Rape Reporting After Reforms: Have Times Really Changed?
Jody Clay-Warner and Callie Harbin Burt
VIOLENCE AGAINST WOMEN 2005; 11; 150
DOI: 10.1177/1077801204271566

The online version of this article can be found at:
http://vaw.sagepub.com/cgi/content/abstract/11/2/150

Published by:
SAGE Publications
http://www.sagepublications.com

Additional services and information for Violence Against Women can be found at:

Email Alerts: http://vaw.sagepub.com/cgi/alerts
Subscriptions: http://vaw.sagepub.com/subscriptions
Reprints: http://www.sagepub.com/journalsReprints.nav
Permissions: http://www.sagepub.com/journalsPermissions.nav

Citations (this article cites 26 articles hosted on the SAGE Journals Online and HighWire Press platforms):
http://vaw.sagepub.com/cgi/content/refs/11/2/150
Rape Reporting After Reforms

Have Times Really Changed?

JODY CLAY-WARNER
CALLIE HARBIN BURT
University of Georgia

Using data from the National Violence Against Women Survey, the authors examine whether rapes committed after reforms were more likely to be reported to police than those committed before reforms. The authors also consider whether the gap between the reporting of simple versus aggravated rape has narrowed. They find that rapes committed after 1990 were more likely to be reported than rapes occurring before 1974. Aggravated rape continues to be more likely to be reported than simple rape, however, and this effect is stable over time. The authors conclude by discussing the implications of these findings for evaluating the success of rape reform statutes.

Keywords: rape reform; rape reporting; sexual assault

Since the mid-1970s, sweeping changes have been made to laws governing forcible rape. These changes include the alteration of evidentiary requirements, establishment of rape shield statutes, modification of resistance requirements, and, in some cases, even redefining the crime of rape (e.g., Spohn & Horney, 1992). Prior to the institution of these reforms, many victims were reluctant to report rapes, fearing mistreatment from the criminal justice system and retaliation from the perpetrator (e.g., Bryden & Lengnick, 1997; Legrand, 1973; Robin, 1977). In fact, under-reporting was so widespread that some estimated that even if every rape reported resulted in a trial, more than 90% of rapists would never see the

AUTHORS’ NOTE: Data used in this article were obtained through the Inter-University Consortium for Political and Social Research housed at the University of Michigan.

VIOLENCE AGAINST WOMEN, Vol. 11 No. 2, February 2005 150-176
DOI: 10.1177/1077801204271566
© 2005 Sage Publications

150
inside of a courtroom (McCahill, Meyer, & Fischman, 1979). Recognizing that justice system reforms are only successful to the extent that victims access the systems, one key goal of the reforms was to increase reporting of rape (Bachman & Paternoster, 1993; Bryden & Lengnick, 1997; Horney & Spohn, 1991). Despite the significance of reporting to the overall success of the reforms, however, only limited empirical research has examined changes in rape reporting across time (Bachman & Paternoster, 1993).

Even less attention has been given to whether extralegal factors that inhibited reporting, such as prior acquaintance with the assailant, continue to exert the same influence today as they did in the prereform era. The influence of extralegal factors is significant, as Estrich (1987) has argued that there has always been a distinction in the legal system between aggravated and simple rape. She defined aggravated rape, following Kalvin and Zeisel (1966; see also Horney & Spohn, 1996), as an assault in which the victim and assailant are unknown to each other, there are multiple assailants, or violence is explicit, as evidenced by use of weapons and victim injury. In simple rapes, none of these circumstances exists. Although modern law has not drawn a distinction between these two forms of sexual violence, considerable evidence suggests that, indeed, aggravated rape and simple rape have been viewed as different crimes by the public and have been treated differently by the courts (Alder, 1987; Bryden & Lengnick, 1997; Estrich, 1987; Kalvin & Zeisel, 1966; LaFree, 1981; McCahill et al., 1979). Estrich also states that certain components of rape reform were written with the explicit intent of reducing the distinction between aggravated and simple rape (e.g., rape shield laws). The question remains, however, whether these reforms have been successful in reducing the reporting gap between simple and aggravated rape.

Here we address the issue of changes in rape reporting by focusing upon two central questions: (a) Has the overall likelihood of rape reporting changed since the advent of reforms? and (b) Has the gap between the reporting of simple and aggravated rape narrowed since the passage of statutory reforms? We begin with a discussion of rape reform legislation and then review prior research on rape reporting. Next, we examine changes in rape reporting behavior through an analysis of data from the National Violence Against Women (NVAW) Survey.
RAPE REFORM LEGISLATION

Feminists, frustrated by the institutionalized sexism in the criminal justice system, and “law-and-order groups,” seeking to increase rates of rape reporting and conviction, began pushing for rape reform in the late 1960s (Spohn & Horney, 1992). Feminists contended that rape victims were hesitant to report their victimization to the police because they feared both derogatory treatment by criminal justice officials and the invasion of privacy associated with rape trials (Robin, 1977). Legal scholars pointed out that the definitional requirements and stringent standards of proof associated with rape made rape statutes unlike any other area of criminal law (Caringella-McDonald, 1985; Robin, 1977). As a result, reporting rates were very low, convictions were difficult to procure, and victims were often revictimized through the criminal justice process (e.g., Caringella-McDonald, 1985; Largen, 1988; Sebba, 1974; Shapo, 1975).

