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

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SUMMARY:

... In this article, Professors Cassell and Fowles find this claim to be supported by FBI data on crime clearance rates. ... Part II then considers the only available long-term measure of Miranda's effects: crime clearance rate data. ... 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%. ... The clearance rate appears to be a reasonable if understated surrogate measure for the confession rate. ... 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. ... In 1995, for example, the violent crime clearance rate was 45.4%. ... There is no reason to suspect, for example, that a heroin-induced robbery would be harder to solve than other robberies. ... 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. ... As a result, we used revised crime rate data to explain unrevised clearance rate data. ...

TEXT:

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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.

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* n1

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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. n2 *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" n3 and prevent the prosecution of countless dangerous criminals. n4 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." n5 Justice White likewise predicted that "in 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." n6 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." n7 And, during the 1968 presidential campaign, then-candidate Richard Nixon charged that *Miranda* "had the effect of seriously [*1058] ham stringing [sic] the peace forces in our society and strengthening the criminal forces." n8

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. n9 In what has been aptly described as an "empirical desert," n10 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 enforcement n11 and that "the *Miranda* dissenters' fears [of harm to law enforcement] did not prove justified." n12 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." n13 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. n14

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 [*1059] protect both the defendant's and society's interests." n15 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. n16

Justice Harlan's dissent was surely right on one point: that "only time [could] tell" what the real effects of Miranda would be. n17 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. n18 Although some have suggested that questions about Miranda's effects "probably lie beyond the capacity of social science methodology to answer," n19 we are not so skeptical. Social science has powerful analytic tools that can and should be brought to bear on this question.

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 [*1060] 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. n20 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. n21 For example, a 1967 research effort in Pittsburgh revealed that confession rates there fell from 49% before the decision to 30% after. n22 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, n23 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). n24

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. n25 Moreover, because the studies rely on data from the months immediately surrounding Miranda, they fail to capture Miranda's long-term effects. n26 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. n27 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. n28

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%. n29 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%. n30 Richard Leo's 1993 study from Berkeley, California found an in-custody questioning success rate by detectives of 64%. n31 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%. n32 A 1977 study of six cities reported a "confession" rate of 40.3%. n33

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. n34 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. n35 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"). n36 Because of this extended range of data, clearance rates might permit a broad perspective on Miranda's effects.

The clearance rate appears to be a reasonable if understated surrogate measure for the confession rate. n37 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. n38 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, "Many 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 [*1064] questioning of other criminal suspects." n39 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." n40 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. n41

Clearance rates have been widely viewed as statistics that would reveal Miranda's effects - especially by defenders of Miranda. n42 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." n43 Many other supporters of Miranda have likewise viewed clearance rates as an appropriate indicator of Miranda's benign effects. n44 Similarly, law enforcement officers at the time of Miranda believed that the decision would lower their clearance rates. n45 We have been unable to locate any article arguing that clearance rates would not capture at least some of the decision's effects. n46

[*1065] 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. n47 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. n48 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. n49 The FBI stopped reporting charging figures in the 1970s.

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. n50 [*1066] However, conviction rates would probably miss many of Miranda's effects. n51 More significantly, conviction rate data in this country are notoriously bad. n52 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. n53

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. n54 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 "study after study confirmed this trend." n55 Although Schulhofer has since repudiated this position, n56 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. n57

[*1067] 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. n58 Neither of these studies provide support for the thesis that clearance rates have returned to pre-Miranda levels. n59 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. n60 He attributed the drop in part to "recent Supreme Court decisions that had limited the admissibility of confessions in court." n61 In "Seaside City," California, crime clearance rates dropped about 3% after Miranda. n62

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" crimes n63 was "virtually unchanged from 1964." n64 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. n65 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. n66 The 1966 drop in clearances was "universally reported by all population groups and by all geographic divisions." n67 The following year, 1967, the UCR continued to report widespread bad news. n68 Again a clearance rate drop was "universally reported by all population groups and by all geographic divisions." n69 In 1968, the UCR acknowledged another fall in clearance rates. n70 In 1969, the UCR reported that most clearance rates declined slightly, n71 and in 1970 the UCR reported that clearance rates were unchanged. n72 Clearance rates have remained roughly stable since 1970. n73

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 [*1069] 47% in 1968. n74 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 rules n75 - simple 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, n76 violent crime clearance rates in fact have been permanently depressed since the decision.

[SEE FIGURE IN ORIGINAL] [*1070] 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.

[SEE FIGURE IN ORIGINAL] [*1071]

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. n77 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. n78

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. n79 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. n80 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 rate n81 will [*1072] now, consistent with their methodological approach, rethink their position and agree that Miranda was indeed harmful to police efforts.

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. n82 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. n83 Comparison of a control group with a subject group is thus not possible. n84

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 "professional econometric studies of the impact of legal rules almost invariably rely on analysis of cross-sectional, not aggregate, data." n85 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. n86 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. n87 It would make no sense to [*1073] apply that methodology here. There simply are no "control" jurisdictions unaffected by Miranda to observe, n88 at least in this country. n89 Moreover, as a practical matter, obtaining appropriate cross-sectional data for the variables in our equations appears to be impossible, n90 and such data as exist might be contaminated by other problems. n91

In any event, interrupted time series analysis is quite appropriate for assessing the effect of a legal reform. n92 This statistical technique is commonly [*1074] used for assessing the effects of legal changes. n93 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. n94

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. n95 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. n96

[*1075] 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. n97 Unsurprisingly, clearance rate statistics have been attacked on the grounds that police can manipulate them in an effort to look good, n98 or that police may simply disagree as to what crimes should be regarded as "cleared." n99 A study in New York City, for example, found that the number of clearances per burglary varied widely from precinct to precinct, probably because individual police commanders defined clearances differently. n100 Another study found large variances in the way that clearances were recorded. n101

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. n102 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. n103 We also report results for the aggregated categories of clear- [*1076] ance rates for "violent crimes" and "property crimes," for which inter-jurisdictional variations in categorizing crimes should be minimized. n104

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. n105 We defer discussion of other issues surrounding our data to the Appendix. n106

What factors influence clearance rates? The literature on clearance rates is surprisingly thin, particularly in assessing clearance rates over time. n107 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. n108 The available literature does, however, offer a few possible control variables.

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 them n109 - variously called the "overload" theory, n110 the "overtaxing" theory, n111 or the "system strain" the- [*1077] ory. n112 Although the theory has intuitive appeal, the empirical support is mixed. n113 Moreover, crime rates rose throughout the 1960s and later, a pattern that does not correspond to the sharp 1966-1968 decline in clearance rates. n114 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 to 1950, as explained at greater length in the Appendix. n115 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. n116 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. n117 To control for any influences [*1078] these factors may have, we collected data on the number of law enforcement employees per capita ("POLICE PERSONNEL") n118 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)").

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. n119 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. n120

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. n121 Most crimes will be committed by [*1079] persons who are in adolescence or early adulthood. n122 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. n123 The age band commonly identified with this effect is fifteen to twenty-four, n124 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"). n125 A control variable of this type has been used in other criminal justice research. n126

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. n127 Similarly, changes in income levels and labor force partici- [*1080] pation might be associated with crime rates n128 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, n129 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. n130 Conceivably, urbanization could have some bearing on clearance rates as well. n131 For instance, clearance rates for index crimes are generally higher in smaller cities. n132 A few studies of clearance rates, however, have found slim predictive power in urbanization. n133 To control for the possible effects of urbanization, we added a variable for the percent of the resident population residing in urban areas ("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. n134 Because smaller cities have higher clearance rates, Schulhofer reasons that a shift in the distribution of crimes [*1081] could bias our results. n135 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"). n136

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, n137 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. n138 We discuss alternative assumptions

about when Miranda began to affect clearance rates at greater length below. n139

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. n140 We discuss below whether "omitted" variables could have influenced our conclusions. n141 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.

[*1082] Using standard ordinary least squares ("OLS") regression techniques, it is possible to develop an equation to explain national crime clearance rates as follows:

[SEE TABLE IN ORIGINAL]

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. n142

C. Crime Clearance Rates by Aggregate and Individual Crime Categories

Table I reports our findings for the aggregate categories of violent and property crimes and shows a statistically significant coefficient n143 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%. n144 The regression equations suggest that the rate would have been 52.1% (45.4% + 6.7%) in the absence of Miranda. Similarly, for prop- [*1083] erty 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%). n145

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. n146 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.

