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THE IMPACT OF RAPE REFORM LEGISLATION

Executive Summary

Final Report to the National Institute of Justice and
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INTRODUCTION

Since the early 1970s there has been growing concern with the response of the criminal justice system to the crime of rape. Feminists, social scientists and legal scholars have questioned the special status of rape as an offense for which the victim, as well as the defendant, is put on trial. They have suggested that the laws and the rules of evidence unique to rape are at least partially responsible for the unwillingness of victims to report rapes and for the low rates of arrest, prosecution and conviction. They also have contended that these laws and rules of evidence result in pervasive skepticism of rape victims' claims and allow 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. They argue, in short, that "it is easy to commit rape and get away with it" (Rodabaugh and Austin, 1981: 17).

There is abundant evidence in support of these claims. Estimates of the ratio of unreported to reported rapes vary from a conservative figure of two to one (McDermott, 1979) to a "probably exaggerated" figure of ten to one (McCahill, Meyer and Fischman, 1979: 84) to Brownmiller's (1975) figure of "possibly" twenty to one. Estrich (1987) argues that the validity of these figures depends on whether "simple" rapes, as well as "aggravated rapes," are counted. She contends that if the simple cases are included -- cases where a woman is forced to have sex by a man she knows who does not beat her or attack her with a gun -- "then rape emerges as a far more common, vastly underreported and dramatically ignored problem" (Estrich, 1987: 10).

Researchers also have documented substantial attrition in rape cases, beginning with the police officer's decision to "unfound" the complaint and ending with the judge or jury's decision to acquit the defendant. The FBI reported that nationally 19 percent of all rape complaints were unfounded by the police in 1975 (U.S. Department of Justice, 1976). This figure has been termed "appreciably higher" for rape than for other crimes (McCahill et al., 1979). It also has been interpreted by some as proof that police are unduly skeptical of the claims of rape complainants (Brownmiller, 1975, McCahill et al., 1979) and by others as evidence that rape victims are more likely than victims of other crimes to lie (MacDonald, 1971). Even if the police decide to file charges, there is a 50 percent chance the offender will not be caught and arrested (U.S. Department of Justice, 1976). Even if an arrest is made, studies show that conviction is unlikely. An analysis of arrests for rape in Washington, D.C., for example, found that only 20 percent resulted in conviction (Williams, 1978). Similarly, only 25 percent of the arrests in New York City (Vera Institute of Justice, 1981), 32 percent of the arrests in Indiana (La Free, 1980) and 34 percent of the arrests in California (Galvin and Polk, 1983) resulted in conviction.

Feminists and social scientists have suggested that this attrition in rape arrests is due in part to the fact that criminal justice officials use legally irrelevant evaluations of the rape victim in decisionmaking. Many researchers have commented upon the effect of extralegal factors in rape cases. They have shown that the treatment of men accused of rape is influenced by victim "misconduct" such as hitchhiking or drinking (Bohmer, 1974; Kalven and Zeisel, 1966; LaFree, 1981; McCahill, et al., 1979; Nelson and Amir, 1975), by the victim's reputation (Amir, 1971; Feild and Bienen, 1980; Feldman-Summers and Linder, 1976; Holmstrom and Burgess, 1978; Kalven and Zeisel, 1966; McCahill, et al., 1979; Reskin and Visher, 1986) and by the victim's age, occupation and education (McCahill, et al., 1979).

Researchers also have found that the relationship between the victim and the accused has a strong effect on the outcome of sexual assault cases; rapes involving strangers are taken more seriously than rapes involving acquaintances. McCahill, et al. (1979) found that police investigate reports of rape by a stranger much more thoroughly than reports of rape by a friend or acquaintance. The prior relationship between the victim and the defendant also has been shown to affect the prosecutor's decision to file charges or not (Battelle Memorial Institute, 1977; Loh, 1980), the decision

to dismiss the charges rather than prosecute fully (Vera Institute of Justice, 1981), and the likelihood the defendant will be convicted (Battelle Memorial Institute, 1977) or incarcerated (McCahill, et al., 1979).

Evidence such as this has led a number of authors to conclude that all rapes are not treated equally (Bohmer, 1974; Estrich, 1987; Griffin, 1977; Ireland, 1978; Williams, 1984). They argue that the response of the criminal justice system is predicated on stereotypes about rape and rape victims and that the most serious dispositions are reserved for "real rapes" involving "genuine victims." Other victims (i.e., those who knew their attacker or who somehow "precipitated" the attack by their dress, behavior or reputation) must prove that they are worthy of protection under the law. As Ireland (1978: 188) notes, in these circumstances "it is the victim rather than the defendant who is placed on trial."

THE RAPE REFORM MOVEMENT

The rape reform movement emerged in the early 1970s in response both to feminists' concerns about the treatment of rape victims and to a nationwide preoccupation with "law and order." Women's groups, led by the National Organization of Women's (NOW) Task Force on Rape, lobbied state legislatures to reform antiquated rape laws "to reflect and legitimate the changing status of women in American society" (Marsh, Geist and Caplan, 1982: 3). They were joined in their efforts by crime-control advocates, notably police and prosecutors, who were alarmed by dramatic increases in rape during the late 1960s and early 1970s and who urged rape reform as a method of encouraging more victims to report rapes and to cooperate with criminal justice officials in prosecuting rapists. Together these groups formed a powerful, although perhaps ill-matched, coalition for change. By the mid-1980s nearly all states had enacted rape reform legislation.

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. The intent was to "counteract the historical bias against rape victims by giving notice that the rights of the rape victim will no longer be subordinated to those of the accused" (Sasko and Sesek, 1975: 502). To accomplish this, states enacted reform statutes which vary in comprehensiveness and encompass a broad range of reforms. The most common changes are: (1) redefining rape and replacing the single crime of rape with a series of graded offenses defined by the presence or absence of aggravating conditions; (2) changing the consent standard by eliminating the requirement that the victim physically resist her attacker; (3) eliminating the requirement that the victim's testimony be corroborated; and (4) placing restrictions on the introduction of evidence of the victim's prior sexual conduct. Reformers expected that these changes would reduce both the skepticism of criminal justice officials toward the claims of rape victims and their reliance on extralegal considerations in decisionmaking. They anticipated that the reforms ultimately would lead to an increase in the number of reports of rape, and would make arrest, prosecution and conviction for rape more likely.

THE IMPACT OF 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. Two studies examined the impact of the 1974 Michigan criminal sexual conduct statute, the most sweeping rape law reform in the country. The more comprehensive of these analyzed monthly data from three years before and three years after the reform (Marsh, et al., 1982). Time-series analysis of these data revealed increases in the number of arrests and convictions for rape, but no change in the number of crimes reported to the police. Caringella-MacDonald (1984) compared post-reform attrition and conviction rates in Kalamazoo County, Michigan, with rates from three jurisdictions with more traditional rape laws. She concluded that the differences in these rates provided "indirect" evidence that the Michigan law had an effect.

Loh (1981) evaluated the less-sweeping rape reform statute enacted in the state of Washington

in 1975. He used a simple before-and-after design to examine the effect of the law on the prosecution of rape cases in King County (Seattle) from 1972 to 1977. Loh found no change in charging decisions and concluded that prosecutors had not altered their standards for determining "convictability." He also found no change in the overall rate of conviction, although more convictions were for rape rather than some other offense such as assault. Finally, the incarceration rate declined slightly after the reform, but commitment to inpatient sexual offender treatment facilities rose.

Mixed results were reported by Polk (1985), who used statewide yearly data on the processing of rape cases from 1975 to 1982 to examine the effect of rape reform statutes in California. He discovered that there had been no significant change in the police clearance rate or the conviction rate. On the other hand, he found that the percentage of arrests for rape that resulted in the filing of a felony complaint was up slightly, as was the incarceration rate for those convicted of rape.

Gilchrist and Horney (1980) used time-series analysis to evaluate the moderately reformed rape statutes enacted by Nebraska in 1975. They found no evidence of a reform-related increase in the proportion of cases reaching the courts or in the conviction rate. The data indicated a slight shift in the kinds of plea bargains being arranged, but did not support the hypothesis that the separation of two degrees of sexual assault would lead to more plea bargaining.

Limitations of Early Studies. These empirical studies provide some evidence on 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. For example, the time-series design used in the Michigan evaluation did not include controls for the "threat of history," i.e., for the possibility that events other than the legal changes could have been responsible for the effects noted. The authors, in fact, stated that "a nagging concern throughout the evaluation of the law reform derived from realization that the changes detected could have happened in the absence of the legal reform" (Marsh, et al., 1982: 82).

The factor most likely to compete with the legal changes as a cause of increased arrests and convictions is the influence of the women's movement. The activities of women's groups during the early 1970s heightened public awareness of the rape problem and of the need for greater sensitivity in the treatment of rape victims. The Michigan reform occurred at the height of this publicity and thus the effects of the two could be confounded. Both the law reform and changes in the processing of rape cases, in other words, might be reflections of changes in public attitudes. In fact, Michigan judges interviewed for the study attributed changes in jurors' willingness to convict in sexual assault cases, not to the law reform, but to changes in public attitudes and the impact of the women's movement (Marsh, et al., 1982: 56).

A further limitation of the studies is the short time span included in the analysis. None of the studies collected data for more than three years following the reform, so it is possible that the effects detected may have been transient ones and that delayed effects may have gone undetected. Casper and Brereton (1984) have pointed out the need for extensive follow-up in legal impact studies. It is not uncommon in the criminal justice system for a reform to produce immediate changes, but for the actors in the system to revert later to old norms of behavior. In their eagerness to evaluate reforms as soon as possible, researchers may miss these changes. There is evidence for such a possibility in the Michigan experience, where total convictions for sexual assault were decreasing during the third year after the reform, at the end of the evaluation period.

Perhaps the most serious deficiency of the studies discussed above is that each was conducted in only one state; no one has compared the impact of different kinds of reforms in different jurisdictions. Each jurisdiction processes rape cases in slightly different ways and these differences could affect the implementation and impact of the reforms. Since many of the reforms are most relevant to rape cases

which go to trial, their impact may be greater in jurisdictions which try a greater percentage of cases. The evidence reforms may have less impact in jurisdictions that use grand juries to return indictments, since it has been suggested that grand jury proceedings are very difficult for rape victims (Battelle Memorial Institute Law and Justice Study Center, 1978). The reforms may have greater impact when a police department or a prosecutor's office considers sexual assaults important enough to warrant setting up a special division for handling those crimes. Untangling the effects of these system variables requires a multi-jurisdiction study.

The inconclusiveness of the studies conducted thus far points to a clear need for additional empirical research on the impact of rape law reforms. The purpose of our study is to provide both breadth and depth of information about the effect of the changes. Breadth is provided by an examination of the impact of the laws on the processing and disposition of rape cases in six major jurisdictions from 1970 to 1985. Depth is provided by supplementing this longitudinal data with information from interviews with criminal justice personnel. Our intent is to provide data which will inform feminists, legal scholars, social scientists and others embroiled in the "continuing national debate about the effectiveness of rape reform legislation" (Feild and Bienen, 1980: 181).

RAPE LAW REFORM IN SIX JURISDICTIONS

The six cities selected for this project represent jurisdictions which enacted different types of rape law reforms. Some jurisdictions embraced all of the reforms described earlier, while others enacted very limited changes. Some states enacted "strong" versions of a particular reform, others "weak" versions. A few legislatures passed comprehensive reform bills, while others adopted individual changes over a number of years. Our purpose in selecting these six jurisdictions, then, was to determine if particular changes, or particular combinations of changes, affected the processing and disposition of sexual assault cases. We also wanted to see if the effect of the reforms varied with the comprehensiveness of the changes.

We categorized the reforms in our six sites as strong, moderate or weak, depending on the types and strength of the changes adopted. Detroit and Chicago represent jurisdictions with strong reforms, Philadelphia and Houston represent states with moderate reforms, and Atlanta and Washington, D.C. represent jurisdictions with weak reforms. The types of reforms enacted in each state are summarized in Table 1. Differences in the reforms are summarized below.

Strong Reforms -- Michigan and Illinois

Although the sexual assault laws in Michigan and Illinois are very similar, Illinois enacted changes in a more piecemeal fashion than did Michigan. The comprehensive Michigan statute enacted in 1975 is regarded by many as a model rape reform law. The 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 statute extends the reach of the sexual assault laws to acts (sexual penetration with an object) and persons (men and married persons who are legally separated) not covered by the old laws.

The Michigan law also delineates the circumstances which constitute coercion, lists the situations in which no showing of force is required (for example, when the victim is physically helpless or mentally defective), and states that the victim need not resist the accused. Since evidence of coercion is seen as tantamount to nonconsent, the law effectively eliminates the requirement that the prosecutor prove the victim resisted and therefore did not consent; the burden of proving the victim acquiesced to the act falls to the defendant. It should be noted, however, that although the law does not state that consent is an affirmative defense to sexual assault, the Michigan Court of Appeals ruled

in 1982 that it was reversible error for a trial judge to fail to instruct the jury on the defense of consent when the defendant had alleged that the complainant consented [People v. Thompson 324 N.W.2nd 22, (Mich. App. 1982)]. The Michigan law further modifies the rules of evidence by eliminating the corroboration requirement and by prohibiting the introduction of most types of evidence of the victim's past sexual conduct. (Variations in the rape shield laws will be discussed in detail below.)

Although Illinois in 1978 implemented a strong rape shield law very similar to the law enacted in Michigan, it did not adopt definitional changes or repeal the resistance requirement until 1984. The Illinois Criminal Sexual Assault Act eliminates seven crimes (rape, deviate sexual assault, indecent liberties with a child, aggravated indecent liberties with a child, contributing to the sexual delinquency of a child, aggravated incest, and sexual abuse by a family member) from the "Sex Offenses" section of the criminal code and adds four (aggravated criminal sexual assault, criminal sexual assault, aggravated criminal sexual abuse, criminal sexual abuse) to the "Bodily Harm" section. The new law defines sexual assault as forcible sexual penetration and sexual abuse as forcible sexual contact; if specified aggravating factors are present (for example, the defendant used a dangerous weapon or seriously injured the victim), the assault or abuse becomes the more serious (i.e., aggravated) offense. The law also allows prosecution for aggravated criminal sexual assault by a spouse, provided that the incident is reported within 30 days.

In contrast to the Michigan law, the Illinois statute specifically provides for a consent defense. However, the law also eliminates the resistance requirement by deleting the phrase "against her will" from the definition of sexual assault and by stating that lack of resistance resulting from the use of force or threat of force does not constitute consent. Presumably, the impact of these provisions will be to shift the burden of proving consent to the defendant.

The strong reforms enacted in Michigan and Illinois, then, redefine rape by providing for staircased sexual assault offenses with graduated penalties and by extending the reach of the laws to acts and persons previously not included. More generally, the reforms shift the focus of inquiry from the behavior of the victim to the behavior of the accused. Under the new laws, police, prosecutors, judges and juries should be more concerned with determining whether the accused had a weapon, had an accomplice, or threatened to injure the victim than with discovering whether the victim resisted, whether her story can be corroborated, or whether she is chaste or promiscuous.

Moderate Reforms -- Pennsylvania and Texas

The reforms enacted in Pennsylvania and Texas are not as comprehensive as those adopted in Michigan and Illinois. Of the two states, Pennsylvania enacted the stronger reforms. In 1976 Pennsylvania passed a strong rape shield law and repealed the corroboration, prompt complaint and resistance requirements. Three years earlier the Lord Hale cautionary instruction had been prohibited. In 1985 a provision concerning spousal sexual assault was added; spousal sexual assault is a felony of the second degree (rape and involuntary deviate sexual intercourse are felonies of the first degree) with a 90-day reporting requirement.

Although these evidentiary changes match or go beyond those adopted in Michigan and Illinois, we categorized the Pennsylvania reforms as "moderate" because Pennsylvania retains traditional Model Penal Code-type definitions of rape and involuntary deviate sexual intercourse. Both definitions focus on the circumstances which define nonconsent: forcible compulsion; threat of forcible compulsion that would prevent resistance; unconsciousness; and mental deficiency. Neither includes penetration with an object. And both crimes are first degree felonies with identical penalties.

We also classified the reforms enacted in Texas as moderate, even though the changes implemented there are closer to the weak reforms adopted in Georgia. In 1975 Texas passed a weak

rape shield law. No further reforms were enacted until 1983, when definitional changes were adopted. The four crimes of rape, aggravated rape, sexual abuse, and aggravated sexual abuse were removed from the "Sexual Offenses" section of the penal code and sexual assault and aggravated sexual assault were added to the "Assaultive Offenses" section.

The old laws defined rape (penile-vaginal intercourse) and sexual abuse (oral or anal sex) in terms of the female's absence of consent and lack of resistance; nonconsent was inferred if the accused compelled the victim to submit "by force that overcomes such earnest resistance as might reasonably be expected under the circumstances" or by any threat "that would prevent resistance by a woman of ordinary resolution . . ." [Texas Penal Code 21.02 (1974)(Supp. 1980)].

The new gender-neutral laws retain the emphasis on consent, but the definition of consent focuses more on the accused's assaultive behavior than on the victim's lack of resistance. It appears that some resistance still is required, however, since the law includes within the definition of nonconsent situations where the victim is mentally or physically unable to resist. The law also specifies that the affirmative defenses to assault include situations where the victim consented or the accused reasonably believed that the victim consented. On a more positive note, the 1983 statute eliminates the marital exemption for spouses who are living apart or legally separated. It also states that corroboration is not required if the victim informed anyone of the assault within six months; under the old law a defendant could be convicted on the uncorroborated testimony of the victim only if the victim made an immediate or prompt outcry.

Although we categorized the reforms adopted in Pennsylvania and Texas as moderate reforms, the statutory changes implemented in the two states are actually very different. The comprehensive evidentiary changes in Pennsylvania clearly might produce different effects than the weak evidentiary and definitional changes in Texas.

Weak Reforms -- Georgia and Washington, D.C.

The reforms enacted in Georgia and Washington, D.C. are much weaker than those adopted in the other jurisdictions, particularly Michigan and Illinois. Both jurisdictions have traditional carnal knowledge statutes which define rape as carnal knowledge of a woman by force and against her will; the definition of rape has been unchanged since 1861 in Georgia and since 1901 in Washington, D.C. Both jurisdictions require penetration of the female sex organ by the male sex organ. Both require resistance by the victim. Georgia has a common-law consent defense, Washington, D.C. a statutory consent defense.

Both jurisdictions have adopted some reforms. Georgia enacted a very weak rape shield law in 1976 and eliminated the corroboration requirement in 1978. Washington, D.C. has not amended its rape laws since 1901, but case law restricts the introduction of evidence of the victim's prior sexual history [S.R. McLean v. United States, 377 A.2d 74 (D.C. App. 1977)] and eliminates the corroboration requirement [J.E. Arnold v. United States, 358 A.2d 335 (D.C. App. 1976)]. These are, however, very modest reforms.

