RAPE LAW REFORM AND INSTRUMENTAL CHANGE IN SIX URBAN JURISDICTIONS

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Concerns about the treatment of rape victims and attrition in rape cases prompted a nationwide movement to reform state rape laws. In this study we evaluate the impact of rape law reforms on reports of rape and the processing of rape cases in six urban jurisdictions—Detroit, Chicago, Philadelphia, Atlanta, Houston, and Washington, D.C. Our results strongly suggest that the ability of rape reform legislation to affect case outcomes is limited. Time-series analyses revealed that predicted results were found in only one of the six jurisdictions, and there the results were limited.

During the past twenty years there has been a sweeping effort to reform rape laws in this country. Reformers questioned the special status of rape as an offense for which the victim, as well as the defendant, was put on trial. They suggested that the laws and rules of evidence unique to rape were at least partially responsible for the unwillingness of victims to report rapes and for the low rates of arrest, prosecution, and conviction (Batelle Memorial Institute 1977; Loh 1980; McCahill, Meyer, and Fischman 1979; Vera Institute of Justice 1981). They cited evidence that these laws and rules of evidence resulted in pervasive skepticism of the victim's claims and allowed criminal justice officials to use legally irrelevant assessments of the victim's status, character, and relationship with the defendant in making decisions regarding the processing and disposition of rape cases (Bohmer 1974; Estrich 1987; Feild and

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Bienen 1980; Feldman-Summers and Lindner 1976; Holmstrom and Burgess 1978; Kalven and Zeisel 1966; LaFree 1981, 1989; McCahill et al. 1979; Reskin and Visher 1986).

Concerns such as these sparked a nationwide, grass-roots movement in which women's groups lobbied for rape law reforms. Their efforts resulted in changes in the rape laws of all fifty states. The overall purpose of the reforms was to treat rape like other crimes by focusing not on the behavior or reputation of the victim but on the unlawful acts of the offender. Advocates of the new laws anticipated that by improving the treatment of rape victims the reforms would ultimately lead to an increase in the number of reports of rape (Cobb and Schauer 1974; Marsh, Geist, and Caplan 1982; Sasko and Sesek 1975). They also expected that the reforms would remove legal barriers to effective prosecution and would make arrest, prosecution, and conviction for rape more likely (Cobb and Schauer 1974; Marsh et al. 1982; Robin 1982).

In this study we address these expectations. Using time-series data on more than twenty thousand rape cases in six major urban jurisdictions, we examine the impact of rape reform legislation on reports of rape and the outcome of rape cases.

RAPE LAW REFORM

States enacted reform statutes that vary in comprehensiveness and encompass a broad range of reforms. The most common changes were (1) changes in the definition of rape; (2) elimination of the resistance requirement; (3) elimination of the corroboration requirement; and (4) enactment of a rape shield law. We briefly describe each of these reforms below.

1. Many states replaced the single crime of rape with a series of offenses graded by seriousness and with commensurate penalties. Historically, rape was defined as "carnal knowledge of a woman, not one's wife, by force and against her will." Thus, traditional rape laws did not include attacks on male victims, acts other than sexual intercourse, sexual assaults with an object, or sexual assaults by a spouse. The new crimes typically are gender neutral and include a range of sexual assaults.

2. A number of jurisdictions changed the consent standard by modifying or eliminating the requirement that the victim resist her attacker. Under traditional rape statutes, the victim, to demonstrate her lack of consent, was required to "resist to the utmost" or, at the very least, exhibit "such earnest resistance as might reasonably be expected under the circumstances" (Tex. Penal Code 1980). Reformers challenged these standards, arguing not only that resistance could lead to serious injury but also that the law should focus on the behavior of the offender rather than on that of the victim. In response, states either eliminated resistance of the victim as an element of the crime to be proved by the prosecutor or

attempted to lessen the state's burden of proving nonconsent by specifying the circumstances that constitute force—using or displaying a weapon, committing another crime at the same time, injuring the victim, and so on.

3. The third type of statutory reform was elimination of the corroboration requirement—the rule prohibiting conviction for forcible rape on the uncorroborated testimony of the victim. Critics cited the difficulty in obtaining evidence concerning an act that typically takes place in a private place without witnesses. They also objected to rape being singled out as the only crime with such a requirement.

4. Most states enacted rape shield laws that placed restrictions on the introduction of evidence of the victim's prior sexual conduct. Under common law, evidence of the victim's sexual history was admissible to prove she had consented to intercourse and to impeach her credibility. Reformers were particularly critical of this two-pronged evidentiary rule and insisted that it be eliminated or modified. Critics argued that the rule was archaic in light of changes in attitudes toward sexual relations and women's role in society. They stressed that evidence of the victim's prior sexual behavior was of little, if any, probative worth (Ireland 1978). Confronted with arguments such as these, state legislatures enacted rape shield laws designed to limit the admissibility of evidence of the victim's past sexual conduct. The laws range from the less restrictive, which permit sexual conduct evidence to be admitted following a showing of relevance, to the more restrictive, which prohibit such evidence except in a few narrowly defined situations. The laws also usually specify procedures for determining the relevance of the evidence; most states require an *in camera* hearing to determine whether the proffered evidence is admissible.

THE IMPACT OF RAPE LAW REFORM

Proponents of rape law reform predicted that the various statutory changes would produce a number of instrumental results. They expected the reforms, particularly the rape shield laws, to improve the treatment of rape victims and thus to prompt more victims to report the crime to the police. They believed that elimination of resistance and corroboration requirements would remove major barriers to conviction; as a result, prosecutors would be more likely to indict and fully prosecute rape cases, and juries and judges would be more likely to convict in rape trials. They expected that conviction would also be facilitated by the enactment of rape shield laws that restricted admission of evidence of the complainant's sexual history. Finally, reformers believed that definitional changes would make it easier to prosecute cases that did not fit traditional definitions of rape, would prevent jury nullification by having penalties commensurate with the seriousness of the

offense, and would lead to more convictions through plea bargaining because appropriate lesser offenses would be available to prosecutors in their negotiations.

Reformers clearly had high hopes for the rape law reforms, but their expectations may have been unrealistic. In fact, the literature on legal impact, which abounds with examples of "the remarkable capacity of criminal courts to adjust to and effectively thwart reforms" (Eisenstein, Flemming, and Nardulli 1988:296), should lead us to predict that the rape law reforms would have only limited effects on reports of rape and the outcome of rape cases.

The chronic failure of reforms aimed at the court system suggests that reformers have misperceptions about the nature of the judicial process (Nimmer 1978). Most reform proposals "assume that we have a hierarchic, centralized, obedient system of courts that will automatically and faithfully adhere to new rules" (Eisenstein et al. 1988:296). These misperceptions cause reformers to overestimate the role of legal rules in controlling the behavior of decisionmakers and to underestimate the role of discretion in modifying the legal rules. Statutory changes like the rape law reforms must be interpreted and applied by decisionmakers who may not share the goals of those who championed their enactment and who therefore may not be committed to their implementation. Numerous studies have demonstrated limited impact of reforms when officials' attitudes were at odds with reformers' goals (e.g., Ross and Foley 1987; Loftin, Heumann, and McDowall 1983).

Even if criminal justice officials agree with the legal change in principle, they may resist if it impinges on interests protected by the courtroom workgroup (Eisenstein and Jacob 1977). Officials may modify or ignore reforms that threaten the status quo by impeding the smooth and efficient flow of cases or that require changes in deeply entrenched and familiar routines. Studies have shown that reforms that interfere with plea bargaining and the production of large numbers of guilty pleas (Carter 1974; Nimmer and Krauthaus 1977) or that attempt to alter the "going rates" established by the workgroup (Church 1976; Heumann and Loftin 1979) are especially at risk of being undermined.

There are other explanations for the failure of court reforms to produce the instrumental results anticipated by reformers. Some reforms are doomed to failure because "the limits altered by the reform are nonessential or irrelevant in practice" (Nimmer 1978:189). Nimmer (1977), for example, found no change in sentences imposed for importation of heroin after a federal statute lowered the minimum and maximum sentences that could be imposed; plea bargaining had been used before the legal change to circumvent the minimum, and the maximum sentence was seldom imposed, so the statutory change was irrelevant to sentence outcomes.

Other reforms may have limited impact because their passage was primarily symbolic. Faced with a vocal constituency demanding action, decisionmakers might adopt a policy with little bite to provide "symbolic reassurance that needs are being attended to, problems are being solved, help is on the way" (Casper and Brereton 1984:124). Policymakers might, for example, placate constituents by enacting a very weak version of the legal change being sought, by adopting a law that differs very little from other laws on the books or from case law, or by adopting a reform that they know full well will not be enforced.

All the foregoing suggest that the advocates of rape law reform may have been overly optimistic about the effects of the reforms. It also suggests that we should approach the task of interpreting the outcomes of the reforms with great care. It obviously is important to consider not only the specific provisions of the laws themselves but also the comprehensiveness of the reforms, the contexts in which the reforms are to be implemented, and the consequences for decisionmakers charged with enforcing the reforms.

Previous Research on Rape Law Reform

Despite the fact that most states have enacted rape law reforms, there has been little empirical research on the effect of these laws. The studies that have been conducted have yielded mixed results. Two studies examined the impact of the 1974 Michigan criminal sexual conduct statute, the most sweeping rape law reform in the country. In an interrupted time-series analysis, Marsh et al. (1982) found increases in the number of arrests for rape and in convictions on the original charge but no change in the number of rapes reported to the police. Caringella-MacDonald (1984) compared postreform attrition and conviction rates in a Michigan jurisdiction with rates from three jurisdictions with more traditional rape laws and concluded that the differences in these rates provided "indirect" evidence that the Michigan law had had an effect.

Studies of other jurisdictions found some changes in officials' attitudes (Largen 1988) but very limited effects on case outcomes. Loh (1981) examined the effect of the Washington state rape reform statute on the prosecution of rape cases in King County (Seattle) and found no change in charging decisions or in the overall rate of conviction, although convictions for rape rather than for other offenses such as assault increased. In a study of California reforms, Polk (1985) analyzed statewide yearly data and found no significant change in the police clearance rate or the conviction rate, but slight increases in the rate of filing felony complaints and in the rate of incarceration for those convicted of rape. Gilchrist and Horney (1980) found no evidence of an increase in indictments

or convictions after rape law reforms were implemented in Nebraska.