[SEE TABLE IN ORIGINAL] [*1084] 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.

[SEE FIGURE IN ORIGINAL]

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.

[*1085]

[SEE FIGURE IN ORIGINAL]

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. The regression results reported in Table II confirm this conclusion. n147

[*1086]

[SEE TABLE IN ORIGINAL]

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.

[*1087]

[SEE FIGURE IN ORIGINAL]

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.

[SEE TABLE IN ORIGINAL] [*1088] [*1089]

D. Explaining the Pattern

Our equations suggest a "Miranda effect" n148 on clearance rates for robbery, larceny, vehicle theft, and burglary, but not homicide, rape, or assault. What could explain this pattern? n149 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. n150 It seems probable that professional criminals would most often commit crimes such as robbery, burglary, and vehicle theft.

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. n151 The ratio of clearances to arrests is well in excess of one for some crimes - specifically burglary, grand larceny, grand larceny vehicle, and robbery. n152 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. n153 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 [*1090] these crimes are, therefore, more susceptible to changes in confession procedures.

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. n154 As a result, maximum "detective-power" is allocated to solve homicides. n155 If Miranda affected clearances generally, one would expect police to respond. To the extent resources affect clearance rates, n156 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. n157

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. n158 Interestingly, the Pittsburgh study found no change in homicide clearances after Miranda and a huge drop in robbery clearance rates. n159 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. n160 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). n161 If the [*1091] 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.

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." n162 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. n163 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. n164 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.

[*1092]

E. Specification of the Equations

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. n165 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." n166

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. n167 While the observers reported improved compliance with Miranda over the summer, they observed full compliance in only a fraction of the interrogations. n168 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." n169 Noncompliance with Miranda was also suggested by a field observation of D.C. police immediately after the decision. n170 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." n171 Finally, a 1969 study of interrogations in Denver reported general police compliance with Miranda, with some excep- [*1093] tions. n172 Interviews with suspects suggested some possible police noncompliance, however. n173 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. n174 Interestingly, all report incomplete implementation.

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, n175 iron-clad evidence on police compliance with Miranda is unavailable. The prevailing view, however, is that police now follow Miranda's requirements n176 and that they began doing so by around 1969. As one scholar who reviewed the available information concluded, implementation problems were largely a "transitional" feature, and "by the end of the 1960s, Miranda advice had become an accepted part of police work." n177 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 [*1094] 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. n178

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*, n179 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," n180 and it is clear that "the scope and meaning of the decision was a matter of strong and widespread disagreement." n181 The limited available evidence suggests that Escobedo had little, if any, effect on confession rates. n182 Nonetheless, it remains possible that Escobedo or its subsequent lower court interpretations n183 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. n184

[*1095] 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. n185 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. n186

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. n187

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. n188

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, [*1096] 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. n189

[SEE TABLE IN ORIGINAL]

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. n190 In short, our findings are not [*1097] 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," n191 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. n192

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.

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. n193

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. n194

[*1098]

[SEE TABLE IN ORIGINAL]

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. n195 Ordinarily, one should use all available data to analyze an issue. n196 It can be instructive, however, to examine whether conclusions change over different time periods because, it has been argued, "if the results of a time series regression analysis are a faithful representation of underlying causal processes, the values of the estimated coef- [*1099] ficients will be independent of the specific time period chosen for the analysis." n197

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. n198 The FBI also revised some of its crime definitions in 1958, meaning that earlier data might not be strictly comparable. n199 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. n200 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." n201 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.

[*1100]

[SEE TABLE IN ORIGINAL]

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." n202 To account for such possibilities, Schulhofer argues that the regression equations must therefore take into account "reserve capacities, lagged responses and the like." n203

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. n204 In contrast, a nonlinear, quantum change model is [*1101] 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. n205 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. n206 But, based on the objectively available data, there was no such capacity. In 1965, for example, roughly 75% of all index crimes were not cleared n207 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 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 [*1102] 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. n208 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. n209 However, the issue posed here is a slightly different one: whether failure to control for possible simultaneity affects our conclusions about the MIRANDA variable.

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. n210 Second, even if simultaneity is an [*1103] 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. n211 In addition, our results are insensitive to the variables that we include or exclude in the equations. n212 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. n213

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. n214 This is a quagmire that we hope, at least for present purposes, to avoid. n215

5. Extreme bounds analysis.

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 [*1104] in an equation. n216 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.

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"). n217 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. n218 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, [*1105] 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. n219

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.

[SEE TABLE IN ORIGINAL]

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. n220 For all of the other criminal justice system [*1106] 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. n221 Evidence of a Miranda effect is accordingly not only statistically significant but also robust with respect to model specification.

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.

[SEE TABLE IN ORIGINAL] [*1107] 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. n222 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%. n223 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. n224 Because there may be some interest in the absolute number of crimes affected, n225 in the last column we estimate how many more crimes would have been cleared in 1995 in the absence of the Miranda effect. n226 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. n227 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.

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. n228 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. n229 We have made an aggressive effort to identify possible "omitted" variables that might influence our conclusions n230 and discuss the various possibilities of such an omission in this section. n231 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.

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." n232 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, n233 but seems an unlikely explanation for relatively sudden shifts in crime clearance rates. n234 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.

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. n235 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. n236 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. n237 They reported that "the detectives unanimously believe [Miranda] will unjustifiably [help the suspect]." n238 They also reported that "the detectives

continually told us that the decision would hurt their clearance rate and they would therefore look inefficient." n239 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. n240 And, in Seaside City, James Witt interviewed forty-three police detectives before 1973. n241 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 statements." n242 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." n243

Concerns about Miranda continue to be expressed by law enforcement. n244 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." n245 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. n246 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. n247 Nonetheless, there are strong reasons for believing that Miranda was the Warren Court decision primarily - although perhaps not exclusively - responsible for declining clearance rates.

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. n248 In the critical 1966-1968 period, they identify, in addition to Miranda, two other Court decisions. The first, *United States v. Wade*, n249 is one of several "lineup" cases decided by the Court in 1967 that suggested that the right to counsel extended to such proceedings. n250 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, n251 with the result that it "is now largely ineffectual." n252 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. n253 The other significant case identified by Ehrlich and Brower is *United States v. Katz*, n254 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," n255 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. n256

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, [*1112] *Hoffa v. United States* n257 dealt with the right to counsel before an indictment. n258 Similarly, *In re Gault* n259 extended various due process procedures to juvenile court trials. n260 *Chapman v. California* n261 set new rules for appellate courts reviewing whether the denial of certain constitutional rights during trial is "harmless error." n262 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. n263 Professor Schulhofer has cited the Court's decisions in *Mapp v. Ohio* n264 and *Gideon v. Wainwright* n265 as possible competing impediments to law enforcement. n266 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. n267 Mapp's main effects, if any, n268 were probably felt in the early 1960s. n269 It is hard to under- [*1113] stand 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.

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, n270 authority that is now restricted. n271 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*, n272 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. n273 Finally, restrictions on police arrests for vagrancy would only indirectly affect clearance for index crimes such as robbery.

[*1114]

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. n274 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. n275 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 connection between drug use and crime is unclear. n276 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. n277

[*1115] 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. n278 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. n279 Any decline in 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,

n280 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. n281

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, n282 while [*1116] 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. n283

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. n284 Our Miranda effect on motor vehicle clearance seems 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. n285 In effect, this claim implies that relations improved rather than deteriorated, because crime reporting is generally linked to greater confidence in the police. n286 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. n287 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 de- [*1117] cline 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. n288

Another possible explanation for the drop in clearance rates would be a change in the definition of crimes "cleared." n289 But FBI data collection criteria appear to have remained the same throughout the relevant period. n290 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." n291 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. n292 While it is possible to cite reports recommending reforms at about the time of Miranda, n293 reform efforts - on record-keeping and other matters - were not narrowly confined to the late 1960s. n294

[*1118] 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, n295 a position that collapsed in the face of FBI data to the contrary. n296 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. n297 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. n298 Subsequently, Schulhofer adopted his current view that, while "many 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." n299 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. n300 To the extent that his claims are

empirically testable, they have been disproven: Our regression analysis suggests that, even 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. n301 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.