Rape Shield Laws

Reformers predicted that the rape shield laws would have a greater impact on the processing and disposition of sexual assault cases than would the other reforms. As Feild and Bienen (1980:103) noted, no other factor "is thought to produce more discriminatory effects in rape trials as the introduction of the victim's sexual reputation or moral character as evidence." In fact, Marsh and her colleagues (1982) found that criminal justice officials cited restrictions on the introduction of sexual history evidence as the most significant aspect of the reforms adopted in Michigan. Because shield

laws clearly have the potential to affect the outcome of sexual assault cases, the differences among the laws are depicted in Table 2 and summarized below.

Strong Shield Laws. Michigan, Illinois, and Pennsylvania all have strong rape shield laws which 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. Evidence of prior sexual activity with persons other than the defendant is inadmissible in Illinois and Pennsylvania and is admissible in Michigan only to show the source of semen, pregnancy or disease. Recent court decisions in Pennsylvania, however, have carved out additional exceptions to the shield; evidence of the victim's prior sexual conduct with third persons can be admitted to show that the victim was biased against the defendant and therefore had a motive to lie [Commonwealth v. Black, 487 A.2d 396 (Pa. Super. 1985)] or to show the source of semen, pregnancy or disease [Commonwealth v. Majorana, 470 A.2d 80 (Pa. Super. 1983)].

All three jurisdictions permit introduction of the victim's past sexual conduct with the defendant, but the standards for determining whether the evidence should be admitted or not vary. The Michigan statute states that the evidence can be admitted if it is material to a fact at issue (generally consent) and if its inflammatory or prejudicial nature does not outweigh its probative value. Similarly, the Pennsylvania law declares that the evidence is admissible if consent is at issue and if the evidence would otherwise be admissible under the rules of evidence. The Illinois law, on the other hand, specifies that the evidence can be admitted only if the court finds that the defense has evidence to impeach the victim in the event that the victim denies having had prior sexual conduct with the defendant.

Weak Shield Laws. If the shield laws enacted in Michigan, Illinois and Pennsylvania lie at the restrictive end of the continuum, the laws adopted in Texas, Georgia and the District of Columbia lie at the permissive end. Texas does not categorically exclude any sexual conduct evidence; rather, such evidence can be admitted only if the judge finds that the evidence is material and that its inflammatory or prejudicial nature does not outweigh its probative value. As a shield, then, the Texas law is fairly permeable. Presumably, any type of evidence -- specific instances of sexual activity with the defendant or with third persons, opinion evidence, or reputation evidence -- could be admitted under the Texas law, as long as the judge rules that it is relevant. In fact, Weddington (1975-76) has pointed out that the Texas law actually changes very little, since prior to the reform prosecutors could use a motion in limine to suppress irrelevant or prejudicial evidence.

The statute enacted in Georgia is more restrictive than the one adopted in Texas, but still gives judges considerable discretion to admit sexual conduct evidence. The Georgia law states that evidence of the victim's past sexual behavior is inadmissible unless the court finds that the evidence concerns behavior with the accused or supports an inference that the accused reasonably could have believed that the victim consented. It is interesting that when the shield law was introduced in the Georgia legislature, it said that sexual conduct evidence was admissible only if it concerned the accused and supported an inference that the accused reasonably could have believed the victim consented. Defense attorneys in the legislature successfully lobbied to change the "and" to "or"; in doing so, they weakened the law.

Comparison of case law before and after the Georgia shield law was enacted substantiates this. The ruling case law prior to the adoption of the shield law was Lynn v. State [(231 Ga. 559, 203 SE2d 221 (1974)]. In this case the Georgia Supreme Court ruled that a rape victim may not be questioned about her prior sexual relations with men other than the defendant. In contrast, in 1981 the Georgia Court of Appeals ruled that the defendant is permitted to offer evidence of the victim's unchaste character if the evidence tends to show that the victim consented to the act [Hardy v. State, 159 Ga. App. 854, 285 SE2d 547 (1981)]. In Georgia, then, case law may have provided a greater degree of

protection to rape victims than the shield law.

We noted earlier that the District of Columbia has not enacted a rape shield law. Case law, however, does limit the admission of sexual conduct evidence. In S.R. McClean v. U.S. [U.S. App. D.C. 377 A.2d 74 (1977)] it was ruled that the victim's prior sexual conduct with third persons is inadmissible; that the victim's reputation for chastity should not be admitted except in unusual situations where its probative value outweighs its prejudicial effect; and that the victim's prior sexual conduct with the defendant is admissible to rebut the government's evidence that the victim did not consent. The protections accorded the rape victim in Washington, D.C., then, are closer to those found in Texas and Georgia than to those found in Michigan, Illinois or Pennsylvania.

Procedural Requirements. The procedures for determining the relevance of past sexual conduct evidence also vary somewhat among the states. The Michigan statute requires the defendant to file a written motion and offer of proof within 10 days of arraignment; the judge then may order an in camera hearing to determine whether the proferred evidence is admissible. The procedures required in Pennsylvania and Illinois are very similar, except that the Pennsylvania law requires a written motion filed at the time of trial, while the Illinois law does not require a written motion and does not specify when the hearing must be held. Both the Texas and Georgia statutes require the defendant to notify the court prior to asking any questions about the prior sexual conduct of the victim; the court is then to conduct an in camera hearing to determine if the evidence is admissible and to limit the questions that can be asked (Texas). No procedures are specified by the case law in the District of Columbia.

The rape reform laws enacted in the six jurisdictions included in our study clearly have the potential to produce different effects on the processing and disposition of sexual assault cases. By examining the impact of these laws over time, we hope to be able to untangle the effects of the various reforms and to begin to delineate the mechanisms by which legal reforms produce changes in attitudes and behavior.

RESEARCH DESIGN AND METHODOLOGY

Jurisdictions

Because the data on prosecution of cases are not generally available on a statewide basis, six jurisdictions were chosen to represent states with various types of law reforms. As described above, jurisdictions were chosen to represent the range of reforms enacted in states across the country. In addition, jurisdictions were selected from states that enacted reforms at one or two specific times (rather than piecemeal across several years), and at several times different from each other. The reform at limited times within a jurisdiction criterion is important to maximize chances of detecting effects in the time-series analysis, while the reforms at different times across jurisdictions criterion is important to control for the threat of history to the design.

Another important consideration in choosing the sample jurisdictions was the number of rape cases handled. Major urban jurisdictions were necessary in order to subject the data to the appropriate statistical analysis. The number of rapes reported to the police in 1982 (according to the Uniform Crime Reports) ranged from 421 in Washington, D.C. to 1,270 in Houston.

The sample jurisdictions vary in some known ways in terms of case processing practices. They include jurisdictions that use grand jury indictments as well as those that use prosecutor's informations for charging. They include jurisdictions that depend heavily on plea bargaining and jurisdictions that dispose of a considerable number of cases at trial. In one jurisdiction (Philadelphia) the prosecutor's office has a special division for prosecuting sexual assault cases.

Data on Rape Cases

Case Selection. All forcible rape cases for which indictments or informations were filed from the years 1970 through 1984 were selected, except for Washington, D.C. and Houston, Texas. In D.C. we had to begin 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 of problems with data collection. In Chicago we also collected data on cases through 1985, because the Illinois law underwent another major change in late 1983.

In addition to forcible rape, other sexual assaults that were not specifically assaults on children were included. The names and definitions of these crimes varied from jurisdiction to jurisdiction. When the rape law reforms included definitional changes the closest equivalent crimes were selected. Table 3 lists the particular offenses included in the analysis for each jurisdiction. Where two offenses are listed, separated by a hyphen, these indicate the closest equivalents after changes in the laws. It should be noted that in some jurisdictions it was not possible to select attempted rapes because the crime was noted in the books only as "attempted felony."

Data Sources. Official court records were the source of data for the time-series analysis, but the procedures used to obtain the data varied from jurisdiction to jurisdiction. In one jurisdiction (Atlanta) all data were obtained from court docket books in which all felony cases were listed. Data collectors looked at all felony cases from 1970 through 1984 and selected the target cases. In two jurisdictions (Chicago and Houston) lists of all target cases had to be made from docket listings of all felony cases and then the case files were pulled to obtain the necessary information. In one jurisdiction (Washington, D.C.) a listing of the target cases was obtained through the prosecutor's computerized system, and then court files were pulled to code the information. In another (Detroit), docket listings of all felony cases were used for the first six years, and after that the target cases were pulled through the court's computerized system (docket listings were not available for the remaining years). Finally, for one jurisdiction (Philadelphia) we were able to obtain all the necessary information from the court's computerized system (no docket books were available). The data on rapes reported to the police in each jurisdiction were obtained from the FBI's Uniform Crime Reports. For Chicago, those data are not available for 1984 and 1985.

Variables Coded. For each target case, the following information was coded:

1. Date of offense
2. Date of indictment/information
3. Date of disposition
4. Most serious charge on indictment/information
5. Second most serious charge
6. Third most serious charge
7. Fourth most serious charge
8. Total number of charges filed
9. Most serious conviction charge
10. Second most serious conviction charge
11. Third most serious conviction charge
12. Fourth most serious conviction charge
13. Type of disposition
    a. guilty plea
    b. guilty by jury
    c. guilty by judge
    d. not guilty by jury
    e. not guilty by judge

f. dismissed
g. no bindover
h. no charge

14. Type of sentence
    a. probation
    b. jail
    c. prison
    d. other
15. Minimum sentence
16. Maximum sentence
17. Sex of victim

Time Series Analysis

The general framework for the statistical analysis was the interrupted time-series design (Campbell and Stanley, 1966). This design has a number of advantages over a simple before-and-after design. By looking at multiple observations of a dependent variable over an extended period of time both before and after some intervention, one can determine whether a change coincident with the interruption represents anything more than a long term trend, statistical regression, or normal variability. One also can determine the duration of any effects, and both level and slope changes in a time series can be detected.

We used interrupted time-series analysis to test for the effect of rape law reforms on a number of dependent variables. In each jurisdiction we examined the impact of the reforms on rape (or the equivalent crime) and on all sex offenses. The dependent variables include: the number of reports of forcible rape; the number of indictments for rape or for sex offenses; the number of convictions for rape or for sex offenses; the number of convictions for the original charge (rape); the number of incarcerations for rape or for sex offenses; the indictment rate (indictments divided by reports); the conviction rate (convictions divided by indictments); the rate of convictions on the original charge (convictions for rape divided by indictments for rape); the incarceration rate (incarcerations divided by convictions); and the average sentence.

In each time-series analysis the interruption was the change in the rape laws of the particular jurisdiction. The number of years before and after the reforms varied somewhat, depending on when the law was reformed in each state. In Washington, D.C. and Atlanta, where rape shield laws were enacted at times different from repeal of corroboration requirements, the impact of both changes was assessed. Two different changes also were examined in Chicago, where definitional changes were implemented several years after the rape shield law.

Each series was analyzed according to procedures specified by McCleary and Hay (1980). The initial step in the analysis is to determine the appropriate statistical model for each of the 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 auto regressive moving average (ARIMA) model is appropriate for testing statistical significance of any effects of the interventions.

Controls. The major weakness of the quasi-experimental time-series design is that it does not control for the "history" threat to internal validity (Campbell and Stanley, 1966). Even though a discontinuity in the series occurs at the time of the intervention, it is quite possible that some other events occurring at about the same time actually led to the effects noted. The major concern with regard to rape law reforms is that 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. The analysis is substantially improved by using a multiple time-series design which includes sets of time-series data with different intervention points for which different patterns of effects can be predicted.

The reformed jurisdictions made their legal changes at different times. If national attention to rape issues causes changes, these should appear at approximately the same time for all the jurisdictions. If each reform jurisdiction shows changes occurring at the time of intervention, however, the evidence for the legal reform being the cause of the changes is greatly strengthened.

Interview Data

To more fully evaluate the rape law reforms we conducted interviews with criminal justice officials in the six jurisdictions included in the study. We conducted lengthy interviews with a purposive sample of 162 judges, prosecutors and public defenders. The interviews covered a wide range of topics. We asked participants if they saw any advantages or disadvantages to the various statutory changes, if they had changed their courtroom tactics or strategies for handling sexual assault cases as a result of the changes, and if they felt there was a need for additional changes. We focused particularly on respondents' attitudes toward the effectiveness of the rape shield laws. We also asked respondents to rate the importance of various types of evidence in sexual assault cases and to respond to a series of hypothetical cases in which evidence of the victim's past sexual behavior was at issue.

RESULTS

The overall goal of the rape reform movement was to treat rape like other crimes by shifting the legal inquiry from the behavior or reputation of the victim to the unlawful acts of the offender. Reformers hoped that the new laws would reduce both the skepticism of criminal justice officials toward the claims of rape victims and their reliance on extralegal considerations in decision making. They anticipated that the reforms ultimately would lead to an increase in the number of reports of rape, and would make arrest, prosecution, and conviction for rape more likely. Both of these expectations are examined below.

Statistical Analysis of the Law Reforms

Table 4 presents a summary of the results of time-series analyses on 16 variables in the six jurisdictions. Consistently significant effects were found only in the jurisdiction which enacted the most comprehensive reforms. Although some effects were found in other jurisdictions, none showed the consistent patterns of change evinced in Detroit.

Detroit. Of the jurisdictions we studied, Michigan enacted by far the most comprehensive reforms in the rape laws. In 1975 the crime of rape was redefined with four degrees of criminal sexual conduct, strong rape shield laws were implemented, and both corroboration and resistance requirements were eliminated. As indicated in Table 4 these changes apparently resulted in considerable impact on the processing of rape cases in Detroit. The details of these results are presented in Table 5, which gives the parameter values for the impact models constructed. Figures 1a, b and c are the time series plots for reported rapes, indictment rate and conviction rate. The months are the 180 months from January, 1970 through December 1984. The vertical dotted line represents the implementation of the comprehensive changes in Michigan rape laws on April 1, 1975.

Figure 1a shows the monthly totals of reported forcible rapes, according to the Uniform Crime Reports. There was a fairly steady increasing trend in reports before the 1975 law. Reports continued

upward briefly after the change and then seemed to stabilize at a rather steady rate. The statistical analysis indicated a significant increase in reports of about 26 reports per month as a result of the new law. In order to compare the pattern in rape reports with general crime trends, we looked at reports of robbery and felony assault for the period 1970 through 1980. The pattern for robbery is quite different from that of reported rapes, and a time-series analysis of reported robberies indicates no change at the time of the 1975 rape reform laws. Reported assaults follow a trend very similar to that of reported rapes, with the increasing trend before 1975 and the stabilization afterwards, but the time-series analysis showed no significant change in level at the time of the intervention. These data provide supporting evidence that the increase in reported rapes may have indeed been due to the change in rape laws and the surrounding publicity.

In Detroit, the variables listed under "rape" in Table 5 are defined 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. Second degree criminal sexual conduct (aggravated sexual contact without penetration) and fourth degree criminal sexual conduct (sexual contact without penetration) were excluded from these variables because they are crimes that would not have been defined as rape before the laws changed. The definitional changes made it impossible to achieve a perfect correspondence between the offenses before and after the reforms. Penetration with an object, for example, is criminal sexual conduct after 1975; before 1975 it could have been charged a number of different ways, and may have been charged as an offense such as assault that included a great many non-sexual crimes. Criminal sexual conduct as defined in the new laws is gender neutral whereas rape under the old common law meant there could be no male victims. Thus, the new offenses are more inclusive than the old crime of rape, even when restricting the measure to first and third degree criminal sexual conduct. Comparing rape, sodomy and gross indecency charges with first and third degree criminal sexual conduct, although imperfect, seemed to be the best option available.

The pattern for total rape indictments in Detroit follows a pattern similar to that of reported rapes, although the increase after the reform of the laws is even more obvious. This greater increase is reflected in the analysis for indictments as a percentage of reported rapes; a significant increase of 20% was found to follow the legal changes (see Figure 1b).

The increase in indictments for rape was also followed by an increase in total convictions, but the prediction that the likelihood of conviction would increase was not borne out (see Figure 1c). Table 5 indicates that the percentage of rape indictments that resulted in convictions did not increase.

As the number of convictions increased after the passage of the new laws, so did the number of convictions on the original charge. There was no change, however, in the percentage of cases that resulted in a conviction on the original charge, and thus, no indication of an increase in plea bargaining. In fact the percentage of cases convicted on the original charge increased after the legislative reforms, although our statistical tests did not show a significant impact.

The legal changes resulted in more of the offenders who were convicted of rape being sentenced to prison, but this impact can be attributed to the increase in convictions rather than a greater likelihood of sentencing to incarceration, because when number of incarcerations is analyzed as a percentage of convictions, there is no significant impact. For those who were sentenced to prison for rape, however, the average sentence was significantly longer after the rape law reforms, with an average increase of 54 months.

When time-series analyses were performed for variables that included all the major sexual assaults the results paralleled those found for rape and first and third degree CSC (Table 5). There were significant increases in total indictments, total convictions, total incarcerations and average sentence, but no significant changes for convictions as a percentage of indictments or for incarcerations

as a percentage of convictions.

In summary, in Detroit, our analysis showed a greater number of reported rapes, a greater likelihood of indictment for the cases reported, more total indictments, convictions, and incarcerations for rape and longer sentences for those sentenced to prison for rape. There was no increase in the likelihood of conviction once indicted, and there was no significant change in whether offenders were convicted on the original charge or on lesser charges. The interpretation of the Detroit results is greatly complicated by the definitional changes that were part of the legislative reforms. As explained above, it was not possible to have perfectly comparable measures of rape before and after the laws changed. Thus it is possible that the observed results (except for the increase in reported rapes) may reflect in part the greater inclusiveness of the post-reform definitions. The problem is exacerbated in our measure of percent indicted, since for that measure we were not tracking cases, but were dividing the number of indictments, as obtained from the court records, by the number of reports, as measured by the Uniform Crime Reports. The UCR definition of forcible rape did not change over time; Detroit police recode the post-reform charges to meet the UCR criteria. Thus in our measure of percent indicted, the denominator is based on the same kinds of cases before and after the intervention, but the numerator is based on a different mix of cases. Without more information on the cases, of the kind that would be found only in police reports, it is impossible to create perfectly equivalent numerators.

