefficient." ²³⁹ Also, law student Gary L. Wolfstone sent letters in 1970 to police chiefs and prosecutors in each state and the District of Columbia. Most agreed that *Miranda* at least raised obstacles to law enforcement.²⁴⁰ And, in Seaside City, James Witt interviewed forty-three police detectives before 1973.²⁴¹ Witt reported that the detectives "were in almost complete agreement over the effect that the *Miranda* warnings were having on the outputs of formal interrogation. Most believed that they were getting many fewer confessions, admissions and

²³⁵. See Otis H. Stephens, Jr., Robert L. Flanders & J. Lewis Cannon, *Law Enforcement and the Supreme Court: Police Perceptions of the Miranda Requirements*, 39 TENN. L. REV. 407 (1972) [hereinafter Stephens et al., *Police Perceptions*]; see also STEPHENS, *supra* note **Error! Bookmark not defined.**, at 179-99 (recounting the survey results first published in *Police Perceptions*, *supra*).

²³⁶. See Stephens et al., *Police Perceptions*, *supra* note **Error! Bookmark not defined.**, at 420 tbl.IV (finding 92% believed the Court decisions created negative effects, 58% attributing this first and foremost to *Miranda*). The percentage fingering *Miranda* as responsible might have been higher but for a legal memorandum on legal restrictions governing search and seizure that circulated shortly before some of the interviews. See *id.* at 421. Seventy-four percent said that advice of defendants' rights had an adverse effect on investigations. See *id.* at 424 tbl.VIII. In individual interviews, the officers surveyed generally gave negative assessments of *Miranda*. See *id.* at 426-29. In light of these findings, Stephens' conclusion that his survey showed little impact from *Miranda*, see *id.* at 430-31, is hard to understand.

²³⁷. See Wald et al., *supra* note **Error! Bookmark not defined.**, at 1528.

²³⁸. *Id.* at 1611.

²³⁹. *Id.* at 1612 n.265.

²⁴⁰. See Gary L. Wolfstone, *Miranda—A Survey of Its Impact*, 7 PROSECUTOR 26, 27 (1971).

²⁴¹. Witt's article was published in 1973. See Witt, *supra* note **Error! Bookmark not defined.**, at 334. He appears to have begun collecting his data sometime after 1968.

statements.”²⁴² Witt also found the detectives to be “quick to refer to a decline in their clearance rate when discussing problems emanating from the *Miranda* decision.”²⁴³

Concerns about *Miranda* continue to be expressed by law enforcement.²⁴⁴

As one experienced detective recently explained, “Most police are not really crazy about *Miranda*. It always gives the criminal that extra incentive not to say anything.”²⁴⁵ But the more telling evidence is what the police thought at the time *Miranda* was handed down, when they had experience both with and without the new rules. Those firsthand, contemporaneous reports are strong evidence that *Miranda* was the cause of the clearance rate drop.

B. Other Supreme Court Decisions

Although police contemporaneously identified *Miranda* as a major obstacle, defenders of *Miranda* might argue that it is impossible to single out *Miranda* as the most harmful decision among the Warren Court’s various rulings.²⁴⁶ This argument suggests that the *MIRANDA* variable is more properly denominated a “Warren Court” variable. Even if this assertion were true, our findings would still be of some importance, as legal academics have generally denied that the Warren Court decisions impeded law enforcement.²⁴⁷

Nonetheless, there are strong reasons for believing that *Miranda* was the Warren Court decision primarily—although perhaps not exclusively—responsible for declining clearance rates.

²⁴². *Id.* at 325.

²⁴³. *Id.* at 330.

²⁴⁴. See Cassell, *supra* note **Error! Bookmark not defined.**, at 1108-10 (discussing 1987 and 1988 studies on police views of *Miranda*); see also Stephen J. Schulhofer, *Confessions and the Court*, 79 MICH. L. REV. 865, 873 (1981) (“[L]aw enforcement authorities continue to press vigorously for the overruling of *Miranda* . . .”). But see Peter Arenella, *Miranda Stories*, 20 HARV. J.L. & PUB. POL’Y 375, 381 (1996) (“[P]olice fighting in the trenches do not seem eager to abandon *Miranda*[.]”); Schulhofer, *supra* note **Error! Bookmark not defined.**, at 507 (claiming that testimony of police officers that *Miranda* did not have any long-term negative impact is “widespread and consistent” since the mid-1970s).

²⁴⁵. Marco Leavitt, *Law Professor Says It Is Time to Throw Off Shackles of Miranda Ruling*, OGDEN STANDARD EXAMINER, June 12, 1996, at 1 (quoting Salt Lake City Police Detective Mike Welch).

²⁴⁶. See Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 369-70 (suggesting that other Warren Court rulings should be taken into account).

²⁴⁷. See, e.g., Kamisar, *supra* note **Error! Bookmark not defined.**, at 245-57 (denying that the Warren Court decisions were responsible for sinking clearance rates in the 1960s). But see

Other Court decisions from the same time seem unlikely to have had as much effect on clearance rates. Ehrlich and Brower cataloged the “significant” Warren Court decisions that possibly affected law enforcement.²⁴⁸ In the critical 1966-1968 period, they identify, in addition to *Miranda*, two other Court decisions. The first, *United States v. Wade*,²⁴⁹ is one of several “lineup” cases decided by the Court in 1967 that suggested that the right to counsel extended to such proceedings.²⁵⁰ But lineups are probably implicated in a small percentage of cases overall, certainly a much smaller percentage than interrogation. Moreover, the Court cut back on the applicability of the doctrine in 1972,²⁵¹ with the result that it “is now largely ineffectual.”²⁵² Therefore, if the lineup decisions were responsible for the drop in clearance rates, those rates should have later rebounded. Such a rebound does not exist.²⁵³ The other significant case identified by Ehrlich and Brower is *United States v. Katz*,²⁵⁴ which involved the unusual investigative technique of placing an electronic listening device on a telephone booth used frequently by a suspect. Although the decision is doctrinally important because of its famous suggestion that the Fourth Amendment “protects people, not places,”²⁵⁵ its effect on day-to-day police operations was probably relatively small; the Court specifically concluded that the law enforcement agents could have done exactly the same thing, provided they obtained a warrant first.²⁵⁶

REYNOLDS, *supra* note **Error! Bookmark not defined.**, at 8 (suggesting that “punishment philosophy softened so much” during the Warren Court years that crime control became more difficult). See generally Robert Weisberg, *Supreme Court Review Foreword: Criminal Procedure Doctrine: Some Versions of the Skeptical*, 76 J. CRIM. L. & CRIMINOLOGY 832 (1985) (discussing skepticism about whether Supreme Court decisions significantly affect the real world of criminal justice).

²⁴⁸. See Ehrlich & Brower, *supra* note **Error! Bookmark not defined.**, at 103 tbl.2.

²⁴⁹. 388 U.S. 218 (1967).

²⁵⁰. See *Stovall v. Denno*, 388 U.S. 293, 296 (1967); *Gilbert v. California*, 388 U.S. 263, 272 (1967); *Wade*, 388 U.S. at 235.

²⁵¹. See *Kirby v. Illinois*, 406 U.S. 682, 690-91 (1972) (refusing to extend counsel requirements to lineup cases where “a person has not been formally charged with a criminal offense”).

²⁵². JOSHUA DRESSLER, UNDERSTANDING CRIMINAL PROCEDURE § 166[B][2], at 352 (1991).

²⁵³. See Figures 1-5 (finding no noticeable rise in clearance rates in 1973 and 1974).

²⁵⁴. 389 U.S. 347 (1967).

²⁵⁵. *Id.* at 351.

²⁵⁶. See *id.* at 354.

Other potentially important cases during the 1966-1968 time period did not restrict police investigations, but rather court adjudicatory processes—which begin only *after* a crime has been cleared. For example, *Hoffa v. United States*²⁵⁷ dealt with the right to counsel before an indictment.²⁵⁸ Similarly, *In re Gault*²⁵⁹ extended various due process procedures to juvenile court trials.²⁶⁰ *Chapman v. California*²⁶¹ set new rules for appellate courts reviewing whether the denial of certain constitutional rights during trial is “harmless error.”²⁶² None of these otherwise important decisions could have had much effect on police clearance rates.

It is also conceivable that earlier decisions affected clearance rates during the 1966-1968 period.²⁶³ Professor Schulhofer has cited the Court’s decisions in *Mapp v. Ohio*²⁶⁴ and *Gideon v. Wainwright*²⁶⁵ as possible competing impediments to law enforcement.²⁶⁶ But the timing of these rulings makes them implausible candidates to explain away the post- *Miranda* clearance rate drop. *Mapp* was decided in 1961 and *Gideon* in 1963. It seems unlikely that they could explain, for example, the sudden changes in robbery clearance rates that appeared in 1966-1968, but not earlier.²⁶⁷ *Mapp*’s main effects, if any,²⁶⁸ were probably felt in the early 1960s.²⁶⁹ It is hard to understand why *Mapp* would cause clearance rates to begin falling at an accelerated pace some five to seven years after the decision. Also, *Gideon* is a particularly poor candidate to influence clearance rates because it dealt not with police investigations but with later court adjudications.

²⁵⁷. 385 U.S. 293 (1966).

²⁵⁸. *See id.* at 309-10.

²⁵⁹. 387 U.S. 1 (1967).

²⁶⁰. *See id.* at 58.

²⁶¹. 386 U.S. 18 (1967).

²⁶². *Id.* at 24.

²⁶³. *But cf.* Kamisar, *supra* note **Error! Bookmark not defined.**, at 242 n.5 (concluding that the Warren Court decisions “caused relatively little furor until applied to the ‘police practice’ phases of the criminal process in the late 1960’s”).

²⁶⁴. 367 U.S. 643, 655-56 (1961) (excluding evidence in a state criminal trial that was the result of an illegal search).

²⁶⁵. 372 U.S. 335, 343-45 (1963) (establishing a right to counsel for indigent defendants).

²⁶⁶. *See* Schulhofer, *supra* note **Error! Bookmark not defined.**, at 512.

²⁶⁷. Note that robbery clearance rates changed little from 1962-1965, even rising slightly from 1964-1965. *See* Figure 4.

²⁶⁸. *See* James D. Cameron & Richard Lustiger, *The Exclusionary Rule: A Cost-Benefit Analysis*, 101 F.R.D. 109, 128 (1984) (stating that the exclusionary rule “does not deter police

It is also possible that the Court's decisions on vagrancy and related issues might have influenced clearance rates. Professor William Stuntz has explained how police once had rather substantial authority to stop and arrest persons on loitering, vagrancy, and related grounds,²⁷⁰ authority that is now restricted.²⁷¹ Although this declining authority to arrest might result in declining opportunities for interrogation and hence confessions, the sharp 1966-1968 clearance rate drop probably did not stem from these restrictions—the timing is off. The leading case striking down vagrancy laws is *Papachristou v. City of Jacksonville*,²⁷² decided in 1972. Also, arrest rates for vagrancy, “suspicion,” disorderly conduct, and drunkenness declined over a much longer time period, from well before *Miranda* to well after, without sharp changes in the late 1960s.²⁷³ Finally, restrictions on police arrests for

misconduct”); Dallin H. Oaks, *Studying the Exclusionary Rule in Search and Seizure*, 37 U. CHI. L. REV. 665, 755 (1970) (arguing that the “exclusionary rule is a failure” in terms of “detering illegal searches and seizures by the police”); see also Paul G. Cassell, *The Mysterious Creation of Search and Seizure Exclusionary Rules Under State Constitutions: The Utah Example*, 1993 UTAH L. REV. 751, 842-46 (collecting evidence that state exclusionary rules are unlikely to have an incremental deterrent effect beyond the federal rules). The Court's ruling in *United States v. Leon*, 468 U.S. 897 (1984) (establishing a good faith exception to the warrant requirement), appears to have “practical day-to-day effects” that are “minimal.” CRAIG D. UCHIDA, TIMOTHY S. BYNUM, DENNIS ROGAN & DONNA M. MURASKY, THE EFFECTS OF *UNITED STATES V. LEON* ON POLICE SEARCH WARRANT POLICIES AND PRACTICES 39 (1986).

²⁶⁹. See Stuart S. Nagel, *Testing the Effects of Excluding Illegally Seized Evidence*, 1965 WIS. L. REV. 283, 285-88 (finding changes in police search and seizure practices due to the exclusionary rule from 1960-1963); Atkins & Rubin, *supra* note **Error! Bookmark not defined.**, at 10 (finding *Mapp* had an effect on crime rates starting in 1961); cf. Bradley C. Canon, *Is the Exclusionary Rule in Failing Health? Some New Data and a Plea Against a Precipitous Conclusion*, 62 KY. L.J. 681, 711-16 (1974) (arguing that the Supreme Court's refinement of the exclusionary rule in its 1969 decision *Chimel v. California*, 395 U.S. 752, produced an increase in use of search warrants from 1968-1973).

²⁷⁰. See William J. Stuntz, *Implicit Bargains, Government Power, and the Fourth Amendment*, 44 STAN. L. REV. 553, 559 (1992) (stating that, prior to the late 1960s, “police could, and did, keep public areas ‘clean’ by stopping or arresting whomever they wished”); William J. Stuntz, *Privacy's Problem and the Law of Criminal Procedure*, 93 MICH. L. REV. 1016, 1076 (1995) (noting that pre-1960s “vagrancy and loitering laws applied to almost any public behavior”).