[*1119]

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. n302 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. n303 Indeed, this finding remains statistically significant over a variety of alternative formulations. n304 In addition, both the FBI and the police on the street contemporaneously identified Miranda as a hindrance to clearing crimes. n305 These law enforcement reports are corroborated by declining confession rates reported in the before-and-after studies of Miranda's impact, n306 lower confession rates reported in this country after Miranda, n307 and higher confession rates reported in countries not following the Miranda rules. n308

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, n309 a claim that the majority did not refute. n310 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.

[*1120]

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.

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. n311 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. n312 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. n313 By the 1950s, coercive questioning had, according to a leading scholar in the area, "diminished considerably." n314 Thus, the Supreme Court's decision curtailing police interrogation techniques

addressed a problem "that was already fading into the past." n315 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." n316 Furthermore, empirical surveys n317 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 [*1121] in police brutality are independent of the Court and started before *Miranda*." n318

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. n319 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. n320 Such motions, however, appear to have been rare around the time of *Miranda*. n321

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." n322 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. n323 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 question- [*1122] ing 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. n324

B. Components of the Clearance Rate Change

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 "clearance rates can be very misleading because before *Miranda*, there is evidence police were using so-called confessions to 'clear' crimes that were never prosecuted." n325 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. n326

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.

[*1123] 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. n327 Kamisar's ultimate claim is that a change in the second component - clearances per arrest - is primarily responsible for the observed clearance rate drop n328 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. n329 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 1965 n330 to 1991 on this issue are plotted in Figure 6.

[*1124]

[SEE FIGURE IN ORIGINAL]

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, n331 it is hard to read the data as showing a permanent, [*1125] post-Miranda drop in clearance rates stemming only, or even primarily, from changes in clearances per arrest.

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. n332 The paucity of police resources makes this unnecessary diversion a possible concern. n333 Multiple confessions are also useful to help police officers discern the modus operandi (or MO) of professional criminals. n334

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. n335 Without a clearance, they will likely continue to fear that they will be victimized again. n336 Victims also have a hard time putting the crime behind them. Victims understandably want to see justice done, n337 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. n338 For all these reasons, the harm to victims from declining clearances is indeed a legitimate social concern. n339

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 [*1126] 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. n340 Miranda does not generally reduce the number of confessions from four to one; it reduces them to zero by occasionally blocking police interrogation entirely. n341 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. n342 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.

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. n343 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. n344

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. n345 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 [*1127] arrested for felonies. n346 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." n347 Measured through clearances, a quite conservative measure of Miranda's effects, n348 there appear to be more uncleared crimes from Miranda than lost convictions from the search and seizure exclusionary rule. n349 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. n350 While other technical adjustments would also have to be made to strictly compare the two figures, n351 it seems quite likely that Miranda's costs exceed those of the exclusionary rule, especially outside of contraband offenses. n352

Taking the perspective of victims of crime underscores the severity of Miranda's costs. n353 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. n354 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 [*1128] nightmare come alive, or robbery as a ruinous matter." n355 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. n356

Finally, it is a truism that an unnecessary cost is a cost that is too high. n357 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.

Today, with the benefit of thirty years of subsequent interpretations, we know the Miranda mandate is not a constitutional requirement. n358 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*, n359 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." n360 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." n361 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. n362 For many years, the Constitution was generally understood as prohibiting the states from introducing coerced or involuntary statements [*1129] 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. n363

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." n364 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, n365 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, n366 and determined that the equally unspecified "benefits" outweigh them. Even under such charitable assumptions, the Court's conclusion is quite debatable. n367 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. n368 The decision itself seemed to invite the continued exploration of such alternatives, promising that "our decision in no way creates a constitutional straitjacket which will handicap sound efforts at reform." n369 This has proven to be an empty promise. In the quarter of a century since Miranda, reform efforts have been virtually non- [*1130] existent. 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:

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. n370

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. n371 At the same time, even when coupled with limited warnings of rights, videotaping does not appear to significantly depress confession rates. n372 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." n373

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; n374 similar proposals have since been made by Judge Walter Schaefer, Judge Henry Friendly, and Akhil Amar and Renee Lettow. n375 Questioning under the supervision of a magistrate would [*1131] 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 issues n376 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. n377

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. n378 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. n379 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. n380

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." n381 The experiment's results are now in. The clearance rate data collected in this study - coupled with the other evidence concerning Miranda's effect n382 - 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.

[*1133]

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." n383 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.

1. National totals.

From 1958-1972, the FBI published a national clearance rate for total crimes. n384 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. n385 This means that there is a discontinuity between the 1972 and 1973 index crime clearance rate series. n386 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.

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. n387 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%. n388 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. n389 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. n390 We made no adjustment for this change in the individual rape data, because we ran our data from 1960 forward, thus eliminating this issue. n391 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.

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 [*1135] country. n392 Thus, data from cities substantially affect the UCR trends. n393 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. n394 However, crime rates showed a wider variation. n395 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. n396

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. n397

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"). n398 In order to take advantage of the [*1136] 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. n399

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. n400

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. n401 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. n402 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. n403 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. n404 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. n405

[*1137] 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. n406 To splice these two data sets together, we started from the 1960 total larcenies reported in the official revision n407 and then used the reported change in the rate of total larcenies for U.S. cities to recalculate the totals back to 1956. n408 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.

Basic data for the number of police personnel come from FBI figures, which report the number of full-time law enforcement employees, including [*1138] civilians, per one thousand inhabitants for all cities. n409 This series has been used in other criminal justice research. n410

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. n411 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. n412

In 1980, the FBI began reporting data not only for city police but also for police throughout the country (denominated "total agencies"). n413 While the total agency percentage covers a substantially larger portion of the

population, n414 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. n415

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 govern- [*1139] ment finances. n416 This series has been used in other criminal justice research. n417 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. n418 However, because these states' overall expenditures were such a small fraction of the total, no adjustment appeared necessary.

The Consumer Price Index ("CPI") was used to adjust expenditures for inflation. While the CPI's accuracy is debated, n419 it appears to be a logical adjusting measure. n420

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. n421 It is expressed as the percentage of the civilian labor force, aged sixteen or older, that is unemployed.

The civilian labor force participation rate comes from the U.S. Bureau of Labor Statistics and reflects the employment status of the civilian population. n422 Income numbers come from the U.S. Bureau of Economic Analy- [*1140] sis. n423 They reflect disposable personal income per capita, in constant 1987 dollars. This measure has been used in other criminal justice research. n424 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. n425 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. n426

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. n427 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." n428 Professor Schulhofer kindly provided us with his data on this series, which he extracted from the FBI's UCR. n429

7. Population variables.

Population data comes from the U.S. Bureau of the Census. n430 The series for resident population was used, consistent with the UCR. n431 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. n432 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 [*1141] 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. n433

8. Time trend variable.

A time trend variable was constructed using standard statistical techniques.

C. Miranda and the Relative Importance of the Variables

As shown previously, the MIRANDA variable is robust with respect to model specification. n434 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. n435 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. n436

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.

[SEE TABLES IN ORIGINAL] [*1142] [*1143] [*1144]

D. Components of the Clearance Rate

As discussed previously, n437 a clearance rate consists of two components: (1) arrests per crime and (2) clearances per arrest.

[SEE EQUATION IN ORIGINAL]

While analytically this formula is straightforward, n438 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. n439 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, n440 a striking 26% increase for a year in which crime rates themselves went up only 11%. n441 Fox probably never ironed out fluctuations in the arrest data because they were ultimately excluded from his model. n442

Fortunately, the FBI has published a revised series of arrest rates for 1965-1992 that appears to be internally consistent. n443 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 [*1145] number of arrests per violent crime was then derived by dividing by the FBI's reported number of violent crimes. n444

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. n445 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. n446 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.