Chicago. The state of Illinois also enacted comprehensive rape reform legislation, but the major changes occurred several years apart. A strong rape shield law was implemented in 1978, and then in 1984 the crime of rape was redefined as criminal sexual assault and aggravated criminal sexual assault, and the resistance requirement was eliminated. The results we found in Chicago, however, were very different from those found in Detroit. The results of our time-series analyses are presented in Table 6. The analyses of the impact of the rape shield law were based on monthly data from January 1970 through June 1984. We excluded data from after the second legal changes (July 1984) because effects of the later reform complicated the modeling of impact of the earlier reform. The vertical line on the plots in Figures 2a, b and c represents the rape shield law that went into effect in January 1978. Our analyses indicate no impact of the rape shield law on total reports, indictments, convictions, or incarcerations. Similarly, there was no impact on indictment rate or on likelihood of conviction once indicted. There were similar patterns for indictments, convictions, convictions on original charge, and incarcerations, with increases in all these measures from 1970 up until 1975, followed by a decrease until after the rape shield legislation in 1978. Then there was another long-term increase up until about 1981 and then another decline. The level reached after the legislative changes was little if any higher than the level reached around 1975. These data tend to follow the pattern in reported rapes (Figure 2a), although the data for reports are quite variable. We are reluctant to attribute the increases after the rape law reforms to the law itself since the change is so gradual (over a three-year period) and since a similar pattern also occurred before the reforms.

The only significant effect in Chicago was an increase of almost 48 months in average maximum sentence for those incarcerated for rape after the rape shield law was enacted. On the other hand, there was no increase in the incarceration rate. In fact, there was almost no variability in incarceration rate since the law in Illinois makes prison a mandatory sentence for those offenders.

The analyses for all sex offenses together, which we extended for one year after the definitional changes of 1984, do not really have comparable offenses before and after the legal reforms. The definition of criminal sexual assault under the new law includes what was previously indecent liberties with a child and incest. We did not collect data on these offenses involving children before the 1984 change, and we were not able to separate those cases out after the reforms. The analyses indicate that after the definitional changes in the Illinois law, there was a higher conviction rate, but the percentage of those convicted who were sentenced to prison decreased. These last results must be evaluated cautiously because they are based on data for only twelve months after the legal changes

took place, and they may merely reflect the different composition of offenses represented.

In summary, the data for Chicago indicate that the passage of the rape shield law in 1978 led only to longer maximum sentences for those convicted of rape. The results of the 1984 definitional changes are inconclusive.

Philadelphia. Pennsylvania enacted what we considered to be moderate rape law reforms. The rape shield law implemented in 1976 is a strong one that prohibits evidence of prior sexual activity with persons other than the defendant; recent court decisions, however, have allowed some exceptions to the shield. Pennsylvania also repealed corroboration, prompt complaint, and resistance requirements, but traditional Model Penal Code-type definitions of rape have been retained. The plots of Philadelphia time-series data appear in Figures 3a, b and c, and Tables 7 and 8 summarize the statistical analyses. The Philadelphia time-series data were somewhat difficult to model; thus the need for two tables to present the results. The series for reports (plotted in Figure 3a), indictments, convictions on original charge, and incarcerations appeared to need differencing before identifying the model parameters. When differenced, the data indicated significant impacts of the rape law reforms, but the model parameters were very close to 1.00, indicating a possible problem with the differencing. We therefore tried to model the series without differencing; we were able to do so for all the series except total incarcerations for rape, but the series were not easily modeled. Table 8 shows in the section "Correct Intervention" that most of the series required two or three autoregressive parameters. With differencing, however, the analyses indicated significant impacts of the legislation. Because the plots of the time-series seemed to show increases occurring before the legal changes took place, we modeled the series again with the dummy variable for the intervention coded as if the intervention occurred in 1974 instead of 1976. With the intervention thus moved back two years, the models for the series were much more parsimonious, requiring either one or no autoregressive parameter in all except one case (which required two parameters), and the intervention parameters became even larger and had much higher values. It seems, therefore, that in Philadelphia some factor other than the legal changes affected the number of cases in the system at least two years before the Pennsylvania statutes were changed.

The models for indictments as a percentage of cases reported (plotted in Figure 3b), convictions as a percent of cases indicted (Figure 3c), and percent convicted on original charge were much more straightforward, and the analyses showed no impact of the legal changes on any of these variables (Table 8). The only evidence of change in Philadelphia associated with the rape law reforms is in the area of sentencing. There was an increase in the percentage of defendants convicted of rape who were sentenced to prison, and their average maximum sentences also increased by just over 10 months. The analyses for all sex offenses together showed no significant impact of the legislative reforms on any of the variables.

Washington, D.C. Figures 4a, b and c present the time-series plots for Washington, D.C. The vertical lines indicate the two interventions in D.C., which were changes in case law rather than statutory changes; a 1976 case abrogated the corroboration requirement for rape cases, and a 1978 case established a rape shield provision. Results of the statistical analyses for D.C. data are summarized in Table 9. The only evidence we found of change in the predicted direction are the significant increases in average sentence for those incarcerated for sex offenses that followed both judicial decisions. The average sentence for all sex offenses increased 69.6 months after the decision eliminating the corroboration requirement and a total of 116.4 months after the decision establishing rape shield provisions.

Two results were in a direction contrary to our predictions. A decrease in the number of reported rapes appeared after the elimination of the corroboration requirement (Figure 4a), and a decrease in total convictions was found in the analyses when either corroboration elimination or the rape shield ruling was modeled as the intervention. We have no theoretical rationale to explain such

decreases; we suspect that they are merely coincidental with the new laws.

Atlanta. The reforms enacted in Georgia are fairly weak. The crime of rape is traditionally defined as carnal knowledge of a woman by force and against her will and resistance by the victim is required. In 1978 the legislature did eliminate the corroboration requirement, and in 1976 a rape shield law was enacted, but that statute still gives judges considerable discretion to admit sexual conduct evidence. The time-series for Atlanta variables are plotted in Figures 5a, b and c and Table 10 summarizes the statistical analyses.

The initial statistical analyses indicated significant increases in total indictments for rape as well as for all sexual assaults, in the percentage of rape indictments resulting in conviction (Figure 5b) (and the same for all sexual assaults), and in the total convictions on the original charge. The plots for these series, however, seemed to show gradual trends that actually started before implementation of the reformed rape laws. We therefore explored these effects further by coding the dummy variable for the legal intervention as "1" two years before the 1976 change in the law. Table 10 shows in the column labeled "Prepost-Moved Back Two Years" that the results remained significant with modeling the intervention two years earlier than it actually occurred. Thus, the original analyses for those variables seemed to be picking up trends that started well before the rape law reforms. All other variables showed no significant changes.

The plot of reported rapes (Figure 5a) shows an increase in reporting occurring around 1973 (month 36) that may be the reason for the earlier increase in total indictments and total convictions. The statistical analysis, however, did not indicate a significant increase in reports. One problem in modeling many of the time-series occurs when a trend starts coincident with the change in laws. If there is an overall trend to a series it must be removed through differencing before the ARIMA models can be identified. When trends shift in the middle of a series, however, the data are often ambiguous in indicating a need for differencing. In general, when differencing produced very large parameter values (over .90 for p or q) we tried to model the series without differencing (McCleary and Hay, 1980), but sometimes that was impossible to accomplish while still meeting other criteria for an acceptable model. In the case of Atlanta, the model for reported rapes required differencing while the other variables could be modeled without differencing (see Table 10). Differencing makes it less likely that a significant impact of the intervention will be detected; thus, this may explain the lack of impact with reported rapes in spite of the appearance of the graph, and the significant impacts determined for the indictment and conviction variables. So it is possible that the increases in indictments and convictions measured merely followed an increase in reported rapes around 1973.

Houston. We also characterized the rape reform laws in Texas as weak, since we were not evaluating the definitional changes that were enacted in 1983. The rape shield law implemented in 1975 does not categorically exclude any sexual conduct evidence; rather, such evidence can be admitted if the judge finds that the evidence is material and that its inflammatory nature does not outweigh its prejudicial effect. The time-series plots for Houston data are presented in Figures 6a, b and c, and the statistical analyses are summarized in Table 11. The initial statistical analyses indicated significant increases for all variables except indictments as a percentage of reports and total incarcerations for all sex offenses. When the graphs for the variables are examined, however, it is apparent that the level for most variables remained fairly constant for several years after the Texas rape law reforms were implemented, and that increases started to show up three to four years later.

In order to determine whether the statistical analysis was reflecting those later increases, the dummy variable for the legal intervention, which is normally coded "0" before the change in law and "1" after the change, was coded "0" before the change, "1", for two years after the change, and "0" for the rest of the series. With that change the significant effects disappeared for most variables, indicating that the effects in the earlier analyses were the later occurring increases (see Table 11 under the

column labelled "Prepost-Two Years". 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 any sign of impact in Houston seems too great to justify concluding that the changes were due to reforms in the laws. One guess is that increases in our case variables may be related to general increases in crime in Houston at that time (approximately 1978 to 1980 or months 96 to 120 on the graphs). Plots of monthly reports of robberies and assaults show similar increases at that time.

Two variables still showed a significant impact even after the recoding of the dummy variable. One was average sentence for rape, with an increase of approximately 53 months after the rape shield law was enacted. The other was reported rapes. The impact for both was still significant after the recoding, but in each case the impact was modeled as a very slow gradual change. It is interesting to note that among the variables for which the later changes were indicated, indictments as a proportion of reports decreased rather than increased as might be predicted (see Figure 6b). Thus, as reports of rapes in Houston increased, the number of indictments did not keep pace.

Summary and Discussion. Interrupted time-series analyses were used to test predictions about how changes in the rape laws of six jurisdictions would affect the reporting of rape and the processing of rape cases. The only impact found across several jurisdictions was an increase in the average sentences imposed for those convicted of sex offenses. Otherwise, consistently significantly effects were found only in Detroit, the jurisdiction that enacted the most comprehensive reforms.

The increase in average sentences was found in all jurisdictions (although not attributed specifically to the law reforms in Atlanta). We suspect that this widespread finding indicates that the sentencing decision is more susceptible to influence by legal reforms and public concern than other decisions made by criminal justice officials. The sentencing decision involves great discretion, little review, and is less dependent on factual input than other decisions. Once an offender has been convicted the law allows judges considerable latitude in determining length of sentence. The enactment of rape law reforms in each jurisdiction represented, at the very least, a public concern that rapes must be treated as very serious offenses. Even though most of the reforms were not directly aimed at increasing sentence length, the resulting increases may reflect the judges' more serious treatment of these cases.

In Detroit the analyses showed increases in reported rapes, in indictments, convictions, convictions on original charge, incarcerations, and average sentences. There was also a significant impact on the indictment rate, as measured by the number of indictments in a month divided by the number of rapes reported to the police. There were no changes in the rates of conviction, conviction on original charge or incarceration.

The Detroit results could be interpreted in different ways. First, we have to assume that the increases in absolute numbers of convictions, convictions on original charge and incarcerations were due primarily to the increase in indictments, since we did not find significant increases in the rates of these variables (i.e. convictions divided by indictments, convictions on original charge divided by convictions, or incarcerations divided by convictions). These aspects of case processing are indeed probably the least likely to be affected by legal reforms.

Convictions, especially, would seem to be resistant to impact. Determinations of guilt or innocence are based on the mass of evidence in a case, of which the evidence affected by the rape shield laws is a tiny portion. Additionally, the rape shield laws are directed at the trial process. The fact that in many jurisdictions few defendants go to trial further reduces the chance of impact on conviction rate. Elimination of corroboration and resistance requirements, which were predicted to increase conviction rates, may not, in practice, have removed major hurdles to conviction in rape cases. In most jurisdictions the case law had reached such broad interpretations of corroboration (such as the

victims telling a third party about the incident), and had been so loose in the requirements for reasonable resistance, that these legal requirements probably were not significant factors leading to acquittals. On the other hand, when juries or judges expect corroboration or resistance, the elimination of the legal requirements does not prevent them from considering these issues in reaching their verdicts.

Some reform advocates predicted that the definitional changes enacted in Michigan would affect conviction rates by facilitating plea bargaining. Under the old laws prosecutors may have considered the difference between rape and any other charge too great to offer pleas to lesser charges. With the gradations of sexual assault and commensurate penalties under the new laws, there might be a greater willingness to offer charge reductions, since the crimes would still be sexual assaults and carry serious penalties. These predictions were not borne out, as we found no significant impact on the rate of convictions on original charges. In fact, although the change was not statistically significant, that rate increased rather than decreased. It is possible that the stress on the seriousness of the crime of rape that permeated the reform legislation created an unwillingness to plea bargain that counteracted the facilitative effects of the definitional changes. In fact, from our Detroit interviews (see Appendix C) we learned that the Wayne County Prosecutor's Office has a plea bargaining policy for rape cases under which there are no plea bargains unless the complainant's approval is obtained. Additionally CSC 1 cases can only be negotiated down to CSC 3 charges except in unusual circumstances, and CSC 3 charges cannot be reduced.

The most important effects of the Michigan laws were the increases in reporting of rapes and in the rate of indictments. Because we had to rely on Uniform Crime Reports we know only that the number of reports increased. It is not possible to know conclusively whether this increase represented an increase in likelihood of reporting, an increase in the actual crime rate, or both. There were no victimization data available to provide other estimates of actual offense rates. Because it is possible that the law reforms coincided with an increase in offense rate, we made the comparisons with reported robberies and reported assaults as described above. The different results for reported rapes encourage us to believe that the observed increase was not just part of a general increase in crime rates. Although it might have been predicted that reporting rates were not likely to be affected by the reforms because the public would be the least likely to know about the legal changes, the results are less surprising in Detroit than they would have been in other jurisdictions. Michigan was the first state to enact dramatic and comprehensive changes in rape laws, and the legislative changes were the result of a well-organized and highly visible effort on the part of women's groups. The changes also occurred at a time when the crime of rape was receiving considerable attention from the media across the country. These factors meant that there was considerable publicity surrounding the Michigan reforms, and therefore a greater likelihood of reporting being influenced.

The increase in indictment rate represents the major impact on the decisionmaking of criminal justice officials. Because we do not have monthly arrest data, we cannot say conclusively whether the result represents decisions by police or by prosecutors, although it seems more likely that prosecutors would be affected since the legal changes were directed more at the factors determining likelihood of obtaining convictions at trial. We also had reports from the Detroit police that prosecutors had been refusing fewer warrants in rape cases since the passage of the laws, and a victim-witness unit respondent believed that more "date rape" cases were getting into the court system (see Appendix C).

The interpretation of the impact on indictment rate is clouded by the definitional changes that were part of the comprehensive reforms. As explained earlier, it was impossible to have perfectly comparable measures of rape before and after the laws changed. Thus it is possible that the observed results may reflect in part the greater inclusiveness of the post-reform definitions. Without more information on the cases, of the kind that would be found only in police reports, it is impossible to create perfectly equivalent numerators. By including sodomy and gross indecency with rape for the

pre-reform measures, and by eliminating second and fourth degree sexual assault from the post-reform measures, we believe we have come quite close to equivalent measures. Both the before and after measures include male victims, non-vaginal penetration, and non-forcible sexual penetration with minors. The offenses probably included in the post-reform measures but not in the pre-reform measures are penetration with an object and incest.

Marsh, et al. (1982), in their study of the Michigan rape law reforms, found somewhat different results from those we have reported. They found no impact on reported rapes but a significant increase in the number of convictions on original charges and a significant decrease in convictions on lesser charges. They also indicated an increase in the rate of convictions as charged (convictions as charged divided by reports). In contrast, we found an increase in reported rapes but no significant increase in the rate of convictions as charged (convictions on original charge divided by indictments). We, like Marsh, et al., found an increase in the total number of convictions on original charge, but we concluded that this result probably reflected the increase in indictments, since we found no significant increase in the rate variable. There are a number of factors that may account for the different results.

First, Marsh, et al., used statewide data from the Michigan police, whereas our data were from the Detroit Recorder's Court; thus it is possible that jurisdictional differences could account for the discrepant outcomes. Second, Marsh, et al., also used interrupted time-series analysis, but their time period was for the three years before and the three years after the reforms, whereas our analysis covered a fourteen-year period. Additionally, their results on the rate of convictions as charged was based only on seven observations of yearly rates and not on a monthly time-series analysis. Our results with 180 monthly observations indicated an increase in the rate, but not a statistically significant increase. Third, we are not sure which offenses were included in the Marsh analyses. The offense of rape was the basis for the pre-reform measure, but their reports do not indicate which of the post-reform criminal sexual assault offenses were included. This would have no bearing on the analysis of reports, but could influence the findings on convictions.

One interpretation of our finding that major impact was limited to Detroit is that the criminal justice system can only be affected by the kind of dramatic, comprehensive changes that were made in Michigan. Detroit was our only jurisdiction in which all were strong reforms. The weaker reforms in other jurisdictions or the piecemeal nature of some of the stronger reforms may have precluded the kind of broad impact on case processing that reformers predicted. From our interview data (see Appendix C) it also appears that criminal justice system officials in Detroit perceived more pressure from organized women's groups than did officials in other jurisdictions. Thus the strong reforms in Michigan were apparently accompanied by a closer monitoring of the system by the advocates of reform.

Attitudes Toward Evidence In Sexual Assault Cases

In this section of the report we investigate the degree to which criminal justice officials use (or think jurors use) legally irrelevant assessments of the victim's character and behavior in making decisions in rape cases. We also examine these officials' attitudes toward restrictions on the use of evidence of the victim's past sexual conduct.

Our analysis is directed both toward broadening our knowledge of the impact of rape law reform and toward responding to critics of the rape shield laws who contend that the laws unconstitutionally infringe on the rights of criminal defendants. Of all the reforms enacted, the rape shield laws clearly have engendered the most controversy. Civil libertarians and legal scholars have harshly criticized the shield laws, especially the more restrictive ones, on the grounds that they prevent the introduction of legally relevant evidence and thus infringe on the defendant's right to confront witnesses against him and to call witnesses in his own behalf (Berger, 1977; Haxton, 1985; Herman, 1976-77; Loftus, 1982; Rudstein, 1976; Tanford and Bocchino, 1980; Williams, S., 1984). The laws have produced a lively

discussion in the legal literature concerning the conflict between the defendant's rights, the rights of the victim to privacy or to the equal protection of the laws, and the state's interest in securing reports of and arrests and convictions for rape.

Most legal scholars have concluded that while the defendant's right to present evidence is not unlimited, it is not likely that the Supreme Court would extend either the right to privacy or the equal protection clause so far that the exclusion of relevant sexual conduct evidence would be constitutionally justified (Berger, 1977; Haxton, 1985). On the other hand, some authors maintain that the state's interests in encouraging victims to report and prosecute might justify the exclusion of evidence, if it could be shown that the shield laws actually furthered this interest. As Haxton points out, however, the lack of empirical evidence on this point means that "there is not a sufficiently compelling governmental interest, nor any constitutional right of the complainant, that justifies the exclusion by rape shield statutes of highly probative evidence of the complainant's past sexual conduct" (Haxton, 1985: 1267-8).