²⁷¹. See Robert C. Ellickson, *Controlling Chronic Misconduct in City Spaces: Of Panhandlers, Skid Rows, and Public-Space Zoning*, 105 YALE L.J. 1165, 1209-14 (1996) (detailing court decisions in the 1960s and 1970s that struck down regulations permitting arrest for minor forms of public disobedience).

²⁷². 405 U.S. 156, 171 (1972).

²⁷³. Arrest rates per 100,000 inhabitants from 1960-1975 were:

YEAR	VAGRANCY	SUSPICION	DISORDERLY	DRUNKENNESS
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vagrancy would only indirectly affect clearance for index crimes such as robbery.

C. *Sundry Other Possible Causes*

Apart from judicial decisions, there remain several other conceivable causes for the 1966-1968 clearance rate declines. But none of them appears to be nearly so strong a candidate as *Miranda*.

One possible factor is changing patterns in illegal drug use. If drug use increased dramatically around the time of *Miranda*, and if this led to a significant increase in crimes, and if drug-related crimes are harder to clear, then perhaps this phenomenon could have caused the clearance rate drops. All of these links are questionable. Here again, the timing does not quite fit. Use of illegal drugs appears to have increased from the early 1960s through the 1970s and 1980s.²⁷⁴ While data on illegal drug usage are notoriously unreliable, one possible measure is provided by the arrest rates for narcotics violations. The data show sharply increasing numbers of arrests from 1965 (the first year for which comparable data are available) to 1974, then a leveling off, then another sharp rise from 1980-1989.²⁷⁵ The available data thus do not suggest a unique, sharp 1966-1968 change in drug usage. Even if there had been a sharp surge in drug usage during this limited period, the

1960	140.8	125.3	437.1	1298.2
1965	89.8	56.9	425.2	1144.7
1966	75.3	64	376.9	1076.6
1967	73.2	65.6	377.2	1040.1
1968	68.2	61.9	408.2	974.5
1969	73.9	61.4	398.8	987.5
1970	66.7	46.3	388.9	997.8
1975	22.1	16.2	252.0	666.2

See UCR-1960, *supra* note **Error! Bookmark not defined.**, at 90 tbl.15 (tables for arrest rates by population groups); UCR-1965, at 108 tbl.18 (same); UCR-1966, at 110 tbl.22 (same); UCR-1967, at 116 tbl.23 (same); UCR-1968, at 110 tbl.22 (same); UCR-1969, at 108 tbl.23 (same); UCR-1970, at 120 tbl.23 (same); UCR-1975, at 180 tbl.29 (same).

²⁷⁴. See Steven B. Duke & Albert C. Gross, *America's Longest War: Rethinking Our Tragic Crusade Against Drugs* 100-02 (1993) (identifying 1964-1978 as "the Age of Aquarius," when drug use expanded within several cultural subgroups, and 1978 to the present as "the Age of Narco Glitz," when commerce in cocaine and crack has posed a threat to the social order).

²⁷⁵. See BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, SOURCEBOOK OF CRIMINAL JUSTICE STATISTICS—1994, at 413 tbl.4.33 (1995) [hereinafter SOURCEBOOK] (reprinting an FBI compilation of 1993 UCR data).

connection between drug use and crime is unclear.²⁷⁶ Finally, it must be remembered that the regression equations already control for the number of crimes committed. Thus, for drug usage to be the *X* factor, drug-related crimes would need not only to have increased around the time of *Miranda*, but also to have been uniquely hard to clear. There is no reason to suspect, for example, that a heroin-induced robbery would be harder to solve than other robberies.

Another possibility is that police-citizen relations deteriorated during the 1960s, affecting crime clearance rates around the time of *Miranda*. Police clashes with Vietnam War protesters and demonstrators for racial justice may have reduced citizen cooperation, thereby drying up witnesses and leads needed to clear crimes.²⁷⁷

Although worsening relations could conceivably have played a role in declining clearance rates during the 1960s, we think it an unlikely candidate for triggering much of the 1966-1968 decline. Although quantification of public attitudes is difficult, it is improbable that police-citizen relations would have deteriorated substantially across the country over such a short period of time. Indeed, the Gallup Poll suggests increased respect for the police around the time of *Miranda*. In April 1965, 70% of respondents across the country had a great deal of respect for the police, a percentage that rose to 77% in August 1967.²⁷⁸ Although we have been unable to locate consistent polling data on public confidence in the police throughout the decade, the polls we have found reported high public confidence in the police.²⁷⁹ Any decline in

²⁷⁶. See, e.g., JOHN KAPLAN, *THE HARDEST DRUG: HEROIN AND PUBLIC POLICY* 57 (1983) (stating that experts cannot “relate the contribution of heroin addiction to the total volume of property crime in the United States—except to say that is far less than the popular literature has portrayed”).

²⁷⁷. See W.S. WILSON HUANG & MICHAEL S. VAUGHN, *SUPPORT AND CONFIDENCE: PUBLIC ATTITUDES TOWARD THE POLICE*, in *AMERICANS VIEW CRIME AND JUSTICE: A NATIONAL PUBLIC OPINION SURVEY* 31, 31 (Timothy J. Flanagan & Dennis R. Longmire eds., 1996) (noting the need for citizen cooperation if a police force is to be effective).

²⁷⁸. See 3 GEORGE H. GALLUP, *GALLUP POLL: PUBLIC OPINION 1935-1971*, at 1935, 2077 (1972).

²⁷⁹. See NATIONAL CTR. ON POLICE AND COMMUNITY RELATIONS, MICHIGAN ST. UNIV., *A NATIONAL SURVEY OF POLICE AND COMMUNITY RELATIONS* 10 (1967) (reprinting a national Harris Poll finding a “good-excellent” rating of 76% for federal law enforcement, 70% for state law enforcement, and 65% for local law enforcement around 1967); note **Error! Bookmark not defined.** *supra* and accompanying text. Polling data also suggest that racial minorities had lower confidence in the police. See *A NATIONAL SURVEY OF POLICE AND COMMUNITY RELATIONS*, *supra*, at 11-13. However, this low confidence existed well before 1966. See *id.* at 16 (citing CALIFORNIA ADVISORY COMM. TO THE U.S. COMMISSION ON CIVIL RIGHTS, *REPORT ON CALIFORNIA: POLICE-MINORITY*

trust for the police would have been a more long-run phenomenon, extending from the late 1950s through the mid-1970s, from the earliest days of the civil rights protests to the waning days of Vietnam War protests and Watergate disillusionment. Such a decline would therefore not explain the sharp decline in clearance rates of the late 1960s. Also, the late 1960s, like the years before and after, were a time of rising public concern about crime,²⁸⁰ which might have produced greater, rather than lesser, willingness to help the police. Finally, it is quite possible that declining confidence in the police was more concentrated in some parts of the country than others and in some kinds of geographical areas than in others (e.g., large cities). Yet the sudden drops in clearance rate in 1966 and 1967 were reported by all population groups and all geographic divisions.²⁸¹

Even if public willingness to assist the police deteriorated in the 1960s, it seems likely to have improved at some point since then. Indirect measures of public support for law enforcement—such as public support for the death penalty or public concern about crime—have risen since the 1960s,²⁸² while crime clearance rates have remained relatively stable. Similarly, the Gallup Poll also found that the public who rated the honesty and ethics of police as high or very high rose from 37% in 1977 to 49% in 1996.²⁸³

A final problem for the public attitude explanation is that it seems unlikely to work for some crimes. For example, motor vehicle theft is a crime that is generally and consistently reported to the police because of insurance requirements.²⁸⁴ Our *Miranda* effect on motor vehicle clearance seems

GROUP RELATIONS (1963), which described poor police relations with African Americans).

²⁸⁰. See SOURCEBOOK, *supra* note **Error! Bookmark not defined.**, at 166 tbl.2.31 (reporting that 49% of Harris Poll respondents felt more uneasy on the streets in 1966 than in the previous year. This number increased to 53% in 1968 and to 55% in 1969).

²⁸¹. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

²⁸². See Dennis R. Longmire, *America's Attitudes About the Ultimate Weapon*, in AMERICANS VIEW CRIME AND JUSTICE, *supra* note **Error! Bookmark not defined.**, at 93, 99 (reporting polls showing that public support for the death penalty has generally risen since 1966); *cf.* SOURCEBOOK, *supra* note **Error! Bookmark not defined.**, at 140 tbl.2.1 (reporting polls showing that public concern about crime rose dramatically in the mid-1990s).

²⁸³. See Leslie McAneny, *The Gallup Poll: Pharmacists Again Most Trusted; Police, Federal Lawmakers Images Improve* (visited Aug. 26, 1997) <<http://www.gallup.com/poll/news/9713.html>>.

²⁸⁴. See CAROLINE WOLF HARLOW, BUREAU OF JUSTICE STATISTICS, SPECIAL REPORT: MOTOR VEHICLE THEFT 4 tbl.10 (1988) (noting 87% of completed vehicle thefts reported to the

accordingly unlikely to have been caused by declining citizen cooperation.

Professor Schulhofer has advanced a hypothesis that appears to be the opposite of the worsening police relations explanation for declining clearance rates. He suggests that reporting of crimes to the police might have actually increased after *Miranda*, thereby bringing weaker, harder to solve cases into the system and, in turn, lowering clearance rates.²⁸⁵ In effect, this claim implies that relations improved rather than deteriorated, because crime reporting is generally linked to greater confidence in the police.²⁸⁶ However, Schulhofer's claim of increased reporting is made without evidentiary support, and there is empirical evidence suggesting reporting has, in fact, remained constant, at least since 1973.²⁸⁷ Moreover, the claim suffers from many of the same flaws as the worsening police relations hypothesis, in that it uses what would seem to be a long-term trend to explain a short-term decline in clearances. Also, the increased reporting theory fails to explain why clearances of motor vehicle theft declined after *Miranda*, when that particular crime is usually consistently reported.²⁸⁸

Another possible explanation for the drop in clearance rates would be a change in the definition of crimes "cleared."²⁸⁹ But FBI data collection

police and 36% of attempted thefts, for 68% overall); Cloninger & Sartorius, *supra* note **Error! Bookmark not defined.**, at 392 (reporting that auto theft showed no appreciable reporting errors in national surveys) (citing Morris Silver, Punishment, Deterrence, and Police Effectiveness: A Survey and Critical Interpretation of the Recent Econometric Literature (unpublished report for the Crime Deterrence and Offender Career Project, New York, 1974)); Scott H. Decker, *Alternate Measures of Police Output*, 1 AM. J. POLICE 23, 27, 30 (1981) (finding little difference between victimizations and reported vehicle thefts); Samuel Nunn, *Computers in the Cop Car: Impact of the Mobile Digital Terminal Technology on Motor Vehicle Theft Clearance and Recovery Rates in a Texas City*, 17 EVALUATION REV. 182, 187 (1993) ("[U]nlike burglary, robbery, and assault, for example, that are subject to both the uncertainty of reporting and nonreporting by victims and changes in the definition of the crimes, motor vehicle thefts are generally reported and are not particularly subject to changes in definition.").

²⁸⁵. See Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 369.

²⁸⁶. See, e.g., ANTHONY V. BOUZA, *THE POLICE MYSTIQUE: AN INSIDER'S LOOK AT COPS, CRIME, AND THE CRIMINAL JUSTICE SYSTEM* 134 (1990) ("[C]itizens report crime more faithfully when confidence in the police is highest . . .").

²⁸⁷. See, e.g., Alfred Blumstein, Jacqueline Cohen & Richard Rosenfeld, *Trend and Deviation in Crime Rates: A Comparison of UCR and NCS Data for Burglary and Robbery*, 29 CRIMINOLOGY 237, 246-48 (1991) (finding, from 1973-1985, strong consistency between reported crimes, as measured by the UCR, and victimizations from crimes, as reported in the National Crime Victimization Survey).

²⁸⁸. See note **Error! Bookmark not defined.** *supra*. The incentive to report auto theft likely stems from insurance requirements.

²⁸⁹. See Ross et al., *supra* note **Error! Bookmark not defined.**, at 494 (noting that in

criteria appear to have remained the same throughout the relevant period.²⁹⁰ A related hypothesis is that improved police professionalism might have caused clearance rates to decline by reducing artificial inflation of clearance rates. For example, Professor Schulhofer has claimed that “the overall movement for greater professionalism . . . occurred, coincidentally or not, almost exactly at the time of *Miranda*.”²⁹¹ It would, indeed, be a “coincidence” to find dramatic and apparently heretofore unrecognized changes in record-keeping procedures that caused significant reductions in clearance rates. Professionalism is a quintessential long-term trend, unlikely to have changed sharply during the relevant period. Bolstering this view is a recurrent theme in the police literature—that police bureaucracies resist change imposed from the outside.²⁹² While it is possible to cite reports recommending reforms at about the time of *Miranda*,²⁹³ reform efforts—on record-keeping and other matters—were not narrowly confined to the late

interrupted time series analysis, changes in the means of measuring a variable can pose difficulties in interpreting before-and-after conditions of that variable); cf. Campbell, *supra* note **Error! Bookmark not defined.**, at 415 (noting the larceny “crime wave” created in Chicago, when a reform-minded administrator improved crime bookkeeping); Walker, *supra* note **Error! Bookmark not defined.**, at 295 (noting that the 1980 change in definition of “clear-up” rates in England means that “research conducted on data recorded before 1980 may require a different interpretation from later research”).