These empirical studies provide some evidence of the impact of rape law reforms in four jurisdictions but leave many unanswered questions about the nationwide effect of the reforms. Design limitations in each study also limit the conclusiveness and generalizability of their results. None of the designs included controls for the history threat to internal validity—that is, for the possibility that events other than the legal changes could have produced the effects noted. Furthermore, none of the studies collected data for more than three years following the reforms, so it is possible that the effects detected may have been transient ones or that delayed effects may have gone undetected (Casper and Brereton 1984). Finally, some studies used simple before-and-after designs, which cannot take long-term trends into account.

Perhaps the most serious problem for interpretation of the studies described above is that, with the exception of Largen's (1988) study of officials' perceptions, each was conducted in only one state and each used a somewhat different design and different measures. Thus we don't know whether the disparate results reflect the varied research strategies or jurisdictional differences in the reforms enacted. Individual states adopted relatively strong or weak reforms; even by the reformers' own expectations, we should anticipate finding greater impact in the jurisdictions with stronger versions of the reform laws. The apparently greater impact of reforms in Michigan would be consistent with the widespread characterization of the Michigan changes as the model for rape law reform. It might also reflect the fact that Michigan adopted a broad, sweeping reform at a single time rather than making changes in a piecemeal fashion as some jurisdictions did. A serious test of the impact of rape law reforms requires a multijurisdiction study.

The Current Study

In this study we assess the impact of rape law reform in six urban jurisdictions. The jurisdictions—Detroit, Michigan; Cook County (Chicago), Illinois; Philadelphia County (Philadelphia), Pennsylvania; Harris County (Houston), Texas; Fulton County (Atlanta), Georgia; and Washington, D.C.—represent states that enacted different kinds of rape law reforms. As Berger, Searles, and Neuman (1988) showed with a factor analysis of reform variables, there is significant variability in the extent to which individual states have reformed their statutes along the different dimensions of the law. Any ranking of jurisdictions in terms of strength of reforms is thus bound to be imprecise because of the difficulty of evaluating the relative importance of the different kinds of reforms. Nevertheless, to be able to make general comparisons of outcomes across jurisdictions, we selected Detroit, Chicago, and

Philadelphia to represent jurisdictions with relatively strong reforms and Atlanta, Washington, D.C., and Houston to represent jurisdictions with weaker reforms. The reforms enacted in the six jurisdictions are summarized below and presented in detail in Table 1.

The Michigan law, considered by many to be the model rape law reform, included all the changes described above. The Michigan statute redefines rape and other forms of sexual assault by establishing four degrees of gender-neutral criminal sexual conduct based on the seriousness of the offense, the amount of force or coercion used, the degree of injury inflicted, and the age and incapacitation of the victim. The law states that the victim need not resist the accused and that the victim's testimony need not be corroborated.

Michigan also enacted a very restrictive rape shield law. Evidence of prior sexual activity with persons other than the defendant is admissible only to show the source of semen, pregnancy, or disease. Evidence of the victim's past sexual conduct with the defendant can be admitted only if a judge determines that it is material to a fact at issue (generally consent) and that its inflammatory or prejudicial nature does not outweigh its probative value.

Although we categorized the reforms adopted in Illinois and Pennsylvania as "strong" reforms, they are neither as broad nor as comprehensive as those enacted in Michigan. The Illinois reforms were incremental; in 1978 the state implemented a strong rape shield law very similar to the law enacted in Michigan, but it was six years later before definitional changes were adopted and the resistance requirement was repealed. In 1976 Pennsylvania passed a strong rape shield law and repealed the corroboration and resistance requirements. Although these are significant changes, Pennsylvania retains Model Penal Code definitions of rape and involuntary deviate sexual intercourse, which many reformers believe still place undue focus on the circumstances that define nonconsent.

The reforms adopted in Washington, D.C., Georgia, and Texas are much weaker. Although corroboration requirements have been eliminated or weakened in each jurisdiction, all three jurisdictions continue to require resistance by the victim. Georgia and Texas passed very weak rape shield laws that give judges considerable discretion to admit sexual conduct evidence. Washington, D.C., has not amended its rape statutes since 1901, but case law restricts the introduction of evidence of the victim's prior sexual conduct. Washington, D.C., and Georgia have traditional carnal knowledge definitions of rape, as did Texas until relatively minor definitional changes were made in 1983.

Table 1. Summary of Rape Law Reforms Evaluated in Six Jurisdictions

	Definition	Law States Resistance Not Required	Law States Corroboration Not Required	Shield Law
Michigan				
4-1-75	Four degrees of criminal sexual conduct defined by penetration vs. contact and by presence or absence of aggravating circumstances (e.g., personal injury, force, coercion, armed with weapon, victim incapacitated)	Yes	Yes	Sexual conduct evidence admissible only if with defendant or to show source of semen, pregnancy; disease; and, in each case, only if prejudicial nature does not outweigh probative value. Written motion required; *in camera* hearing may be held.
Pennsylvania				
6-17-76		Yes	Yes	Sexual conduct evidence admissible only if with defendant where consent is issue and only if admissible pursuant to other rules of evidence. Written motion and *in camera* hearing required.
Illinois[a]				
1-4-78		No	No	Sexual conduct evidence admissible only if with defendant. *In camera* hearing required to determine if defense has evidence to impeach in the event conduct denied by complainant.
Georgia				
7-1-76				Past sexual behavior (including marital history, mode of dress, sexual reputation) admissible if it supports inference that accused could reasonably have believed victim consented; sexual behavior involving the accused admissible. Motion and *in camera* hearing required.
7-1-78		No	Yes	
District of Columbia				
5-3-76[b]		No	Yes	
9-2-77[b]				Reputation evidence admissible only in unusual cases where probative value outweighs prejudicial effect; behavior with defendant admissible if consent is issue. No procedural requirements.
Texas[c]				
9-1-75		No	No	Specific acts of sexual activity, opinion evidence, reputation evidence admissible if prejudicial nature does not outweigh probative value. Notice and *in camera* hearing required.

[a] Illinois adopted a statute stating that resistance is not required, but we were not able to evaluate this reform, which occurred in 1984.

[b] Changes in case law.

[c] Texas adopted rather minor definitional changes in 1983; we did not evaluate the impact of this reform.

Hypotheses

In this study we evaluate the impact of rape law reform on reports of rape and the outcome of rape cases. We test reformers' expectations that the reforms would result in increases in (1) the reporting of rapes to the police, (2) the indictment of rape cases by prosecutors, and (3) the conviction of offenders. Although most of the reform laws did not deal explicitly with sentencing, we consider the possibility that the attention focused on the seriousness of the crime of rape would lead judges to incarcerate more offenders and to impose more severe sentences on those incarcerated.

We noted earlier that there are a number of reasons for suggesting that reformers' expectations for the rape law reforms were unrealistic. If the resistance to change inherent in the criminal justice system is to be overcome, strong reforms will be required. We therefore hypothesize that the stronger reform laws of Michigan, Illinois, and Pennsylvania will have greater impact than those of Georgia, Texas, and Washington, D.C.

We also consider whether impact is greatest with comprehensive reform or incremental change. Reformers viewed the Michigan reform as a model for other states; the Michigan legislature addressed reformers' concerns in one far-reaching revision of the statutes. The Michigan reform was so dramatic and broad in scope that a powerful message was sent to decisionmakers in the criminal justice system. If the attitudes of system participants are to change, this kind of strong statement might be necessary. Yet Nimmer (1978:181) has asserted that "the probability of system change is inversely related to the degree of change sought by a reform" and that "a series of limited reforms is more likely to generate systemic change than a single, far-reaching reform."

We compare outcomes in Detroit, where the reforms were enacted at a single time, with outcomes in Chicago and Philadelphia, where rape shield laws of comparable strength were enacted, but where fewer of the other reforms were adopted at the same time. If Nimmer is correct, we should find greater impact in Chicago and Philadelphia than in Detroit. If, however, it takes dramatic, comprehensive change to overcome the predisposition to resist change, then we should find the greatest impact in Detroit.

RESEARCH DESIGN AND METHODOLOGY

Jurisdictions

As noted above, we selected six jurisdictions to represent states that enacted various types of law reforms. Other factors also influenced our choice of these six cities. To control for the threat of history to the design (discussed below), we chose jurisdictions in which reforms were implemented at several different times. Also,

to obtain an adequate number of cases for the statistical analysis, we selected major urban jurisdictions.

Case Selection

We gathered court records data[1] on rape cases processed from 1970 through 1984[2] in the six jurisdictions. We collected data on rapes reported to the police during the same time period from the FBI's Uniform Crime Reports (UCR).[3] In each jurisdiction we collected data on forcible rape cases and on other sexual assaults that were not specifically assaults on children. We performed all analyses for both forcible rape and total sexual assaults. Because the pattern of results did not differ with the inclusion of other sexual assaults (and because the types of offenses included varied from jurisdiction to jurisdiction), we present here only the results for forcible rape cases since they are the most comparable. In Michigan, where the reforms included definitional changes, we selected the closest equivalent crimes for the forcible rape analysis (details described below).

Dependent Variables

The dependent variables include the number of reports of forcible rape; the indictment ratio (indictments divided by reports); the percentage convicted (convictions divided by indictments); the percentage convicted on the original charge (convictions for rape divided by indictments); the percentage incarcerated (incarcerations divided by convictions); and the average sentence (average maximum sentence—in months—for defendants incarcerated after a conviction for rape).[4]

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[1] The procedures used to obtain the data varied from jurisdiction to jurisdiction. In Atlanta we obtained the data from court docket books that contained detailed information on all felony cases. In Chicago and Houston we used docket listings to select the target cases and then pulled case files to obtain the necessary information. In Washington, D.C., we obtained a listing of the target cases from the prosecutor's computerized system and then went to the case files to obtain detailed information on each case. In Detroit we used docket listings of all felony cases for the first six years and computer printouts of case dispositions for the remainder of the period (docket listings were not available for these years). Finally, in Philadelphia we were able to obtain all the necessary information from the court's computerized system.