These criticisms certainly are not without merit. The rights of criminal defendants, many of whom face long prison terms if convicted, should not be sacrificed to protect the privacy and emotional well being of the victim. But these criticisms assume that relevant evidence of past sexual conduct is excluded by the rape shield laws, and particularly by the more restrictive laws. The criticisms assume, in other words, that prosecutors routinely challenge defense attorneys' attempts to introduce probative sexual history evidence and that judges routinely side with prosecutors. By examining judges', prosecutors' and defense attorneys' attitudes toward evidence in sexual assault cases, we hope to address the validity of these assumptions.

We noted above that the six cities selected for this study represent jurisdictions which enacted different types of rape law reforms. We categorized the reforms as strong (Detroit and Chicago), moderate (Philadelphia and Houston), or weak (Atlanta and Washington, D.C.), depending on the types and strength of the changes adopted. The reforms enacted in the different jurisdictions, then, clearly have the potential to produce different effects on the disposition of rape cases and on the attitudes of criminal justice personnel. In fact, we expect to find attitudinal differences. We expect officials in the jurisdictions with strong laws to be more supportive of the laws and less willing to circumvent either the substantive or the procedural restrictions contained in them.

Ratings of the Importance of Evidence. We asked prosecutors in each jurisdiction to rate the importance of various types of evidence to the decision to file charges in sexual assault cases. In addition, we asked all respondents to assess the importance of the evidence to persuading juries to convict. The types of evidence and the respondents' ratings are presented in Table 12, which categorizes the evidence as either legal or extralegal. We define legal evidence as evidence necessary or helpful to proving elements of the crime, extralegal evidence as evidence legally irrelevant to proving elements of the crime. The evidence is arrayed from most to least important, based on the respondents' beliefs about the degree to which the evidence influences the jury's decision to convict.

As expected, the types of evidence deemed most important are legal factors closely related to the elements of the crime which the prosecutor must prove. This applies both to the decision to file charges and the decision to convict. The victim's ability to identify the suspect, the fact that the victim reported the crime promptly and the existence of physical evidence or of evidence the victim was physically injured all affect the likelihood that the prosecutor will be able to prove the identity of the suspect, lack of consent, and penetration. The various types of extralegal evidence, all of which concern legally irrelevant characteristics of the victim or of the relationship between the victim and the suspect, are seen as less important.

The data presented in Table 12 also reveal that each type of evidence was seen as more

important to the jury's decision to convict than to the prosecutor's decision to file charges. This is not surprising. At this early stage in the criminal justice process, prosecutors may be reluctant to reject the charge against the defendant simply, for example, on the basis of evidence that the victim used drugs or alcohol at the time of the incident or had a prior sexual relationship with the defendant. They may reason that evidentiary problems such as these will be taken into account at the preliminary hearing or trial. Respondents apparently believe that jurors, as the final arbiters in cases that go to trial, will be more influenced by the presence or absence of certain types of evidence.

It is interesting that the largest differences in the ratings of the importance of the evidence to prosecutors and jurors are found for evidence affected either directly or indirectly by rape law reforms. These criminal justice officials believe that jurors are much more likely than prosecutors to be swayed by evidence that the victim resisted (a difference of .58), that the suspect and victim were strangers (.50), that the suspect and victim had no previous sexual relationship (.39), that the victim does not have a reputation for sexual promiscuity (.39), and that the victim was physically injured (.39). All of these types of evidence should be less probative, and therefore less persuasive, in the post-reform era.

Coupled with statements made during the interviews, these differences suggest that criminal justice officials believe reform legislation has affected their attitudes more than the attitudes of the general population from which jurors are chosen. As one prosecutor in Detroit noted, "Jurors still expect some resistance or some explanation as to why there was none, especially if it was a date gone sour." More to the point, a judge in Chicago commented that "Jurors are still looking for corroborating evidence -- physical injury, a weapon, an hysterical phone call to the police. Old habits and old attitudes die hard. We can change the law but we can't necessarily change attitudes."

Further evidence of the impact of rape law reform can be found by comparing the ratings in the six jurisdictions. Generally, the ratings either are fairly consistent from jurisdiction to jurisdiction or vary randomly among the jurisdictions. For the types of evidence directly affected by rape law reform, however, the ratings manifest clear patterns. For example, evidence that the victim resisted is seen as less persuasive to jurors in Detroit, Chicago and Philadelphia than to jurors in Houston, Atlanta and Washington, D.C. These differences clearly reflect differences in rape laws. Michigan, Illinois and Pennsylvania have statutorily eliminated the resistance requirement and judges there instruct jurors that lack of resistance does not constitute consent; in Texas, Georgia and the District of Columbia, on the other hand, resistance still is implicitly or explicitly required.

Similar patterns are found for evidence of a prior sexual relationship between the victim and suspect and evidence of the victim's reputation for sexual promiscuity. Once again, the differences found among the jurisdictions reflect differences in rape laws. Michigan, Illinois and Pennsylvania all have strong rape shield laws; Texas, Georgia and Washington, D.C. have weak ones. In particular, shield laws in the former jurisdictions generally prohibit the introduction of evidence of the victim's reputation, while those in the latter jurisdictions permit it upon a showing of relevance. Given this, it is not surprising that evidence concerning the victim's sexual reputation is seen as more likely to influence juries in Houston, Atlanta and D.C. than in Detroit, Chicago and Philadelphia; if the evidence is seldom or never ruled admissible, its effect on jurors obviously will be negligible.

Responses to Hypothetical Cases. Additional evidence of the impact of the rape shield laws is found in Table 13, which summarizes the responses of judges, prosecutors and defense attorneys to a series of hypothetical cases in which evidence of the victim's past sexual history is at issue. (The text of the hypothetical cases can be found at the end of the executive summary.) These hypothetical cases, which were adapted from Herman's (1976-77) discussion of the constitutionally of the rape shield laws, represent what many would agree are the difficult questions raised by the rape shield laws. They represent situations in which the probative worth of the sexual history evidence is debatable.

Respondents were asked to indicate whether they personally believed the sexual history evidence should be admitted and to assess the likelihood that the evidence would be admitted in their jurisdiction. Examination of the mean responses for all respondents reveals that criminal justice officials clearly do not attach the same probative weight to each of the six types of evidence. Most believe that accusations of prior sex with men the victim met at singles bar should not be admitted, but large majorities think that threats implying prior sexual behavior, the results of semen tests and prior sex with the defendant should be admitted. Respondents were more evenly split over the question of whether prior sexual experiences with men similar to the alleged suspect (maintenance men) or with groups of men should be allowed.

To explain why they felt the latter two types of evidence should be admissible, even in the face of shield laws prohibiting the evidence, a number of respondents cited either the "unusual" nature of the behavior or the fact that the evidence revealed a pattern of behavior. One prosecutor in Chicago, for example, justified his belief that a gang rape victim's prior sexual experiences with groups of men should be introduced by noting the "bizarreness" of the behavior. On the same issue, a Philadelphia public defender stated that "This is the kind of thing the appellate courts have created an exception for -- a common behavioral pattern. If this were true it obviously would be relevant and the defendant could not get a fair trial without it."

Assessments of the likelihood that the various types of evidence would be admitted also varied. Respondents were convinced that allegations of prior sex with men the victim met at singles bars would not be admitted, but that evidence of prior sex with the defendant would be. They were doubtful that accusations of prior sex with men similar to the defendant or with groups of men would be allowed in, but felt that testimony regarding threats or the results of semen tests probably would be permitted. Many respondents, particularly judges and prosecutors, labelled evidence of prior casual sex with men the victim met in bars "the classic example of the type of evidence the shield laws were designed to keep out." A number of defense attorneys, on the other hand, questioned the shield laws' impenetrability with respect to evidence of prior sex with men similar to the defendant or with groups of men. A public defender in Philadelphia stated that defense attorneys' chances of winning rape cases had diminished "as a result of using the rape shield law as a technical weapon to keep out probative testimony rather than as an instrument for protecting the victim."

If we compare the attitudes of criminal justice officials in the six jurisdictions, a number of interesting findings emerge. First, there is relatively little disagreement among the respondents that evidence of the victim's prior sexual encounters with men she met at singles bars should not and would not be admitted. Even in Atlanta and Houston, jurisdictions with weak shield laws, respondents believe that this type of testimony probably would not be permitted. And in Washington, D.C., which has not enacted a shield law but which relies on case law to prohibit the introduction of this type of evidence, respondents are convinced that the evidence definitely would not be admitted. If this is indeed the classic example of the type of evidence shield laws were designed to prohibit, and if we assume this type of evidence was permitted in the pre-reform era, then the reforms clearly have been effective.

A second finding is that respondents in the various jurisdictions generally agreed that evidence of a prior sexual relationship with the defendant, as well as evidence of threats implying prior sexual conduct, should and would be admitted. A number of officials noted that evidence of a past sexual relationship with the defendant was relevant to the issue of consent. Others said that evidence of prior threats was relevant to the question of whether the victim was biased against the defendant or had a motive to lie about the incident. Criminal justice officials, in other words, believe these types of evidence are probative and, consequently, would be admitted.

The data presented in Table 13 also reveal that reactions to the other three hypothetical situations were more variable. This is particularly true of responses to case #2 (prior sex with men

similar to the defendant) and case #3 (prior sex with groups of men), which most respondents agreed were the "tough calls." These differences appear to be related to differences in the shield laws. Michigan and Illinois have the strongest shield laws, with absolute prohibitions against the introduction of evidence of the victim's prior sexual conduct with persons other than the defendant or evidence concerning the victim's sexual reputation. Respondents in these two jurisdictions reported that the victim's prior sexual encounters with men similar to the defendant or with groups of men probably would not be admitted.

The other four jurisdictions have weaker shield laws. The Texas, Georgia and District of Columbia laws permit the introduction of testimony concerning the victim's past sexual behavior or sexual reputation if the evidence is found to be relevant; the law in Pennsylvania prohibits admitting such evidence, but appellate courts recently have carved out a number of exceptions to the prohibitions. Reflecting their laws, officials in these four cities, and particularly in Houston, Atlanta and Washington, D.C., are more likely to believe that the evidence cited in case #2 or #3 would be admitted.

Since slightly different proportions of judges, prosecutors and defense attorneys were interviewed in each jurisdiction and since attitudes toward the relevance of the victim's past sexual behavior obviously might vary among the three groups, we speculated that the results presented in Table 13 might reflect this disparity. Accordingly, we re-analyzed the data, controlling for jurisdiction, for the respondents' occupational group and for a number of other independent variables. We reasoned that respondents' attitudes might be affected by their gender; by the number of years they had been judges, prosecutors or defense attorneys; and (for the analysis of their beliefs about whether the evidence would be admitted) by their beliefs about whether the evidence should be admitted. We then ran regressions on each of the dependent variables controlling for these independent variables. Using a technique analogous to the procedure used to compute adjusted means in multiple classification analysis or in analysis of covariance (see Andrews et al., 1973; Miller and Erickson, 1974), we computed the adjusted means for each of the various types of evidence. (See footnote a, Table 14 for a discussion of the calculation of these means).

While controlling for the independent variables caused the mean responses to shift slightly in some jurisdictions, the data presented in Table 13 are not significantly different from the data presented in Table 14. The conclusions drawn earlier stand: first, there is little disagreement among respondents in the six jurisdictions concerning the irrelevance of evidence that the victim had prior sexual encounters with men she met at bars or the relevance of evidence that the victim had a prior sexual relationship with the defendant; and second, the responses to the tough cases reflect the strength or weakness of the rape shield law in each jurisdiction.

Despite the apparent correlation between the strength of the shield laws and officials' assessments of the likelihood that various types of evidence would be admitted, it is clear that the restrictions found in the shield laws can be circumvented. For example, given the absolute prohibition against introducing evidence of the victim's past sexual behavior with persons other than the defendant found in the Michigan and Illinois laws, it is surprising that <u>any</u> respondents in these jurisdictions believed the evidence at issue in case #2 or case #3 would be admitted. That some officials did feel the evidence might be admitted suggests that the impenetrable shield fashioned by legislators in these jurisdictions can be lowered by judges who believe the evidence is relevant.

It also is clear that procedural guidelines contained in the shield laws have been circumvented. All of the laws allow prior sexual conduct between the victim and the defendant to be introduced following a judicial finding of relevance in an <u>in camera</u> hearing. Interviews with judges, prosecutors and defense attorneys, however, revealed that <u>in camera</u> hearings were rarely if ever held in these cases. This may be due at least in part to the fact that criminal justice officials themselves believe the

evidence is relevant to the issue of consent. As shown in Table 14, large majorities of the respondents in each city believe that evidence of a prior sexual relationship between the victim and the defendant should be admitted.

In interview after interview, prosecutors in each jurisdiction admitted that they concede the relevance of this type of evidence. As a prosecutor in Detroit explained, "Most of the time I won't contest it if it's with the defendant, even though technically the judge is supposed to rule on the relevance of the information. I don't oppose it because I think it's relevant that they've had a prior sexual relationship." A prosecutor in Atlanta noted that there was no point in asking for a hearing since the judge will always let it in, adding "even I agree that this (conduct between the victim and defendant) is relevant and see no point in trying to keep it out." And a district attorney in Philadelphia somewhat cynically explained that if this type of evidence is offered "judges generally will admit it since they're afraid of being overruled on appeal."

These comments were echoed by both judges and defense attorneys. Judges in Chicago, Philadelphia and Houston readily admitted that evidence of a prior sexual relationship between the victim and the defendant is admitted without a hearing if the defense is consent, and defense attorneys in each jurisdiction cited instances where they were able to "get the evidence in" without a hearing on its relevance. It thus appears that the members of the courtroom workgroups in each jurisdiction have developed an informal policy to circumvent the formal requirements of the law.

Implications. Our examination of criminal justice officials' attitudes toward evidence in sexual assault cases yielded a number of findings that merit elaboration. First, the rape reform movement seems to have played a role in the socialization of criminal justice officials. Indirect evidence for this is seen in respondents' beliefs that extralegal factors, and particularly those explicitly affected by rape reform legislation, are more important to the jury's decision to convict than to the prosecutor's decision to charge. Direct evidence for this is found in respondents' personal beliefs about the relevance of testimony concerning the victim's past sexual conduct. Although some officials were unable or unwilling to untangle their own beliefs about whether the evidence should be admitted from beliefs about whether the evidence would be admitted under the law, most, when pressed, were able to do so. The differences in responses among the jurisdictions range from subtle (prior sex with the defendant) to dramatic (prior sex with groups of men).

These differences are particularly apparent between Detroit, the jurisdiction with the most restrictive law, and the three jurisdictions with the most permissive laws. For example, only 27 percent of the officials in Detroit believed that prior sex with groups of men should be admitted, compared to 77 percent of the respondents in Washington, D.C. Laws restricting the use of sexual history evidence, then, may have shaped or molded the attitudes of criminal justice officials toward victims of sexual assaults.

The data also indicate that the rape shield laws have the potential to influence the outcome of sexual assault cases. Although respondents in the six cities agreed that three types of sexual history evidence generally would be admitted while one type usually would not be, they were not in complete agreement even in these cases. Evidence of prior sexual encounters with men the victim met in bars, for example, was given a greater chance of being admitted in Houston than in Detroit or Chicago. And in the tough cases -- cases involving a pattern of sexual behavior with men similar to the defendant or unusual sexual behavior -- the differences among the jurisdictions were much more pronounced. If the likelihood that evidence such as this will be admitted varies, and if it is true that the evidence, if admitted, would incline a judge or jury toward acquittal, then prohibiting or admitting the evidence obviously could affect the disposition of sexual assault cases.

This possibility goes to the heart of criticisms of the rape shield laws. Critics worry that highly

probative evidence will be excluded in the interest of protecting the victim. Our data indicate, however, that the most probative types of evidence probably would be admitted in each jurisdiction. Evidence of threats against the defendant is relevant to the question of whether the victim has a motive to lie or is biased against the defendant, semen test results help establish the identity of the rapist, and prior sexual encounters between the victim and the defendant may be relevant to the issue of consent. In each city the probability of these types of evidence being admitted is high. This applies even in Detroit and Chicago. If these six jurisdictions are typical, "highly probative" evidence of the victim's past sexual history probably will not be excluded under the rape shield laws.

On the other hand, it is possible that potentially relevant evidence will be excluded under the shield statutes. Respondents in each jurisdiction were troubled by the hypothetical cases involving sexual history evidence of more questionable relevance. In each of the cities except Detroit, for instance, either a large minority or a majority of the officials believed that testimony concerning the victim's prior sexual behavior with groups of men should be allowed, but admitted that the law probably would exclude it. An oft-heard comment was that if the allegations were true they should be heard by the judge or jury, but judges probably would not allow it. As one public defender said, "This prevents the defendant from having his day in court."

This allegation is tempered, however, by our finding that the restrictions contained in the rape shield laws can and will be ignored. This is possible at least in part because of the vast amount of discretion accorded officials in the criminal justice system. Prosecutors and judges troubled by the substantive or procedural restrictions found in the shield laws can simply disregard them. Prosecutors can concede the relevance of sexual history evidence and not challenge defense attorneys who either attempt to introduce admissible evidence without requesting an in camera hearing or attempt to use inadmissible evidence during the trial. Likewise, judges can use their discretion to overlook the in camera hearing requirement or to overrule prosecutor's objections to the introduction of the evidence. If these things occur with any regularity, and the data collected for this project indicate that they happen more than one would expect, then the shield laws may be considerably weaker in practice than they appear on paper. While this may appease critics of the statutes, it also may alarm proponents of the reform.

DISCUSSION OF RESULTS

Criticisms of the treatment of rape victims and the processing of rape cases prompted states to reform their rape laws. By the mid-1980s most states had modified the rules of evidence relevant to rape and many states had redefined the crime of rape. The overall purpose of these 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. Reformers expected that the legal changes would reduce both the skepticism of criminal justice officials toward the claims of rape victims and their reliance on extralegal considerations in decisionmaking. They anticipated that the reforms ultimately would lead to an increase in the number of reports of rape and would make arrest, prosecution and conviction for rape more likely.

Reformers expected that the rape reform statutes would have both indirect and direct effects on the processing and disposition of rape cases. The statutes would affect rape cases indirectly by altering attitudes toward the crime of rape and toward rape victims. Redefining rape, modifying or eliminating the resistance and corroboration requirements, and placing restrictions on the use of evidence of the victim's prior sexual conduct, in other words, would alter criminal justice officials' and jurors' perceptions of "real rapes" with "genuine victims." Rather than focusing on whether the victim was black or white, married or single, chaste or promiscuous, decision makers would focus on whether the offender used a weapon, injured the victim, or had an accomplice. In short, the changes were expected to counteract the assumption that when men force you to have sex against your will "it isn't

rape so long as they know you and don't beat you nearly to death in the process" (Estrich, 1987: 4).