Legal Topics:

For related research and practice materials, see the following legal topics:

Criminal Law & Procedure Interrogation Miranda Rights Custodial Interrogation Criminal Law & Procedure Interrogation Miranda Rights Self-Incrimination Privilege Criminal Law & Procedure Interrogation Voluntariness

FOOTNOTES:

n1. 384 U.S. 436, 504, 517 (1966) (Harlan, J., dissenting).

n2. 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).

n3. *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).

n4. .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).

n5. *Miranda*, 384 U.S. at 504 (Harlan, J., dissenting).

n6. *Id.* at 542 (White, J., dissenting).

n7. S. Rep. No. 90-1097, at 25 (1968), reprinted in 1968 U.S.C.C.A.N. 2112, 2123.

n8. 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).

n9. 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.").

n10. H. Richard Uviller, *Tempered Zeal: A Columbia Law Professor's Year on the Streets with the New York City Police* 198 (1988).

n11. 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").

n12. Yale Kamisar, *Remembering the "Old World" of Criminal Procedure: A Reply to Professor Grano*, 23 *U. Mich. J.L. Reform* 537, 585 (1990).

n13. 1 Wayne R. LaFare & 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).

n14. 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).

n15. *Moran v. Burbine*, 475 U.S. 412, 433 n.4 (1986).

n16. 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").

n17. *Miranda v. Arizona*, 384 U.S. 436, 504 (Harlan, J., dissenting).

n18. We will not discuss other, perhaps less quantifiable, harms of Miranda. See, e.g., Grano, *supra* note 9, 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).

n19. Caplan, *supra* note 9, at 281.

n20. See Cassell, *supra* note 13. For discussion of this assessment, compare the criticism of this analysis in Schulhofer, *supra* note 16, 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).

n21. 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 9, at 825.

n22. See Cassell, *supra* note 13, 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)).

n23. See Cassell, *supra* note 13, 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 16, 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*).

n24. See Cassell, *supra* note 13, at 437-38. But cf. Schulhofer, *supra* note 16, 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% confession rate drop multiplied by 19% confession necessity rate)).

n25. Leo, *supra* note 14, at 676 n.243.

n26. See Cassell, *supra* note 13, at 450-51.

N27. See, e.g., Schulhofer, *supra* note 16, at 507-10.

n28. See Cassell, *supra* note 20, at 1088-89 (raising this possibility).

n29. 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).

n30. See Cassell & Hayman, *supra* note 29, 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 29, at 944-53.

n31. 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 14.

n32. See Cassell & Hayman, *supra* note 29, at 926-30 (discussing Leo, *Police Interrogation in America*, *supra* note 31, at 255-68). For criticism of the downward adjustment of Leo's figures, see Thomas, *supra* note 29, at 953-54.

n33. See Gary D. LaFree, *Adversarial and Nonadversarial Justice: A Comparison of Guilty Pleas and Trials*, 23 Criminology 289 tbl.2 (1985).

n34. See Cassell & Hayman, *supra* note 29, at 871-76.

n35. See Thomas, *supra* note 29, at 954-56 (raising this possibility).

n36. 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).

n37. See Gerald N. Rosenberg, *The Hollow Hope: Can Courts Bring About Social Change?* 328 (1991) ("One 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 47-49 *infra* and accompanying text.

n38. 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 22, 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.

n39. Fred E. Inbau, John E. Reid & Joseph P. Buckley, *Criminal Interrogation and Confessions* at xiv (3d ed. 1986) (some letters capitalized in original).

n40. Leo, *Police Interrogation in America*, *supra* note 31, at 373.

n41. See Michael Wald, Richard Ayres, David W. Hess, Mark Schantz, Charles H. White- head 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 325-331 *infra* and accompanying text.

n42. See, e.g., Seeburger & Wettick, *supra* note 22, 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).

n43. 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).

n44. 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 11, at 18 n.93 & 19 n.99 (citing clearance and confession rates to argue Miranda did not harm law enforcement); cf. Leo, *supra* note 14, at 645 (suggesting that Miranda has not significantly affected crime clearance rates, although "in some instances" they may have dropped).

n45. See notes 232-245 *infra* and accompanying text (collecting law enforcement views on Miranda and clearance rates).

n46. 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 "differences 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 103 *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) [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).

n47. See Cassell, *supra* note 13, at 398-99 (arguing that clearance rates understate Miranda's adverse effect on law enforcement).

n48. See Cassell & Hayman, *supra* note 29, at 908 tbl.15 (finding a statistically significant difference in prosecutorial charging decisions between suspects who were successfully interrogated and those who were not).

n49. 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 36, at 104 tbl.16 (44,641 persons charged with violent crimes, derived by summing four violent crime categories), with UCR-1967, *supra* note 36, at 109 tbl.16 (50,515 persons charged with violent crimes, derived by summing four violent crime categories).

n50. 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).

n51. See Cassell, *supra* note 13, at 396-98 (discussing why conviction rates cannot show how Miranda hinders the investigative process).

n52. 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").

n53. 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).

n54. See Leo, *supra* note 14, at 645-46 (concluding that the 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").

n55. Schulhofer, *Reconsidering Miranda*, *supra* note 43, at 456.

n56. See note 78 *infra* and accompanying text.

n57. Kamisar, *supra* note 12, 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 43, 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 43, 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 43, at 456)).

n58. See Schulhofer, Fifth Amendment, *supra* note 43, at 954 n.17; Schulhofer, *Reconsidering Miranda*, *supra* note 43, at 456 n.52.

n59. 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 22, at 24.

n60. See Bernard Weinraub, Crime Reports up 72% Here in 1966; Actual Rise Is 6.5%, N.Y. Times, Feb. 21, 1967, at A1.

n61. *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 38, at 79-80 (questioning whether Miranda

could have caused the reported changes in New York's clearance rate).

n62. See Witt, *supra* note 42, 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).

n63. See note 384 *infra* and accompanying text for a description of grand total crimes. We do not consider arson, which was only added to the Crime Index in the 1970s.

n64. UCR-1965, *supra* note 36, 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 36, 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").

n65. See UCR-1966, *supra* note 36, 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.").

n66. See *id.*

n67. *Id.*

n68. See UCR-1967, *supra* note 36, 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")

n69. *Id.*

n70. See UCR-1968, *supra* note 36, 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.").

n71. See UCR-1969, *supra* note 36, at 28.

n72. See UCR-1970, *supra* note 36, at 30.

n73. See, e.g., UCR-1975, *supra* note 36, at 37; UCR-1980, *supra* note 36, at 180; UCR-1985, *supra* note

36, at 154.

n74. 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).

n75. See notes 165-178 *infra* and accompanying text.

n76. See Schulhofer, *Reconsidering Miranda*, *supra* note 43, at 456.

n77. See Schulhofer, *Bashing Miranda*, *supra* note 46, at 356; Schulhofer, *Clearance Rates*, *supra* note 46, at 279.

n78. See Schulhofer, *Bashing Miranda*, *supra* note 46, at 364-72; Schulhofer, *Clearance Rates*, *supra* note 46, at 280-85.

n79. 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.

n80. See Gary A. Mauser & Richard A. Holmes, *An Evaluation of the 1977 Canadian Firearms 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).

n81. See notes 54-57 *supra* and accompanying text.

n82. 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).

n83. 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).

n84. 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 29, at 922 (concluding that Miranda prevented research on important interrogation questions).

n85. Schulhofer, Clearance Rates, *supra* note 46, at 291.

n86. 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 52, 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 46, at 281 n.12 (noting that all graphical data "are drawn from" Fox, *supra* note 36 (providing a national time series econometric equations attempting to forecast crime)).

n87. See Schulhofer, Clearance Rates, *supra* note 46, 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)).

n88. 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 87, 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 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 13, 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).

n89. 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 13, 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 29, 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).

n90. 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 36, at 102 tbl.13. State-by-state clearance data appear to be unpublished for the relevant time period. See Brewer Letter, *supra* note 53, 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").

n91. See Nagin, *supra* note 61, 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 102-105 *infra* and accompanying text (noting the advantages of national clearance rate data).

n92. 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.").

n93. 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 82, at 526-32; Zimring, *supra* note 80, at 171-94.

n94. 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 80, at 381-90 (discussing the observations of Britt et al., *supra*).

n95. 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 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 *Mirandawas* decided.

n96. See Fox, *supra* note 36, at 8 ("Although 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 52, 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").