²⁹⁰. The 1962 *UCR Handbook* defines a “cleared” crime and provides examples. See FEDERAL BUREAU OF INVESTIGATION, U.S. DEP’T OF JUSTICE, UNIFORM CRIME REPORTING HANDBOOK: HOW TO PREPARE UNIFORM CRIME REPORTS 48-49 (1962) (defining “offenses cleared by arrest” and “exceptional clearances”). The 1965 and 1966 handbooks contain virtually identical definitions. See, e.g., FEDERAL BUREAU OF INVESTIGATION, U.S. DEP’T OF JUSTICE UNIFORM CRIME REPORTING HANDBOOK: HOW TO PREPARE UNIFORM CRIME REPORTS 50 (1966) [hereinafter 1966 UCR HANDBOOK]. Later versions appear to be substantively indistinguishable. See, e.g., FEDERAL BUREAU OF INVESTIGATION, U.S. DEP’T OF JUSTICE, UNIFORM CRIME REPORTING HANDBOOK 41-42 (1984). It appears that the definition of a “cleared” crime was slightly broadened some time before the 1960s by allowing police to count as cleared crimes cases in which the offender was “turned over to the court for prosecution” rather than actually requiring the prosecution to take place. See Sherman, *supra* note **Error! Bookmark not defined.**, at 380.

²⁹¹. Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 369.

²⁹². See generally BOUZA, *supra* note **Error! Bookmark not defined.**; SKOLNICK, *supra* note **Error! Bookmark not defined.**; JEROME H. SKOLNICK & JAMES J. FYFE, ABOVE THE LAW: POLICE AND THE EXCESSIVE USE OF FORCE (1993).

²⁹³. See Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 368 (citing PRESIDENT’S COMM’N ON LAW ENFORCEMENT AND ADMIN. OF JUSTICE, THE CHALLENGE OF CRIME IN A FREE SOCIETY 106-15 (1967)).

1960s.²⁹⁴

None of these explanations, either collectively or individually, begins to compete with *Miranda* as the cause of the clearance rate declines. But, as a final note, it is worth observing that *Miranda*'s defenders have articulated a welter of conflicting theories on clearance rates. Professor Schulhofer's shifting claims are illustrative. His initial position, widely adopted by *Miranda*'s defenders, was that clearance rates did not decline permanently after *Miranda*,²⁹⁵ a position that collapsed in the face of FBI data to the contrary.²⁹⁶ Schulhofer next maintained that "we need *only* turn to trends in levels of crime and police resources during the [post-1965] period" to understand the clearance rate decline.²⁹⁷ However, empirical evidence demonstrated that the 1966-1968 drop in clearance rates did not follow the pattern of rising crime rates throughout the 1960s and early 1970s.²⁹⁸ Subsequently, Schulhofer adopted his current view that, while "[m]any forces contributed to clearance rate trends . . . there is no reason to think that one particular factor—*Miranda*—was among the factors playing a causal role."²⁹⁹

In addition to rising crime rates and reduced law enforcement resources, he has pointed to the limited "capacity" of the criminal justice system, urbanization, the proportion of crimes in small cities, and other less easily quantifiable factors.³⁰⁰ To the extent that his claims are empirically testable, they have been disproven: Our regression analysis suggests that, even

²⁹⁴. See ROBERT FOGELSON, *BIG-CITY POLICE* 219-42 (1977) (discussing the wave of police reforms from the 1930s to the 1970s); Richard A. Leo, *From Coercion to Deception: The Changing Nature of Police Interrogation in America*, 18 *CRIME L. & SOC. CHANGE* 35, 49-52 (1992) (discussing the growth and gradual entrenchment of police professionalism from the 1930s to the mid-1960s).

²⁹⁵. See notes **Error! Bookmark not defined.** & **Error! Bookmark not defined.** *supra* and accompanying text (noting Schulhofer's claim to this effect).

²⁹⁶. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

²⁹⁷. Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 280 (emphasis added); *see id.* ("[S]oaring rates of violent crime and stagnant levels of police resources *easily explain* the observed clearance-rate trends.") (emphasis added); Stephen J. Schulhofer, *Pointing in the Wrong Direction*, *LEGAL TIMES*, Aug. 12, 1996, at 24 ("[T]here is no reason—none—to blame *Miranda*, rather than precipitously shrinking resources, for the decline in clearance rates during the late 1960s.").

²⁹⁸. See Cassell, *supra* note **Error! Bookmark not defined.**, at 307, 308 fig.2 ("[C]rime rates were increasing well before *Miranda* was handed down and continued to climb afterwards.").

²⁹⁹. Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 357-58.

³⁰⁰. *See id.* at 365-67; Schulhofer, *Clearance Rates*, *supra* note **Error! Bookmark not defined.**, at 291-92.

controlling for all the quantifiable factors, *Miranda* had a significant depressing effect on clearance rates, an effect found more consistently than that for Schulhofer's competing causes.³⁰¹ The robustness of the *Miranda* explanation compared to the fragility of the testable, competing explanations is itself strong evidence in favor of the handcuffing-the-cops position.

D. *The Logic of Miranda As a Cause*

Apart from *Miranda*, then, we have been unable to identify any strong candidate as the cause for the post-*Miranda* drop in clearance rates. Because we have been focusing on a host of specific and narrow issues, it might be useful to step back for a moment to look at the big picture: Is it logical to view *Miranda* as a causal factor?

The conclusion that *Miranda* caused a significant part of the 1966-1968 decline in clearance rates is supported by a wide range of information. To begin with, there is an obvious, sharp drop in clearance rates between 1966 and 1968, at exactly the time when the drop should have occurred if *Miranda* harmed law enforcement.³⁰² Next, the regression equations indicate that the drop for robbery, vehicle theft, larceny, and burglary cannot be explained by major criminal justice or socioeconomic variables.³⁰³ Indeed, this finding remains statistically significant over a variety of alternative formulations.³⁰⁴ In addition, both the FBI and the police on the street contemporaneously identified *Miranda* as a hindrance to clearing crimes.³⁰⁵ These law enforcement reports are corroborated by declining confession rates reported in the before-and-after studies of *Miranda*'s impact,³⁰⁶ lower confession rates reported in this country after *Miranda*,³⁰⁷ and higher confession rates reported

³⁰¹. See, e.g., Tables II-IV (finding a statistically significant *Miranda* effect on violent and property crimes, but no statistically significant effect from crime rate, police variables, capacity variables, or crime in small cities—urbanization was significant only for violent crimes).

³⁰². See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

³⁰³. See text accompanying notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra*.

³⁰⁴. See *id.*

³⁰⁵. See text accompanying notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra*.

³⁰⁶. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

³⁰⁷. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

in countries not following the *Miranda* rules.³⁰⁸

Finally, plain old common sense points to precisely the same result. Our conclusion is simply that when the Supreme Court imposed unprecedented restrictions on an important police investigative technique, the police became less effective. This was exactly what the dissenters predicted in *Miranda*,³⁰⁹ a claim that the majority did not refute.³¹⁰ In short, we are not asserting anything unusual; instead, we are merely suggesting the obvious. The proposition that *Miranda* harmed law enforcement should be uncontroversial. Instead, it is the contrary academic wisdom that receive our skepticism.

V. THE CLEARANCE RATE DECLINE AS A SOCIAL COST

Part IV suggested that *Miranda* caused a significant drop in crime clearance rates. This part contends that the drop in clearance rates should be regarded as a social cost.

³⁰⁸. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 876-78 (noting confession rates of over 60% in Great Britain and Canada); cf. Richard S. Frase, *Comparative Criminal Justices As a Guide to American Law Reform: How Do the French Do It, How Can We Find Out, and Why Should We Care?*, 78 CAL. L. REV. 542, 590-94 (1990) (reporting French clearance rates higher than American rates in three of four categories but concluding that differences are relatively modest).

³⁰⁹. See *Miranda v. Arizona*, 384 U.S. 436, 516 (Harlan, J., dissenting) (“There can be little doubt that the Court’s new code would markedly decrease the number of confessions.”); *id.* at 541 (White, J., dissenting) (discussing clearance rates and concluding that “[t]he rule announced today will measurably weaken the ability of the criminal law to perform these tasks”).

³¹⁰. See *id.* at 481 (claiming that the decision “should not constitute an *undue* interference with a proper system of law enforcement” (emphasis added)).

A. *The Implausibility of Declining Coercion*

Sometimes it is argued that clearance rates declined after *Miranda*, because the police were forced to abandon unconstitutionally coercive questioning techniques.³¹¹ On this view, declining clearance rates measure not the social cost of criminals unfairly escaping, but rather the price of the social benefit of police abandoning impermissible questioning techniques. Note that this argument implicitly concedes that clearance rates did fall because of *Miranda*. However, this explanation of why *Miranda* produced the drop is far-fetched for several reasons.

First, genuinely coerced confessions were statistically rare at the time of *Miranda*.³¹² Of course, one cannot consult an FBI tally of the number of coerced confessions each year. Yet it appears to be common ground that, as the result of increasing judicial oversight and police professionalization, coercive questioning methods began to decline in the 1930s and 1940s.³¹³ By the 1950s, coercive questioning had, according to a leading scholar in the area, “diminished considerably.”³¹⁴ Thus, the Supreme Court’s decision curtailing police interrogation techniques addressed a problem “that was already fading into the past.”³¹⁵ Chief Justice Warren’s majority opinion in *Miranda*, though noting historical police abuses, acknowledged that such abuses are “undoubtedly the exception now” and that “the modern practice of in-custody interrogation is psychologically rather than physically oriented.”³¹⁶

Furthermore, empirical surveys³¹⁷ provide good support for Professor Gerald Rosenberg’s assessment: “Evidence is hard to come by but what evidence there is suggests that any reductions that have been achieved in police brutality are independent of the Court and started before *Miranda*.”³¹⁸

³¹¹. See, e.g., *Transcript: The Goldwater Institute and the Federalist Society: Federalism and Judicial Mandates*, 28 ARIZ. ST. L.J. 17, 160 (1996) (remarks of Professor David Rudovsky) [hereinafter Rudovsky] (raising this possibility).

³¹². To be clear, we believe that even isolated instances of coerced confessions should be strongly condemned.

³¹³. Cassell, *supra* note **Error! Bookmark not defined.**, at 473-75 (describing police reform efforts in the twentieth century); Leo, *supra* note **Error! Bookmark not defined.**, at 47-53 (same).

³¹⁴. Leo, *supra* note **Error! Bookmark not defined.**, at 51.

³¹⁵. FRED P. GRAHAM, *THE SELF-INFLICTED WOUND* 22 (1969); see also Fred E. Inbau & James P. Manak, *Miranda v. Arizona—Is It Worth the Cost?*, PROSECUTOR, Spring 1988, at 31, 36 (“Prior to *Miranda*, the movement was under way to improve the quality of police interrogation practices.”).

³¹⁶. *Miranda v. Arizona*, 384 U.S. 436, 447-48; see also *id.* at 499-500 (Clark, J., dissenting).

Coercive police questioning is not only limited to physical brutality, but also includes other techniques. It seems unlikely, however, that such forms of coercion were so widespread that their elimination would have greatly changed clearance rates.³¹⁹ In addition, statistics on motions to suppress confessions confirm that coercive techniques were infrequently used by the time of *Miranda*. We should find frequent challenges to the voluntariness of confessions even before *Miranda* if coerced confessions were in fact frequent.³²⁰ Such motions, however, appear to have been rare around the time of *Miranda*.³²¹

However, the majority continued on to state that police abuses “are [still] sufficiently widespread to be the object of concern.” *Id.* at 447-48.

³¹⁷. For a discussion of this empirical evidence, see Cassell, *supra* note **Error! Bookmark not defined.**, at 474-75.

³¹⁸. GERALD N. ROSENBERG, *THE HOLLOW HOPE: CAN COURTS BRING ABOUT SOCIAL CHANGE?* 326 (1991).

³¹⁹. Professor Wayne R. LaFave reported the year before the *Miranda* decision that “[i]n the great majority of in-custody interrogations observed, the possibility of coercion appeared slight.” WAYNE R. LAFAVE, *ARREST: THE DECISION TO TAKE A SUSPECT INTO CUSTODY* 386 (1965); *see also* Edward L. Barrett, Jr., *Police Practices and the Law—From Arrest to Release or Charge*, 50 CAL. L. REV. 11, 42 (1962) (reporting California data in 1960 that most interrogations lasted under two hours). Similarly, the student observers in New Haven in 1966, assessing all forms of police “tactics,” found “a low level of coerciveness in most questioning.” Wald et al., *supra* note **Error! Bookmark not defined.**, at 1558.

³²⁰. Such challenges are rarely made today and even more rarely granted. *See* Cassell, *supra* note **Error! Bookmark not defined.**, at 392-93 (collecting the studies on this point).

³²¹. *See id.* at 476 (citing studies showing few rejections of defendants’ statements by prosecutors or by judges at preliminary hearings).

Besides the relative scarcity of coercion, there is another reason to believe that clearance rate reductions were not caused by fewer coerced confessions: the nature of the *Miranda* rules themselves. *Miranda* was not particularly well-designed as a shield against coercion. As Justice Harlan pointed out in his *Miranda* dissent, “Those who use third-degree tactics and deny them in court are equally able and destined to lie as skillfully about warnings and waivers.”³²² In other words, police who used coercive tactics to obtain involuntary confessions would not necessarily have felt compelled after *Miranda* to change their already-improper methods.³²³ And even if they did so, it was unlikely to have been so rapid a change as to produce a quick decrease in confession rates. Considering the low coerciveness of questioning even before *Miranda* and the ineffectiveness of the *Miranda* requirements in preventing coercion, it is unlikely that the 1966-1968 clearance rate drop is a manifestation of a change from a coercive to a noncoercive system.³²⁴

B. *Components of the Clearance Rate Change*

³²². *Miranda v. Arizona*, 384 U.S. 436, 505 (Harlan, J.,dissenting).