Reformers also anticipated that the changes would directly affect the processing of rape cases. Replacing the crime of rape with a series of graded offenses with commensurate penalties, for example, was expected to produce an increase in convictions. The availability of appropriate lesser charges would enhance the prosecutor's ability to achieve convictions through plea bargaining and would reduce the likelihood of jury nullification in cases where the charge of rape did not seem to fit the circumstances of the crime. Changing the resistance and corroboration requirements would make it easier to prove that the victim was raped, thus increasing the likelihood of arrest, prosecution and conviction. And restricting the use of evidence of the victim's past sexual conduct would prompt more victims to report, would encourage police and prosecutors to proceed with cases with sexually promiscuous victims, and would reduce the likelihood of an acquittal based on the victim's sexual reputation.

Summary of Results

In evaluating the rape reform statutes, we examined both the indirect and direct effects of the changes. Interviews in the six jurisdictions revealed that criminal justice personnel are aware of and support most of the changes in their states' rape laws. Most of those interviewed said they approve of the evidentiary changes, which they believe have resulted in more appropriate treatment of men accused of rape, as well as more humane treatment of the victims of rape. Respondents agreed that rape victims in the post-reform era should not and would not be subject to overt harassment by the defense attorney. They also agreed that evidence which the new laws deem irrelevant should not be taken into consideration during the decisionmaking process. Officials in each jurisdiction spoke approvingly of these revised attitudes toward rape cases and rape victims. The standard line, which we heard over and over again, was that "even a prostitute can be raped."

Despite this general acceptance of the changes, however, there were clear inter-jurisdictional variations in attitudes and in compliance with the substantive and procedural restrictions contained in the laws, and these differences are related to the type of law reform enacted. This is especially true of compliance with restrictions on the introduction of evidence of the victim's past sexual conduct. When questioned about a series of hypothetical cases where evidence of the complainant's past sexual history was at issue, officials in Detroit and Chicago, the two cities with the most restrictive shield laws, consistently reported that the more questionable types of evidence should not and would not be admitted. In Atlanta and Houston, the two cities with the weakest shield laws, on the other hand, respondents were less convinced that these types of evidence should or would be excluded.

These findings suggest that the rape reform laws have had indirect effects on the processing of rape cases. They indicate that the reforms have shaped or molded the attitudes of criminal justice officials toward victims of sexual assaults. The findings also suggest, however, that even these indirect effects are associated with the type of law reform enacted. The stronger reforms seem to have played a greater role in the socialization of criminal justice officials than the weaker reforms.

We also examined the direct effects of the legal changes. We used an interrupted time-series design to assess the impact of the changes on reports of rape, and on indictments, convictions, and sentences for rape. We found that the changes produced few significant effects in the four jurisdictions which enacted moderate or weak reforms -- Pennsylvania, Texas, Georgia, and Washington, D.C. Our analysis also revealed few changes in the jurisdiction which enacted strong reforms at two different points in time. We found that passage of the 1978 Illinois rape shield law resulted in a statistically significant increase in the average sentence, but had no effect on any of the other dependent variables; similarly, definitional changes implemented in 1984 produced no effects in the predicted direction. On the other hand, the legal changes in the jurisdiction (Michigan) with the strongest and most

comprehensive reforms produced a number of significant effects; there were increases in the number of reports, indictments, convictions, and incarcerations, and in the indictment rate and average sentence.

The types of direct effects anticipated by the reformers, then, were found only in Detroit. This, coupled with the fact that criminal justice officials in Detroit expressed more support for the evidentiary changes than officials in any of the other jurisdictions, suggests that the impact of rape reform statutes will be confined primarily to jurisdictions which enact strong and comprehensive changes. The reasons for this are explored below. We first discuss why the weak evidentiary changes enacted in Texas, Georgia and Washington, D.C. failed to produce significant results. We then explain why the strong evidentiary changes enacted in Illinois and Pennsylvania did not produce the expected results, while the comprehensive changes implemented in Michigan did.

Reform in Texas, Georgia and Washington, D.C.

The lack of impact in Houston, Atlanta and Washington, D.C. can be explained by the weak nature of the reforms enacted in these jurisdictions. Georgia and Washington, D.C. retain traditional carnal knowledge statutes and Texas until 1983 defined rape and sexual abuse in terms of the female's absence of consent and degree of resistance. While all of the jurisdictions have repealed or modified the corroboration requirement, none of them has explicitly repealed the resistance requirement.

All three jurisdictions also have very weak restrictions on the use of sexual history evidence. The Texas law is often cited as an example of the most permissive type of law (Berger, 1977; Galvin, 1986). Texas does not categorically exclude any sexual conduct evidence; rather, such evidence can be admitted only if the judge finds that the evidence is relevant. As a shield, then, the Texas law is fairly permeable.

The statute enacted in Georgia is more restrictive than the one adopted in Texas, but still gives judges considerable discretion to admit sexual conduct evidence. The Georgia law states that evidence of the victim's past sexual conduct is admissible unless the court finds that the evidence concerns behavior with the accused or supports an inference that the accused reasonably could have believed the victim consented. Prosecutors in Atlanta suggested that the shield law actually was weaker than case law in effect prior to the law's passage.

While Washington, D.C. has not enacted a shield law, case law does limit the admission of sexual conduct evidence. According to a 1977 case, the victim's prior sexual conduct with third persons is inadmissible, the victim's reputation for chastity should not be admitted except where its probative value outweighs its prejudicial effect, and the victim's prior sexual conduct with the defendant is admissible to rebut the government's evidence that the victim did not consent. The law in Washington, D.C., then, is somewhat more restrictive than the law in Texas or Georgia. As case law, on the other hand, it may not have the same potential for impact as a major legislative reform.

Given the weak nature of the reforms enacted in these three jurisdictions, then, it is not surprising that they produced no direct effects on the processing and disposition of rape cases. All three states enacted some evidentiary changes but retained traditional definitions of rape and assumptions about the importance of resistance by the victim. The shield laws adopted in each state continue to allow judges nearly unfettered discretion in deciding whether or not to admit sexual history evidence. Since the reforms did not substantially alter the "rules" for handling rape cases, they have little potential to directly affect the outcomes of these cases. They can be viewed as " . . . largely symbolic reassurance that needs are being attended to, problems are being solved, help is on the way . . ." (Casper and Brereton, 1984: 124).

Reform in Illinois, Pennsylvania and Michigan

While it is fairly easy to explain the lack of impact in the three jurisdictions which enacted weak reforms, it is more difficult to explain the results in the three jurisdictions which adopted stronger reforms. We noted earlier that we found the types of direct effects anticipated by the reformers only in Detroit. This result is somewhat puzzling. We felt the restrictive rape shield laws enacted in Illinois, Pennsylvania and Michigan had the potential to produce similar results. 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 (Feild and Bienen, 1980). And Marsh and her colleagues (1982) found that criminal justice officials cited restrictions on the introduction of sexual history evidence as the most significant aspect of the reforms adopted in Michigan.

The shield 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 is irrelevant and therefore inadmissible. Unlike the laws enacted in Texas, Georgia and Washington, D.C., they also limited the discretion of judges to admit certain types of evidence.

Contrary to our expectations, these similar laws did not produce similar results. Perhaps this is because the reform packages adopted in each jurisdiction are very different. Illinois implemented the shield law in 1978 but did not adopt definitional changes or repeal the resistance requirement until 1984. The Pennsylvania reform included a number of evidentiary changes; a shield law was adopted and the corroboration, prompt complaint and resistance requirements were repealed in 1976. The Michigan reform included both evidentiary and definitional changes; the comprehensive statute enacted in 1975 redefined rape and established four degrees of gender-neutral criminal sexual conduct, eliminated the corroboration and resistance requirements, and placed restrictions on the use of sexual history evidence. By comparing the effect of the rape reform statutes in these three jurisdictions, then, we can assess the effects of three different types of changes: a rape shield law only; a rape shield law and other evidentiary changes; and a comprehensive overhaul of the rape laws.

The Effect of the Rape Shield Laws. Our findings indicate that while a rape shield law may be the most important component of a comprehensive reform package, it cannot by itself affect the processing and disposition of rape cases. In fact, our results strongly suggest that evidentiary changes alone will not alter the outcomes of rape cases.

There are a number of reasons why the rape shield laws cannot produce the types of changes envisioned by reformers. First, the shield laws are designed to prevent the admission of sexual history evidence at trial. Although there may be spillover effects on arresting and charging decisions, the shield laws will primarily affect cases which go to trial and, particularly, the small percentage of cases tried before a jury. This is complicated by the fact that 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 directly affect 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 upon a trial.

Even if we assume that these types of cases are fairly common, it does not necessarily follow that the passage of a shield law will result in significant changes in the processing of rape cases

overall. Although respondents in each jurisdiction stated that the law prevents blatant attempts by the defense attorney to harass or embarrass the rape victim, most could not recall many pre-reform cases in which defense attorneys used this tactic. If there weren't many of these egregious cases before, elimination of some, or even all, of them wouldn't show up in a statistical analysis designed to measure the impact of the law on the outcomes of all rape cases.

The effect of the rape shield law might also be tempered by prior case law. If court rulings had begun to restrict the use of sexual history evidence, the effect of the statutory change would not be as noticeable. Respondents in Chicago and Detroit stated that case law provided some protection for the victim prior to passage of the shield law. They also noted, however, that the shield law contained a stronger message than case law. One judge in Detroit offered the opinion that it may have taken the law to foster "a stronger judicial no-nonsense attitude."

The situation is further complicated by the fact that the procedural requirements of the shield laws can be circumvented. All of the statutes allow prior sexual conduct between the victim and the defendant to be introduced following a judicial finding of relevance in an in camera hearing. Interviews with judges, prosecutors and defense attorneys, however, revealed that in camera hearings were rarely if ever held in these cases. Instead, prosecutors 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. In the three jurisdictions which permit the introduction of other types of sexual history evidence, prosecutors also use the motion in limine to prevent the defendant from introducing irrelevant evidence, thus precluding the need for the in camera hearing. Similarly, judges use their discretion to overlook the in camera requirement or to overrule prosecutor's 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." The overriding goal of the courtroom workgroup is to process cases is quickly and as efficiently as possible. 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.

This lack of compliance can also be explained by the fact that judges and prosecutors have few, if any, incentives to comply. 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 and prosecutors to err in favor of the defendant.

Finally, noncompliance might also be attributed to prosecutor's and judge's beliefs that evidence of a prior sexual relationship between the victim and the defendant is, the law notwithstanding, relevant to the issue of consent. Respondents in each jurisdiction admitted that this type of evidence generally is regarded as probative. If those who are to enforce the law disagree with it, the likelihood of the law being effectively implemented is obviously reduced. This is particularly true in a system, like the criminal justice system, where participants have vast amounts of discretion. Reformers should not assume that judges and prosecutors will comply with the formal requirements of the law. As Nimmer (1978: 179) notes, "compliance is preceded by interpretation, which permits injection of the judge's

preferences." A judge in Chicago put it more succinctly when he said, "Well, the law's the law, but fair is fair."

Reformers hoped that the rape shield laws would significantly affect the processing and disposition of rape cases. It seems clear, however, that this was an unrealistic expectation. The effect of the shield laws is limited by the types of cases they apply to and by noncompliance with their substantive and procedural requirements. Even a strong law like the one adopted in Illinois apparently cannot by itself affect the outcomes of rape cases.

The Effect of Other Evidentiary Changes. Pennsylvania's restrictive rape shield law was accompanied by elimination of the resistance, prompt complaint and corroboration requirements. The Pennsylvania reform, in other words, was broader than the Illinois reform. As such, it had greater potential to affect the outcomes of rape cases. This package of evidentiary changes, however, did not have an impact on the processing of rape cases in Philadelphia.

There are a number of reasons why eliminating the resistance and corroboration requirements might not produce the types of results anticipated. Reformers felt these changes would make it easier to prove that the victim was raped, thus increasing the likelihood of arrest and prosecution. However, court decisions in most jurisdictions, including Pennsylvania, already had loosened these requirements. Courts had ruled that the victim is not required to put her life in jeorpardy by resisting and that evidence of force on the part of the offender is tantamount to proof of nonconsent by the victim. Court rulings also had loosened the corroboration requirement; a prompt complaint or physical evidence of intercourse, for example, could corroborate the victim's testimony. It is possible, then, that neither of these requirements was a significant hurdle in the pre-reform era. Consequently, their elimination would not result in significant changes in decisionmaking.

Reformers also anticipated that eliminating the resistance and corroboration requirements would increase the likelihood of conviction; they felt that jurors would be more likely to convict if these evidentiary hurdles were removed. Many respondents stated that these reforms were important and may have had an impact on jury verdicts. They explained that under the new laws it was possible to include in the jury instructions statements that the victim need not resist and that her testimony need not be corroborated. They felt it was important that jurors hear this from the judge. As one prosecutor in Philadelphia explained, "When the judge says it explicitly to the jury, the jury listens and takes it more seriously." If this is true, then it is essential that these statements routinely be included in jury instructions. From our interviews, however, it is clear that this is not the case. In some jurisdictions all judges routinely instruct jurors that resistance and corroboration are not required. In other jurisdictions, however, some judges always include these instructions while others do so only if requested to by the prosecutor. This type of discretion obviously can mitigate the effect of the reforms.

The effect of the reforms also will be limited if jurors, in spite of the law, continue to expect resistance and corroboration. We noted earlier that criminal justice officials believe reform legislation has affected their attitudes more than the attitudes of the general population from which jurors are chosen. They believe that many jurors are suspicious of a rape case in which the victim did not resist or cannot offer corroborating testimony. As one judge in Chicago said, "Jurors are still looking for corroborating evidence -- physical injury, a weapon, an hysterical phone call to the police. Old habits and old attitudes die hard. We can change the law but we can't necessarily change attitudes."

All of these factors help explain why eliminating the resistance and corroboration requirements, even in combination with a strong rape shield law, did not significantly affect the processing of rape cases in Philadelphia. Prior court rulings, judicial discretion in instructing the jury, and juror resistance to change all serve to dampen the effect of the reforms.

The Effect of Comprehensive Changes. In 1975 Michigan implemented a comprehensive rape reform statute. The reform included a strong rape shield law and elimination of the corroboration and resistance requirements. It also established four degrees of gender-neutral criminal sexual conduct defined by 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 statute extends the reach of the sexual assault laws to acts (sexual penetration with an object) and persons (men and married persons who are legally separated) not covered by the old laws. Clearly, the Michigan law is broader than either the Illinois or the Pennsylvania laws.

We noted above that reformers expected the legal changes to affect both the attitudes of criminal justice officials toward rape victims and the actual processing and disposition of rape cases. It seems clear that the Michigan reforms produced both types of effects.

Our interviews revealed strong support for the reforms among criminal justice officials in Detroit. They also revealed a greater level of compliance with the substantive and procedural requirements of the rape shield law in Detroit than in the other five jurisdictions. When questioned about a series of hypothetical cases where evidence of the complainant's past sexual history was at issue, officials in Detroit consistently reported that the more questionable types of evidence should not and would not be admitted. For example, only 27 percent of the officials in Detroit believed the victim's prior sexual activities with groups of men should be admitted, compared to 77 percent of the respondents in Washington, D.C.

Nimmer (1978) maintains that criminal justice officials will be more likely to comply with legal changes of which they approve. Our study provides support for this. Officials in Detroit strongly support the changes and are inclined to comply with them. This may provide a partial explanation for the impact of reform legislation in Detroit. That is, the comprehensive legal changes engendered attitude change which led to compliance.

The Michigan reform also had more direct effects. It produced a statistically significant increase in reports of rape and in indictments, convictions and incarcerations for rape. It also resulted in a significant increase in the indictment rate and the average sentence. We feel these results can be attributed both to the comprehensiveness of the Michigan reform and to the professionalism of the Detroit criminal justice system.

Unlike the changes in either Illinois or Pennsylvania, the Michigan reform broadened the acts which could be charged as rape and brought additional groups under the protection of the law. The new statute also clearly spelled out the circumstances defining each crime. Increases in numbers of reports, indictments, convictions, and incarcerations probably reflect this greater inclusiveness. We tried to limit our analysis to equivalent crimes by comparing rape, sodomy and gross indecency cases (before the reform) with first and third degree criminal sexual conduct cases (after the reform). Nevertheless, the crimes we examine are not perfectly equivalent; for example, some crimes with child victims would not be charged as "rape" prior to the reform, but might be charged as first or third degree criminal sexual conduct after the reform. Our results suggest, then, that the Michigan reform resulted in more crimes being charged and prosecuted as forcible rape.

We also found that the reforms resulted in a significant increase in the indictment rate. This is an important finding. We noted above that reports of rape increased in the post-reform era. While we can only speculate, presumably some of these additional cases were the types of cases victims were reluctant to report prior to the passage of reform legislation: cases involving acquaintances, cases involving sexually promiscuous women or men, cases with little or no corroborating evidence, and so on. Given this assumption, we might have expected the indictment rate to decrease. The fact that it increased suggests that prosecutors are more willing to file charges in borderline cases.

This greater willingness to file charges can be attributed both to the evidentiary reforms and to the fact that definitions of the various degrees of criminal sexual conduct are much clearer than the old definition of rape. The new Michigan law provides clear guidelines for prosecutors to follow in screening rape cases. It carefully defines the elements of each offense, specifies the circumstances which constitute coercion, and lists the situations in which no showing of force is required. The judges, prosecutors and defense attorneys we interviewed in Detroit all 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 which constitute sexual assault, the circumstances which imply coercion and nonconsent and the types of evidence which are unnecessary or irrelevant, the Michigan law may have dissuaded police from unfounding complaints and prosecutors from rejecting charges.

Contrary to reformers expectations, the Michigan law did not result in an increase in the conviction rate. However, given that the indictment rate increased, the fact that the conviction rate did not decline following the changes is an important finding. If we assume that at least some of the cases charged following the reform would have been rejected before the reform, we might have expected the conviction rate to fall. It is particularly interesting that the rate of conviction for the original charge not only did not decline but increased substantially following the reform; 35.6 percent of the defendants were convicted of the original charge after the legal changes, compared to only 19.5 percent before the changes. Although these differences did not show up as a statistically significant effect of the reform legislation, they suggest that there is less plea bargaining in the post-reform period. Other data confirm this. Guilty pleas declined from 50.5 percent to 43.1 percent and the percent of guilty pleas where the severity of the charge was reduced declined form 84.4 percent to 51.6 percent. Since plea bargaining tends to produce more lenient sentences, these results also provide an explanation for the increase in the average sentence following the reform; as guilty pleas declined, sentences increased.

In the post-reform period, then, a greater percentage of rape defendants are being charged, fewer of them are pleading guilty, more of them are being convicted of the original charge and sentences are more severe. These clearly are significant results. They indicate that the reform legislation enacted in Michigan produced the types of results hoped for by reformers.