[2] In all jurisdictions except Washington, D.C., we selected all sexual assault cases for which indictments or informations were filed beginning in 1970. In Washington, D.C., we had to begin data collection with cases from February 1973, the date when the Superior Court took over jurisdiction of felony cases. In Houston we had to end the data file in August 1982, because a set of data collection forms covering the remainder of the period were lost in the mail, and we did not have the resources necessary for collecting the data again. Since the Texas reform was the second earliest, taking effect in 1975, we still had seven postreform years for our analysis, so we are confident that the missing data did not affect our conclusions.

[3] For Chicago those data are not available for 1984 because of reporting irregularities.

[4] We performed all analyses for total indictments, total convictions, total

Our unit of analysis was the indicted case (the term indictment will be used broadly to include informations filed in those jurisdictions in which the grand jury is not used). When we calculate the indictment ratio, we use data from two sources—the UCR and our population of cases from the court files. Thus we do not have the perfect correspondence that we would have if we had been able to follow individual cases from report through court filing. Because most indictments seem to follow the reports fairly closely in time and because there is no good model for making other assumptions, we divided the number of indictments filed in a given month by the number of rapes reported in that same month. In all other analyses the data are based on the indicted cases, and month of indictment is used for the time variable. Thus when we calculate convictions as a percentage of indictments, we are looking at the percentage of cases indicted in a particular month that resulted in conviction.

Time-Series Analysis

We used interrupted time-series analysis to evaluate the impact of the rape law reforms on the dependent variables. We analyzed monthly data over the fifteen-year period to see whether changes in the rape laws produced increases or decreases in the level of the series. In each time-series analysis the interruption was the change in the rape laws of the particular jurisdiction.[5] The number of years before and after the reforms varied somewhat, depending on when the law was reformed in each state.

Each series was analyzed according to procedures specified by McCleary and Hay (1980).[6] We tested three basic models of impact

convictions on original charge, and total incarcerations as well as for the percentage variables reported in the text. Because all the absolute variables depended on the number of reported crimes, those time series would not be independent of any effects on reported crimes. We therefore focused on the percentage variables; in all cases, however, the analyses of the absolute variables did not lead to any different conclusions. Because the upper and lower bounds with our percentage variables may cause problems for assumptions of the ARIMA modeling, we reanalyzed our data for the key variables of indictment ratio and percentage convicted, using a logistic transformation of the data. These analyses led to the same conclusions we reached using the percentage variables.

[5] We analyzed two interventions in Atlanta and in Washington, D.C., where changes occurred at two different times. We were not able to analyze the second changes (definitional) that occurred in Chicago in 1984 and in Houston in 1983 because we did not have adequate post-intervention data. In addition, the unavailability of UCR data for Chicago for 1984 created problems for analyzing the second wave of changes there.

[6] The initial step in the analysis is to determine the appropriate statistical model for the noise component of the time series, based on the relationship among the data points. Autocorrelations and partial autocorrelations are computed, and if these differ significantly from white noise, then there is evidence of dependence among the observations. In such a case, it must then be determined from the pattern of autocorrelations and partial autocorrelations which autoregressive moving average (ARIMA) model is appropriate for testing sta-

for each series—an abrupt, permanent change; a gradual, permanent change; and an abrupt, temporary change. Interventions are modeled with dummy variables. The simplest model, for example, represents an abrupt, permanent change by coding the dummy variable "0" for all observations before the intervention and "1" for all observations following the intervention. The same coding is used in modeling a gradual, permanent change, but a denominator factor is added to the equation representing the series. The abrupt, temporary model uses a pulse variable—one coded "0" for all observations except the observation at the time of the intervention, which is coded "1".

Missing Data

Because the time-series analysis cannot be performed with missing observations, we had to decide how to handle missing data. Missing data generally were produced when there were no cases during a month on which to base calculations or because a calculation would involve division by zero. For example, there may have been five indictments in a given month but no convictions. The variables based on convictions—percentage incarcerated and average sentence—would then be missing for that month. Even though there were zero convictions (and thus zero incarcerations) in a given month, it would have been misleading to code that month's missing values for percentage incarcerated and average sentence as zeroes. In addition, monthly observations were occasionally missing from the Uniform Crime Reports data because of inappropriate reporting procedures and other reasons. In each of these instances, we used the mean of the previous five observations to replace the missing value.

Controls

The major weakness of the time-series design is that it does not control for the "history" threat to internal validity (Campbell and Stanley 1966). Even when a discontinuity in the series occurs at the time of the intervention, other events occurring at about the same time actually may be responsible for the effects noted. In the case of rape law reforms, increased national attention to the problems surrounding the prosecution of rape cases might have sensitized criminal justice officials and led to any observed changes in processing. We were able to control for history in this research by using a multiple time-series design because the reformed jurisdictions made their legal changes at different times. If national attention to rape issues led to changes in the processing of rape cases, these changes should appear at approximately the same time for all jurisdictions. If, on the other hand, these changes coincide

tistical significance of any effects of the intervention. All our analyses were performed with the SAS statistical package.

with the legal reforms in each jurisdiction, we have strong evidence that the legal reforms caused the changes.

Interviews with Criminal Justice Officials

To more fully evaluate the rape law reforms, in 1985 and 1986 we interviewed criminal justice officials in the six jurisdictions. We conducted lengthy, structured, face-to-face interviews with a sample of 162 judges, prosecutors, and defense attorneys (Spohn and Horney 1991). We selected officials who had experience with rape cases before and after the legal reforms went into effect or who had handled a substantial number of rape cases in the postreform period. We also interviewed police officers and rape crisis center personnel in each jurisdiction.

RESULTS

The results of the time-series analyses are summarized in Table 2.[7] The results of our analyses indicate that, contrary to reformers' expectations, the reforms had little effect on reports of rape or the processing of rape cases. The only clear impact of the laws was in Detroit, and even there the effects were limited. Below we discuss the results for Detroit in detail and then briefly summarize the results for the other five jurisdictions.

The statutory changes adopted by Michigan in 1975 produced some of the results anticipated by reformers. There was an increase in the number of reports of rape and in the ratio of indicted to reported cases. Additionally, the maximum sentence for those incarcerated increased. On the other hand, there was no change in the percentages of indictments resulting in conviction or in conviction on the original charge or in the percentage of convictions resulting in incarceration.

Our analysis of monthly reports of rape revealed that the new law produced a significant increase of about twenty-six reports per month (see Fig. 1). Because our measure of reports did not allow us to separate changes in reporting from changes in crime rates, we compared reports of rape with reports of robbery and felony assault for the period 1970 through 1980. If the increase in reported rapes reflected a general trend in violent crimes, we should have seen similar increases for these other crimes. Such increases were not evident. The pattern for felony assault reports was much like that for reported rapes, but the time-series analysis indicated no significant change coincident with changes in the rape laws. The pattern for reported robberies was quite different, and there was no significant change at the time when reporting of rapes increased.

Our results also indicate that the reforms had some effects on

[7] Detailed results including ARIMA parameter values are available on request from the authors.

Table 2. Summary of the Results of the Time-Series Analysis

Jurisdiction	Reports	Indictment Ratio	% Convicted	% Convicted–Orig. Charge	% Incarcerated	Average Sentence
Detroit	26.53**	.18***	−2.26	0.07	−.15	62.54***
Chicago[a]	0.95	−.42	−0.16	1.03	NA[b]	47.67***
Intervention moved back one year						49.20***
Philadelphia[c]	1.65	.04	−0.01	0.19	.07*	10.35**
Intervention moved back one year					.09***	13.46**
Washington, D.C.						
Shield law	−0.80	.002	0.02	0.01	.06	35.33
Corroboration	−5.07**	−.003	0.04	0.06	.08	70.86
Atlanta[a]						
Shield law	−3.94	−.09	0.01***	−0.05	−.003	29.17
Corroboration	0.02	−.05	0.01	−0.07**	.01	42.74*
Intervention moved back one year			0.01***	−0.07**		42.86*
Houston Num =	1.30***	−.14***	0.12***	0.09***	.06*	73.63***
Den =	0.99***					
First two years after reform	17.25***	−.08*	0.06	0.02	.07	84.94**

NOTE: For each jurisdiction we present the intervention coefficient. Detailed results are available from the authors.

[a] In Chicago the percentage indicted variable was logged.

[b] We did not analyze the percentage incarcerated in Chicago because almost all values were 100 percent.

[c] In Philadelphia the percentage convicted on the original charge variable was logged.

[d] In Atlanta the percentage indicted and the percentage convicted variables were logged.