n97. See James Q. Wilson & Barbara Boland, *The Effect of the Police on Crime*, 12 L. & Soc'y Rev. 367, 368 (1978).

n98. 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 41, 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).

n99. See *Controlling Crime Hearings*, *supra* note 62 (statement of Robert W. Johnson, County Attorney, Anoka County, Minnesota) ("Cleared 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 97, at 368)).

n100. See Peter W. Greenwood, *An Analysis of the Apprehension Activities of the New York City Police Department* 18 (1970).

n101. See Bernard Greenberg, Oliver S. Yu & Karen I. Lang, *Enhancement of the Investigative Function* 10 (1972).

n102. Cf. Yehuda Grunfeld & Zvi Griliches, *Is Aggregation Necessarily Bad?*, 42 *Rev. Econ. & Stat.* 1, 10 (1960) ("Aggregation is not necessarily bad if one is interested in the aggregates.").

n103. See Fox, *supra* note 36, 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.").

n104. 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 90, at 124 n.4 (noting that most econometric studies employ aggregated crime indicators).

n105. See Fox, *supra* note 36, 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."

n106. See Appendix.

n107. 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.").

n108. 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 90, at 174 (concluding after extensive modeling that "the models used to predict the clearance rates in general were very poorly specified").

n109. See Fox, *supra* note 36, 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 108, at 21 (suggesting that the number of clearances might be positively related to the number of crimes, i.e., the number of potential clearances).

n110. Michael Geerken & Walter R. Gove, *Deterrence, Overload, and Incapacitation: An Empirical Evaluation*, 56 *Soc. Forces* 424, 429-31 (1977).

n111. Nagin, *supra* note 61, at 119.

n112. Charles H. Logan, *Arrest Rates and Deterrence*, 56 *Soc. Sci. Q.* 376, 384 (1975).

n113. Compare Pogue, *supra* note 108, 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 90, 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 "the overload theory ... cannot explain the U.S. pattern" of crime).

n114. See Cassell, *supra* note 83, at 307-08 (noting that, although crimes rates rose throughout the 1960s and early 1970s, the drop in clearance rates occurred in a shorter time span); Cassell, *supra* note 46, 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).

n115. See notes 398-408 *infra* and accompanying text.

n116. See, e.g., Fox, *supra* note 36, at 26 (hypothesizing that clearance rates depend on the size of police forces); Bennett, *supra* note 113, at 179 (hypothesizing that "personnel levels and police expenditures intuitively relate to the ability of the police to clear crimes").

n117. See, e.g., Bennett, *supra* note 113, at 187 (reporting cross-national analysis suggesting that police variables have minimal effect on clearance rates); Pogue, *supra* note 108, 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 90, 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).

n118. Ideally, we should control for the number of police personnel on the streets. See Wilson & Boland, *supra* note 97, 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.

n119. See Schulhofer, *Clearance Rates*, *supra* note 46, at 291.

n120. 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 46, 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 46, at 339 tbl.1 (reporting preliminary regression results).

n121. . 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 90, at 30, 163-64 (concluding from the existing literature that the impact of

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

n122. 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).

n123. 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).

n124. See Kamisar, *supra* note 98, at 246 (identifying the 15-24 age band as having the most influence on crime rates); Steffensmeier & Harer, *supra* note 123, at 331 (same).

n125. 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.

n126. 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 103, 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 123, at 336 (same).

n127. 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 36) (arguing that Fox improperly excluded unemployment from crime forecasting models); Wilson & Boland, *supra* note 97, 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 117, at 41 (finding an association between unemployment and crime), with Fox, *supra* note 36, at 29 (reviewing literature and concluding that "the absence of an impact of the unemployment rate on the rate of crime appears at this time to be unequivocal"); Ehrlich, *supra* note 126, 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: 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 104, at 309 (reporting that unemployment rate had little explanatory power in criminal justice models).

n128. See *Effects of Economic Change*, *supra* note 127, 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 104, at 309 (finding that "median income relates significantly with fewer violent crimes but not fewer property crimes").

n129. Cf. Sullivan, *supra* note 90, at 165 (finding a relationship between murder clearances and the percentage of families with single mothers).

n130. See Kamisar, *supra* note 98, at 247.

n131. See Schulhofer, *Bashing Miranda*, *supra* note 46, at 366-68 (noting that "clearance 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).

n132. See UCR-1995, *supra* note 36, 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)).

n133. See Pogue, *supra* note 117, at 33 tbl.5 (finding urbanization not statistically significant in explaining clearance rates and population density related only to robbery clearance rates); Sullivan, *supra* note 90, at 161-62 (finding urban variables "virtually insignificant" in explaining most clearance rates); cf. Pogue, *supra* note 108, 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).

n134. See Schulhofer, *Bashing Miranda*, *supra* note 46, at 366-67 (suggesting this control for violent crimes).

n135. See *id.* at 367.

n136. 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.

n137. 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).

n138. See McDowall et al., *supra* note 80, at 387-88 (defending the use of an abrupt, permanent change model and the law's effective date in gun control time series research).

n139. See text accompanying notes 165-194 *infra*.

n140. 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").

n141. See text accompanying notes 228-301 *infra*.

n142. See text accompanying notes 208-215 *infra*.

n143. 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.

n144. See UCR-1995, *supra* note 36, at 199 tbl.25.

n145. See *id.* (listing the clearance rate for property crimes as 17.6%).

n146. Cf. Lott & Mustard, *supra* note 87, at 7 (raising a similar concern in a gun control study).

n147. 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.

n148. In Part IV below, we discuss in detail why we attribute our results to a Miranda effect rather than some other cause.

n149. 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 216-221 *infra* and accompanying text (presenting extreme bounds analysis of the equations).

n150. See Cassell, *supra* note 13, at 464-66 (citing studies that show that repeat offenders may be less likely to confess than those without criminal records).

n151. See Greenwood, *supra* note 100, at 24 tbl.3.

n152. 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.

n153. For further discussion of this issue, see notes 325-331 *infra* and accompanying text.

n154. See David Simon, *Homicide: A Year on the Killing Streets* 17-19 (1991) (giving an example of this for the Baltimore police).

n155. See Kamisar, *supra* note 98, 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").

n156. See note 117 *supra* (collecting conflicting evidence on this issue); cf. Tables I & II (finding sporadic effect of police resources on clearances).

n157. See Sullivan, *supra* note 90, at 8 (suggesting this possibility).

n158. These figures are derived from subtracting the post-Miranda rates from the pre-Miranda rates listed in Seeburger & Wettick, *supra* note 22, at 11 tbl.1.

n159. See *id.* at 20 (finding overall clearances in Pittsburgh higher after Miranda than before).

n160. See *id.* at 21 tbl.9.

n161. 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.

n162. UCR-1993, *supra* note 36, 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:

[SEE TABLE IN ORIGINAL]

See *id.* at 285 tbls.5.3-5.5.

n163. 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).

n164. 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.

n165. See McDowall et al., *supra* note 79, at 75 (concluding that many social science impacts will be gradual).

n166. Schulhofer, *supra* note 16, at 513.

n167. See Wald et al., *supra* note 41, at 1550-56 (discussed in Cassell, *supra* note 13, at 407-08).

n168. See *id.* at 1550 tbl.3 (showing increasing compliance with Miranda from June through August, but not full compliance in every interrogation).

n169. Richard J. Medalie, Leonard Zeitz & Paul Alexander, *Custodial Police Interrogation in 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 14, at 639.

n170. 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.

n171. *Taping Police Interrogations*, *supra* note 38, at 75-76.

n172. 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.

n173. See *id.* at 30 (reporting allegations by suspects that police continued interrogations after requests for counsel).

n174. 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 46, 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 29, 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 31, 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 20, at 1088 n.26 (collecting citations).

n175. See Cassell & Hayman, *supra* note 29, at 848-49 (finding almost no studies on police interrogations since 1970); Thomas, *supra* note 9, at 822 (same).

n176. See Cassell & Hayman, *supra* note 29, at 887-92 (collecting references and evidence to this effect).

n177. Van Kessel, *supra* note 18, at 102-03; see also Leo, *supra* note 14, 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 29, 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").