³²³. See Evelle J. Younger, *Prosecution Problems*, 53 A.B.A. J. 695, 698 (1967) (“*Miranda* will not affect the brutal or perjurious policeman—he will continue to extract confessions without reference to the intonations of the Supreme Court; and when he testifies, he will simply conform his perjury to the latest ground rules.”); cf. Leiken, *supra* note **Error! Bookmark not defined.**, at 22 (finding defendants claimed, after *Miranda*, that police had made promises or threats to get confessions). Shortly after *Miranda*, Neal Milner studied the police response in Wisconsin and found that “generally most interrogations continued to operate under rules formalized prior to the *Miranda* decision.” MILNER, *supra* note **Error! Bookmark not defined.**, at 228; cf. George C. Thomas III & Marshall D. Bilder, *Aristotle’s Paradox and the Self-Incrimination Puzzle*, 82 J. CRIM. L. & CRIMINOLOGY 243, 278 (1991) (noting the paradoxical quality of the *Miranda* debate in which the decision’s defenders frequently argue it has had little effect.)

³²⁴. Note that we are only arguing that the clearance rate decline found from 1966-1968 is not explained by a sudden, contemporaneous reduction in coerciveness. “One could believe that police interrogation has generally become less coercive over the last several decades and still accept this claim.” Cassell, *supra* note **Error! Bookmark not defined.**, at 478 n.533.

Even if crime clearance rates fell after *Miranda*, defenders of *Miranda* might argue that law enforcement was not really harmed. Instead, they might claim, all that changed was the ability of law enforcement officers to get suspects to confess to, and thus “clear,” old unsolved crimes. Professor Yale Kamisar has taken this view. Responding to a preliminary presentation of our thesis, he was quoted as saying that our interpretation of the clearance rate drop is too simple in that “[c]learance rates can be very misleading because before *Miranda*, there is evidence police were using so-called confessions to ‘clear’ crimes that were never prosecuted.”³²⁵ In other words, clearance rates appeared high before *Miranda* because police could get a suspect to confess many crimes, even though there was never sufficient evidence to be sure that the suspect committed all of those crimes.³²⁶

It is important to recognize that the minor premise of Professor Kamisar’s argument is that the confession rate did in fact decline after *Miranda*. Clearance rates for burglary, for example, would fall only if police were less successful in interrogating burglars. Kamisar’s argument, therefore, appears to concede implicitly that *Miranda* is an impediment to productive police investigation.

³²⁵. Joe Costanzo, *Has Miranda Handcuffed the Cops?*, DESERETNEWS, June 12-13, 1996, at A1 (quoting Kamisar). Kamisar has discussed the issue of the reliability of clearance rates at greater length in Kamisar, *supra* note **Error! Bookmark not defined.**, at 252-53 (citing SKOLNICK, *supra* note **Error! Bookmark not defined.**, at 178-79, and suggesting the unreliability of clearance rate statistics); Yale Kamisar, *On the Tactics of Police-Prosecution Oriented Critics of the Courts*, 49 CORNELL L.Q. 436, 466 n.173 (1964) (discussing clearances through confessions to multiple crimes).

³²⁶. See, e.g., SKOLNICK, *supra* note **Error! Bookmark not defined.**, at 178-79 (describing a burglar giving multiple clearances to police); Wald et al., *supra* note **Error! Bookmark not defined.**, at 1593 n.197, 1595-96 (finding that solving other crimes is the most important reason for questioning other than gathering evidence for trial and giving examples of questioning to clear crimes); Rudovsky, *supra* note **Error! Bookmark not defined.**, at 160 (noting that the impact on the defendant from confessing to additional crimes is minimal, while it significantly benefits the police because they are able to “clear the books” of many unsolved cases); Weinraub, *supra* note **Error! Bookmark not defined.** (describing police use of questioning to clear other crimes); cf. PAULINE MORRIS, ROYAL COMM’N ON CRIMINAL PROCEDURE, POLICE INTERROGATION: REVIEW OF LITERATURE 11 (1980) (stating that, in Britain, interrogation may lead to the clearance of crimes other than the one being investigated); Bottomley & Coleman, *supra* note **Error! Bookmark not defined.**, at 50-52 (attributing part of drop in British clearance rates to the introduction of new confession procedures reducing confessions to multiple crimes).

Professor Kamisar is analytically correct in suggesting that clearance rates could have fallen because law enforcement was hampered in its ability get confessions to previously unsolved crimes. To examine this possibility more precisely, it is important to understand that the clearance rate consists of two components: (1) the number of arrests per crime and (2) the number of clearances per arrest.³²⁷ Kamisar's ultimate claim is that a change in the second component—clearances per arrest—is primarily responsible for the observed clearance rate drop³²⁸ and, apparently, that such a change is not a major law enforcement concern. Both of these positions are doubtful.

The theory that only the rate of clearances per arrest fell after *Miranda* is itself too simple. At bottom, this theory is simply speculation about one possible explanation for the clearance rate drop after *Miranda*. No hard evidence suggests that there were, at the time of *Miranda*, sufficient numbers of confessions to multiple crimes to account for the clearance rate changes we have reported.³²⁹ More specifically, the available data suggest that clearance rates fell immediately after *Miranda* not only because police were obtaining fewer statements from suspects but also because they were making fewer arrests per crime. In other words, both clearances per arrest and arrests per crime fell after *Miranda*. FBI data for violent crimes from 1965³³⁰ to 1991 on this issue are plotted in Figure 6.

³²⁷. Cf. GREENWOOD, *supra* note **Error! Bookmark not defined.**, at 24 (discussing differences between clearance rates and arrest rates).

³²⁸. As an illustration, assume that the violent crime clearance rate before *Miranda* was 60% because, of every 100 crimes, the police arrested 40 suspects and cleared 1.5 crimes per arrest. After *Miranda*, if police arrested 40 suspects and cleared only one violent crime per arrest, then the clearance rate would fall to 40%.

³²⁹. The limited quantitative empirical evidence on whether such multiple confessions were sufficiently prevalent to make Kamisar's speculation even theoretically possible is mixed. Compare TAPING POLICE INTERROGATIONS, *supra* note **Error! Bookmark not defined.** (finding that, after *Miranda*, only 12 unsolved crimes were solved through 1460 interrogations), with Wald et al., *supra* note **Error! Bookmark not defined.**, at 1595 n.203 (finding, after *Miranda*, about 10-15% of all unsolved crimes during observation period were solved through interrogations). See generally Weinraub, *supra* note **Error! Bookmark not defined.** (reporting that a police spokesperson attributed the 10% decline in clearance rates from 1965-1966 in New York City in part to Supreme Court decisions restricting the ability to get multiple confessions).

³³⁰. The FBI has published a consistent revision of its arrest data that extends back only to 1965. The revised arrest data are used in preference to unadjusted data for reasons discussed in the Appendix. See notes **Error! Bookmark not defined.-Error! Bookmark not defined.** *infra* and accompanying text. Violent crime data is used because Professor Kamisar was discussing a

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As can be seen, there was a substantial decline in the number of arrests per violent crime at the time of *Miranda*. In fact, as far as we can tell from the available data, more than half of the 1966-1968 drop in violent crime clearance rates stemmed from reductions in arrests per crime. The number of arrests per crime fell sharply from 1965-1972 and has continued to do so modestly since then. Clearances per arrest, on the other hand, have bounced around over the last several decades. Although more research into this area is plainly warranted,³³¹ it is hard to read the data as showing a permanent, post-*Miranda* drop in clearance rates stemming only, or even primarily, from changes in clearances per arrest.

presentation of our thesis that used violent crimes and because an aggregate grouping tended to smooth some puzzling fluctuations in the year-to-year data. See note **Error! Bookmark not defined.** *infra*.

³³¹. Figure 6 shows large and apparently unpredictable fluctuations in both clearances per arrest and arrests per crime, fluctuations that were even larger in some individual crime categories. This suggests that the data on these variables may not be consistent over time. Also, from about 1977 onward, arrests per crime modestly decline while clearances per arrest modestly rise. Because the two figures are reciprocally related, see notes **Error! Bookmark not defined.** **Error! Bookmark not defined.** *infra* and accompanying text, these movements may be, in part, a statistical artifact rather than a reflection of some real-world trend. However, declining arrest rates from 1974-1986 for various crimes were also found in Patrick A. Langan, *America's Soaring Prison Population*, 251 SCIENCE 1568, 1572 tbl.4 (1991).

Even were Professor Kamisar correct in pinpointing the cause of the clearance rate drop, his other claim—that society is unharmed when police officers solve fewer crimes from each arrest—is wrong. Fewer cleared crimes leave police investigators less able to focus effectively on crimes that require their attention. They are forced to spend hours or days attempting to solve crimes that would have been solved by talking with a suspect for a few minutes.³³² The paucity of police resources makes this unnecessary diversion a possible concern.³³³ Multiple confessions are also useful to help police officers discern the modus operandi (or MO) of professional criminals.³³⁴

Uncleared crimes also harm crime victims, who never receive the reassurance that their attackers have been apprehended. Evidence suggests that the principle harms suffered by victims of crime include psychological trauma.³³⁵ Without a clearance, they will likely continue to fear that they will be victimized again.³³⁶ Victims also have a hard time putting the crime behind them. Victims understandably want to see justice done,³³⁷ and that is often not possible until the crime is cleared. Victims of property crimes also benefit from each clearance, because the confession creates the possibility of the return of stolen property.³³⁸ For all these reasons, the harm to victims from declining clearances is indeed a legitimate social concern.³³⁹

³³². See Richard A. Leo, *Police Interrogation and Social Control*, 3 SOC. & LEGAL STUD. 93, 99 (1994) (reporting police views that “getting a confession makes the investigator’s job a lot easier If he gets a confession . . . he doesn’t have to spend hours tracking down witnesses, running fingerprints, putting together line-ups, etc.”).

³³³. This conclusion assumes that police might be able to solve other crimes through additional investigation. *But cf.* Table II (showing police resources generally had little effect on clearances); Wald et al., *supra* note **Error! Bookmark not defined.**, at 1596 (questioning whether investigation of uncleared crimes is effective).

³³⁴. See GREENBERG ET AL, *supra* note **Error! Bookmark not defined.**, at 45-46 (reporting detectives’ views to this effect).

³³⁵. See LESLIE SEBBA, *THIRD PARTIES: VICTIMS AND THE CRIMINAL JUSTICE SYSTEM* 80-81 (1996) (reaching this conclusion after a review of several studies).

³³⁶. See PRESIDENT’S TASK FORCE ON VICTIMS OF CRIME, *FINAL REPORT* 4 (1982) (describing fear of “composite” victim of crime); *see also id.* at 28 (noting that victims suffer when police fail to solve crimes).

³³⁷. *See id.* at 5.

³³⁸. See GREENBERG ET AL, *supra* note **Error! Bookmark not defined.**, at 45 (reporting detectives’ views that the possibility of returning property to burglary victims justified attempts to obtain multiple clearances).

³³⁹. *Cf. Morris v. Slappy*, 461 U.S. 1, 14 (1983) (“[I]n the administration of criminal justice,

Finally, a drop in clearances per arrest will inevitably be followed by a drop in convictions. If police clear fewer crimes because they obtain fewer statements from suspects, subsequent prosecutions will be impaired. Kamisar conceivably could be correct that it makes little difference whether an arrested burglar confesses to four crimes or to one. But it makes a considerable difference whether he confesses to one crime or none, as a confession is quite important to prosecutors.³⁴⁰ *Miranda* does not generally reduce the number of confessions from four to one; it reduces them to zero by occasionally blocking police interrogation entirely.³⁴¹ It seems hard to view such declining clearances as anything other than a serious social cost.

VI. MOVING BEYOND *MIRANDA*

Having analyzed the data on crime clearance rates, we are now in a position to return to the question we posed at the outset: Has *Miranda*, in fact, handcuffed the cops? The answer to the question turns on what “handcuffed” means. It is clear that our analysis provides no support for the overheated rhetoric of some of *Miranda*’s detractors, who predicted immediately after the decision that law enforcement would grind to a halt.³⁴² But we understand *Miranda*’s pragmatic critics today to make a more modulated claim: that *Miranda* has seriously impeded police effectiveness in ways that could be avoided through reasonable changes in the *Miranda* rules.

courts may not ignore the concerns of victims.”).

³⁴⁰. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 909-16 (finding that whether police obtain an incriminating statement makes a significant difference in case outcome).

³⁴¹. See Cassell, *supra* note **Error! Bookmark not defined.**, at 494-96 (presenting data on what percentage of suspects do not waive rights and thus cannot be interrogated).

³⁴². See BAKER, *supra* note **Error! Bookmark not defined.**, at 200-01 (collecting examples of such responses to *Miranda*).

Our findings support this more tempered attack. Our regression equations and accompanying causal analysis suggest that, without *Miranda*, the number of crimes cleared would be substantially higher—by as much as 6.6-29.7% for robbery, 6.2-28.9% for burglary, 0.4-11.9% for larceny, and 12.8-45.4% for vehicle theft.³⁴³ Moreover, applied to the vast numbers of cases passing through the criminal justice system, these percentages would produce large numbers of cleared crimes. As many as 36,000 robberies, 82,000 burglaries, 163,000 larcenies, and 78,000 vehicle thefts remain uncleared each year as a result of *Miranda*.³⁴⁴

³⁴³. See Table VIII.