*$p < .05$ **$p < .01$ *** $p < .001$

case processing in Detroit. The case processing variables are measured for the offenses of rape, sodomy, and gross indecency before the 1975 legal changes and for the offenses of first- and third-degree criminal sexual conduct after the changes.[8] Figure 2 presents

[8] The definitional changes that were a major component of reform legislation made it impossible to achieve a perfect correspondence between the offenses before and after the reforms. We excluded second-degree criminal sexual conduct (aggravated sexual contact without penetration) and fourth-degree criminal sexual conduct (sexual contact without penetration) from the measurement of variables because they are crimes that would not have been defined as rape before the laws changed. We included sodomy and gross indecency in addition to rape in the prereform measures because they define sexual acts that are included in first- and third-degree criminal sexual conduct under the new laws. Since both first- and third-degree offenses include acts that were previously defined as rape, it is not possible from the court records alone to sort out these offenses. We included sodomy and gross indecency also because the old offense of rape did not include males as victims whereas the criminal sexual conduct offenses are gender neutral. The correspondence is still imperfect; penetration with an object, for example, is included in criminal sexual conduct under the reform laws, but may have been charged as a non-

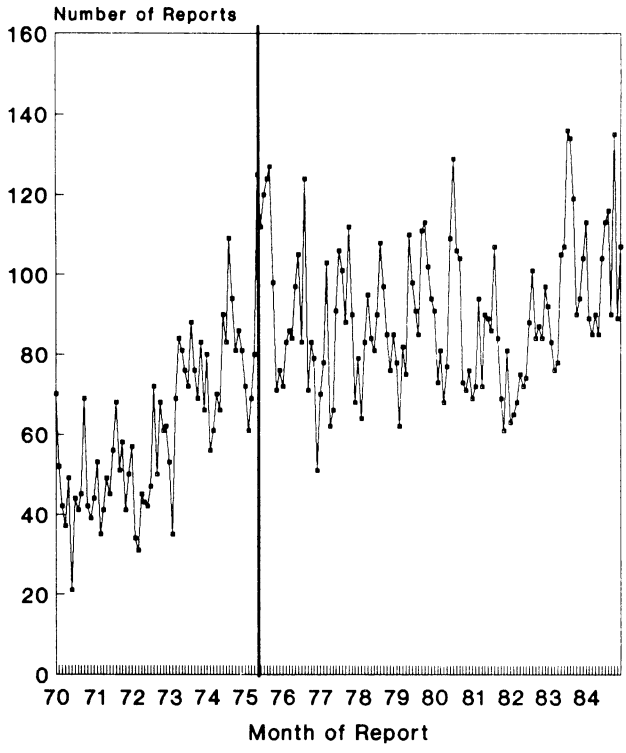

Figure 1. Reports of rape, Detroit, Michigan, 1970–1984

the plot of the ratio of indictments to reported rapes. The time-series analysis of these data indicated that the indictment ratio increased by .18. Thus, not only were there more indictments simply because of an increase in the number of cases reported, but prosecution of these cases was more likely following the legislative changes.

The likelihood of conviction, on the other hand, did not change as a result of the reforms (Fig. 3). With the increase in reports and indictments, however, the steady conviction rate indicates that prosecutors were obtaining more *total* convictions in the postreform period. This was confirmed by a statistical analysis of the absolute number of convictions. We also found that the reforms did not change the likelihood of incarceration but that the average sentence received by those incarcerated increased by about sixty-three months.

Some reformers predicted that the definitional reforms would lead to an increase in plea bargaining, since the graded criminal sexual conduct offenses would make it possible to reduce original charges to charges still within the sexual offense category. When

sexual offense such as assault before the legal changes. The conservative comparisons we chose, although imperfect, seemed to be the best option available.

we examined the percentage of cases convicted on original charges, we found no evidence of a decrease that would correspond to a greater reliance on plea bargaining. In fact, the percentage of cases convicted on the original charge increased after the new laws went into effect, although the increase was not statistically significant.

Michigan's strong and comprehensive reforms produced some, but not all, of the effects anticipated by reformers. The strong evidentiary changes enacted in Illinois (1978) and Pennsylvania (1976), in contrast, had no effect on reports of rape or the processing of rape cases in Chicago[9] or Philadelphia.[10] Figures 4–7 present the plots for reports and indictment ratio for those two cities.[11]

The three cities with weaker reforms also showed almost no evidence of impact for the changes in rape laws. The only significant effect found for Washington, D.C., was a decrease in reported rapes after the elimination of the corroboration requirement. We have no theoretical rationale to explain such a decrease; we suspect that it was merely coincidental with the new law. In Atlanta there were some changes in the processing of rape cases but none that could be attributed to the legal reforms.[12]

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[9] As with the Detroit data, the initial analysis of average sentence for those incarcerated for rape in Chicago indicated a significant impact of the law. Because the plot of the data indicated an increase before the legal reforms, we modeled the series with the intervention moved back one year earlier than the actual reform date. The size of the effect was even larger and still significant, indicating that the effect should not be attributed to the legal reform.

[10] In Philadelphia the only significant impact of the law reform, according to our initial statistical analysis, was on percentage of convicted offenders who were incarcerated and on average sentence for those incarcerated for rape. Both of these effects, however, appeared in the plots to have actually occurred before the changes in Pennsylvania rape laws. We therefore modeled the reform as occurring a year earlier than the actual reform date; both effects were still significant in this analysis, indicating that they were, in fact, due to other factors.

[11] Because the numerator and denominator of our indictment ratio did not necessarily represent the same populations of cases, it was possible for the ratio to be greater than 1. Because this occurred very infrequently (with one observation for Philadelphia and with three observations for Chicago) and because it created outlier values in the time series, we set these values equal to 1 in plotting these figures. The time-series results we present were obtained using the actual values, although we also substituted less extreme values for the outliers and found no difference in the results.

[12] The initial statistical analysis of the Atlanta data indicated that the rape shield law had a significant impact on the likelihood of conviction and that elimination of the corroboration requirement resulted in a smaller proportion of convictions on the original charge. It also revealed that elimination of the corroboration requirement resulted in an increase in the average sentence. In the case of percentage convicted and percentage convicted on original charge, we were concerned about interpretation of these results because in Atlanta we were dealing with such a small number of cases. For much of the prereform period there were fewer than five indictments per month for rape; therefore percentages computed on these values are quite variable. In order to determine if the statistical effects might be reflecting stabilization of percentages due to more cases in the system, we modeled the series with the reforms moved back one year before they actually occurred. In both cases, the effect remained just as large and significant, indicating that these effects cannot be