Although we feel that the effect of the Michigan reform can be attributed primarily to the comprehensiveness of the legal changes, we also believe it was affected by the professionalism of the Detroit court system. Detroit differs from the other five jurisdictions in a number of important ways. First, both the chief judge of Detroit's Recorder's Court and the docket control center of the State Court Administrative Office exercise administrative control over the judges. Although the system is designed to monitor case processing, there appear to be spillover effects on the overall operation of the court system. We suspect that compliance with the rape shield laws is higher in Detroit in part because of this administrative oversight. Second, Detroit does not have a public defender system; instead, criminal cases are assigned to a private defender corporation or to private attorneys. In addition, defense attorneys are not assigned to courtrooms, as they are in other jurisdictions. The major actors in the Detroit courtroom workgroup, then, are judges and prosecutors; defense attorneys play a less important role. This arrangement can have obvious effects on case processing.

The results discussed thus far indicate that reformers had unrealistic expectations about the ability of rape reform legislation to affect the outcomes of rape cases. Given the nature of the criminal justice system, in fact, it is somewhat surprising that the reforms produced results in any of the jurisdictions. The criminal justice system is dominated by a courtroom workgroup composed of autonomous decision makers who possess large amounts of discretion and who are primarily motivated by a desire to process cases as quickly and as efficiently as possible. In a system like this, "formal rules, evidence requirements, statutory definitions of offenses, and jury instructions may be largely

irrelevant to the way decisions are made" (Feild and Bienen, 1980: 183). In order to affect this system, a reform must limit officials' discretion and/or provide incentives sufficient to overcome their motivations. As we have demonstrated, the reforms in five of the six jurisdictions were not strong or comprehensive enough to accomplish this.

Implications of the Results

We have shown that the ability of rape reform legislation to affect case processing is limited. Evidentiary reforms alone cannot produce the types of results anticipated by reformers. Only in Detroit, with its comprehensive legal changes and professional court system, did we find consistent significant effects on the processing and disposition of rape cases.

This is not to say, however, that the rape reform laws have produced no effects in the other five jurisdictions. We indicated that the laws have had an important impact on the attitudes of criminal justice officials. Judges, prosecutors and defense attorneys in each jurisdiction stressed that rape cases are taken more seriously and that rape victims are treated more humanely as a result of the legal changes. As reformers had hoped, the laws appear to have altered officials' perceptions of rape cases and rape victims.

These educative effects clearly are important. 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. The rape reform movement sought to refute these offensive common law principles and thus to dissuade officials from making decisions based on the victim's character or behavior. Interviews in six very different jurisdictions indicate that the reforms have achieved these goals. Criminal justice officials in all of the jurisdictions, and particularly in the jurisdictions with the stronger evidentiary changes, are convinced that the outcomes of sexual assault cases should depend, not on the behavior or reputation of the victim, but on the unlawful behavior of the offender.

Our findings suggest that the rape shield laws have had an especially important effect on the attitudes and behavior of criminal justice officials. The purpose of the rape shield law was to prevent harassment of the rape victim by a defense attorney bent on proving that her past sexual conduct is indicative of consent. Our interviews revealed that the shield laws do protect the rape victim by precluding the use of irrelevant sexual history evidence. A defense attorney in Houston, for example, described the situation before the adoption of the Texas rape shield law as "a nightmare," explaining that "a lot of women were worked over and made to feel like whores because they were assaulted. This just doesn't happen anymore." One Detroit judge commented that before the legal change "I wouldn't have let my daughter report and testify."

Criminal justice officials in all six jurisdictions agreed that evidence of the victim's prior sexual encounters with men she met at singles bars should not and would not be admitted. If this is indeed the classic example of the type of evidence shield laws were designed to prohibit, and if we assume this type of evidence was admitted at least occasionally in the pre-reform era, then the reforms obviously have been effective. As a judge in Houston noted, "The days when rape victims are blasted by defense attorneys are gone."

There is additional evidence that the shield laws have been effective. Judges in every jurisdiction stated that defense attorneys don't even attempt to introduce the more questionable types 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." Even in Houston, with its weak rape shield law, the consensus among judges was that defense attorneys don't request in camera hearings to determine the relevance of sexual history

evidence because "they realize it just wouldn't do any good."

The shield laws also prevent the jury from inadvertently hearing irrelevant sexual history evidence. In the pre-reform period a defense attorney could prejudice the jury by simply asking a question about the victim's prior sexual conduct. Even if the prosecutor objected and the judge sustained the objection, the damage was done. The existence of the shield law apparently discourages defense attorneys from using this tactic. According to one judge, "The mere availability of the rule heads off the damages."

These conclusions should please advocates of rape reform legislation. They indicate that the legal changes have had important effects on the attitudes of criminal justice officials. They suggest that in the post-reform era rape victims will be treated like other victims of crime. They will not be forced to prove they are deserving of protection under the law. They will not themselves be placed on trial.

TABLE 1
COMPARISON OF RAPE LAWS IN SIX STATES

State	Definition of Offenses	Shield Law	Corroboration Requirement	Resistance Requirement
Michigan	Prior to 4-1-75--traditional carnal knowledge statute; rape and sodomy After 4-1-75--1st dg. criminal sexual conduct (aggravated penetration), 2nd degree criminal sexual conduct aggravated contact), 3rd degree criminal sexual conduct (penetration), 4th degree criminal sexual conduct (contact)	4-1-75	Eliminated 4-1-75	Eliminated 4-1-75
Illinois	Prior to 7-1-84--rape, deviate sexual intercourse; emphasis on victim's nonconsent After 7-1-84--criminal sexual assault (rape and deviate sexual intercourse), aggravated criminal sexual assault, criminal sexual abuse (contact), and aggravated criminal sexual abuse; emphasis on degree of force used by accused	1-4-78	Still appears to be required	Eliminated 7-1-84
Pennsylvania	1970 to 1973--traditional carnal knowledge statute 1973--Model Penal Code statute with rape, involuntary deviate sexual intercourse	6-17-76	Repealed 6-17-76	Repealed 6-17-76
Texas	Prior to 9-1-83--rape, aggravated rape, sexual abuse, aggravated sexual abuse; emphasis on resistance of victim After 9-1-83--sexual assault (rape and sexual abuse), aggravated sexual assault (aggravated rape and aggravated sexual abuse); emphasis on victim's nonconsent	9-1-75	Not required if victim informed any person within six months 9-1-83	Definition of consent still refers to resistance
Georgia	Traditional carnal knowledge statute; rape and sodomy	7-1-76	Repealed 7-1-78	Not specifically; law says carnal knowledge must be against her will
D.C.	Traditional carnal knowledge statute; rape and sodomy	9-2-77 (Case law)	Repealed 5-3-76 (Case law)	Law states carnal knowledge must be against her will

TABLE 2
COMPARISON OF RAPE SHIELD LAWS IN SIX STATES

State	Date	Types of Evidence Deemed Inadmissible For Any Purpose	Types of Evidence Admissible Upon Showing of Relevance	Types of Evidence Presumed Relevant-- No Proof of Relevance Required	Procedure For Determining Relevance
Michigan	4-1-75	Specific instances of sexual conduct; opinion evidence; reputation evidence	Conduct with defendant; specific acts to show source of semen, pregnancy, disease	None	written motion and offer of proof within 10 days of arraignment; in camera hearing to ensure that prejudicial nature does not outweigh probative value
Illinois	1-4-78	Prior sexual activity or reputation	Conduct with defendant	None	In camera hearing to determine if defense has evidence to impeach victim in event prior sexual conduct with defendant denied
Pennsylvania	6-17-76	Specific instances of sexual conduct; opinion evidence; reputation evidence	Conduct with defendant where consent is an issue	None	Written motion and offer of proof; in camera hearing to determine if evidence admissible pursuant to rules of evidence
Texas	9-1-75	None	Specific acts of sexual activity; opinion evidence; reputation evidence	Prior felony convictions involving sexual conduct; evidence of promiscuous conduct of a child betwee 14 and 17	In camera hearing to ensure that prejudicial nature does not outweigh probative value and to limit questions
Georgia	7-1-76	None	Past sexual behavior, including marital history, mode of dress, reputation for promiscuity, non-chastity, or sexual mores contrary to community standards; past sexual behavior involving the accused	None	In camera hearing; to be admitted evidence must involve accused or support inference that accused could reasonably have believed that victim consented
D.C.	No shield but case law [S.R. McLean v. U.S. (U.S. App. D.C. 377 A.2d 74 (1977))] limiting admissibilty of evidence. Reputation evidence inadmissible except in unusual cases where probative value outweigh prejudicial effect; evidence of prior sexual conduct with third parties inadmissible; evidence of prior sexual conduct with defendant admissible to rebut government's evidence that victim did not consent on the particular occasion.				

TABLE 3

OFFENSES CODED

Atlanta, Georgia

Rape
Aggravated assault with intent to rape
Aggravated sodomy

Chicago, Illinois

Rape -- aggravated criminal sexual assault, criminal sexual assault
Deviate sexual assault -- aggravated criminal sexual assault, criminal sexual assault

Detroit, Michigan

Rape -- first or third degree criminal sexual conduct
Sodomy -- first or third degree criminal sexual conduct
Gross indecency -- first or third degree criminal sexual conduct
Second degree criminal sexual conduct
Attempted rape
Attempted first or second degree criminal sexual conduct
Attempted gross indecency
Assault with intent to commit rape or sodomy
Assault with intent to commit criminal sexual conduct

Houston, Texas

Rape
Aggravated rape
Sexual abuse
Aggravated sexual abuse
Sodomy
Attempted aggravated rape
Assault with intent to rape
Burglary with intent to rape

Philadelphia, Pennsylvania

Rape
Attempted rape
Involuntary deviate sexual intercourse
Attempted involuntary deviate sexual intercourse
Assault and burglary with intent to ravish
Burglary with intent to ravish and rape
Assault with intent to commit sodomy

Washington, D.C.

Rape
Rape while armed
Sodomy
Sodomy while armed
Assault with intent to rape while armed
Assault with intent to commit sodomy while armed
Assault with intent to rape

TABLE 4

TIME-SERIES DATA

SUMMARY OF IMPACT ANALYSIS

	DETROIT	CHICAGO		PHILADELPHIA	HOUSTON	ATLANTA		WASHINGTON, D.C.	
Variable		SHIELD	DEF			SHIELD	CORR	SHIELD	CORR
Rape									
Reports	*	—	—	—	*	—	—	—	—
Indictments	*	—	—	—	—	—	—	—	—
% Indicted	*	—	—	—	—	—	—	—	—
Convictions	*	—	—	—	—	—	—	—	—
% Convicted	—	—	—	—	—	—	—	—	—
Convictions on Original Charge	*	—	—	—	—	—	—	—	—
% Convicted on Original Charge	—	—	—	—	—	—	—	—	—
Incarcerations	*	—	—	—	—	—	—	—	—
% Incarcerated	—	—	---	*	—	—	—	—	—
Average Sentence	*	*	*	*	*	—	—	*	*
ALL SEX OFFENSES									
Indictments	*	—	—	—	—	—	—	—	—
Convictions	*	—	—	*	—	—	—	—	—
% Convicted	—	—	—	—	—	—	—	—	—
Incarcerations	*	—	—	—	—	—	—	—	—
% Incarcerated	—	—	—	—	—	—	—	—	—
Average Sentence	*	*	—	—	—	—	—	*	*

TABLE 5

DETROIT

SUMMARY OF INTERVENTION MODELS

Variable	Arima Model for Noise Component	Arima Parameters	Prepost
RAPE			
Reports	$(0,1,1)(0,1,1)_{12}$	MA1 = .62*** MA2 = .68***	26.53**
Indictments	(1,1,1)	MA1 = -.74*** AR1 = -.17*	8.62*
% Indicted	(0,0,3)	MA1 = -.13* MA2 = -.22*** MA3 = -.31***	.18***
Convictions	(1,1,1)	MA1 = .82*** AR1 = -.21**	5.30*
% Convicted	(0,0,0)		-2.26
Convictions on Original Charge	(0,1,1)	MA1 = .88***	5.67***
% Convicted on Original Charge	(0,1,1)	MA1 = .85***	.07
Incarcerations	(0,0,0)		9.43***
% Incarcerated	(0,1,1)	MA1 = .82***	-.15
Average Sentence	(0,0,1)	MA1 = -.18**	62.54***
ALL SEX OFFENSES			
Indictments	(0,1,1)	MA1 = .80***	11.44**
Convictions	(0,1,1)	MA1 = .88***	7.20**
% Convicted	(0,0,0)		-.56
Incarcerations	(0,0,0)		9.78***
% Incarcerations	(0,1,1)	MA1 = .82***	-.13
Average Sentence	(0,0,1)	MA1 = -.17**	72.4***

***p < .01 **p < .05 *p < .10

TABLE 6

CHICAGO

SUMMARY OF INTERVENTION MODELS

Variable	Arima Model for Noise Component	Arima Parameters	Prepost-Shield	Prepost-Definition
RAPE				
Reports	(2,0,0)	AR1 = .18** AR2 = .26***	.95	
Indictments	(0,1,1)	MA1 = .68***	-6.18	
% Indicted	(0,1,1)	MA1 = .84***	-.42	
Convictions	(0,1,1)	MA1 = .75***	-1.10	
% Convicted	(0,0,0)		-.16	
Convictions on Original Charge	(0,1,1)	MA1 = .79***	.75	
% Convicted on Original Charge	(0,0,0)		1.03	
Incarcerations	(0,1,1)	MA1 = .79***	.89	
% Incarcerated[a]				
Average Sentence	(0,0,0)		47.67***	
ALL SEX OFFENSES				
Indictments (logged)	(0,1,1)	MA1 = .71***	-6.68	.24
Convictions	(0,1,1)	MA1 = .76***	-1.43	6.63
% Convicted	(0,0,0)		-.27	Num =11.65** Den = -.79**
Incarcerations	(0,1,1)	MA1 = .77***	-.04	2.96
% Incarcerated	(0,0,0)		.01	-9.04***
Average Sentence	(0,0,2)	MA1 = NS MA2 = -.16**	35.74***	-4.82

[a]Time series analysis not performed because almost all values were 100%

TABLE 7

PHILADELPHIA

SUMMARY OF INTERVENTION MODELS

Variable	Arima Model for Noise Component	Arima Parameters	Prepost
RAPE			
Reports	(1,1,1)	MA1 = .81*** AR1 = -.30***	1.65
Indictments	(0,1,1)	MA1 = .88**	1.42
% Indicted	(0,0,0)		.04
Convictions	(0,1,1)	MA1 = -.91***	-.23
% Convicted	(0,0,0)		-1.05
Convictions on Original Charge	(0,1,1)	MA1 = .89***	1.43
% Convicted on Original Charge (logged)	(0,0,0)		.19
Incarcerations	(0,1,1)	MA1 = .86***	-.68
% Incarcerated	(0,0,0)		.07***
Average Sentence	(0,0,0)		10.35**
ALL SEX OFFENSES			
Indictments	(0,1,1)	MA1 = .83***	7.17
Convictions	(0,1,1)	MA1 = .85***	-.06
% Convicted (logged)	(0,0,0)		.05*
Incarcerations	(0,1,1)	MA1 = .88***	-.27
% Incarcerated	(0,1,1)	MA1 = .88***	.04
Average Sentence	$(0,0,0)(0,0,1)_{12}$	MA1 = -.22***	5.9

***p < .01 **p < .05 *p < .10

TABLE 8

PHILADELPHIA

SUMMARY OF INTERVENTION MODELS
(WITHOUT DIFFERENCING)

	Correct Intervention			Intervention Moved Back Two Years		
	Arima Model for Noise Component	Arima Parameters	Intervention	Arima Model for Noise Component	Arima Parameters	Intervention
RAPE						
Reports	(3,0,0)	AR1 = NS AR2 = .35*** AR3 = .11*	13.7***	(2,0,0)	AR1 = NS AR2 = .27***	20.07***
Indictments	(3,0,0)	AR1 = .16** AR2 = .17** AR3 = .22***	5.14**	(2,0,0)	AR1 = NS AR2 = .13*	8.55***
Convictions	(1,0,0)	AR1 = .21***	2.82***	(0,0,0)		4.77***
Convictions on Original Charge	(2,0,0)	AR1 = .20*** AR2 = .18**	1.82**	(0,0,0)		3.76***
Incarcerations	----[a]			(1,0,0)	AR1 = .13*	3.77***
ALL SEX OFFENSES						
Indictments	(3,0,0)	AR1 = .25*** AR2 = .14* AR3 = .23***	8.70***	(1,0,0)	AR1 = .19***	14.38***
Convictions	(2,0,0)	AR1 = .25*** AR2 = .15**	5.16***	(1,0,0)	AR1 = .18**	7.12***
Incarcerations	(3,0,0)	AR1 = .25*** AR2 = NS AR3 = .20***	4.82***	(0,0,0)		7.52***

[a]could not be modelled satisfactorily without differencing

TABLE 9

WASHINGTON, D.C.