³⁴⁴. See *id.*

To put *Miranda*'s costs into some perspective, it is useful to compare them to the costs of the Fourth Amendment exclusionary rule, long considered a major—if not *the* major—judicial impediment to effective law enforcement.³⁴⁵ At the time the Supreme Court created the “good faith” exception to the Fourth Amendment exclusionary rule, it cited statistics indicating that the rule resulted in the failure to convict from 0.6-2.35% of individuals arrested for felonies.³⁴⁶ The Court pointed out that these “small percent- ages . . . mask a large absolute number of felons who are released because the cases against them were based in part on illegal searches or seizures.”³⁴⁷ Measured through clearances, a quite conservative measure of *Miranda*'s effects,³⁴⁸ there appear to be more uncleared crimes from *Miranda* than lost convictions from the search and seizure exclusionary rule.³⁴⁹ Exact comparisons of the percentages are problematic because of differing variables under study—persons arrested in the exclusionary rule studies and crimes cleared in our *Miranda* study. However, this asymmetry likely understates the relative size of the *Miranda* effect, because there are more crimes cleared than there are arrests made.³⁵⁰ While other technical adjustments would also have to be made to strictly compare the two figures,³⁵¹ it seems quite likely that *Miranda*'s costs exceed those of the exclusionary rule, especially outside of contraband offenses.³⁵²

³⁴⁵. See generally AKHIL REED AMAR, *THE CONSTITUTION AND CRIMINAL PROCEDURE: FIRST PRINCIPLES* 20-31 (1997) (discussing the Fourth Amendment exclusionary rule).

³⁴⁶. See *United States v. Leon*, 468 U.S. 897, 907 n.6 (1984) (citing Thomas Y. Davies, *A Hard Look at What We Know (and Still Need to Learn) About the “Costs” of the Exclusionary Rule: The NIJ Study and Other Studies of “Lost” Arrests*, 1983 AM. B. FOUND. RES. J. 611, 621, 667 tbl.6).

³⁴⁷. *Id.* at 908 n.6.

³⁴⁸. See note **Error! Bookmark not defined.** *supra* and accompanying text.

³⁴⁹. Compare note **Error! Bookmark not defined.** *supra* and accompanying text with note **Error! Bookmark not defined.** *supra*.

³⁵⁰. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

³⁵¹. Cf. Davies, *supra* note **Error! Bookmark not defined.**, at 656-68 (comparing estimates of the effects of illegal searches to exclusionary rule studies).

³⁵². See Dallin H. Oaks, *Studying the Exclusionary Rule in Search and Seizure*, 37 U. CHI. L. REV. 665, 681 (1970) (noting that suppression motions are most frequently filed in cases involving narcotics and weapons).

Taking the perspective of victims of crime underscores the severity of *Miranda*'s costs.³⁵³ A society concerned for victims is obligated to do its best to avoid such miscarriages of justice as when a child abuser is set free because of a *Miranda* technicality.³⁵⁴ Hearing about cases in which confessions are obtained and then suppressed puts a human face on *Miranda*'s costs. But the far more frequent form of *Miranda* suppression occurs even before that, when a suspect fails to even make the statement that could prevent the crime from going unsolved or unpunished. In concentrating on statistical quantification of *Miranda*'s costs, we hope we have not underplayed the human tragedies involved. As Professor Gerald Caplan has concluded, the statistical studies "reduce crime to something remote and abstract, a string of numbers, an event that one reads about in the newspapers, something that happens in another part of town. There is no hint of rape as a nightmare come alive, or robbery as a ruinous matter."³⁵⁵ Moreover, we have not discussed the inequitable distribution of the social consequences of these unsolved crimes. It is likely that these costs fell most heavily on those in the worst position to bear them, including racial minorities and the poor.³⁵⁶

Finally, it is a truism that an unnecessary cost is a cost that is too high.³⁵⁷ If we can protect other values in our criminal justice system while eliminating the costs of *Miranda*, regardless of whatever size they might be, then there is no reason to cling to its constraints. What converts *Miranda*'s harm into an undeniable tragedy is that these uncleared crimes are largely unnecessary.

³⁵³. See generally Paul G. Cassell, *Balancing the Scales of Justice: The Case for and the Effects of Utah's Victims' Rights Amendment*, 1994 UTAH L. REV. 1373 (considering the crime victim's perspective); DOUGLAS E. BELOOF, VICTIMS IN CRIMINAL PROCEDURE (forthcoming 1998) (discussing victims and the exclusionary rule).

³⁵⁴. For an example, see *State v. Oldham*, 618 S.W.2d 647 (Mo. 1981). See also 136 CONG. REC. S9027 (daily ed. June 28, 1990) (statement of Sen. Hatch describing this case).

³⁵⁵. Gerald M. Caplan, *Miranda Revisited*, 93 YALE L.J. 1375, 1384-85 (1984) (reviewing BAKER, *supra* note **Error! Bookmark not defined.**).

³⁵⁶. Cf. CHARLES MURRAY, *LOSING GROUND: AMERICAN SOCIAL POLICY, 1950-1980*, at 117 (1984) (reviewing crime statistics and concluding: "Put simply, it was much more dangerous to be black in 1972 than it was in 1965, whereas it was not much more dangerous to be white.").

³⁵⁷. Cf. Supplemental Brief for the United States as Amicus Curiae Supporting Reversal at 46, *Illinois v. Gates*, 462 U.S. 213 (1983) (No. 81-430) ("[T]he freeing of even one guilty defendant by virtue of an irrational rule may exact a greater cost than society should be expected to bear . . .").

Today, with the benefit of thirty years of subsequent interpretations, we know the *Miranda* mandate is not a constitutional requirement.³⁵⁸ Rather, the Court has held specifically that *Miranda* rules are only “safeguards,” whose purpose is to reduce the risk that police will violate the Constitution during custodial questioning. As explained in *Michigan v. Tucker*,³⁵⁹ *Miranda* established a “series of recommended ‘procedural safeguards’ The [Miranda] Court recognized that these procedural safeguards were not themselves rights protected by the Constitution but were instead measures to insure that the right against compulsory self-incrimination was protected.”³⁶⁰ Based on this rationale, the *Tucker* Court found that failure to give warnings that complied with *Miranda* “did not abridge respondent’s constitutional privilege . . . but departed only from the prophylactic standards later laid down by this Court in *Miranda* to safeguard that privilege.”³⁶¹ Thus, the *Miranda* rules can be changed without impinging on the Fifth Amendment.

The *Miranda* rules have no firm roots in the constitutional history of this country.³⁶² For many years, the Constitution was generally understood as prohibiting the states from introducing coerced or involuntary statements against a defendant. Courts assessed “voluntariness” by looking at such factors as police pressure, the length of questioning, and the suspect’s mental and physical condition. Police compliance with an elaborate set of warning-and-waiver requirements was not thought relevant to that determination. As one ardent defender of *Miranda* has conceded, the decision was a “radical departure” from the assumptions of the times.³⁶³

³⁵⁸. See GRANO, *supra* note **Error! Bookmark not defined.**, at 173-98 (discussing how the prophylactic nature of *Miranda* differs from most constitutional law decisions).

³⁵⁹. 417 U.S. 433 (1974).

³⁶⁰. *Id.* at 443-44.

³⁶¹. *Id.* at 446; *see also* Withrow v. Williams, 507 U.S. 680, 690-91 (1993) (collecting numerous cases describing *Miranda* rights as “prophylactic” in nature”).

³⁶². See GRANO, *supra* note **Error! Bookmark not defined.**, at 173-98; OFFICE OF LEGAL POLICY, U.S. DEP’T OF JUSTICE, REPORT TO THE ATTORNEY GENERAL ON THE LAW OF PRE-TRIAL INTERROGATION 3-41 (1986) [hereinafter OLP PRE-TRIAL INTERROGATION REPORT].

³⁶³. Schulhofer, *supra* note **Error! Bookmark not defined.**, at 552 n.214.

The Court has taken *Miranda* beyond the bounds of the Constitution based on a purely pragmatic, cost-benefit assessment. The *Miranda* decision, the Court has claimed, “embodies a carefully crafted balance designed to fully protect both the defendant’s and society’s interests.”³⁶⁴ Thus, the Court weighs the costs and benefits, as it sees them, of extending the “safeguards” of *Miranda*. Although the Court has never articulated with any precision what “costs” it is willing to tolerate in this cost-benefit calculation,³⁶⁵ it has in all likelihood significantly underestimated their magnitude. Presumably the Court has simply plugged into its equation the supposed “negligible” costs of the decision, as recounted in the conventional academic wisdom,³⁶⁶ and determined that the equally unspecified “benefits” outweigh them. Even under such charitable assumptions, the Court’s conclusion is quite debatable.³⁶⁷ But, if substantial numbers of uncleared crimes are counted among the costs of *Miranda*, it is far more doubtful that the calculus would produce the same conclusion.

The Court’s calibration of *Miranda*’s costs and benefits becomes even more problematic when the possibility of reasonable, less harmful approaches to regulating police questioning is factored in. When the Court announced *Miranda* in 1966, significant efforts to reform the rules regarding interrogations were already underway.³⁶⁸ The decision itself seemed to invite the continued exploration of such alternatives, promising that “[o]ur decision in no way creates a constitutional straitjacket which will handicap sound efforts at reform.”³⁶⁹ This has proven to be an empty promise. In the quarter of a century since *Miranda*, reform efforts have been virtually nonexistent. The reasons are not hard to imagine. No state is willing to risk possible invalidation of criminal convictions by departing from *Miranda*. As a result, the Office of Legal Policy concludes:

³⁶⁴. Moran v. Burbine, 475 U.S. 412, 433 n.4. (1986).

³⁶⁵. See Cassell & Hayman, *supra* note **Error! Bookmark not defined.**, at 921-22; cf. Davies, *supra* note **Error! Bookmark not defined.**, at 626 (noting that “there is no empirical content to this ‘balancing’ approach to considering the costs and benefits” of the search and seizure exclusionary rule).

³⁶⁶. See note **Error! Bookmark not defined.** *supra* and accompanying text.

³⁶⁷. See 18 U.S.C. § 3501 (1994) (a federal statute rejecting *Miranda* in favor of voluntariness test); GRANO, *supra* note **Error! Bookmark not defined.**, at 199-222 (advocating overruling of *Miranda* on purely doctrinal grounds).

³⁶⁸. See *Miranda v. Arizona*, 384 U.S. 436, 523 (Harlan, J., dissenting) (noting the “ironic untimeliness” of the Court’s new confession rules in view of ongoing “massive re-examination of

The *Miranda* decision has petrified the law of pre-trial interrogation for the past twenty years, foreclosing the possibility of developing and implementing alternatives that would be of greater effectiveness both in protecting the public from crime and in ensuring fair treatment of persons suspected of crime. . . . Nothing is likely to change in the future as long as *Miranda* remains in effect and perpetuates a perceived risk of invalidation for any alternative system that departs from it.³⁷⁰

The failure to explore other approaches cannot be attributed to lack of viable options. The states might, for example, be allowed to videotape interrogations as a substitute for the *Miranda* procedures. Videotaping would better protect against police brutality, end the “swearing contest” about what happened in secret custodial interrogation, and allow suspects who are manipulated into falsely confessing to prove their innocence.³⁷¹ At the same time, even when coupled with limited warnings of rights, videotaping does not appear to significantly depress confession rates.³⁷² Videotaping thus might be the “win-win” solution to the problem of regulating police interrogations envisioned in *Miranda*’s encouragement to “Congress and the States to continue their laudable search for increasingly effective ways of protecting the rights of the individual while promoting efficient enforcement of our criminal laws.”³⁷³

criminal law enforcement procedures on a scale never before witnessed”); OLP PRE-TRIAL INTERROGATION REPORT, *supra* note **Error! Bookmark not defined.**, at 40-41 (discussing American Law Institute efforts to draft a Model Code of Pre-arraignment Procedure).

³⁶⁹. *Miranda*, 384 U.S. at 467.

³⁷⁰. OLP PRE-TRIAL INTERROGATION REPORT, *supra* note **Error! Bookmark not defined.**, at 96.

³⁷¹. See JUDGE HAROLD J. ROTHWAX, GUILTY: THE COLLAPSE OF CRIMINAL JUSTICE 237 (1996) (urging abandonment of *Miranda* and replacement with videotaping); Cassell, *supra* note 13, at 486-92 (advocating videotaping and modified warnings as a replacement for *Miranda*); Paul G. Cassell, *Protecting the Innocent from False Confessions and Lost Confessions—and from Miranda*, 88 J. CRIM. L. & CRIMINOLOGY 497 (1998) (suggesting that videotaping as a replacement for *Miranda* is a superior solution to the problem of false confessions and lost confessions); Paul G. Cassell, *Balanced Approaches to the False Confessions Problem: A Brief Comment on Ofshe, Leo, and Alschuler*, 74 DENV. U. L. REV. 1123 (1997) (raising videotaping as a replacement for *Miranda*); Richard J. Ofshe & Richard A. Leo, *The Social Psychology of Police Interrogation: The Theory and Classification of True and False Confessions*, 16 STUD. LAW, POL. & SOC. 189, 238 (Austin Sarat & Susan S. Sibley eds., 1997) (suggesting a “taping” of interrogatories as one of the safeguards against false confessions).

³⁷². See Cassell, *supra* note **Error! Bookmark not defined.**, at 489-92.

³⁷³. *Miranda*, 384 U.S. at 467.