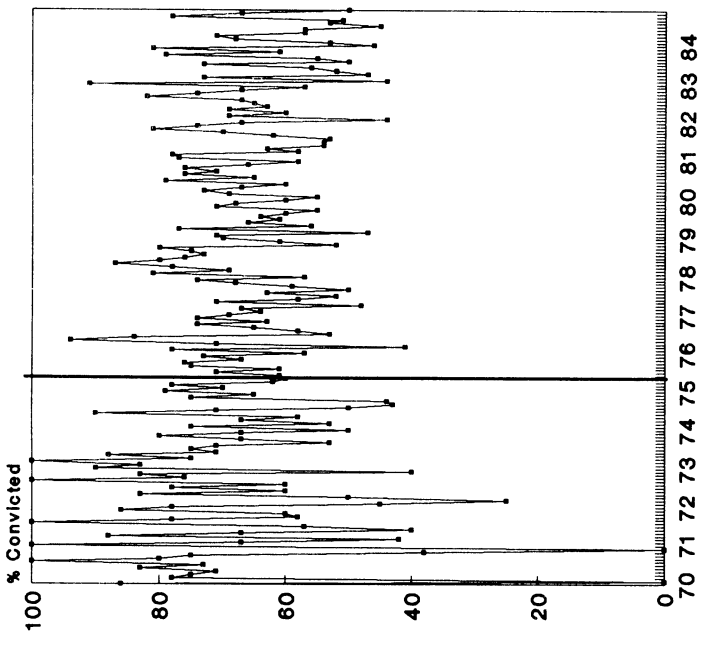

Figure 3. Percentage convicted for rape, Detroit, Michigan, 1970–1984

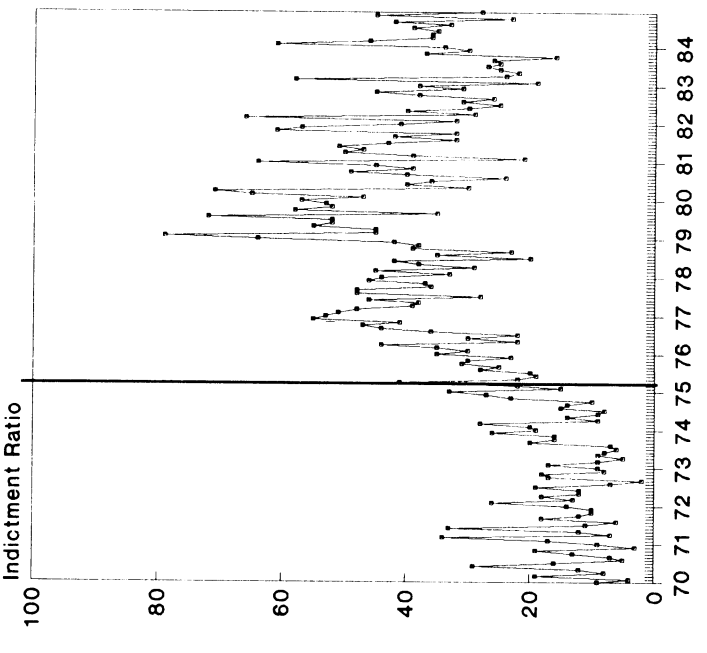

Figure 2. Indictment ratio for rape, Detroit, Michigan, 1970–1984

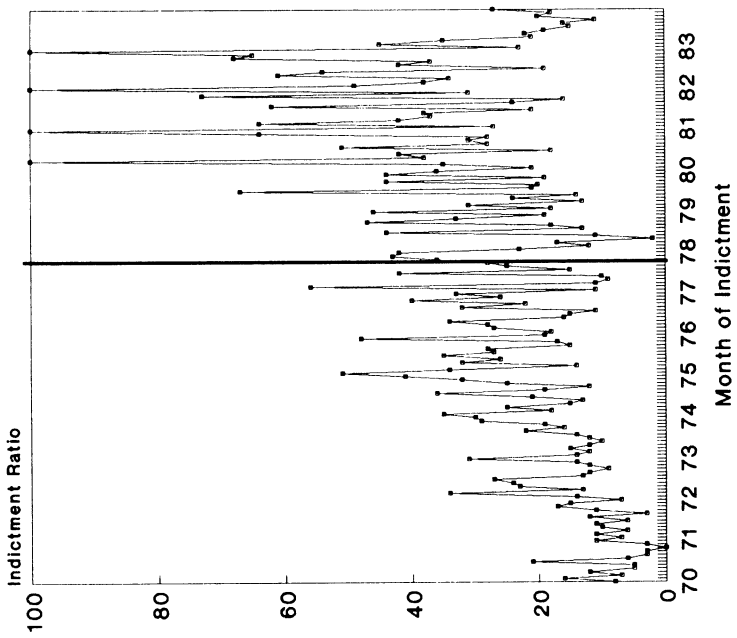

Figure 5. Indictment ratio for Rape, Chicago, Illinois, 1970–1983

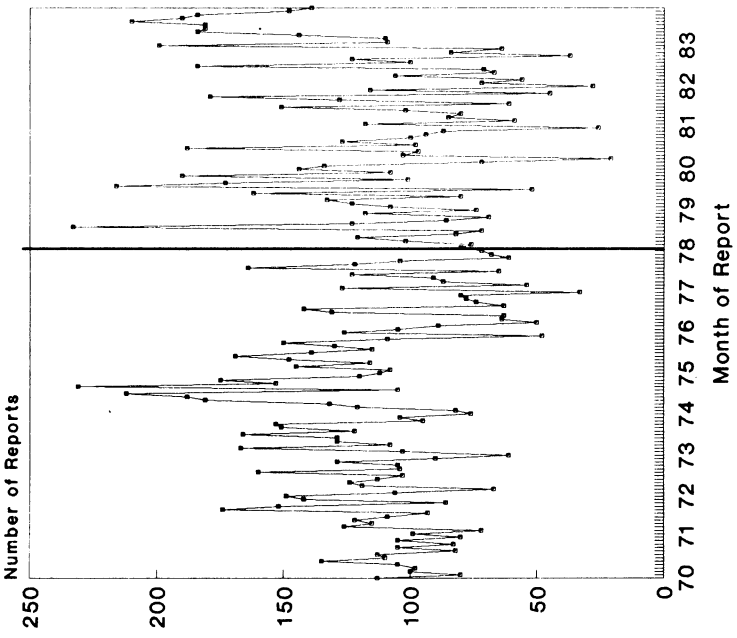

Figure 4. Reports of rape, Chicago, Illinois, 1970–1983

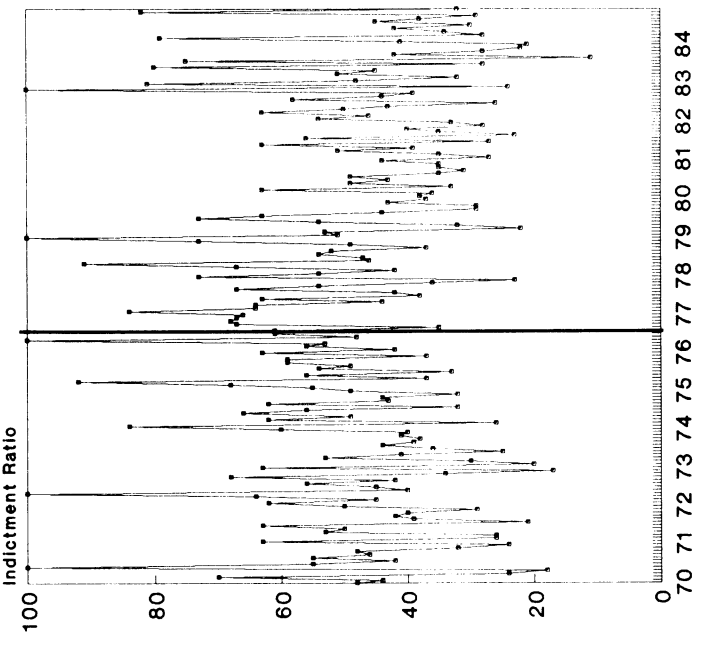

Figure 7. Indictment ratio for rape, Philadelphia, Pennsylvania, 1970–1984

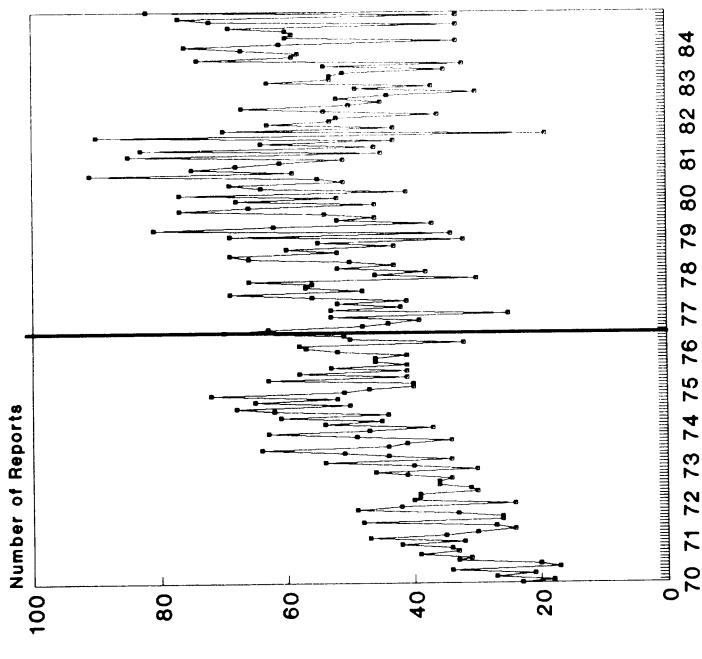

Figure 6. Reports of rape, Philadelphia, Pennsylvania, 1970–1984

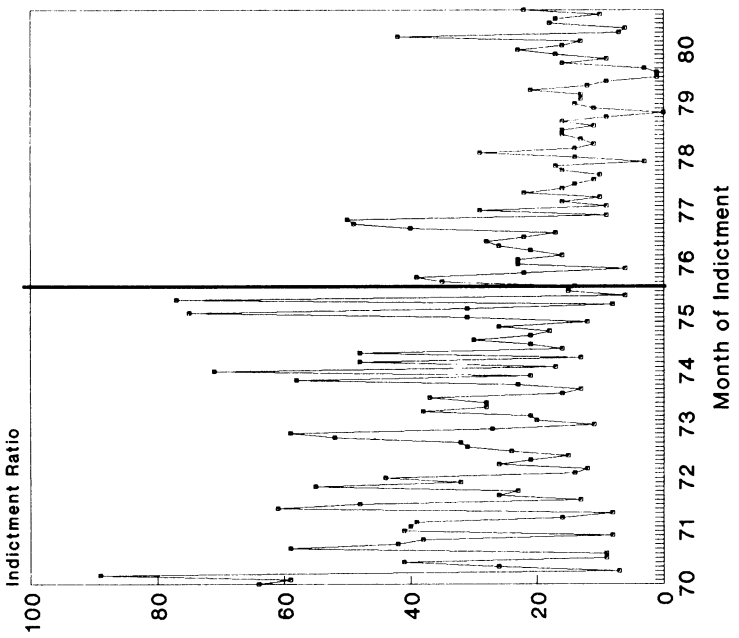

Figure 9. Indictment for rape, Houston, Texas, 1970–1980

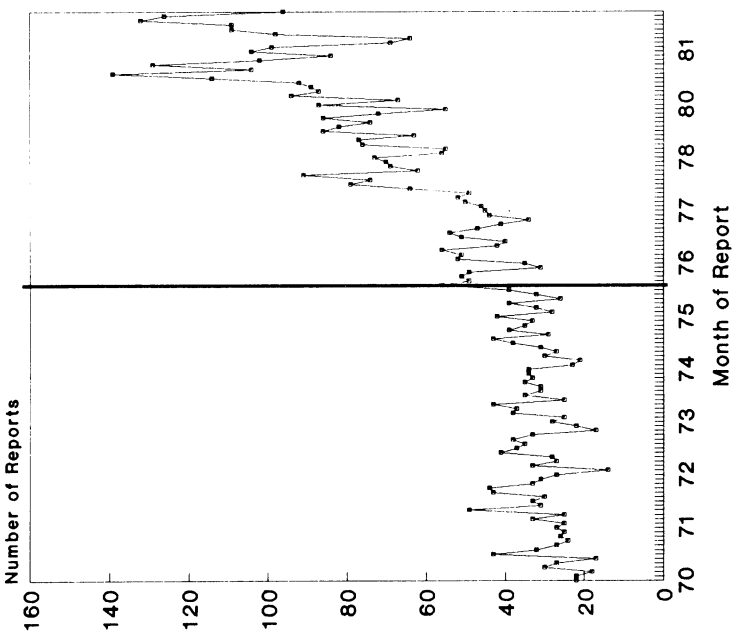

Figure 8. Reports of rape, Houston, Texas, 1970–1981

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We found a number of changes in Houston, but most occurred years after implementation of the new laws, suggesting that they were due to other causes.[13] Still, some changes occurred at the time of the rape law reform.[14] The number of reported rapes increased by an average of 17.25 reports per month, and the indictment ratio *decreased* by .08. The average sentence for those incarcerated for rape increased by almost eighty-five months.

The graph of reported rapes (Fig. 8) shows a long-term increasing trend, and the statistical model that best fit the data was one representing a very gradual increase. Such an effect is quite unlikely to be produced by a legal reform. To test whether the trend might simply be part of a general increase in crime in Houston during those years, we looked at the monthly data for reported robberies and reported assaults for the same period. The plots show long-term trends similar to the trend for reported rapes, but without the increase in level apparent for reported rapes immediately after the law reforms. That slight increase in reported rapes thus might have been produced by the publicity surrounding the reforms.

The significant decrease in the indictment ratio was not what reformers predicted. We suspect that it was a result of the increase in the number of rape reports; as reports of rapes increased in Houston, the number of indictments did not keep pace (Fig. 9). Similarly, the impact on sentences probably follows from the decrease in the indictment ratio. As more cases came into the system and as prosecutors became more selective, it is quite likely that the average case being prosecuted was more serious, producing an increase in average sentence length.

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attributed to the passage of the law reforms. The same thing occurred for the effect of removal of the corroboration requirement on average sentence. Even in the original analysis that variable was significant only at the .10 level.