n178. 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 13, at 450-51 ("There is some support for the hypothesis that the population has now, in effect, been "Mirandized."").

n179. 378 U.S. 478 (1964).

n180. Yale Kamisar, Wayne R. LaFare & Jerold H. Israel, *Modern Criminal Procedure* 465 (8th ed. 1994).

n181. *Id.*

n182. The evidence is collected in Cassell, *supra* note 13, 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.

n183. 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).

n184. See Schulhofer, Clearance Rates, *supra* note 46, at 291-92 (noting the potential effects of an inappropriate time period selection on regression analysis); see also Britt et al., *supra* note 94, 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 80, at 385-86 (same).

n185. See, e.g., McDowall et al., *supra* note 80, at 386.

n186. See Sir Maurice Kendall & J. Keith Ord, *Time Series* 222 (3rd ed. 1990).

n187. See *id.*

n188. 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 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 174, 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 13, at 494-96.

n189. 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.

n190. Cf. McDowall et al., *supra* note 80, 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).

n191. Schulhofer, *supra* note 16, at 507.

n192. See Kendall & Ord, *supra* note 186, at 222.

n193. See *id.*

n194. Cf. Britt et al., *supra* note 94, 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).

n195. 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 80, at 389-90.

n196. See, e.g., McDowall et al., *supra* note 80, at 389.

n197. 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).

n198. See notes 398-408 *infra* and accompanying text.

n199. See Crime and Its Impact, *supra* note 123, at 20 ("Figures prior to 1958 ... must be viewed as neither fully comparable with nor nearly so reliable as later figures."); UCR-1958, *supra* note 36, 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 36, at 2. It has also been reported that the definition of larceny changed in 1960. See Cohen & Felson, *supra* note 164, at 601.

n200. See note 433 *infra* and accompanying text (noting this problem).

n201. Schulhofer, Clearance Rates, *supra* note 46, at 292.

n202. Id.

n203. Id. at 293.

n204. 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.

n205. See, e.g., Fox, *supra* note 36; 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").

n206. See Schulhofer, *Clearance Rates*, *supra* note 46, at 293.

n207. See UCR-1965, *supra* note 36, at 18.

n208. See generally Gary S. Becker, *Crime and Punishment: An Economic Approach*, 78 *J. Pol. Econ.* 169 (1968); cf. Atkins & Rubin, *supra* note 87, at 20 (finding the creation of the exclusionary rule had an effect on crime rates starting in 1961).

n209. For good introductions to the problem, see Nagin, *supra* note 61, at 117-29, and Swimmer, *supra* note 104, at 293-303.

n210. 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 109, at 398-99 (providing step regression equations finding that rates for some crimes were responsive to clearance rates); Decker & Kohfeld, *supra* note 96, at 15-18 (finding increased clearance rates deter robbery, but not burglary or larceny); Ehrlich & Brower, *supra* note 52, 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 87, 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 97, 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 113, at 849 (using panel models to deal with simultaneity issues and concluding that "aggregate 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 117, 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, 1995) (reviewing the literature on the police effect on crime and concluding that the literature "does not speak with an authoritative voice").

n211. See Ehrlich & Brower, *supra* note 52, at 104-06.

n212. See notes 216-221 *infra* and accompanying text.

n213. 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 90, at 171 (suggesting that certain models "can be used to predict even if they cannot be used to establish the causal relationship").

n214. See, e.g., Cook, *supra* note 213, at 676; Nagin, *supra* note 61, at 129 (hoping that information for dealing with simultaneity issues in deterrence will develop).

n215. 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.

n216. 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).

n217. 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).

n218. For another example of this approach, see Fowles & Merva, *supra* note 216, at 168.

n219. We included in our "doubtful" set all variables for which we had data over the relevant time period.

n220. 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 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.

n221. See Fowles & Merva, *supra* note 216, at 167-68.

n222. We caution against drawing firm conclusions from the aggregate category of "violent" crimes for the reasons noted at note 146 *supra* and accompanying text.

n223. See Table VI.

n224. $24.2\% + (1.7-7.2\%)/24.2\%$.

n225. See note 347 *infra* and accompanying text (noting the Supreme Court's interest in absolute numbers of crimes affected by the exclusionary rule).

n226. 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 396 *infra* and accompanying text (noting the general parallel between nationwide and city data).

n227. See notes 47-49 *supra* and accompanying text.

n228. 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.").

n229. See Britt et al., *supra* note 94, at 362-63 (expounding this point); cf. McDowall et al., *supra* note 80, at 382-84 (explaining that controls are useful but not required for time series inferences).

n230. 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 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 20, 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.

n231. See Ross et al., *supra* note 92, at 504 ("When 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.").

n232. UCR-1967, *supra* note 36, at 30.

n233. Cf. Sullivan, *supra* note 90, at 161 (finding that travel time to work was negatively associated with clearance rates for auto theft).

n234. 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).

n235. 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 46, at 179-99 (recounting the survey results first published in *Police Perceptions*, *supra*).

n236. See Stephens et al., *Police Perceptions*, *supra* note 235, 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.

n237. See Wald et al., *supra* note 41, at 1528.

n238. *Id.* at 1611.

n239. *Id.* at 1612 n.265.

n240. See Gary L. Wolfstone, *Miranda - A Survey of Its Impact*, 7 *Prosecutor* 26, 27 (1971).

n241. Witt's article was published in 1973. See Witt, *supra* note 42, at 334. He appears to have begun collecting his data sometime after 1968.

n242. *Id.* at 325.

n243. *Id.* at 330.

n244. See Cassell, *supra* note 20, 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) ("Law 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) ("Police fighting in the trenches do not seem eager to abandon Miranda[.]"); Schulhofer, *supra* note 16, 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).

n245. 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).

n246. See Schulhofer, *Bashing Miranda*, *supra* note 46, at 369-70 (suggesting that other Warren Court rulings should be taken into account).

n247. See, e.g., Kamisar, *supra* note 98, at 245-57 (denying that the Warren Court decisions were responsible for sinking clearance rates in the 1960s). But see Reynolds, *supra* note 210, 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).

n248. See Ehrlich & Brower, *supra* note 52, at 103 tbl.2.

n249. 388 U.S. 218 (1967).

n250. 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.

n251. 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").

n252. Joshua Dressler, *Understanding Criminal Procedure* 166[B][2], at 352 (1991).

n253. See Figures 1-5 (finding no noticeable rise in clearance rates in 1973 and 1974).

n254. 389 U.S. 347 (1967).

n255. *Id.* at 351.

n256. See *id.* at 354.

n257. 385 U.S. 293 (1966).

n258. See *id.* at 309-10.

n259. 387 U.S. 1 (1967).

n260. See *id.* at 58.

n261. 386 U.S. 18 (1967).

n262. *Id.* at 24.

n263. But cf. Kamisar, *supra* note 98, 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").

n264. 367 U.S. 643, 655-56 (1961) (excluding evidence in a state criminal trial that was the result of an illegal search).

n265. 372 U.S. 335, 343-45 (1963) (establishing a right to counsel for indigent defendants).

n266. See Schulhofer, *supra* note 16, at 512.

n267. Note that robbery clearance rates changed little from 1962-1965, even rising slightly from 1964-1965. See Figure 4.

n268. 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 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).

n269. 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 87, 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).

n270. 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").

n271. 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).

n272. 405 U.S. 156, 171 (1972).

n273. Arrest rates per 100,000 inhabitants from 1960-1975 were:

[SEE TABLE IN ORIGINAL]

See UCR-1960, *supra* note 36, 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).

n274. 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).

n275. 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).

n276. 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").

n277. 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).

n278. See 3 George H. Gallup, *Gallup Poll: Public Opinion 1935-1971*, at 1935, 2077 (1972).

n279. 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 278 *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 Group Relations* (1963), which described poor police relations with African Americans).

n280. See *Sourcebook*, *supra* note 275, 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).

n281. See notes 65-69 *supra* and accompanying text.

n282. See Dennis R. Longmire, *America's Attitudes About the Ultimate Weapon*, in *Americans View Crime and Justice*, *supra* note 277, at 93, 99 (reporting polls showing that public support for the death penalty has generally risen since 1966); *cf.* *Sourcebook*, *supra* note 275, at 140 tbl.2.1 (reporting polls showing that public concern about crime rose dramatically in the mid-1990s).