As another example, the states might be allowed to bring an arrested suspect before a magistrate for questioning. Such a proposal was made more than sixty years ago by Paul Kauper;³⁷⁴ similar proposals have since been made by Judge Walter Schaefer, Judge Henry Friendly, and Akhil Amar and Renée Lettow.³⁷⁵ Questioning under the supervision of a magistrate would offer more judicial oversight than *Miranda* but might also be structured in such a way as to result in more evidence useful for society. However, just as with videotaping, the threat of lurking constitutional issues³⁷⁶ and the Court's failure to indicate whether this might be a permissible alternative to *Miranda*, has relegated this proposal to nothing more than a favorite "hypothetical" for criminal procedure professors.³⁷⁷

Finally, the Court might simply abandon the grand social experiment of *Miranda* and return to the long-standing "voluntariness" test for the admissibility of confessions.³⁷⁸ After all, this was the generally prevailing approach to assessing confessions in this country for almost two centuries and is supported by the conventional notion that the Constitution ought to be interpreted consistently with the Framers' intent.³⁷⁹ The voluntariness standard is also adopted by an explicit (although as of yet largely untested) congressional directive, making it the touchstone for admitting confessions in federal cases.³⁸⁰

³⁷⁴. See Paul G. Kauper, *Judicial Examination of the Accused—A Remedy for the Third Degree*, 30 MICH. L. REV. 1224 (1932).

³⁷⁵. See WALTER V. SCHAEFER, *THE SUSPECT AND SOCIETY: CRIMINAL PROCEDURE AND CONVERGING CONSTITUTIONAL DOCTRINES* 76-81 (1967) (arguing for an interpretation of the Fifth Amendment that would permit magistrates to question suspects and prosecutors to use resulting nontestimonial fruits at trial); Henry J. Friendly, *The Fifth Amendment Tomorrow: The Case for Constitutional Change*, 37 U. CIN. L. REV. 671, 721-25 (1968) (similar); AKHIL REED AMAR, *THE CONSTITUTION AND CRIMINAL PROCEDURE* 76-77 (1997) (similar); Akhil Reed Amar & Renée B. Lettow, *Fifth Amendment First Principles: The Self-Incrimination Clause*, 93 MICH. L. REV. 857, 898-928 (1995) (similar).

³⁷⁶. See Yale Kamisar, *Kauper's "Judicial Examination of the Accused" Forty Years Later—Some Comments on a Remarkable Article*, 73 MICH. L. REV. 15, 33-35 n.70 (1974) (discussing constitutional problems with Kauper's proposal in light of contemporary doctrine).

³⁷⁷. See, e.g., RONALD J. ALLEN, RICHARD B. KUHNS & WILLIAM J. STUNTZ, *CONSTITUTIONAL CRIMINAL PROCEDURE: AN EXAMINATION OF THE FOURTH, FIFTH, AND SIXTH AMENDMENTS AND RELATED AREAS* 1229 (3d ed. 1995) (raising the Kauper proposal as a possible alternative to *Miranda*).

³⁷⁸. One way to accomplish this result is to "disincorporate" the *Miranda* doctrine and allow the states to determine whether to follow it. See Barry Latzer, *Toward the Decentralization of*

The evidence presented in this article argues in favor of earnestly considering these alternatives to *Miranda*. Justice Harlan's dissenting opinion warned that the decision was a hazardous experiment with the country's safety, with "harmful consequences" that "only time can tell."³⁸¹ The experiment's results are now in. The clearance rate data collected in this study—coupled with the other evidence concerning *Miranda*'s effect³⁸²—strongly suggest that *Miranda* has seriously harmed society by hampering the ability of the police to solve crimes. Indeed, based on national crime clearance rates, a rough and conservative measure of its impact, *Miranda* may be the single most damaging blow inflicted on the nation's ability to fight crime in the last half century. In short, it appears that *Miranda* has, as its critics charge, handcuffed the cops. It is time to consider removing those shackles and regulating police interrogation in less costly ways.

Criminal Procedure: State Constitutional Law and Selective Disincorporation, 87 J. CRIM. L. & CRIMINOLOGY 63, 101-11 (1996) (suggesting this approach).

³⁷⁹. Cf. Paul G. Cassell, *The Rodney King Trials and the Double Jeopardy Clause: Some Observations on Original Meaning and the ACLU's Schizophrenic Views of the Dual Sovereign Doctrine*, 41 UCLA L. REV. 693, 717-19 (1994) (discussing role of original meaning in constitutional criminal procedure).

³⁸⁰. See 18 U.S.C. § 3501 (1994); see also *Davis v. United States*, 512 U.S. 452, 465 (1994) (Scalia, J., concurring) (arguing that section 3501 "reflect[s] the people's assessment of the proper balance to be struck between concern for persons interrogated in custody and the needs of effective law enforcement"); *United States v. Crocker*, 510 F.2d 1129 (10th Cir. 1975) (concluding that section 3501 is constitutional); *United States v. Rivas-Lopez*, 988 F. Supp. 1424 (D. Utah 1997) (same); Eric D. Miller, Comment, *Should Courts Consider 18 U.S.C. § 3501 Sua Sponte?*, 665 U. CHI. L. REV. (forthcoming 1998) (concluding that federal courts must follow section 3501).

³⁸¹. *Miranda v. Arizona*, 384 U.S. 436, 504 (Harlan, J., dissenting).

³⁸². See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

APPENDIX. DATA SOURCES FOR THE REGRESSION VARIABLES

It has been suggested that “it is of the utmost importance when reporting statistical results that authors report in the fullest possible manner all relevant information that would enable others to judge the quality of the results and, if necessary, to replicate them.”³⁸³ To that end, we explain in some detail the methodology for compiling our data.

A. *The Dependent Variables—Clearance Rates*

The dependent variables for the regressions were national crime clearance rates, published in the FBI’s annual *UCR*.

³⁸³. Pyle & Deadman, *supra* note **Error! Bookmark not defined.**, at 207.

1. *National totals.*

From 1958-1972, the FBI published a national clearance rate for total crimes.³⁸⁴ This “grand total” included the seven index crimes (i.e., murder, rape, robbery, assault, burglary, larceny \$50 and over, and auto theft), but also included manslaughter by negligence and larceny under \$50. The FBI stopped reporting the clearance rate for total crimes in 1973.

From 1965 to date, the FBI has published a national clearance rate for the seven index crimes. It is possible to extend the crime index clearance rate backward to 1962 by taking the percentages reported in the crime clearance table and “backing out” manslaughter by negligence and larcenies of less than \$50. However, the reporting format before 1962 does not easily permit this adjustment, because only clearance rates for total larcenies were reported.

From 1965-1972, the index crime of larceny was defined as larceny of \$50 and over. In 1973, the FBI changed the crime index definition to include all larcenies, not just larcenies of \$50 and over.³⁸⁵ This means that there is a discontinuity between the 1972 and 1973 index crime clearance rate series.³⁸⁶

As a result, we analyzed clearance rates for individual crime categories and the aggregate categories of violent and property crimes rather than for the crime index.

2. *Violent and property crimes.*

³⁸⁴. See UCR-1958, *supra* note **Error! Bookmark not defined.**, at 76 tbl.12; Brewer Letter, *supra* note **Error! Bookmark not defined.**, at 1.

³⁸⁵. See UCR-1973, *supra* note **Error! Bookmark not defined.**, at 1; Brewer Letter, *supra* note **Error! Bookmark not defined.**, at 1.

³⁸⁶. Fox appears to ignore this discontinuity. See FOX, *supra* note **Error! Bookmark not defined.**, at 6.

The FBI has reported clearance rate data for “violent” crime (i.e., nonnegligent homicide, rape, robbery, and assault) and “property” crime (i.e., burglary, larceny, and auto theft) from 1969 to date, although from 1969-1972 the property crime category appears to include only clearance of larcenies \$50 and over. For pre-1969 data, we used Dean James Alan Fox’s published data for 1950-1974.³⁸⁷ Dean Fox’s data are based on a weighted average of reported material from the FBI. We then switched to the FBI data from 1975 to date. Despite this “splicing,” the Fox compilation corresponded quite well with the reported FBI violent crime data, and corresponded exactly with the property data. For example, in 1974, the last year of Fox’s tabulation, Fox’s violent crime clearance rate was 44.9%; whereas the FBI’s was 45.2%. The 1974 property crime clearance rate as tabulated in both sources was 18.5%.³⁸⁸ The difference in the violent crime category may stem from Fox’s elimination 1957 of statutory rape from the rape category, and hence the violent crime category in 1957.³⁸⁹ In view of the small difference that this appeared to make in the overall violent crime data, we made no adjustment to the post-1975 FBI data.

3. *Individual crimes.*

The FBI has reported clearance data for individual crime categories continuous from 1950 to date. A few caveats should be noted. For the murder clearance rate, we used only “murder and nonnegligent manslaughter,” excluding “manslaughter by negligence,” apparently comprised primarily of traffic fatalities. In addition, while the FBI has continuous reported data on rape, in 1958 it eliminated statutory rape from the total.³⁹⁰ We made no adjustment for this change in the individual rape data, because we ran our data from 1960 forward, thus eliminating this issue.³⁹¹ For larceny, while the FBI has sometimes reported clearance rate data for larcenies \$50 and over, it has consistently reported data for total larcenies. Accordingly, total larceny data, and not the inconsistently defined crime index larceny data, were used for the larceny clearance rate in this study.

³⁸⁷. *Id.* at 86 (based on UCR data).

³⁸⁸. Compare *id.* with UCR-1974, *supra* note **Error! Bookmark not defined.**, at 166.

³⁸⁹. See FOX, *supra* note **Error! Bookmark not defined.**, at 7.

³⁹⁰. See UCR-1958, *supra* note **Error! Bookmark not defined.**, at 2 (limiting rape figures to those rapes involving force).

³⁹¹. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and

4. *City versus agency data.*

From 1950-1979, the FBI only reported clearance rates for cities, not for the entire country. As a result, we used city clearance rate data for the dependent variables in our equations. However, the independent variables generally rest on data not just from cities but from the entire country.

While this slight mismatch might be a problem in theory, in practice we think it is not. The clearance rates for the entire country probably track clearance rates for cities rather closely for three reasons. First, *UCR* figures are crime figures, and cities have a large percentage of the crimes in the country.³⁹² Thus, data from cities substantially affect the *UCR* trends.³⁹³ Second, clearance rates do not vary as greatly from jurisdiction to jurisdiction as crime rates. For example, in 1995 the clearance rate for violent crimes for all agencies was within 5% of the clearance rate for cities, and the clearance rates for property crimes were virtually indistinguishable.³⁹⁴ However, crime rates showed a wider variation.³⁹⁵ Finally, it does not seem unreasonable to assume that trends affecting city clearance rates would generally affect other police agencies as well.

To test the convergence of city clearance data with national clearance data, we compared the two sets of data from 1980, when the FBI began reporting these numbers, to the present. The correlation coefficient for the two series is 0.985. It therefore seems unlikely that our use of city data significantly affects our conclusions.³⁹⁶

accompanying text.

³⁹². See, e.g., UCR-1995, *supra* note **Error! Bookmark not defined.**, at 181 tbl.12 (showing that 80.1% of offenses known to police were reported by cities (percentage derived from *id.*)).

³⁹³. Cf. Fox, *supra* note **Error! Bookmark not defined.**, at 123 n.3 (concluding that since city data is weighed in proportion to population in the *UCR* data, inclusion of small cities should contribute little to the analytic results).

³⁹⁴. See UCR-1995, *supra* note **Error! Bookmark not defined.**, at 199 tbl.25 (reporting clearance rate data for cities of 43.5% for violent crimes and 17.7% for property crimes and data for all agencies of 45.4% for violent crimes and 17.6% for property crimes).

³⁹⁵. See *id.* at 187 tbl.16 (reporting crime rate data, per 100,000 population, for cities of 919.9 for violent crimes and 5793.0 for property crimes and data for all agencies of 746.5 for violent crimes and 4877.8 for property crimes).

³⁹⁶. It is possible that use of city data in our regression equations may have slightly increased the magnitude of the *Miranda* effect that we detected. The available empirical evidence suggests that *Miranda* depressed confession rates more in bigger cities. See Cassell, *supra* note **Error! Bookmark not defined.**, at 447-50 (analyzing the evidence of post-*Miranda* reduction of such rates).

B. *Independent Variables*

1. *Miranda variables.*

Various dummy variables were used to measure *Miranda*'s effects starting around July 1, 1966, as explained in more detail in the text.³⁹⁷

2. *Crime rates.*

Since 1950, the FBI has published an annual total number of crimes for the United States in the *UCR*. Because not all law enforcement agencies reported figures to the FBI during the 1950s, the totals during those years are estimates. In 1975, the FBI undertook a revision of its earlier published figures in order to produce a consistent time series. As a result, a continuous series of revised data from 1960 to date is now available for the FBI's seven index crimes (the "official revision").³⁹⁸ In order to take advantage of the FBI's latest calculation of total crime numbers, we have used the revised series here. It should be noted that even the unrevised data have been used in other criminal justice research.³⁹⁹

Unfortunately, the FBI does not appear to have published a revision of the clearance rates. As a result, we used *revised* crime rate data to explain *unrevised* clearance rate data. This does not affect our conclusions, however, because our other option using unrevised crime rate data would lead to the same results; the unrevised crime rate data do not explain the changes in clearance rates.⁴⁰⁰

³⁹⁷. See notes **Error! Bookmark not defined.**-**Error! Bookmark not defined.** *supra* and accompanying text.

³⁹⁸. The modification is published in UCR-1975, *supra* note **Error! Bookmark not defined.**, at 49 tbl.2, and in some, but not all, subsequent *UCRs*.

³⁹⁹. See, e.g., Jacob & Rich, *supra* note **Error! Bookmark not defined.**, at 122 (using *UCR* city data on robberies for nine cities without any adjustment).