[13] The initial statistical analyses of the Houston data indicated significant increases for all variables except indictment ratio, which decreased. When we examined the graphs, however, it was apparent for several of the variables that the level remained fairly constant for several years after the Texas law reforms went into effect, and that changes started to show up three to four years later. To determine whether the statistical analysis was reflecting those later increases, we reanalyzed the data, excluding all the months that were more than two years after the law changed. With the truncated series the significant effects disappeared for the variables percentage convicted, percentage convicted on original charge and percentage incarcerated, indicating that the effects found in the original analysis reflected changes that occurred well after the law reforms. Although it would not be unusual to have delayed effects in the implementation of a new law, the length of time between the legal changes in Texas and the signs of impact on those three variables in Houston seems too great to justify concluding that the changes were due to reforms in the laws.

[14] Effects remained for these variables when the truncated series were analyzed.

DISCUSSION

Our analysis of the impact of rape law reforms in six major urban jurisdictions revealed that legal changes did not produce the dramatic results anticipated by reformers. The reforms had no impact in most of the jurisdictions. While the greatest, albeit limited, impact was found in Detroit, where a single reform dramatically changed all the rape laws, a simple strong reform-weak reform distinction cannot explain the pattern of results. We found no greater impact in two jurisdictions with relatively strong reforms—Chicago and Philadelphia—than in the three jurisdictions with relatively weak reforms.

As noted earlier, many reforms have failed because reformers assumed that the behavior of decisionmakers in the criminal justice system is controlled by legal rules. A failure to appreciate the role of discretion in case processing often leads to reforms that do not include adequate incentives for changing behavior. In order to understand our results, we look first at the specific provisions of the rape law reforms, considering how they actually affected decisionmakers.

Definitional Changes

Reformers anticipated that replacing the single crime of rape with a series of gender-neutral graded offenses with commensurate penalties would lead to an increase in convictions. They predicted that the availability of appropriate lesser charges would enable prosecutors to obtain more convictions through plea bargaining and would discourage jury nullification by providing other options to juries reluctant to convict for forcible rape.

We found no evidence of an increased likelihood of convictions in Detroit, where definitional changes took effect, or in any of the other jurisdictions. The fact that we found no decrease in the proportion of cases resulting in convictions on the original charge indicates that there was no increase in plea bargaining. Our interviews led us to believe that the reforms' implicit focus on the seriousness of the crime of rape may have created an unwillingness to plea bargain that counteracted the facilitative effects of the definitional changes. In Detroit, in fact, the Wayne County Prosecutor's office has an explicit policy restricting plea bargaining. The policy requires the complainant's approval prior to reducing charges. In addition, the policy provides that charges of criminal sexual conduct in the first degree may only, except in unusual circumstances, be negotiated down to criminal sexual conduct in the third degree (CSC3) and that CSC3 charges may not be reduced.

Reformers also expected that the new laws would encourage juries to convict on lesser charges in cases that might otherwise have produced an acquittal for forcible rape. This assumes that prosecutors will ask for instructions on lesser included charges.

Prosecutors in Detroit, however, said they were reluctant to ask for these instructions because they feared that jurors would be hopelessly confused if given definitions for criminal sexual conduct in the first, second, third, and fourth degrees. Thus the complexity of the law, considered important for its inclusiveness, may have undermined one of the reformers' goals.

Elimination of Corroboration and Resistance Requirements

Reformers predicted that eliminating the requirements for corroboration and resistance would make it easier to prosecute cases and therefore more likely that prosecutors would file charges and obtain convictions. We believe that reformers were overly optimistic about the effects of these largely symbolic changes. For one thing, court decisions over the years had already considerably loosened both requirements. Officials in every jurisdiction reported that a prompt report or physical evidence of intercourse could corroborate the victim's testimony; thus, it was almost always possible to get past a motion for a judgment of acquittal. As one judge stated, "The case law was so broadly interpreted that a scintilla of corroboration was satisfying." Similarly, courts had ruled that a victim was not required to put her life in jeopardy by resisting an attack, and that evidence of force on the part of the offender was tantamount to proof of nonconsent by the victim. By the mid-1970s the corroboration and resistance requirements could be viewed as minor hurdles if prosecutors wanted to proceed with a case, and formal elimination of the statutory requirements was therefore irrelevant in practice.

More important, elimination of the requirements does nothing to constrain the discretion of decisionmakers. As Nimmer (1978:176) observed, reformers often assume that removing alleged legal obstacles will allow decisionmakers to behave in the "correct" way, when in fact "problems are typically not the product of artificial barriers or constraints but of conscious behavioral choices made both individually and as a group by professionals within the system." As one of our respondents explained, the law may no longer require corroboration, but that does not mean that the prosecutor will file charges when the complainant's story is totally uncorroborated.

Prosecutors often make charging decisions based on their estimates of whether cases could be won before a jury; if they believe a jury will look for corroboration and resistance, they will continue to require them for charging. Many prosecutors we interviewed believed, in fact, that jurors are unlikely to convict in the absence of these factors. As one prosecutor noted, "Juries still expect some resistance or some explanation as to why there was none. This is especially true if it was a date gone sour; if we can't show some resistance in this case we're in a lot of trouble." Con-

cerning corroboration, another stated, "If you're talking about consent defenses, jurors are still looking for corroborating evidence—physical injury, a weapon, a hysterical call to the police; old habits and old attitudes die hard, and we can change the law but we can't necessarily change attitudes."

Juries might be influenced if instructed that the victim need not resist her attacker and that her testimony need not be corroborated. Many officials we interviewed believed, in fact, that it could be very important for jurors to hear this, not from the prosecutor, but from the judge. Even when a statute explicitly states the lack of a need for resistance or corroboration, however, such instructions are given at the discretion of the judge and the prosecutor. Some judges routinely give the instruction; others instruct only if requested to do so by the prosecutor. Some judges reported that the prosecutor always asks for the instruction; some said that prosecutors never do. Thus, the potential for impact of the reforms is again diminished by the discretionary nature of the criminal justice system.

Analysis of elimination of corroboration and resistance requirements, then, suggests that reformers had unrealistic expectations concerning their impact. Although these reforms may have sent an important symbolic message, they did not significantly alter the decisionmaking context. Neither requirement was an insurmountable hurdle before the reforms, and the reforms themselves did not constrain the discretion of prosecutors or jurors. In the postreform period, as in the prereform period, corroboration and resistance evidence may still be important to the successful prosecution of at least some kinds of rape cases.

Rape Shield Laws

Reformers predicted the rape shield laws would have a greater impact on the processing and disposition of sexual assault cases than would the other reforms. They anticipated that the restrictions on evidence damaging to the complainant would prompt more victims to report rapes to the police and would lead directly to an increase in convictions and indirectly to an increase in arrests and prosecutions.

Effects of Weak Shield Laws

We did not expect the weak shield laws of Washington, D.C., Georgia, and Texas to have a significant impact on case processing. The laws adopted in each of these states continue to allow judges considerable discretion in deciding whether to admit sexual history evidence. Case law in Washington, D.C., for example, excludes evidence of the victim's prior sexual conduct with parties other than the defendant but allows evidence of the victim's reputation for chastity if the judge determines that its probative value outweighs

its prejudicial effect. The Georgia law allows evidence of the victim's sexual reputation or sexual conduct with third parties if the judge finds it supports an inference that the accused reasonably could have believed the victim consented. And the Texas law is often cited as an example of the most permissive kind of law (Berger 1977; Galvin 1986). Texas does not categorically exclude any sexual conduct evidence; rather, such evidence can be admitted if the judge finds that the evidence is relevant.

By leaving so much to the judge's discretion, the shield laws enacted in these jurisdictions did little to alter the "rules" for handling rape cases. Weddington (1975–76) observed that the Texas shield law in essence made no change. The motion *in limine* had always been available to prosecutors to exclude irrelevant evidence, and the judge always determined relevance. Prosecutors in Atlanta suggested that Georgia's rape shield law was actually weaker than the case law in effect prior to the law's passage. In both states, much stronger reforms had been presented to legislators. The weak shield laws that were passed can be viewed as symbolic policies designed to placate the interest groups lobbying for change.

Effect of Strong Shield Laws

The rape shield laws enacted in Michigan, Illinois, and Pennsylvania are much stronger. The laws in all three states generally prohibit the introduction of evidence of the victim's past sexual conduct. The prohibition includes evidence of specific instances of sexual activity, reputation evidence, and opinion evidence. There are only very narrow exceptions to the shield. All three jurisdictions permit introduction of the victim's past sexual conduct with the defendant, but only if the judge determines that the evidence is relevant. The shield laws enacted in these states, then, sent a strong message to defense attorneys, prosecutors, and judges. They clearly stated that certain types of sexual history evidence are inadmissible. Unlike the laws adopted in Texas, Georgia, and the District of Columbia, they also attempted to place meaningful limits on judges' discretion to admit certain kinds of evidence.

One important procedural aspect of the rape shield laws is the requirement of an *in camera* hearing for determining admissibility of evidence relating to the victim's sexual history.[15] Our interviews with judges, prosecutors and defense attorneys, however, revealed that *in camera* hearings are rarely if ever held, especially if the ev-

[15] It should be noted that two of the strongest rape shield laws (in Michigan and Illinois) have fairly weak procedural requirements. The Michigan statute states that the judge *may* hold an *in camera* hearing to determine admissibility if the defense makes a written motion to offer sexual history evidence. The Illinois statute provides that the judge shall hold a hearing to determine admissibility in the event that the complainant denies the sexual relationship with the defendant.

idence concerns sexual conduct between the victim and the defendant. Prosecutors reported that they generally concede the relevance of evidence of a prior sexual relationship between the victim and the defendant and do not challenge defense attorneys who attempt to introduce the evidence without requesting a hearing. Similarly, judges use their discretion to overlook the *in camera* requirement or to overrule prosecutors' objections to the introduction of the evidence.

It is not surprising that criminal justice officials have found ways to circumvent the formal procedural requirements of the shield laws. As Casper and Brereton (1984:123) note, "implementors often engage in adaptive behavior designed to serve their own goals and institutional or personal needs." *In camera* hearings are time consuming and would be a waste of time if judges routinely rule that evidence of a prior relationship between the victim and the defendant is relevant. Rather than going through the motions of challenging the evidence and perhaps alienating other members of the courtroom workgroup, prosecutors concede the point.