SUMMARY OF INTERVENTION MODELS

Variable	Arima Model for Noise Component	Arima Parameters	Prepost-Shield	Prepost-Corroboration
Reports	$(0,0,0)(0,1,1)_{12}$	MA1 = .73***	-.80	-5.07**
Indictments (logged)	(0,0,0)		-.24	-.21
% Indicted	(0,0,1)	MA = .16**	.002	-.003
Convictions	(0,0,0)		-1.13**	-1.35**
% Convicted	(0,0,0)		.02	.04
Convictions on Original Charge	(0,0,0)		-.64	-.13
% Convicted on Original Charge	(0,0,0)		.01	.06
Incarcerations	(0,0,1)	MA1 = -.16**	-.16	-.01
% Incarcerated	(0,0,1)	MA1 = -.17**	.06	.08
Average Sentence				
ALL SEX OFFENSES				
Indictments (logged)	(0,0,0)		-.15	-.20*
Convictions (logged)	(0,0,0)		-.02	.00
% Convicted	(0,0,0)		.03	.05
Incarcerations (logged)	(0,0,0)		-.04	-.05
% Incarcerated	(0,1,1)	MA1 = .77***	.19	-.09
Average Sentence	(0,0,0)		69.6**	116.4***

***p < .01 **p < .05 *p < .10

TABLE 10

ATLANTA

SUMMARY OF INTERVENTION MODELS

Variables	Arima Model for Noise Component	Arima Parameters	Prepost-Shield	Prepost-Corroboration	Prepost-Moved Back Two Years
RAPE					
Reports	$(0,1,1)(0,1,1)_{12}$	MA1 = .85*** MA2 = .89***	-3.94	.24	----
Indictments	(0,0,0)		3.77***	3.68***	3.28***
% Indicted (logged)	(0,0,1)	MA1 = -.13*	-.09	-.05	----
Convictions	(0,0,0)		Num = .21** Den = .94***	3.37***	Num = .10** Den = .98***
% Convicted (logged)	(0,0,0)		.01***	.01**	.01***
Convictions on Original Charge	(0,0,0)		1.11***	1.05***	1.07***
% Convicted on Original Charge	(0,0,0)		-.05	-.07**	-.07**[a]
Incarcerations	(0,0,0)		1.13***	1.15***	1.11***
% Incarcerated	(0,0,0)		-.003	.01	----
Average Sentence	(0,0,2)	MA1 = NS MA2 = -.14**	29.17	42.74*	42.86*[a]
ALL SEX OFFENSES					
Indictments	(0,0,0)		4.12***	.76	3.29***
Convictions	(0,0,0)		Num = .34* Den = .92***	.87	3.31***
% Convicted (logged)	(0,0,0)		.46**	.19	.58***
Incarcerations	(0,0,0)		2.75***	.67	2.34***
% Incarcerated	(0,0,0)		8.07***	1.78	7.14**
Average Sentence	(0,0,0)		14.34	-35.00	----

***p < .01 ** p < .05 * p < .10

[a]Moved back one year from corroboration intervention

TABLE 11

HOUSTON

SUMMARY OF INTERVENTION MODELS

Variable	Arima Model for Noise Component	Arima Parameters	Prepost	Prepost-Two Years
RAPE				
Reports	(1,0,0)	AR1 = .36***	Num =1.30*** Den = .99***	Num =1.29*** Den = .99***
Indictments	(0,0,0)		2.88***	-.03
% Indicted	(0,0,0)		-.14***	
Convictions	(0,0,0)		3.00***	-.19
% Convicted	(0,0,0)		.12***	-.01
Convictions on Original Charge	(0,0,0)		2.09***	.09
% Convicted on Original Charge	(0,0,1)	MA1 = .15*	.09***	-.04
Incarcerations	(0,0,0)		1.97***	.68
% Incarcerated	(0,0,2)	MA1 = NS MA2 = .21***	.06*	.04
Average Sentence	(0,0,0)		73.63***	52.93*
ALL SEX OFFENSES				
Indictments	(0,0,0) logged		.05	.02
Convictions	(0,0,0)		3.04***	.95
% Convicted	(0,0,0)		.11***	0.00
Incarcerations	(0,0,3)	MA1 = NS MA2 = -.18* MA3 = -.23***	3.16***	.80
% Incarcerated	(0,0,0)		.09**	.08*
Average Sentence	(0,0,0)		64.31***	24.99

***p < .01 **p < .05 *p < .10

TABLE 12
Assessments of the Importance of Evidence
In Sexual Assault Cases

	Prosecutors--File Charges[a] (N=51)	Influence on Jury's Decision to Convict: All Respondents[b] (N=152)	Detroit (N=25)	Chicago (N=32)	Philadelphia (N=26)	Houston (N=29)	Atlanta (N=26)	D.C. (N=25)
LEGAL EVIDENCE								
Victim can identify suspect	3.38	3.62	3.43	3.36	3.79	3.92	3.60	3.62
Victim reported promptly	2.88	3.23	3.17	3.13	3.33	3.15	3.56	3.04
Physical evidence	2.74	3.03	2.91	2.87	3.12	3.08	3.20	3.00
Documented physical injury	2.52	2.91	2.91	3.00	2.79	2.77	3.04	2.92
No inconsistencies in victim's story	2.61	2.89	2.87	2.86	3.00	2.85	2.88	2.88
Corroborating witnesses	2.44	2.70	2.87	2.73	2.38	2.50	3.00	2.71
Evidence that victim resisted	2.08	2.67	2.52	2.52	2.42	2.75	3.08	2.75
Suspect used dangerous weapon	2.18	2.54	2.65	2.57	2.29	2.50	2.84	2.38
EXTRALEGAL EVIDENCE								
Victim did not use alcohol or drugs at time of incident	2.12	2.51	2.56	2.50	2.46	2.35	2.60	2.58
Suspect and victim had no previous sexual relationship	2.18	2.46	2.35	2.34	2.38	2.65	2.56	2.46
Victim has no prior felony convictions	1.96	2.29	2.13	2.33	2.29	2.15	2.64	2.17
Suspect and victim were strangers	1.78	2.28	2.35	2.21	2.17	2.46	2.40	2.12
Victim does not have a reputation for sexual promiscuity	1.72	2.15	2.09	1.86	1.78	2.38	2.42	2.38

[a]Prosecutors only were asked to rate the importance of the various types of evidence to the decision to file charges.
[b]Includes 58 prosecuting attorneys, 50 defense attorneys and 44 judges.
[c]Mean responses where 1=irrelevant, 2=helpful, 3=important, and 4=essential.

TABLE 13
Attitudes Toward Introducing Evidence
Of the Victim's Past Sexual Behavior

	All Respondents (N=162)	Detroit (N=25)	Chicago (N=32)	Phila-delphia (N=26)	D.C. (N=25)	Atlanta (N=26)	Houston (N=28)
Case #1							
Prior sex with men victim met at singles bars							
Should be admitted[a]	.11	.05	.10	.17	.15	.13	.12
Would be admitted[b]	1.55	1.36	1.31	1.54	1.19	1.84	2.07
Case #2							
Prior sex with men similar to the defendant							
Should be admitted	.34	.05	.32	.35	.46	.48	.33*[c]
Would be admitted	2.26	1.68	1.91	2.23	2.21	2.73	2.82**
Case #3							
Prior sex with groups of men							
Should be admitted	.49	.22	.50	.42	.73	.50	.56**
Would be admitted	2.55	2.00	2.48	2.44	2.71	2.77	2.89
Case #4							
Threat against brother-in-law							
Should be admitted	.74	.67	.66	.75	.88	.78	.67
Would be admitted	3.50	3.25	3.12	3.83	3.80	3.32	3.82
Case #5							
Semen test results							
Should be admitted	.76	.79	.59	.72	.96	.78	.71
Would be admitted	3.77	3.74	3.34	4.05	4.50	3.16	4.00**
Case #6							
Prior sex with defendant							
Should be admitted	.79	.73	.72	.69	.88	.88	.89
Would be admitted	4.16	3.68	4.28	4.23	4.36	4.23	4.14

[a]Respondents were asked whether they personally believed that the evidence should be admitted. Yes=1 and no=0.
[b]Respondents were asked how likely it was that the evidence would be admitted. 1=definitely would not be admitted, 2=probably would not be admitted, 3=50/50 chance, 4=probably would be admitted, and 5=definitely would be admitted.
[c]* $p < .05$; ** $p < .01$ for differences among the six jurisdictions.

TABLE 14

Adjusted Means[a]--Attitudes Toward Introducing Evidence Of the Victim's Past Sexual Behavior

	Detroit (N=25)	Chicago (N=32)	Phila-delphia (N=26)	D.C. (N=25)	Atlanta (N=26)	Houston (N=28)
Case #1						
Prior sex with men victim met at singles bars						
Should be admitted[b]	.13	.15	.16	.22	.18	.19
Would be admitted[c]	0.91	0.89	1.14	0.81	1.39	1.60
Case #2						
Prior sex with men similar to defendant						
Should be admitted	.14	.32	.29	.46	.46	.37*[b]
Would be admitted	1.81	1.92	2.44	2.18	2.59	2.64*
Case #3						
Prior sex with groups of men						
Should be admitted	.27	.49	.37	.77	.47	.55**
Would be admitted	2.16	2.48	2.70	2.54	2.81	2.73
Case #4						
Threat against brother-in-law						
Should be admitted	.70	.65	.73	.90	.77	.72
Would be admitted	3.19	3.24	3.94	3.63	3.31	3.72
Case #5						
Semen test results						
Should be admitted	.78	.71	.72	1.00	.78	.73
Would be admitted	3.61	3.67	4.15	4.12	3.10	3.98**
Case #6						
Prior sex with defendant						
Should be admitted	.73	.71	.68	.87	.90	.86
Would be admitted	3.72	4.41	4.44	4.22	4.09	4.02

[a]These adjusted figures were computed in the following way. We created dummy variables for five of the six jurisdictions (Atlanta, Chicago, Detroit, D.C. and Philadelphia), for males,and for two of the three occupational groups (prosecutors and defense attorneys). We then ran regressions on each of the dependent variables controlling for these dummy variables and for the number of years the respondent had been a prosecutor, defense attorney, or judge in the jurisdiction. For the analysis of the respondent's belief that the evidence would be admitted we also controlled for the respondent's belief about whether or not the evidence should be admitted. The adjusted figures were calculated using the following formulas:

$b_1 = -[(b_2)(prop_2) + (b_3 prop_3) + (b_4)(prop_4) + (b_5)(prop_5) + (b_6)(prop_6)]$

$adjmean_1 = M + b_1$

$adjmean_2 = adjmean^1 + b_2 \quad . \ . \ . \quad adjmean_6 = adjmean^1 + b_6$

Where:

b_1 =the adjusted unstandardized regression weight (b weight) for the omitted category (Houston);

b_2, b_3, b_4, b_5, b_6 = the b weights for the dummy jurisdictional variables in the regression;

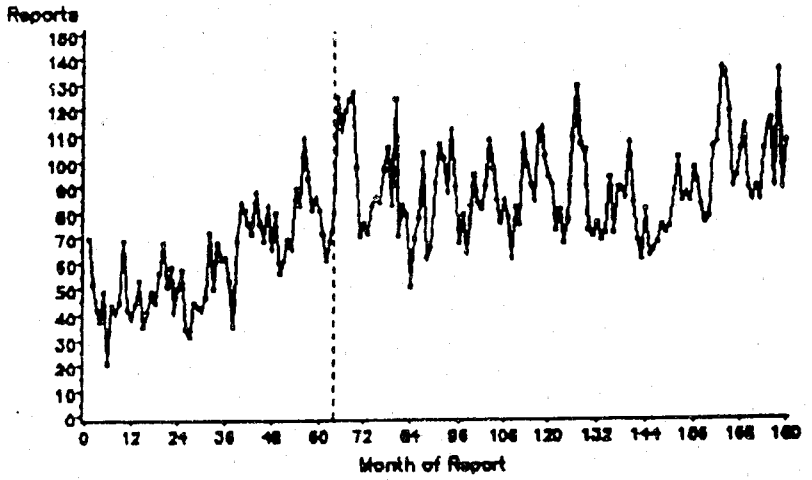
$prop_2$, $prop_3$, $prop_4$, $prop_5$, $prop_6$ = the means of the dummy variables (or the proportion of respondents scoring 1 on the dummy variable;

M= the mean of the dependent variables;

$adjmean_1$, $adjmean_2$, $adjmean_3$, $adjmean_4$, $adjmean_5$, $adjmean_6$ = the adjusted means for each jurisdiction.

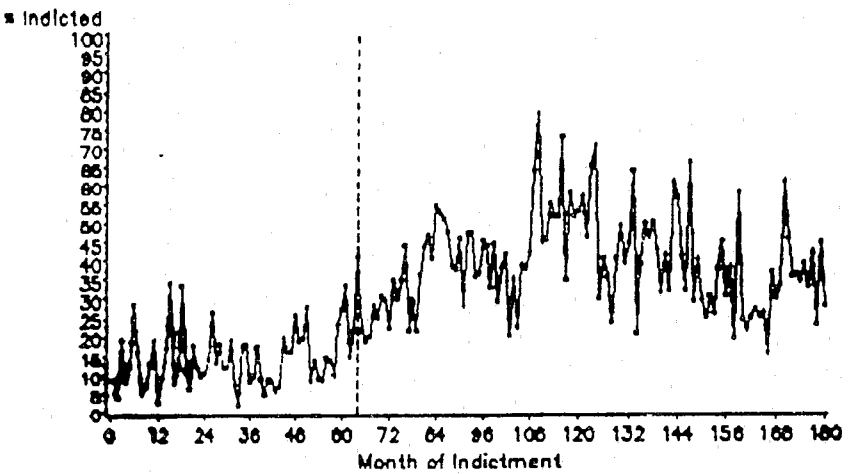
[b]* $p < .05$; ** $p < .01$ for differences among the jurisdictions.

FIGURE 1a
Monthly Reports of Rape
Detroit, Michigan
1970–1984



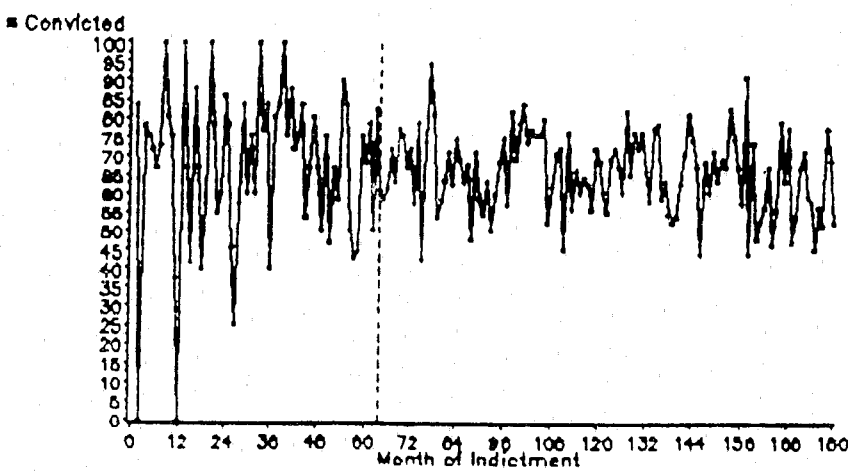
Broken line indicates date reforms implemented

FIGURE 1b
Percent Indicted Monthly for Rape, Sodomy, Gross Indecency,
1st or 3rd Degree Criminal Sexual Conduct
Detroit, Michigan
1970–1984



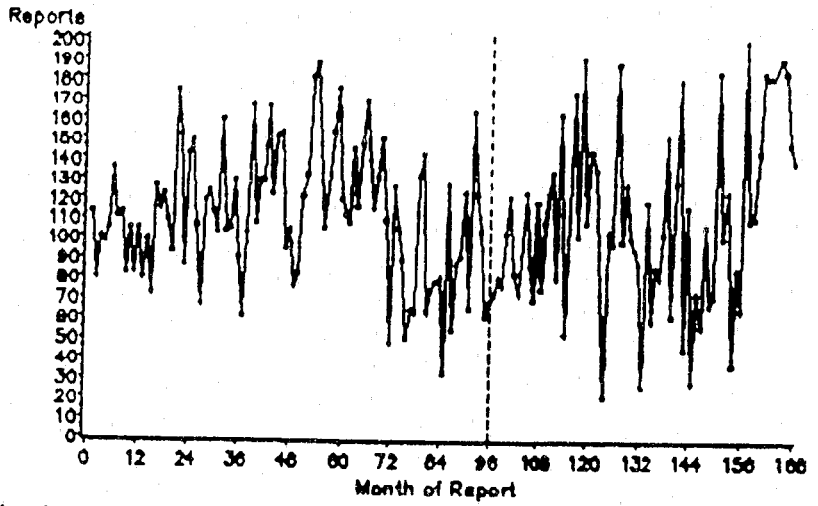
Broken line indicates date reforms implemented

FIGURE 1c
Percent Convicted Monthly for Rape, Sodomy, Gross Indecency,
1st or 3rd Degree Criminal Sexual Conduct
Detroit, Michigan
1970–1984



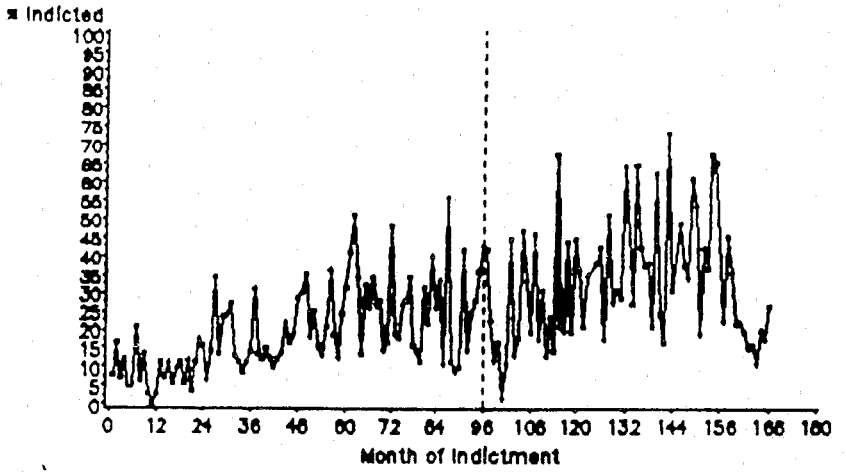
Broken line indicates date reforms implemented

FIGURE 2a
Monthly Reports of Rape
Chicago, Illinois
1970–1983



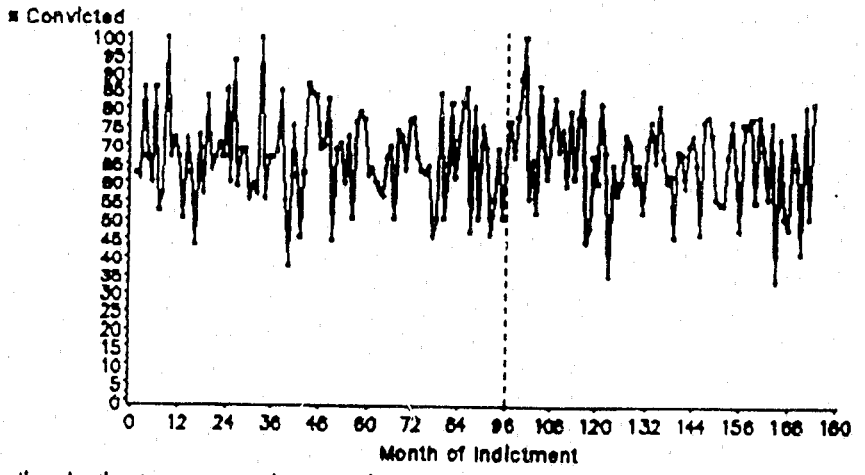
Broken line indicates date shield law implemented