⁴⁰⁰. See Cassell, *supra* note **Error! Bookmark not defined.**, at 334-38 (discussing Schulhofer's reliance upon unrevised crime rate data to construct "clearance capacity" charts, which do not track clearance rate changes).

To extend the revised series back from 1960-1950, we used an FBI revision that has not been formally published (the “unpublished revision”). The revision is reprinted in Brower’s dissertation and extends from 1933-1972.⁴⁰¹ For all of the seven index crimes except larceny, the unpublished revision figures appeared to correspond closely to the FBI’s official revised figures for the overlap period of 1960-1972.⁴⁰² In order to splice the unpublished 1950-1959 data with the official revision, we first determined the ratio of the unpublished revision to the official revision figures for each year for each index crime during the overlap period.⁴⁰³ The mean ratio during the overlap period was then applied to the 1950-1959 unpublished revision data for each index crime.

The unpublished revision for larceny did not appear as suitable for time series analysis for three reasons. First, during the overlap period, the unpublished crime rate was approximately one-third of that in the official revision.⁴⁰⁴ It is unclear whether the changed definition of larceny (e.g., in excess of \$50 only) or some other factor produced this disparity. Second, in contrast to the other crime categories, the ratio of the unpublished revision to the official revision shifted significantly during the overlap period, from 27.3% in 1960 to 44.3% in 1972.⁴⁰⁵

⁴⁰¹. See Brower, *supra* note **Error! Bookmark not defined.**, at 172 tbl.47, 175 tbl.48, 178 tbl.49, 181 tbl.50, 184 tbl.51, 187 tbl.52 & 190 tbl.53.

⁴⁰². See note **Error! Bookmark not defined.** *infra* (providing ratios of official revised figures to unofficial revised figures).

⁴⁰³. The mean ratio of the unofficial revision to official revision for the seven index crimes was: 0.991 for murder; 0.991 for rape; 0.995 for robbery; 0.989 for aggravated assault; 0.987 for burglary; and 0.933 for auto theft. The ratios did not vary much during the overlap period. For instance, for assault the ratios ranged from a low of 0.9887 to a high of 0.9890.

⁴⁰⁴. Compare Brower, *supra* note **Error! Bookmark not defined.**, at 187 tbl.52 (reporting that unpublished revision of larceny for 1960 was 507,300), with UCR-1975, *supra* note **Error! Bookmark not defined.**, at 49 tbl.2 (reporting that official revision of larceny for 1960 was 1,855,400).

⁴⁰⁵. The 1960 ratio was $507,300 / 1,855,400 = 27.3\%$; the 1972 ratio was $1,837,800 / 4,151,200 = 44.3\%$. Compare Brower, *supra* note **Error! Bookmark not defined.**, at 187 tbl.52 (providing unpublished revised data), with UCR-1975, *supra* note **Error! Bookmark not defined.**, at 49 tbl.2 (providing official revised data).

In view of these difficulties with the unpublished revision of the larceny data, we used an alternative methodology to construct a data series of estimated total larcenies for 1950-1959. The FBI had earlier published estimates of total larcenies in the United States between 1950 and 1957, but this series is also not directly comparable to the official revision.⁴⁰⁶ To splice these two data sets together, we started from the 1960 total larcenies reported in the official revision⁴⁰⁷ and then used the reported change in the rate of total larcenies for U.S. cities to recalculate the totals back to 1956.⁴⁰⁸ We then compared the 1956 and 1957 numbers in this series to the FBI's earlier estimated larceny data for 1956 and 1957. A ratio for each of the two years was determined and then averaged. We then applied the average ratio the 1950-1955 estimated data to complete the series.

With the number of crimes determined, we then calculated crime rates by dividing by resident population for each year. The official FBI revision does this for 1960 to date. We used Census data for the years 1950-1959 to calculate crime rates during that period.

3. *Number of police personnel.*

⁴⁰⁶. See UCR-1958, *supra* note **Error! Bookmark not defined.**, at 1-3 (noting a change in estimation techniques and larceny definition).

⁴⁰⁷. See UCR-1975, *supra* note **Error! Bookmark not defined.**, at 49 tbl.2 (reporting total larcenies for 1960 as 1,855,400).

⁴⁰⁸. See UCR-1960, *supra* note **Error! Bookmark not defined.**, at 79 tbl.5 (reporting an 11.7% increase in total larcenies in 1960) (percentage derived from *id.* by combining larceny under and over \$50 and calculating the percent increase); UCR-1959, *supra* note **Error! Bookmark not defined.**, at 75 tbl.5 (reporting a 0.8% decrease in total larcenies in 1959) (percentage derived from *id.* by combining larceny under and over \$50 and calculating the percent increase); UCR-1958, *supra* note **Error! Bookmark not defined.**, at 69 tbl.5 (reporting a 9.4% increase in total larcenies in 1958) (percentage derived from *id.* by combining larceny under and over \$50 and calculating the percent increase); UCR-1957, *supra* note **Error! Bookmark not defined.**, at 79 tbl.26 (reporting an 8.0% increase in total larcenies in 1957). Data for cities rather than the entire country were used because they were consistently reported for total larcenies, not just larcenies over \$50, in urban data.

Using national estimates rather than city data would produce only a modest change in numbers used here. See UCR-1960, *supra* note **Error! Bookmark not defined.**, at 35 tbl.2 (reporting a 14.2% increase in larcenies over \$50 nationwide in 1960); UCR-1959, *supra* note **Error! Bookmark not defined.**, at 4 (reporting a 3% increase in larcenies over \$50 nationwide in 1959); UCR-1958, *supra* note **Error! Bookmark not defined.**, at 1 (reporting a 10.3% increase in larcenies over \$50 nationwide in 1958); UCR-1957, *supra* note **Error! Bookmark not defined.**, at 71 tbl.24 (reporting an 8.4% increase in total larcenies nationwide in 1957).

Basic data for the number of police personnel come from FBI figures, which report the number of full-time law enforcement employees, including civilians, per one thousand inhabitants for all cities.⁴⁰⁹ This series has been used in other criminal justice research.⁴¹⁰

Because of variation in the dates at which the number of employees was determined (e.g., July 1, October 31, or December 31), Dean Fox standardized the figures to July 1 from 1950-1974.⁴¹¹ This article uses the Fox figures for these years to ensure consistency during the critical period surrounding *Miranda*. For the years from 1975-1993, the FBI figures were used, although they were determined as of October 31. We did not attempt to adjust these figures to July 1, because we found that the Fox standardized figures mirrored the FBI figures for the overlap years of 1964-1974. In addition, this article uses the rate of police personnel rather than the total number of personnel to eliminate spikes in the data that would otherwise occur from differences in the number of agencies reporting.⁴¹²

⁴⁰⁹. For the early 1950s, this information appears in a table labeled "Number of police department employees." See, e.g., Semiannual Bulletin, *supra* note **Error! Bookmark not defined.**, at 21 tbl.11. For the years from 1958-1971, this information appears in a table labeled "Full-Time Police Department Employees." See, e.g., UCR-1971, *supra* note **Error! Bookmark not defined.**, at 158 tbl.51. For the years after 1971, the table is labeled "Full-Time Law Enforcement Employees." See, e.g., UCR-1972, *supra* note **Error! Bookmark not defined.**, at 161 tbl.54.

⁴¹⁰. See, e.g., Jacob & Rich, *supra* note **Error! Bookmark not defined.**, at 122 app. (using the UCR's figures for number of sworn officers).

⁴¹¹. See FOX, *supra* note **Error! Bookmark not defined.**, at 6 (using the "number of full-time law enforcement employees per 1000 population").

⁴¹². For instance, the reported number of police department employees went from 186,539 in 1959 to 195,109 in 1960 to 189,093 in 1961. However, the rate of police employees per thousand inhabitants during those years remained relatively constant, moving from 1.8 in 1959 to 1.9 in 1960 to 1.9 in 1961. See UCR-1959, *supra* note **Error! Bookmark not defined.**, at 105 tbl.23; UCR-1960, *supra* note **Error! Bookmark not defined.**, at 105 tbl.31; UCR-1961, *supra* note **Error! Bookmark not defined.**, at 108 tbl.33.

In 1980, the FBI began reporting data not only for city police but also for police throughout the country (denominated “total agencies”).⁴¹³ While the total agency percentage covers a substantially larger portion of the population,⁴¹⁴ this article uses only the police personnel figures for cities to maintain consistency in the time trend and also to maintain consistency with the clearance rate numbers, which also come from city data. This approach does not appreciably influence our results because the reported police rates for total agencies roughly track the city data.⁴¹⁵

4. *Dollars spent on police protection.*

A data series on annual government expenditures for police protection year is available from the U.S. Bureau of the Census compilation of government finances.⁴¹⁶ This series has been used in other criminal justice research.⁴¹⁷ The particular series used here was developed by adding local expenditures to state expenditures. Federal expenditures were not included because they are not likely to be closely associated with crime clearance rates for index crimes in major cities. A missing observation for 1951 was derived by averaging the immediately preceding and following observations. Data for 1994 and 1995 were derived by assuming a continuation of the rate of expenditure growth from 1990-1993. The series includes Alaska beginning in 1959 and Hawaii beginning in 1960.⁴¹⁸ However, because these states’ overall expenditures were such a small fraction of the total, no adjustment appeared necessary.

⁴¹³. See UCR-1980, *supra* note **Error! Bookmark not defined.**, at 264 tbl.68.

⁴¹⁴. In 1980, for instance, total agency population was 211 million; total city population was 143 million. See *id.* at 262, 264.

⁴¹⁵. See, e.g., *id.* at 264 tbl.68; UCR-1995, *supra* note **Error! Bookmark not defined.**, at 248 tbl.74; UCR-1990, *supra* note **Error! Bookmark not defined.**, at 242 tbl.69.

⁴¹⁶. For 1950-1970, the data are available in 1 BUREAU OF THE CENSUS, *supra* note **Error! Bookmark not defined.**, at 416 ser.H:1012-1027. For later years, the data are available in BUREAU OF THE CENSUS, U.S. DEP’T OF COMMERCE, GOVERNMENT FINANCES, Series GF, No. 5, which is in turn sometimes reprinted in the annual BUREAU OF THE CENSUS, U.S. DEP’T OF COMMERCE, STATISTICAL ABSTRACT OF THE UNITED STATES (hereinafter STATISTICAL ABSTRACT-[year]). Where revisions of earlier data appeared, we used the revisions. Since 1962, the data are for government fiscal years, which generally end June 30 (e.g., the data for 1994 would be for the fiscal year ending in June 1994). This means that the data do not align precisely with the crime variables in our equations, which rest on calendar year compilations.

⁴¹⁷. See, e.g., FOX, *supra* note **Error! Bookmark not defined.**, at 5, 127 n.5; Jacob & Rich, *supra* note **Error! Bookmark not defined.**, at 122 app.; Pogue, *supra* note **Error! Bookmark not**

The Consumer Price Index (“CPI”) was used to adjust expenditures for inflation. While the CPI’s accuracy is debated,⁴¹⁹ it appears to be a logical adjusting measure.⁴²⁰

5. *Socioeconomic variables.*

Several socioeconomic variables were included in the equations. The unemployment rate is the standard annual measurement collected by the U.S. Bureau of Labor Statistics.⁴²¹ It is expressed as the percentage of the civilian labor force, aged sixteen or older, that is unemployed.

defined., at 44 app..

⁴¹⁸. See U.S. DEP’T OF COMMERCE, GOVERNMENT FINANCES IN 1962, G-GF62 No. 2, at 19 n.1 (1963).

⁴¹⁹. See ADVISORY COMM’N TO STUDY THE CONSUMER PRICE INDEX, FINAL REPORT TO THE SENATE FINANCE COMM.: TOWARD A MORE ACCURATE MEASURE OF THE COST OF LIVING (1996); cf. Scott H. Decker & Carol W. Kohfeld, *Fox Reexamined: A Research Note Examining the Perils of Quantification*, 19 J. RES. CRIME & DELINQ. 110, 117-20 (1982) (debating whether the CPI is a useful predictor of crime rates); Fox, *supra* note **Error! Bookmark not defined.**, at 126 (debating same in response).

⁴²⁰. See FOX, *supra* note **Error! Bookmark not defined.**, at 127 n.6 (finding the CPI to be a logical adjuster, but using an alternative measure because the CPI was to be used as a predictor in that particular regression model).

⁴²¹. See BUREAU OF LABOR STATISTICS, U.S. DEP’T OF LABOR, EMPLOYMENT AND EARNINGS (January issues, published annually); see also STATISTICAL ABSTRACT, *supra* note **Error! Bookmark not defined.**

The civilian labor force participation rate comes from the U.S. Bureau of Labor Statistics and reflects the employment status of the civilian population.⁴²² Income numbers come from the U.S. Bureau of Economic Analysis.⁴²³ They reflect disposable personal income per capita, in constant 1987 dollars. This measure has been used in other criminal justice research.⁴²⁴ Data on live births to unmarried women come from the U.S. National Center for Health Statistics, which measures births to residents based on marital status.⁴²⁵ The percentage of the resident population residing in urban areas was derived by fitting a third-degree polynomial function to the decadal data from the U.S. Bureau of the Census.⁴²⁶

6. *Crime in small cities.*

The percentage of violent crime in small cities was added to respond to a criticism of our earlier model raised by Professor Schulhofer.⁴²⁷ Accordingly, we tracked his specification of the variable, which was “the portion of the total [of violent crimes] that was reported in cities with less than 250,000 residents.”⁴²⁸ Professor Schulhofer kindly provided us with his data on this series, which he extracted from the FBI’s *UCR*.⁴²⁹

7. *Population variables.*

⁴²². See, e.g., STATISTICAL ABSTRACT-1995, *supra* note **Error! Bookmark not defined.**, at 399 tbl.627.