Noncompliance might also be attributed to agreement among prosecutors and judges that evidence of a prior sexual relationship between the victim and the defendant is always relevant to the issue of consent. Reformers believed that the relevance of this kind of evidence would depend on factors such as the nature and duration of the sexual relationship or the separation in time from the alleged rape. We believe decisionmakers have developed a much simpler rule based on shared norms of relevance and fairness in evidentiary issues. Their "admissibility rule" states that if the sexual conduct was with the defendant, it is relevant. Like "going rates" in sentencing (Feeley 1979; Loftin et al. 1983) or "normal crime" categories in charging (Mather 1974; Sudnow 1965), the rule routinizes and simplifies the decisionmaking process.

The disregard of the requirement for hearings contradicts Nimmer's (1978) assertion that legal rules are most effective when they specify procedural steps in case processing. The *in camera* hearings required by rape shield laws, however, differ from other procedural requirements in one important way. While the laws mandate hearings in certain situations and clearly specify the procedures to be followed, they do not provide for review or sanction of judges who fail to follow the law. Moreover, if a defendant is acquitted because the judge violated the law and either admitted potentially relevant evidence without a hearing or allowed the defense attorney to use legally inadmissible evidence, the victim cannot appeal the acquittal or the judge's decisions. If, on the other hand, the judge followed the law and refused to admit seemingly irrelevant sexual history evidence, the defendant can appeal his conviction. All the consequences, in other words, would lead judges to err in favor of the defendant.

The avoidance of *in camera* hearings clearly undermines the reforms to the extent that issues of relevance are not debated if the sexual conduct was between the victim and the *defendant*. Their absence does not mean legally prohibited evidence of sexual conduct between the victim and *third parties* is being admitted. To the contrary, it appears that hearings are avoided on these issues because the members of the workgroup agree that such evidence cannot be admitted. The other side of the admissibility rule, in other words, is that sexual conduct between the victim and parties other than the defendant is not relevant. Judges in every jurisdiction stated that defense attorneys don't even attempt to introduce the more questionable kinds of sexual history evidence. As one judge in Chicago explained, "Attorneys are warned that I will interpret the law strictly and they don't even try to bring it up unless it concerns the victim and the defendant."[16]

If evidence clearly proscribed by the law is effectively excluded, we must consider other explanations for the lack of impact of the strong rape shield laws. Why is it that even these strong laws did not produce the types of changes envisioned by reformers? For one thing, the shield laws primarily affect cases that go to trial and, particularly, the small percentage of cases tried before a jury. Moreover, sexual history evidence is only relevant in cases where the defense is consent. Since it is unlikely that consent will be the defense when a woman is raped by a total stranger, this means that sexual history evidence will be relevant only when the victim and the defendant are acquainted. The shield laws, then, have the potential to affect directly only the relatively few rape cases in which the victim and the defendant are acquainted, the defendant claims the victim consented, and the defendant insists on a trial.

Unfortunately, no data are available on how often a complainant's sexual history entered into cases before the rape shield laws were enacted. Although reformers cited horror stories regarding harrassment of victims in court, most respondents in jurisdictions we studied could recall few, if any, prereform cases in which defense attorneys used this tactic. If testimony regarding the victim's sexual history with third parties was rarely introduced, then restricting the use of such evidence would produce little change. Respondents in several jurisdictions reported, in fact, that previous case rulings had accomplished much of what the rape shield laws were designed to do.

We have discussed the weaknesses of the individual reforms and have explained why the reforms did not produce the instrumental results anticipated by reformers. These results might also

[16] We also presented officials with hypothetical cases (Spohn and Horney 1991), and they occasionally said that evidence of special relations with third parties might be admitted, but this occurred only in fairly unusual cases, and few respondents rated admissibility as very likely even in those cases.

be due to the fact that the reforms had the potential to affect only certain types of cases. Estrich (1987) has emphasized the importance of the distinction between aggravated rape and "simple" rape. Aggravated rapes are those that involve strangers, multiple assailants, or armed force; simple rapes are committed by unarmed acquaintances, acting alone. She has suggested that traditional rape law provisions represented "a set of clear presumptions applied against the woman who complains of simple rape" (Estrich 1987:28). She has also asserted (ibid., p. 29) that historically the processing of rape cases has not been characterized by indiscriminate sexism, but that there has been and still is

> a far more sophisticated discrimination in the distrust of women victims: all women and all rapes are not treated equally. As the doctrines of rape law were developed in the older cases, distinctions were drawn, explicitly and implicitly, between the aggravated, jump-from-the-bushes stranger rapes and the simple cases of unarmed rape by friends, neighbors, and acquaintances. It was primarily in the latter cases that distrust of women victims was actually incorporated into the definition of the crime and the rules of proof.

Estrich maintains that resistance and corroboration requirements were loosened in aggravated rape cases and that evidence of a victim's past sexual conduct was only considered relevant in simple rape cases.

If Estrich is correct, then it follows that most of the rape law reforms have been directed at simple rape cases, and thus the greatest impact should have been seen in these cases. If, as we argue above, the laws fail to place meaningful constraints on the discretion of decisionmakers, then impact could only be achieved by modifying decisionmakers' basic distrust of victims of simple rape. Further research should address this issue.

Evidence of Impact

Two sites did show some evidence of impact. In Detroit we found increases in reports of rape, in the ratio of indictments to reports, and in the length of the maximum sentence. In Houston also we found some evidence of increases in reporting and sentence length, but they were accompanied by a decrease in the indictment ratio.

Reports

We were surprised to find increased reports of rape in the jurisdiction with the weakest reforms (Houston) as well as in the jurisdiction with the strongest reforms (Detroit). The appearance of the increase just as the new laws went into effect suggests that the increases may have resulted from the publicity surrounding the re-

forms.[17] If an increase in reporting resulted from actual improved treatment of victims due to the legal changes, we would expect a more gradual impact on reporting as knowledge of the improved treatment spreads through the community. Unfortunately, the scope of our study limits our ability to interpret these results. Because the legal reforms in Houston and Detroit were among the earliest in the country and because they coincided with extensive national media attention to the crime of rape, we suspect that their implementation may have occasioned more publicity than resulted in the other jurisdictions. However, we have no data to support this speculatve explanation.

It is also possible that the increases in reporting we detected actually reflect changes in police behavior rather than in the behavior of victims. We had to rely on Uniform Crime Reports to measure reporting by victims, and these data are a product not only of victim complaints, but also of police decisions on whether to count complaints as valid reports of criminal incidents. Again, however, we do not have data on police department operations that would allow us to test this possibility.

Indictments

The most interesting results are our findings on the impact of reforms on the likelihood of indictment. Our analysis revealed a significant increase in the ratio of indictments to reports in Detroit, but a significant decrease in the likelihood of indictment in Houston. As we noted earlier, it seems very likely that the decrease in Houston represents a failure to keep up with the increase in reported rapes. If prosecutors simply continued to prosecute about the same number of cases, an increase in cases entering the system would result in a decrease in the indictment ratio. In Texas, indictment is by grand jury; the additional burden of taking a case to the grand jury may have affected judgments about how many cases could be prosecuted.

In Detroit we found an increase in the indictment ratio even as the number of reported cases increased. The results could have been produced by changes in police or prosecutor decisions or by both. It seems more likely that prosecutors would be most affected, because the legal changes generally involved evidentiary rules affecting the likelihood of obtaining convictions at trial. Our interviews tended to confirm this. Detroit police reported that prosecutors were refusing fewer warrants in rape cases since pas-

[17] Our finding of an increase in reports in Detroit is seemingly at odds with the findings of no change reported by Marsh et al. (1982). Marsh and her colleagues, however, used statewide data while our data were only for Detroit. Thus, it is possible that jurisdictional differences account for the discrepant outcomes.

sage of the laws, and a victim-witness unit respondent said that more "date rape" cases were getting into the court system.[18]

This increase in the indictment ratio suggests that prosecutors are more willing to file charges in borderline cases. We can speculate that some of the additional cases being reported in the postreform period were the kinds of cases victims were reluctant to report prior to the passage of reform legislation: cases involving acquaintances, cases involving sexually promiscuous men or women, cases with little or no corroborating evidence, and so on. Presumably, some of these additional cases were the simple rape cases that Estrich (1987) argues were so difficult to prove in the prereform era. Given this assumption, we might have expected the indictment rate to decline. The fact that it increased even as reports increased is thus an important finding.

The greater willingness to prosecute might be due in part to the fact that the definitions of the various degrees of criminal sexual conduct are much clearer than the old definition of rape. The current Michigan law provides clear guidelines for prosecutors to follow in screening rape cases. It carefully defines the elements of each offense, specifies the circumstances that constitute coercion, and lists the situations in which no showing of force is required. Although the judges, prosecutors, and defense attorneys we interviewed in Detroit thought the new laws might be confusing to jurors, they nevertheless spoke approvingly of the clarity and precision of the new statute. One prosecutor commented that "the elements of force and coercion are clearly spelled out." Another explained that the law "sets out with greater particularity what the elements of the offense are." By spelling out the acts that constitute sexual assault, the circumstances that imply consent and nonconsent, and the types of evidence that are unnecessary or irrelevant, the Michigan laws may have made it easier to recognize acceptable cases.

Although our finding of no increase in the conviction ratio[19] in Detroit represents a failure of reformers' expectations, the stabil-

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[18] One possible interpretation of the impact on indictment rate is that it was produced by definitional changes that were part of the comprehensive reforms, making it impossible to have perfectly comparable measures of rape before and after the laws changed. We think that this explanation is unlikely, however, because we were able to come very close in matching the pre- and postreform measures.

[19] Marsh et al. (1982), who also studied rape law reform in Michigan, reported a significant increase in the number of convictions on original charges and a significant decrease in convictions on lesser charges. They also reported an increase in the rate of convictions as charged (measured as convictions as charged divided by reports). We found no change in the the overall rate of convictions (measured as convictions divided by indictments), and no significant increase in the percentage of convictions on the original charge.