FIGURE 2b
Percent Indicted Monthly for Rape
Chicago, Illinois
1970–1984*



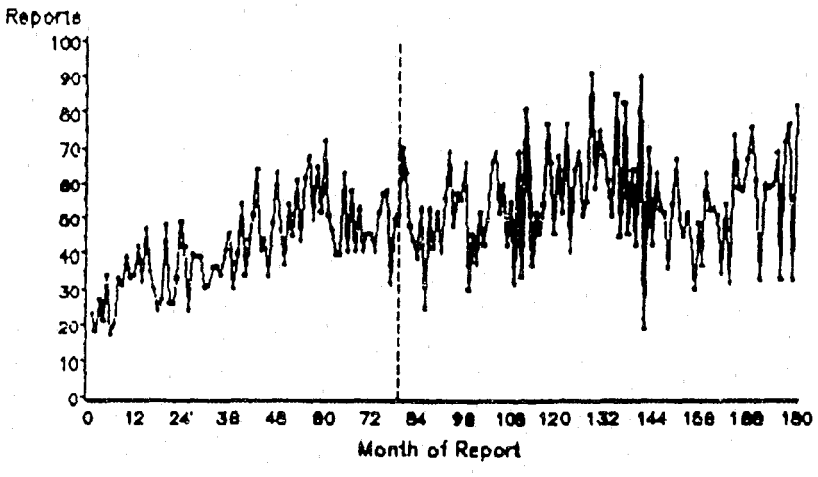
Broken line indicates date shield law implemented
*Through 6–31–84

FIGURE 2c
Percent Convicted Monthly for Rape
Chicago, Illinois
1970–1984*



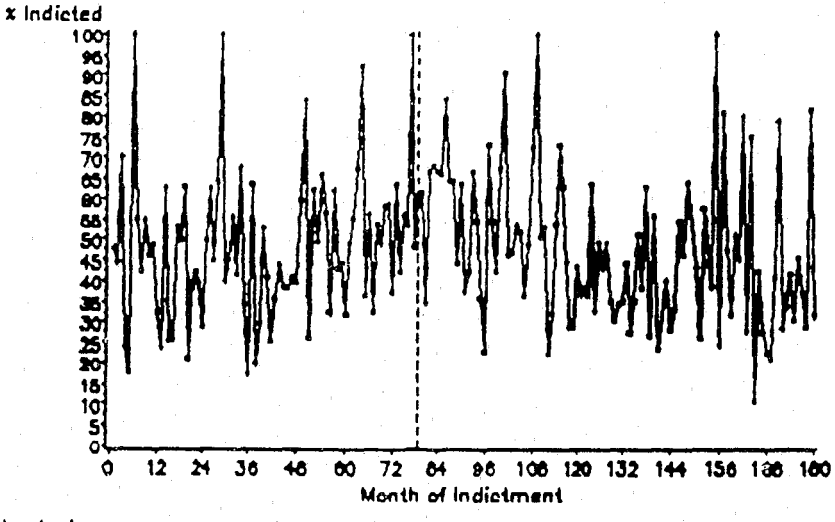
Broken line indicates date shield law implemented
*Through 6–31–84

FIGURE 3a
Monthly Reports of Rape
Philadelphia, Pennsylvania
1970–1984



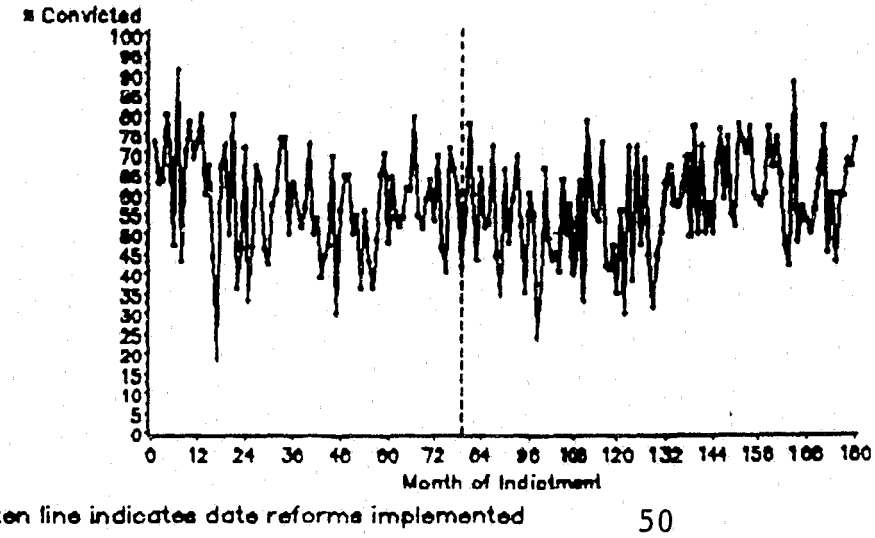
Broken line indicates date reforms implemented

FIGURE 3b
Percent Indicted for Rape by Month
Philadelphia, Pennsylvania
1970–1984



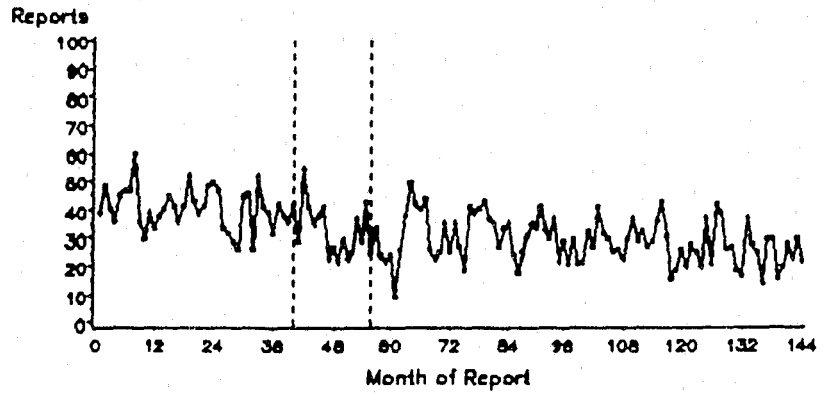
Broken line indicates date reforms implemented

FIGURE 3c
Percent Convicted for Rape by Month
Philadelphia, Pennsylvania
1970–1984



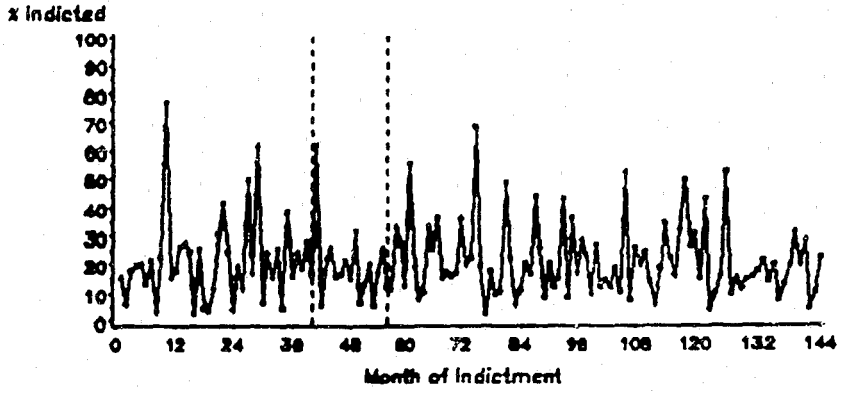
Broken line indicates date reforms implemented

FIGURE 4a
Monthly Reports of Rape
Washington, D.C.
1973–1984



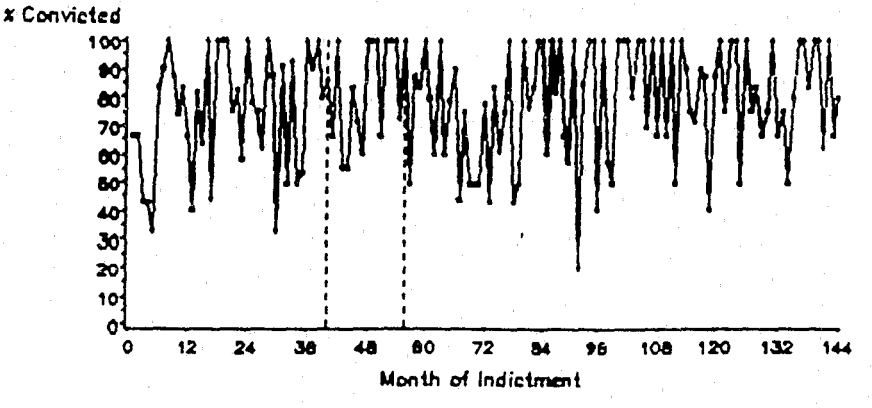
Broken lines indicate dates of court rulings
Month 40 = Corroboration
Month 56 = Shield

FIGURE 4b
Percent Indicted for Rape by Month
Washington, D.C.
1973–1984



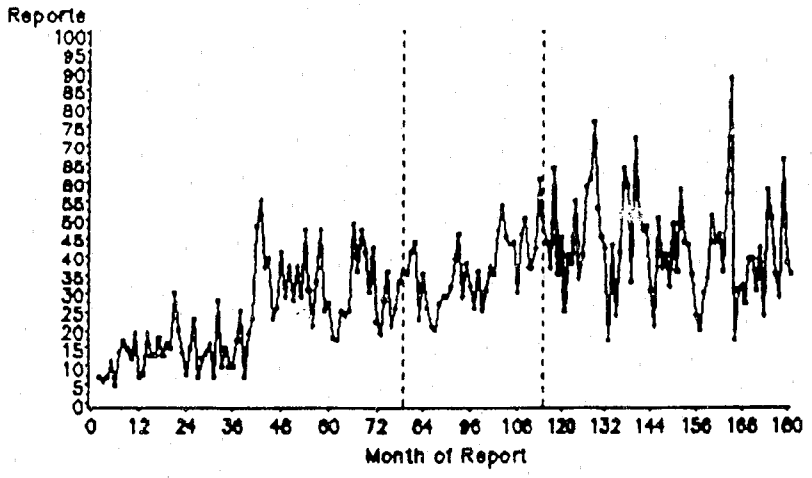
Broken lines indicate dates of court rulings
Month 40 = Corroboration
Month 56 = Shield

FIGURE 4c
Percent Convicted for Rape by Month
Washington, D.C.
1973–1984



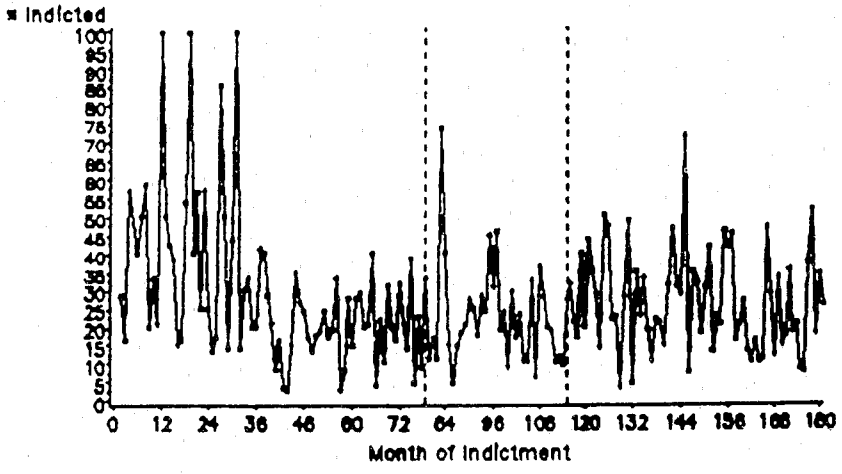
Broken lines indicate dates of court rulings
Month 40 = Corroboration
Month 56 = Shield

FIGURE 5a
Monthly Reports of Rape
Atlanta, Georgia
1970–1984



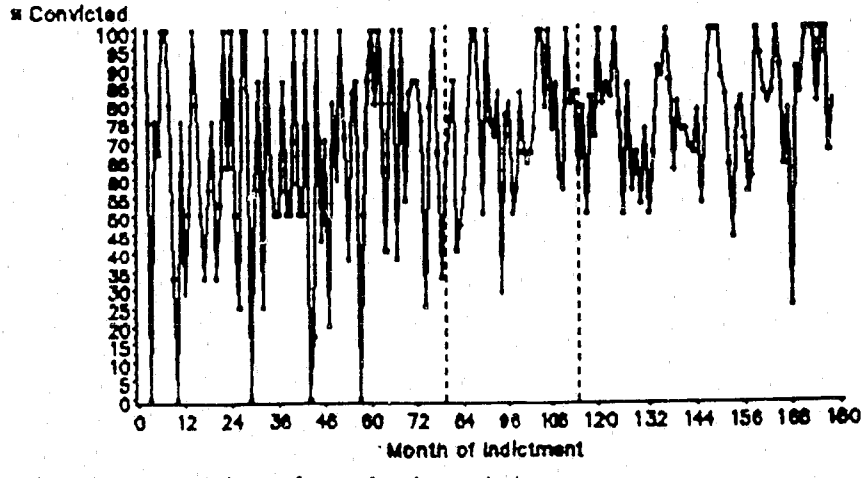
Broken lines indicate dates reforms implemented
Rape shield law – month 79; corroboration change – month 115

FIGURE 5b
Percent Indicted Monthly for Rape
Atlanta, Georgia
1970–1984



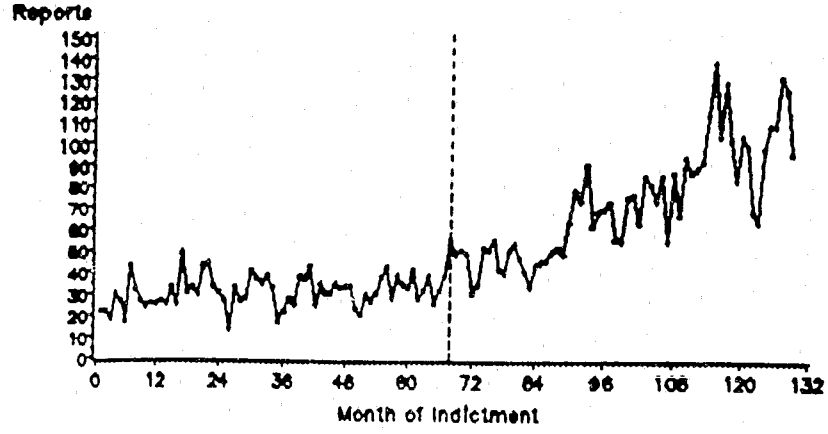
Broken lines indicate dates reforms implemented
Rape shield law – month 79; corroboration change – month 115

FIGURE 5c
Percent Convicted Monthly for Rape
Atlanta, Georgia
1970–1984



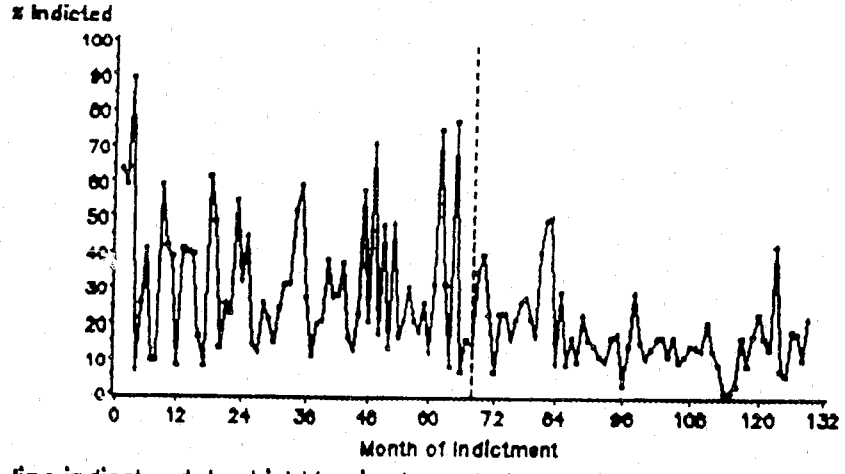
Broken lines indicate dates reforms implemented
Rape shield law – month 79; corroboration change – month 115

FIGURE 6a
Monthly Reports of Rape
Houston, Texas
1970–1980*



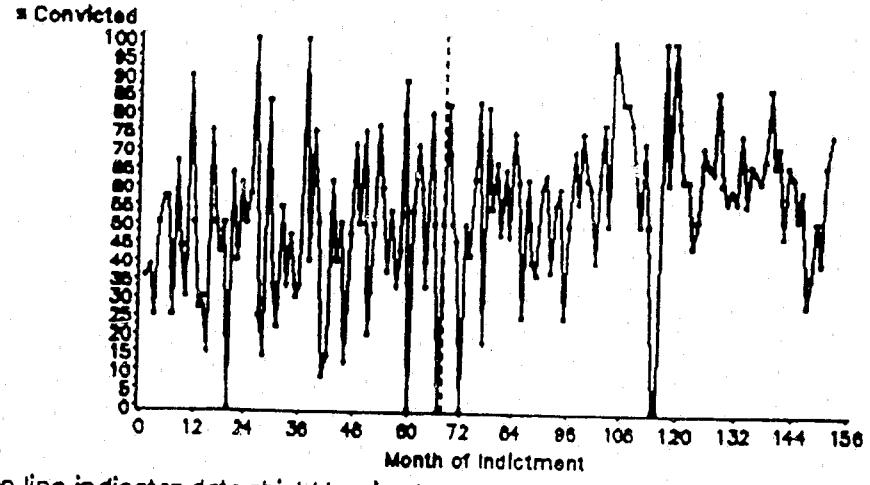
Broken line indicates date shield law implemented
*Through 10–31–80

FIGRE 6b
Percent Indicted Monthly for Rape
Houston, Texas
1970–1980*



Broken line indicates date shield law implemented
*Through 10–31–80

FIGURE 6c
Percent Convicted Monthly for Rape
Houston, Texas
1970–1982*



Broken line indicates date shield law implemented
*Through 8–31–82

Text of the Hypothetical Cases

Case #1

The complainant testifies that she met the defendant at a singles bar, danced and drank with him, and accepted his offer to drive her home. She testifies that at the front door he refused to leave, forced his way into her apartment, and raped her. The defendant claims consent and wants to prove that the complainant previously had consented to intercourse with casual acquaintances she had met at singles bars.

Case #2

The complainant, a resident of a posh building, testifies that she was raped by a maintenance man who was working in her apartment. The defendant claims consent and wants to prove that the complainant previously had consented to intercourse with building employees whom she had invited into her apartment.

Case #3

The complainant testifies that she was gang-raped at a party by several men she had not met before. The defendants claim consent and want to prove that both before and after the alleged rape the complainant had consented to intercourse with groups of men.

Case #4

The complainant, a married woman, testifies that she was raped by her brother-in-law. The defendant claims consent and wants to prove that the complainant recently had consented to intercourse with other men; that she had been criticized for her conduct by her sister, who threatened to tell the complainant's husband; and that the complainant had responded by threatening to charge her brother-in-law with rape.

Case #5

The complainant testifies that she was raped by a stranger who entered her room through an open window in the middle of the night. The defendant claims he was incorrectly identified and wants to prove that the complainant, earlier that same night, had intercourse with a man she had just met at a party, and that this other man was the source of semen found during a medical exam.

Case #6

The complainant testifies that she went to a movie with the defendant, whom she had known for several years. She testifies that at her front door he refused to leave, forced his way into her apartment, and raped her. The defendant claims consent and wants to prove that the complainant had consented to intercourse with him once several months earlier.