⁴²³. See generally BUREAU OF ECON. ANALYSIS, U.S. DEP’T OF COMMERCE, SURVEY OF CURRENT BUSINESS; see also STATISTICAL ABSTRACT, *supra* note **Error! Bookmark not defined.**

⁴²⁴. See e.g., Fowles & Merva, *supra* note **Error! Bookmark not defined.** (discussing the correlation between wage inequality and crime and reviewing relevant literature).

⁴²⁵. See, e.g., *id.* at 77 tbl.94 (reprinting National Center for Health Statistics data).

⁴²⁶. See STATISTICAL ABSTRACT-1975, *supra* note **Error! Bookmark not defined.**, at 19 tbl.20 (reporting that the urban population of the U.S. was 64.0% in 1950, 69.9% in 1960, and 73.5% in 1970); STATISTICAL ABSTRACT-1995, *supra* note **Error! Bookmark not defined.**, at 43 tbl.44 (reporting that urban population of the U.S. was 73.7% in 1980 and 75.2% in 1990). The function was of the form $y = 0.0003x^3 - 0.0719x^2 + 5.81x - 84.449$.

⁴²⁷. See note **Error! Bookmark not defined.** *supra* and accompanying text.

⁴²⁸. Schulhofer, *Bashing Miranda*, *supra* note **Error! Bookmark not defined.**, at 367.

⁴²⁹. We extended Schulhofer’s series to 1995 by calculating data for that year. See *UCR-1995*, *supra* note **Error! Bookmark not defined.**, at 187 tbl.16 (providing relevant data in table entitled “Crime Rates, Offense Known to the Police, Population Group”).

Population data comes from the U.S. Bureau of the Census.⁴³⁰ The series for resident population was used, consistent with the *UCR*.⁴³¹ The population data was used to generate a crime rate, officer rate, dollars-on-law-enforcement rate, juvenile rate, and illegitimate birth rate.

Data on the population aged fifteen to twenty-four comes from the U.S. Bureau of the Census.⁴³² In 1959, Alaskan and Hawaiian figures were included. This made a small difference in size of the percentage, increasing it by roughly 0.4%. To eliminate, albeit imperfectly, this small incompatibility, the ratio of the population with fifty states to the population with forty-eight states was determined for 1959. The 1950-1958 forty-eight-state population percentage was then multiplied by that ratio. During the 1970s, the Census Bureau appears to have categorized their age group differently, reporting data on the age groups 14-24, rather than for 15-24. For these years, we calculated the ratio of persons aged 14-24 to persons aged 15-24 for 1970 and 1980. We then estimated population data for the 1970s by assuming that the average moved constantly between the two ratios during the decade.⁴³³

8. *Time trend variable.*

A time trend variable was constructed using standard statistical techniques.

⁴³⁰. See STATISTICAL ABSTRACT, *supra* note **Error! Bookmark not defined.**

⁴³¹. See UCR-1995, *supra* note **Error! Bookmark not defined.**, at 58 tbl.1, n.1 (indicating that the population data was supplied by the Bureau of the Census).

⁴³². See STATISTICAL ABSTRACT, *supra* note **Error! Bookmark not defined.**

⁴³³. In 1970, the ratio was 1.1 (40.3 million to 35.50 million). In 1980, the ratio was 1.09 (46.5 million to 42.5 million). See STATISTICAL ABSTRACT-1981, *supra* note **Error! Bookmark not defined.**, at 26 tbl.29 & 27 tbl.30. We assumed that the ratio closed constantly from 1970-1980.

C. *Miranda and the Relative Importance of the Variables*

As shown previously, the *MIRANDA* variable is robust with respect to model specification.⁴³⁴ Additionally, we tested the statistical importance of the *Miranda* effect from a predictive perspective using a forward stepwise regression procedure. Stepwise regression is an automatic search mechanism for determining an optimal specification based on a sequence of regression models.⁴³⁵ The forward search method begins by finding the most important explanatory variable in terms of reducing the total variation of the dependent variable. At each stage, an explanatory variable is considered for inclusion and the program examines whether or not any of the preceding variables should be dropped in light of the new performance of the most recent variable.⁴³⁶

As shown in Table IX (violent crimes) and Table X (property crimes), the *MIRANDA* variable ranked high in order of explanatory power (in the top three) in the regression models for which a statistically significant *Miranda* effect was found. This supports the hypothesis that *Miranda* is a relatively important factor in explaining these clearance rates.

⁴³⁴. See notes **Error! Bookmark not defined.-Error! Bookmark not defined.** *supra* and accompanying text.

⁴³⁵. See JOHN NETER, MICHAEL H. KUTNER, CHRISTOPHER J. NACHTSHEIM & WILLIAM WASSERMAN, *APPLIED LINEAR REGRESSION MODELS* 347-52 (1996) (discussing the merits of the procedure and explaining its mechanics).

⁴³⁶. See *id.* at 348-52.

TABLE IX. EXPLANATORY VARIABLES BY ORDER OF IMPORTANCE:
 VIOLENT CRIMES (1950-1995)
 Stepwise OLS Regressions on Clearance Rates for Cities

Variable	Violent	Murder	Rape	Robbery	Assault
1st	MIRANDA	TREND OVER TIME	PERSONNEL CAPACITY	MIRANDA	CRIME RATE
2nd	CRIME RATE	CRIME IN SMALL CITIES	MIRANDA	CRIME RATE	BIRTHS TO UNMARRIED
3rd	DOLLAR CAPACITY (REAL)	URBANIZATION	CRIME IN SMALL CITIES	UNEMPLOYMENT	UNEMPLOYMENT
4th	TREND OVER TIME	CRIME RATE	JUVENILE	PERSONNEL CAPACITY	JUVENILES
5th	CRIME IN SMALL CITIES	JUVENILES	TREND OVER TIME	CRIME IN SMALL CITIES	DOLLAR CAPACITY (REAL)
6th	POLICE PERSONNEL	UNEMPLOYMENT	POLICE DOLLARS	BIRTHS TO UNMARRIED	POLICE PERSONNEL
7th	JUVENILES	POLICE PERSONNEL	POLICE PERSONNEL	DOLLAR CAPACITY (REAL)	POLICE DOLLARS
8th	URBANIZATION	MIRANDA	—	TREND OVER TIME	PER CAPITA INCOME (REAL)
9th	LABOR FORCE PARTICIPATION	—	—	POLICE DOLLARS	TREND OVER TIME
10th	UNEMPLOYMENT	—	—	PER CAPITA INCOME (REAL)	CRIME IN SMALL CITIES
11th	PER CAPITA INCOME (REAL)	—	—	POLICE PERSONNEL	—
12th	PERSONNEL CAPACITY	—	—	—	—

Blank boxes indicate no other variable met the 0.500 significance level for entry into the model.

TABLE X. EXPLANATORY VARIABLES BY ORDER OF IMPORTANCE:
PROPERTY CRIMES (1950-1995)
Stepwise OLS Regressions on Clearance Rates for Cities

Variable	Property	Burglary	Larceny	Vehicle
1st	PERSONNEL CAPACITY	CRIME RATE	JUVENILES	CRIME RATE
2nd	MIRANDA	UNEMPLOYMENT	CRIME IN SMALL CITIES	MIRANDA
3rd	DOLLAR CAPACITY	MIRANDA	MIRANDA	CRIME IN SMALL CITIES
4th	TREND OVER TIME	TREND OVER TIME	UNEMPLOYMENT	LABORFORCE PARTICIPATION
5th	UNEMPLOYMENT	DOLLAR CAPACITY	URBANIZATION	BIRTHS TO UNWED WOMEN
6th	BIRTHS TO UNWED WOMEN	POLICE DOLLARS	PER CAPITA INCOME	URBANIZATION
7th	PER CAPITA INCOME	JUVENILES	PERSONNEL CAPACITY	TREND OVER TIME
8th	URBANIZATION	URBANIZATION	DOLLAR CAPACITY	JUVENILES
9th	CRIME IN SMALL CITIES	PERSONNEL CAPACITY	TREND OVER TIME	PER CAPITA INCOME
10th	UNEMPLOYMENT	PER CAPITA INCOME	CRIME RATE	POLICE PERSONNEL
11th	PER CAPITA INCOME (REAL)	LABORFORCE PARTICIPATION	—	UNEMPLOYMENT
12th	PERSONNEL CAPACITY	POLICE PERSONNEL	—	—
13th	—	BIRTHS TO UNWED WOMEN	—	—

Blank boxes indicate no other variable met the 0.500 significance level for entry into the model.

D. *Components of the Clearance Rate*

As discussed previously,⁴³⁷ a clearance rate consists of two components: (1) arrests per crime and (2) clearances per arrest.

$$\frac{\text{cleared crimes}}{\text{crimes}} = (1) \frac{\text{arrests}}{\text{crimes}} \times (2) \frac{\text{cleared crimes}}{\text{arrests}}$$

While analytically this formula is straightforward,⁴³⁸ finding a consistent data series for arrests is problematic. For instance, Professor Fox's tabulation of FBI data reveals some significant gyrations from 1959-1963.⁴³⁹ Fox's arrest data also appear to be flawed during the critical 1965-1966 juncture. He reports that arrest rates for violent crimes increased from 110.6 per 100,000 in 1965 to 139.9 per 100,000 in 1966,⁴⁴⁰ a striking 26% increase for a year in which crime rates themselves went up only 11%.⁴⁴¹ Fox probably never ironed out fluctuations in the arrest data because they were ultimately excluded from his model.⁴⁴²

⁴³⁷. See note **Error! Bookmark not defined.** *supra* and accompanying text.

⁴³⁸. A complicating factor is the possibility that police can claim a clearance "by exception" when some element beyond police control precludes arrest and prosecution (e.g., a victim's failure to cooperate). See 1966 UCR HANDBOOK, *supra* note **Error! Bookmark not defined.**, at 50. Such circumstances will be captured in the clearances per arrest component; ideally they should be broken out separately. Unfortunately, no regularly published national data on exceptional clearances are available. Data from Chicago for 1966 suggest that exceptional clearances are more likely for crimes for which we failed to find a *Miranda* effect (e.g., rape, aggravated assault) than for crimes for which we found a *Miranda* effect (e.g., robbery, burglary). See ALBERT J. REISS, JR., THE POLICE AND THE PUBLIC 81 tbl.2.3 (1971). Therefore, we do not expect that the inclusion of exceptional clearances has had any influence on our finding a *Miranda* effect.

⁴³⁹. The arrest rate for violent crimes inexplicably fluctuates wildly from 1959-1963. The violent crime arrest rate per 100,000 population is 92.2 in 1959, 112.8 in 1960, 117.8 in 1961, 112.8 in 1962, and 97.0 in 1963. See FOX, *supra* note **Error! Bookmark not defined.**, at 86 tbl.A-1, col.EV.

⁴⁴⁰. See *id.*

⁴⁴¹. See UCR-1966, *supra* note **Error! Bookmark not defined.**, at 4.

⁴⁴². See FOX, *supra* note **Error! Bookmark not defined.**, at 127 n.9.

Fortunately, the FBI has published a revised series of arrest rates for 1965-1992 that appears to be internally consistent.⁴⁴³ To derive (1), arrests per crime, we converted the arrest rates for violent crimes to actual arrests for violent crimes by multiplying by Census Bureau population data. The number of arrests per violent crime was then derived by dividing by the FBI's reported number of violent crimes.⁴⁴⁴

To derive (2), clearances per arrest, we multiplied the clearance rate by the FBI's reported number of violent crimes. We then divided by the number of actual arrests for violent crimes as determined above.

These conversions required certain assumptions, particularly with respect to the number of clearances per arrest. As with other aspects of our study, we assumed that the clearance rate from cities is comparable to that from other parts of the country.⁴⁴⁵ We also assumed that the cities used for the clearance rate are comparable to the cities used for the arrest rate. The revised arrest rate series relies on a larger population base than does the clearance rate series.⁴⁴⁶ There appears to be no way to determine the extent to which these issues might confound our analysis of clearances per arrest. However, the percentage for arrests per crime is not affected by either of these problems.

⁴⁴³. See UNIFORM CRIME REPORTING PROGRAM, U.S. DEP'T OF JUSTICE, AGE-SPECIFIC ARREST RATES AND RACE-SPECIFIC ARREST RATES FOR SELECTED OFFENSES 1965-92, at 173 (1993) [hereinafter AGE-SPECIFIC ARREST RATES AND RACE-SPECIFIC ARREST RATES]. The number of agencies reporting in each of the various years does vary, but the extent of variation appears to be relatively small. See *id.* at 205-07.

⁴⁴⁴. We used the FBI's revised series of crimes for this calculation. See notes **Error! Bookmark not defined.-Error! Bookmark not defined.** *supra* and accompanying text (discussing the revised series).

⁴⁴⁵. See notes **Error! Bookmark not defined.-Error! Bookmark not defined.** *supra* and accompanying text (discussing the relative soundness of this assumption).

⁴⁴⁶. Compare, e.g., UCR-1966, *supra* note **Error! Bookmark not defined.**, at 100 tbl.12 (reporting 1966 clearance rate data based on 2857 cities with a population of 99.4 million), with AGE-SPECIFIC ARREST RATES AND RACE-SPECIFIC ARREST RATES, *supra* note **Error! Bookmark not defined.**, at 205 (reporting 1966 arrest rate based on 4048 cities with population of 132.4 million).