n283. 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>>.

n284. 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 police and 36% of attempted thefts, for 68% overall); Cloninger & Sartorius, *supra* note 109, 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) ("Unlike 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.").

n285. See Schulhofer, *Bashing Miranda*, *supra* note 46, at 369.

n286. See, e.g., Anthony V. Bouza, *The Police Mystique: An Insider's Look at Cops, Crime, and the Criminal Justice System* 134 (1990) ("Citizens report crime more faithfully when confidence in the police is highest").

n287. 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).

n288. See note 94 *supra*. The incentive to report auto theft likely stems from insurance requirements.

n289. See Ross et al., *supra* note 292, at 494 (noting that in 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 292, at 415 (noting the larceny "crime wave" created in Chicago, when a reform-minded administrator improved crime bookkeeping); Walker, *supra* note 50, 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").

n290. 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 99, at 380.

n291. Schulhofer, *Bashing Miranda*, *supra* note 46, at 369.

n292. See generally Bouza, *supra* note 286; Skolnick, *supra* note 98; Jerome H. Skolnick & James J. Fyfe, *Above the Law: Police and the Excessive Use of Force* (1993).

n293. See Schulhofer, *Bashing Miranda*, *supra* note 46, 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)).

n294. 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).

n295. See notes 43 & 55 *supra* and accompanying text (noting Schulhofer's claim to this effect).

n296. See notes 63-76 *supra* and accompanying text.

n297. Schulhofer, *Clearance Rates*, *supra* note 46, at 280 (emphasis added); see *id.* ("Soaring 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 ("There is no reason - none - to blame Miranda, rather than precipitously shrinking resources, for the decline in clearance rates during the late 1960s.").

n298. See Cassell, *supra* note 83, at 307, 308 fig.2 ("Crime rates were increasing well before Miranda was handed down and continued to climb afterwards.").

n299. Schulhofer, *Bashing Miranda*, *supra* note 46, at 357-58.

n300. See *id.* at 365-67; Schulhofer, *Clearance Rates*, *supra* note 46, at 291-92.

n301. 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).

n302. See notes 54-76 *supra* and accompanying text.

n303. See text accompanying notes 77-227 *supra*.

n304. See *id.*

n305. See text accompanying notes 232-245 *supra*.

n306. See notes 20-24 *supra* and accompanying text.

n307. See notes 29-33 *supra* and accompanying text.

n308. See Cassell & Hayman, *supra* note 29, 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).

n309. 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 "the rule announced today will measurably weaken the ability of the criminal law to perform these tasks").

n310. See *id.* at 481 (claiming that the decision "should not constitute an undue interference with a proper system of law enforcement" (emphasis added)).

n311. 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).

n312. To be clear, we believe that even isolated instances of coerced confessions should be strongly condemned.

n313. Cassell, *supra* note 13, at 473-75 (describing police reform efforts in the twentieth century); Leo, *supra* note 294, at 47-53 (same).

n314. Leo, *supra* note 294, at 51.

n315. 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.").

n316. *Miranda v. Arizona*, 384 U.S. 436, 447-48; see also *id.* at 499-500 (Clark, J., dissenting). However, the majority continued on to state that police abuses "are [still] sufficiently widespread to be the object of concern." *Id.* at 447-48.

n317. For a discussion of this empirical evidence, see Cassell, *supra* note 13, at 474-75.

n318. Gerald N. Rosenberg, *The Hollow Hope: Can Courts Bring About Social Change?* 326 (1991).

n319. Professor Wayne R. LaFare reported the year before the *Miranda* decision that "in the great majority of in-custody interrogations observed, the possibility of coercion appeared slight." Wayne R. LaFare, *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 41, at 1558.

n320. Such challenges are rarely made today and even more rarely granted. See Cassell, *supra* note 13, at 392-93 (collecting the studies on this point).

n321. See *id.* at 476 (citing studies showing few rejections of defendants' statements by prosecutors or by judges at preliminary hearings).

n322. *Miranda v. Arizona*, 384 U.S. 436, 505 (Harlan, J., dissenting).

n323. 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 172, 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 59, 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.)

n324. 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 13, at 478 n.533.

n325. Joe Costanzo, *Has Miranda Handcuffed the Cops?*, *Deseret News*, 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 98, at 252-53 (citing Skolnick, *supra* note 98, 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).

n326. See, e.g., Skolnick, *supra* note 98, at 178-79 (describing a burglar giving multiple clearances to police); Wald et al., *supra* note 41, 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 311, 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 60 (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 99, at 50-52 (attributing part of drop in British clearance rates to the introduction of new confession procedures reducing confessions to multiple crimes).

n327. Cf. Greenwood, *supra* note 100, at 24 (discussing differences between clearance rates and arrest rates).

n328. 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%.

n329. 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 38 (finding that, after Miranda, only 12 unsolved crimes were solved through 1460 interrogations), with Wald et al., *supra* note 41, 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 60 (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).

n330. 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 398-400 *infra* and accompanying text. Violent crime data is used because Professor Kamisar was discussing a 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 331 *infra*.

n331. 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 437-438 *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).

n332. 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.").

n333. 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 41, at 1596 (questioning whether investigation of uncleared crimes is effective).

n334. See Greenberg et al., *supra* note 101, at 45-46 (reporting detectives' views to this effect).

n335. See Leslie Sebba, *Third Parties: Victims and the Criminal Justice System* 80-81 (1996) (reaching this conclusion after a review of several studies).

n336. 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).

n337. See *id.* at 5.

n338. See Greenberg et al., *supra* note 101, at 45 (reporting detectives' views that the possibility of returning property to burglary victims justified attempts to obtain multiple clearances).

n339. Cf. *Morris v. Slappy*, 461 U.S. 1, 14 (1983) ("In the administration of criminal justice, courts may not ignore the concerns of victims.").

n340. See Cassell & Hayman, *supra* note 29, at 909-16 (finding that whether police obtain an incriminating statement makes a significant difference in case outcome).

n341. See Cassell, *supra* note 13, at 494-96 (presenting data on what percentage of suspects do not waive rights and thus cannot be interrogated).

n342. See Baker, *supra* note 13, at 200-01 (collecting examples of such responses to Miranda).

n343. See Table VIII.

n344. See *id.*

n345. See generally Akhil Reed Amar, *The Constitution and Criminal Procedure: First Principles* 20-31 (1997) (discussing the Fourth Amendment exclusionary rule).

n346. 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).

n347. *Id.* at 908 n.6.

n348. See note 47 *supra* and accompanying text.

n349. Compare note 343 *supra* and accompanying text with note 346 *supra* .

n350. See notes 151-153 *supra* and accompanying text.

n351. Cf. Davies, *supra* note 346, at 656-68 (comparing estimates of the effects of illegal searches to exclusionary rule studies).

n352. 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).

n353. 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).

n354. 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).

n355. Gerald M. Caplan, *Miranda Revisited*, 93 *Yale L.J.* 1375, 1384-85 (1984) (reviewing Baker, *supra* note 13).

n356. 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.").

n357. 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) ("The freeing of even one guilty defendant by virtue of an irrational rule may exact a greater cost than society should be expected to bear").

n358. See Grano, *supra* note 9, at 173-98 (discussing how the prophylactic nature of Miranda differs from most constitutional law decisions).

n359. 417 U.S. 433 (1974).

n360. *Id.* at 443-44.

n361. *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").

n362. See Grano, *supra* note 9, 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].

n363. Schulhofer, *supra* note 16, at 552 n.214.

n364. *Moran v. Burbine*, 475 U.S. 412, 433 n.4. (1986).

n365. See Cassell & Hayman, *supra* note 29, at 921-22; cf. Davies, *supra* note 29, 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).

n366. See note 11 *supra* and accompanying text.

n367. See 18 U.S.C. 3501 (1994) (a federal statute rejecting Miranda in favor of voluntariness test); Grano, *supra* note 9, at 199-222 (advocating overruling of Miranda on purely doctrinal grounds).