The Marsh time-series results for convictions are for the absolute number of convictions as charged and the absolute number of convictions for lesser charges. When we analyzed the total number of convictions, we also found a significant increase. We concluded, however, that this increase simply re-

ity of that ratio is important. If, as we suggested, the increase in indictments that occurred resulted from more borderline cases entering the system, we might have expected a decline in the overall likelihood of conviction. The fact that the total number of convictions kept pace with the increase in indictments suggests that defendants in these borderline cases *are* being convicted.

Sentencing

We found an increase in average sentence coinciding with the change in law in Houston and Detroit and increases in sentence length over the time period studied but not attributable specifically to the law reforms in other jurisdictions. We suspect that these widespread increases reflect changes in attitudes toward the crime of rape rather than changes in the laws of rape. The enactment of rape law reforms, while not aimed directly at increasing sentences for rape, reflected public demands that rape be treated as a very serious offense. Judges may have responded by imposing more severe sentences on those convicted for rape. These changes in attitudes toward the crime of rape might also have fostered a reluctance to plea bargain—and a consequent increase in sentence severity—in rape cases.

The Effect of Comprehensive Changes

Although we found some evidence in Houston of increases in reporting and in sentence length, the impact we found in Detroit, although limited, stands out as the only example of the reforms affecting official decisionmaking in the manner predicted by reformers. One interpretation of this finding is that the criminal justice system can only be affected by the kind of dramatic, comprehensive changes enacted in Michigan. Clearly, the Michigan reform was broader than those adopted in the other five jurisdictions we studied. It also was accomplished in one major revision of state codes. Although we have explored the weaknesses of the individual legal changes, it may be that only a comprehensive reform package, by sending a strong and unambiguous message to deci-

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flected the increase in reports and indictments, since we did not find an increase in the percentage of indictments resulting in convictions.

Marsh and her coauthors also considered the conviction rate by looking at the number of convictions divided by the number of reports. They did no time-series analysis of that rate, however, but presented the yearly figures for seven years. Our analysis of the conviction rate showed no evidence of any increase across those years, but there is a key difference in the variables we analyzed. They considered convictions divided by reports, while we measured convictions divided by indictments. Since our study indicated that the percentage of reported cases being indicted increased, the increase over the seven years that Marsh et al. (1982) show in the conviction rate probably reflects the greater likelihood of cases getting into the system, rather than a greater likelihood of conviction once the cases are in court.

sionmakers, can overcome the resistance to change inherent in the system.

Our findings clearly contradict Nimmer's (1978:181) prediction that "the probability of system change is inversely related to the degree of change sought by a reform." Nimmer and others (Eisenstein et al. 1988) have asserted that a reform involving drastic change will encounter greater resistance within the system, especially when the behavior affected is perceived as important. Nimmer cited as an example the impact of required plea bargaining conferences in Detroit and Denver. The reform in Detroit, which required relatively minor changes in behavior, was successful, while the Denver reform, which sought to drastically shift the time to disposition, had no impact on timing of guilty pleas. The kinds of reforms involving speedy dispositions are very different from the rape law reforms, which attempted to effect major changes in attitudes as well as behavior. We suspect that if attitudes are to be changed, dramatic, comprehensive changes that demand attention may be required. Marsh et al. (1982) quoted a judge who described the Michigan reform: "The law was so different from the previous statute, it was so much more comprehensive and complex that it required a total administrative change. It became important for every person in the system to attend seminars and training sessions to determine what the new law would mean for him or her."

We do not have measures of officials' attitudes before and after the law reforms, but we did conduct interviews across our six jurisdictions following the reforms. Although respondents in all jurisdictions expressed attitudes generally favorable toward the legal reforms, Detroit officials took the strongest positions on excluding complainant's sexual history when they were questioned about a series of hypothetical cases (Spohn and Horney 1991). We cannot be sure, of course, that those attitudes were the product of the comprehensive reforms and not a causal factor that led to their enactment.

The Effect of System Variables

To understand the impact of a reform it is important to understand not only the characteristics of the reform itself but also the structure of the system on which it is imposed. The jurisdictions we studied differed in a number of ways. They varied, for example, by method of judicial selection, by whether prosecutors screened arrest charges, by whether charging was by grand jury or preliminary hearing, by the predominance of trials versus plea bargaining, and by whether the prosecutor's office had a special unit for handling rape cases. Because the rape law reforms were directed toward trial proceedings, we might have expected that the jurisdiction with a much higher rate of trials (Philadelphia) would have

shown more impact. This was not the case. Similarly, we might have predicted greater change in that same city because of the special rape prosecution unit. Our primary finding was the overall lack of impact of rape law reforms in spite of these differences.

Detroit, however, differs from the other five jurisdictions in a number of ways that might be relevant to the effectiveness of the reforms in that jurisdiction. First, although every jurisdiction had a rape crisis center, only in Detroit is the Rape Counseling Center run through the Police Department. This close association gives the Counseling Center earlier and greater access to victims than in many cities and thus potentially greater influence in encouraging reporting and pressing for prosecution.

A second noteworthy difference is the centralized policy orientation of the Wayne County Prosecutor's office. The Wayne County Prosecutor was unusual in requiring formal training of new prosecutors, having formal policies on practices such as plea bargaining, and having supervisors carry out formal review of assistant prosecutors' decisions. These factors may be more important than having a special unit for rape cases in ensuring that the goals of rape law reform are met. We suspect that such centralized control serves to greatly weaken the kind of courtroom workgroup effects that operate to resist change. In fact, a sensitivity to the role of courtroom workgroups led the Wayne County Prosecutor, in 1984, to implement a policy of rotating deputies to different courtrooms every four months. Defense attorneys had always been assigned to cases rather than courtrooms. Rather than a typical public defender system, Detroit has a private defender corporation that handles about 25 percent of the indigent cases, while the rest are handled by private attorneys. With less chance for workgroups to develop and function autonomously, reforms may have a greater chance to effect instrumental change.

The fact that the rape law reforms had an impact on case processing in Detroit but not in the other five jurisdictions, then, can be attributed to a combination of factors. The strong and comprehensive laws enacted in Michigan were more than a symbolic response to a vocal constituency clamoring for change. The laws defined new crimes, mandated new procedures, and limited the discretion of criminal justice officials. Although, as we argue above, individual components of the reform package had limited potential to affect case outcomes, the comprehensive nature of the reform may have overcome resistance to change inherent in the criminal justice system. Coupled with the weaker workgroups and other contextual factors which distinguish Detroit from the other jurisdictions, this comprehensive approach to reform may explain our results.

CONCLUSION

We have shown that the ability of rape reform legislation to produce instrumental change is limited. In most of the jurisdictions we studied, the reforms had no impact. Our results are not surprising in light of the large body of literature detailing the failure of legal reforms. We found, like many others who have studied reforms aimed at the court system, that the rape law reforms placed few constraints on the tremendous discretion exercised by decisionmakers in the criminal justice system.

The results of our study suggest that instrumental change will be especially difficult to achieve when reforms are designed to remove legal barriers to prosecution and conviction. If, for example, workgroup norms support prosecution and conviction of rape cases in which the victim did not resist or her allegations cannot be corroborated, then officials find ways to circumvent the legal barriers, and as a result the official removal of these barriers will have little effect in practice. If, on the other hand, informal norms oppose prosecution and conviction of these kinds of cases, simply removing the legal barriers will be ineffective unless the discretion that allows informal norms to guide decisionmaking is constrained or meaningful incentives to change the norms are created.

Reforms aimed at improving the treatment of victims may be less likely than other reforms to provide even minimal constraints on discretion or incentives to change. Victim-oriented reforms are unlikely to facilitate the smooth and efficient flow of cases through the system, and they may conflict with values concerning the rights of defendants. In our legal system, the defendant has considerably more "power" than the victim. Not only are the rights of the defendant constitutionally protected, but in defending those rights the defendant has, at least in theory, an advocate in the defense attorney. The prosecutor does not play the same role for the victim, but is instead an advocate for the state, and the interests of the state may often conflict with those of the victim. The protections afforded the defendant are further guaranteed by the process of appellate review. Judicial decisions that negatively affect the defendant can be appealed to a higher court; decisions that impinge on a victim by decreasing the likelihood of conviction are legally unreviewable.

This suggests that the advocates of rape law reform may need to create incentives for change by monitoring implementation of the reforms and by applying public pressure on criminal justice officials who fail to comply. In some jurisdictions the officials we interviewed said that there was intense monitoring at the time of the law reforms but that the public and the media lost interest shortly thereafter. These officials perceived very little continuing attention to their handling of rape cases. Such attention may be necessary to produce and preserve change.

The fact that the rape law reforms did not produce the broad effects anticipated by reformers does not mean, of course, that the reforms had no impact. Most respondents in the six jurisdictions expressed strong support for the reforms, which they felt had resulted in more sensitive treatment of victims of rape. Officials believed that passage of the reforms sent an important symbolic message regarding the treatment of rape cases and rape victims.

In the long run, this symbolic message may be more important than the instrumental change that was anticipated but generally not accomplished. Under the old laws it was assumed that chastity is relevant to consent and credibility; that corroboration is required because women tend to lie about being raped; and that resistance is required to demonstrate nonconsent. These assumptions clearly were archaic in light of changes in attitudes toward sexual relations and toward the role of women in society. Those who lobbied for rape law reform sought to refute these common law principles and to shift the focus in a rape case from the reputation and behavior of the victim to the unlawful acts of the offender. In doing so, the reforms may have produced long-term attitude change that is difficult to measure in a legal impact study.

We did find more than symbolic impact in Detroit. We speculated that the increase in the indictment ratio there represented a greater willingness to prosecute in "borderline" cases. Because we were not able to examine the impact of the reforms on different kinds of cases, we could not test this hypothesis directly. We also could not test for more subtle kinds of impact that might have been masked when we analyzed case outcomes overall. We have discussed Estrich's (1987) focus on the distinction between "aggravated" rape and "simple" rape and her assertion that these cases have always been dealt with in very different ways. If the proportion of real and simple rapes entering the courts has changed, some effects of the rape law reform could have been masked. Further research should address this issue.

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