In 1974, Michigan became the first state to respond to these criticisms by enacting rape reform legislation (Galvin, 1985). Many states followed suit; in the years subsequent to the commencement of rape reforms, substantial changes have been made to statutes in every state (Field & Bienen, 1980, p. 153; see also Berger, Neuman, & Searles, 1994; Berger, Searles, & Neuman, 1988). Although the reforms and laws enacted vary between states, they all seek to shift the focus away from the victim and toward the behavior of the defendant (e.g., Caringella-McDonald, 1985; Futter & Mebane, 2001; Searles & Berger, 1987). This goal is reflected in four general themes of rape reform legislation: (a) a redefinition of rape, (b) the elimination or modification of the resistance requirement, (c) the elimination or modification of the corroboration requirement, and (d) the establishment of rape-shield laws that limited or prohibited the admissibility of a victim’s sexual history on cross-examination (Horney & Spohn, 1991).

Traditional rape laws have generally defined forcible rape as “an act of sexual intercourse undertaken by a man with a woman, not his wife, by force and against her will” (Bohmer, 1991, p. 318). Most states’ definitions of rape now encompass acts other than the “traditional” rape such as marital rape and forced oral or anal sex (Marsh, Geist, & Caplan, 1982; Searles & Berger, 1987), and they have replaced the single crime of rape with a series of graded
offenses. These graded offenses reflect the continuum of violence in sexual assault crimes, allow punishments to be prescribed that are commensurate with the crime, and reduce the amount of discretion given to criminal justice officials (Marsh et al., 1982). An additional modification was the replacement of the term rape with a more general term, such as sexual assault, with the intentions of emphasizing both that rape is a crime of violence, rather than one resulting from uncontrollable sexual urges, and equating rape with other violent assaults (Searles & Berger, 1987).

The resistance requirement also came under fire. In traditional American legal theory, rape consisted in the concurrence of a criminal act (the nonconsensual sexual intercourse) with criminal intent (the intention or knowledge of having the intercourse without the victim’s consent; Estrich, 1987). Thus, there was no rape unless the victim explicitly did not consent to sexual intercourse and the perpetrator both recognized and disregarded this fact. The most facile way of proving nonconsent was through victim resistance. As a result, many jurisdictions required victims to demonstrate resistance for the crime to qualify as rape. Many criticized this requirement, because it is unique to the crime of rape. As a consequence of the reform movement, most states eliminated this requirement and/or reduced the state’s burden of proof by stipulating the circumstances that constitute force (Horney & Spohn, 1991).

Another significant evidentiary reform was the removal of the corroboration requirement, which had prohibited conviction for rape on the uncorroborated testimony of the victim. Legal corroboration was confirmation that “(a) the event occurred and is thus not a complete fabrication, (b) the defendant has been identified as the rapist, (c) there was actual ‘penetration’ of the woman, and (d) force was present and ‘consent’ was absent” (Robin, 1977, p. 138). As with the resistance requirement, reformers criticized this condition by pointing to the fact that rape was the only crime with such a requisite (Horney & Spohn, 1991). Every state, with the exception of Nebraska, eventually eliminated the condition that a rape complainant’s testimony be corroborated in the case of forcible rape (Bryden & Lengnick, 1997).

Perhaps both the most controversial and noteworthy reform was the creation of rape shield laws. In theory, these laws prevent the introduction of the victim’s past sexual conduct and evidence
of character and reputation into cross-examination (Caringella-McDonald, 1985; Hibey, 1973). Under traditional rape laws, a propensity to consent to sexual intercourse, established by prior acts as well as the victim’s character and reputation, were considered probative evidence for the jury to utilize to determine whether the victim was the type of person who would have willingly engaged in intercourse with the defendant (Caringella-McDonald, 1985). Thus, the defense used evidence of prior sex acts as being tantamount to acquiescence by contending that if a woman had had sex with various men on many different occasions, it is likely that she consented during the incident in question. As a result of this focus on the victim, many women described the trial as a “second rape” (e.g., Bryden & Lengnick, 1997; Legrand, 1973; Madigan & Gamble, 1991; J. E. Williams & Holmes, 1981).