n368. 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 criminal law enforcement procedures on a scale never before witnessed"); OLP Pre-Trial Interrogation Report, *supra* note 362, at 40-41 (discussing American Law Institute efforts to draft a Model Code of Pre-arraignment Procedure).

n369. *Miranda*, 384 U.S. at 467.

n370. OLP Pre-Trial Interrogation Report, *supra* note 362, at 96.

n371. 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).

n372. See Cassell, *supra* note 13, at 489-92.

n373. *Miranda*, 384 U.S. at 467.

n374. See Paul G. Kauper, *Judicial Examination of the Accused - A Remedy for the Third Degree*, 30 Mich. L. Rev. 1224 (1932).

n375. 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 & Renee B. Lettow, *Fifth Amendment First Principles: The Self-Incrimination Clause*, 93 Mich. L. Rev. 857, 898-928 (1995) (similar).

n376. 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).

n377. 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*).

n378. 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 Criminal Procedure: State Constitutional Law and Selective Disincorporation*, 87 J. Crim. L. & Criminology 63, 101-11 (1996) (suggesting this approach).

n379. 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).

n380. 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 "reflects 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).

n381. *Miranda v. Arizona*, 384 U.S. 436, 504 (Harlan, J., dissenting).

n382. See notes 302-310 *supra* and accompanying text.

n383. Pyle & Deadman, *supra* note 292, at 207.

n384. See UCR-1958, *supra* note 36, at 76 tbl.12; Brewer Letter, *supra* note 53, at 1.

n385. See UCR-1973, *supra* note 36, at 1; Brewer Letter, *supra* note 53, at 1.

n386. Fox appears to ignore this discontinuity. See Fox, *supra* note 36, at 6.

n387. *Id.* at 86 (based on UCR data).

n388. Compare *id.* with UCR-1974, *supra* note 36, at 166.

n389. See Fox, *supra* note 36, at 7.

n390. See UCR-1958, *supra* note 36, at 2 (limiting rape figures to those rapes involving force).

n391. See notes 198-201 *supra* and accompanying text.

n392. See, e.g., UCR-1995, *supra* note 36, at 181 tbl.12 (showing that 80.1% of offenses known to police were reported by cities (percentage derived from *id.*)).

n393. Cf. Fox, *supra* note 90, 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).

n394. See UCR-1995, *supra* note 36, 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).

n395. 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).

n396. 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 13, at 447-50 (analyzing the evidence of post-Miranda reduction of such rates).

n397. See notes 191-194 *supra* and accompanying text.

n398. The modification is published in UCR-1975, *supra* note 36, at 49 tbl.2, and in some, but not all, subsequent UCRs.

n399. See, e.g., Jacob & Rich, *supra* note 210, at 122 (using UCR city data on robberies for nine cities without any adjustment).

n400. See Cassell, *supra* note 46, at 334-38 (discussing Schulhofer's reliance upon unrevised crime rate data to construct "clearance capacity" charts, which do not track clearance rate changes).

n401. See Brower, *supra* note 96, at 172 tbl.47, 175 tbl.48, 178 tbl.49, 181 tbl.50, 184 tbl.51, 187 tbl.52 & 190 tbl.53.

n402. See note 403 *infra* (providing ratios of official revised figures to unofficial revised figures).

n403. 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.

n404. Compare Brower, *supra* note 96, at 187 tbl.52 (reporting that unpublished revision of larceny for 1960 was 507,300), with UCR-1975, *supra* note 36, at 49 tbl.2 (reporting that official revision of larceny for 1960 was 1,855,400).

n405. 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 96, at 187 tbl.52 (providing unpublished revised data), with UCR-1975, *supra* note 36, at 49 tbl.2 (providing official revised data).

n406. See UCR-1958, *supra* note 36, at 1-3 (noting a change in estimation techniques and larceny definition).

n407. See UCR-1975, *supra* note 36, at 49 tbl.2 (reporting total larcenies for 1960 as 1,855,400).

n408. See UCR-1960, *supra* note 36, 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 36, 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 36, 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 36, 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 36, at 35 tbl.2 (reporting a 14.2% increase in larcenies over \$ 50 nationwide in 1960); UCR-1959, supra note 36, at 4 (reporting a 3% increase in larcenies over \$ 50 nationwide in 1959); UCR-1958, supra note 36, at 1 (reporting a 10.3% increase in larcenies over \$ 50 nationwide in 1958); UCR-1957, supra note 36, at 71 tbl.24 (reporting an 8.4% increase in total larcenies nationwide in 1957).

n409. For the early 1950s, this information appears in a table labeled "Number of police department employees." See, e.g., Semiannual Bulletin, supra note 36, 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 36, 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 36, at 161 tbl.54.

n410. See, e.g., Jacob & Rich, supra note 210, at 122 app. (using the UCR's figures for number of sworn officers).

n411. See Fox, supra note 36, at 6 (using the "number of full-time law enforcement employees per 1000 population").

n412. 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 36, at 105 tbl.23; UCR-1960, supra note 36, at 105 tbl.31; UCR-1961, supra note 36, at 108 tbl.33.

n413. See UCR-1980, supra note 36, at 264 tbl.68.

n414. In 1980, for instance, total agency population was 211 million; total city population was 143 million. See id. at 262, 264.

n415. See, e.g., id. at 264 tbl.68; UCR-19985, supra note 36, at 248 tbl.74; UCR-1990, supra note 36, at 242 tbl.69.

n416. For 1950-1970, the data are available in 1 Bureau of the Census, supra note 234, 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.

n417. See, e.g., Fox, supra note 36, at 5, 127 n.5; Jacob & Rich, supra note 210, at 122 app.; Pogue, supra note 108, at 44 app..

n418. See U.S. Dep't of Commerce, Government Finances in 1962, G-GF62 No. 2, at 19 n.1 (1963).

n419. 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 90, at 126 (debating same in response).

n420. See Fox, *supra* note 36, 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).

n421. See Bureau of Labor Statistics, U.S. Dep't of Labor, Employment and Earnings (January issues, published annually); see also Statistical Abstract, *supra* note 416.

n422. See, e.g., Statistical Abstract-1995, *supra* note 416, at 399 tbl.627.

n423. See generally Bureau of Econ. Analysis, U.S. Dep't of Commerce, Survey of Current Business; see also Statistical Abstract, *supra* note 416.

n424. See e.g., Fowles & Merva, *supra* note 216 (discussing the correlation between wage inequality and crime and reviewing relevant literature).

n425. See, e.g., *id.* at 77 tbl.94 (reprinting National Center for Health Statistics data).

n426. See Statistical Abstract-1975, *supra* note 416, 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 416, 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 $y = 0.0003x[su'3'] - 0.0719x[su'2'] + 5.81x - 84.449$.

n427. See note 134 *supra* and accompanying text.

n428. Schulhofer, Bashing Miranda, *supra* note 46, at 367.

n429. We extended Schulhofer's series to 1995 by calculating data for that year. See UCR-1995, *supra* note 36, at 187 tbl.16 (providing relevant data in table entitled "Crime Rates, Offense Known to the Police, Population Group").

n430. See Statistical Abstract, *supra* note 416.

n431. See UCR-1995, *supra* note 36, at 58 tbl.1, n.1 (indicating that the population data was supplied by the Bureau of the Census).

n432. See Statistical Abstract, *supra* note 416.

n433. 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 416, at 26 tbl.29 & 27 tbl.30. We assumed that the ratio closed constantly from 1970-1980.

n434. See notes 218-221 *supra* and accompanying text.

n435. 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).

n436. See *id.* at 348-52.

n437. See note 327 *supra* and accompanying text.

n438. 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 290, 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.

n439. 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 36, at 86 tbl.A-1, col.EV.

n440. See *id.*

n441. See UCR-1966, *supra* note 36, at 4.

n442. See Fox, *supra* note 36, at 127 n.9.

n443. 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.

n444. We used the FBI's revised series of crimes for this calculation. See notes 398-400 *supra* and accompanying text (discussing the revised series).

n445. See notes 392-396 *supra* and accompanying text (discussing the relative soundness of this assumption).

n446. Compare, e.g., UCR-1966, *supra* note 36, 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 443, at 205 (reporting 1966 arrest rate based on 4048 cities with population of 132.4 million).