In response, rape shield laws were passed to limit or prevent the defense from focusing on the behavior of the defendant rather than on the behavior of the victim (Caringella-McDonald, 1985). When legislatures passed rape shield laws, their reasons cited included encouraging the reporting of rapes, protecting the privacy of rape victims, and thwarting the humiliation of victims during trial, particularly in cases of acquaintance rape (Berger et al., 1994; Kessler, 1992).

As Bachman and Paternoster (1993) noted, reformers believed that changes in rape laws, especially rape shield statutes, would make victims more likely to report sexual assaults to the authorities. Women would no longer fear the humiliation at trial that often results from focusing on the behavior and characteristics of the victims. Women would also be more willing to come forward as a result of changes in evidentiary requirements that made rape convictions easier to obtain, because they would no longer see their reporting as a futile effort (Bryden & Lengnick, 1997; Fairstein, 1993; Horney & Spohn, 1991).

**CHANGES IN REPORTING**

Research investigating the success of rape-reform legislation in increasing reporting has been inconclusive. Early state-level studies found rape reforms to be ineffective. In Michigan, where the first and often considered the most comprehensive reforms were enacted, researchers found no change in the number of rapes
reported to the police (Marsh et al., 1982). Additionally, in a time-series analysis of data pre- and postreforms and in interviews with criminal justice officials, Marsh et al. (1982) found no evidence that the new laws affected reporting rates.

Spohn and Horney (1992) examined the effects of the reforms in six cities: Chicago; Detroit; Philadelphia; Washington, D.C.; Atlanta; and Houston. The first three cities were in states with strong reforms, whereas the last three cities were located in jurisdictions with weak reforms. Examining monthly data, they found that in the years from 1970 to 1984, legal reforms had no positive effect in Chicago, Philadelphia, and Atlanta. In Washington, D.C., most likely by coincidence, there was a decrease in reporting after the removal of the corroboration requirement. Spohn and Horney observed in Detroit, the state with the strongest reforms, and in Houston, the jurisdiction with the weakest reforms, an increase in reporting rates that appeared in some way correlated with the reforms. Further inquiry revealed, however, that the changes in reporting rates were not related to the “substantive content” of the reforms but, rather, suggested that “the increases resulted from publicity surrounding the reforms rather than gradually acquired knowledge of improved treatment of victims under the new laws” (Spohn & Horney, 1992, pp. 101-102). This explanation is plausible, because in both cities, the rate of reporting increased immediately after the passage of the reforms and then stabilized (Bryden & Lengnick, 1997).

Berger et al. (1994) examined the relationship between the strength of rape reform legislation and reporting rates using the 1985 rape and sexual assault statutes in 48 states. Seven variables were selected and arranged on an ordinal continuum ranging from traditional to progressive statutes with regard to feminist objectives for the reforms. They then tested whether the strength of a state’s reform statute was related to either rape rates or to a percentage increase in rape reporting in the postreform period. Finding neither relationship to be statistically significant, Berger et al. concluded that reforms had not appreciably affected official rape rates. They did find, however, that the rape law reforms that extended the range of persons protected by law were positively associated with official rape rates. As a result, they contended that although the effects of rape reform were limited, it
would be premature to dismiss the reforms as ineffective (see also Caringella-MacDonald, 1985).

Other research on changes in reporting has utilized national-level data. Bryden and Lengnick (1997) considered crime statistics from both the Uniform Crime Reports (UCR) and the National Crime Victimization Survey (NCVS) from the years 1973 to 1994. When examining the NCVS, which interviews individuals 12 years of age and older from a stratified sample of U.S. households, Bryden and Lengnick found that the number of reported rapes declined piecemeal during that period. In the UCR, however, which tallies the number of reported crimes, the number of rapes reported increased from 24.5 in 1973 to 39.2 per 100,000 inhabitants in 1994.4 Bryden and Lengnick’s explanation for the apparent discrepancy is that reports of sexual assault have increased.

Bachman and Paternoster (1993) analyzed the NCVS, the UCR, the National Prisoners Statistics program (NPS), and the National Corrections Reporting Program (NCRP) to examine the extent to which rape reporting changed in the years from 1973 to 1990 compared to the change in reporting for robbery and nonsexual assaults.5 The NCVS data demonstrated that rape victims who reported to police increased by 10% compared to a 4% increase for assault and a 12% decrease for robbery. The UCR data corroborated this slight increase by revealing a 13% increase in rape reporting during the years from 1973 to 1990, whereas assault reports increased 46% and robbery 6% over the same time period. Based on these findings, Bachman and Paternoster concluded that “rape victims were slightly more likely to report their victimizations after statutory reforms were in place” (1993, p. 566).

In sum, studies of rape law reform have been unable to document significant increases in reports that could be directly attributed to the legislation. Most notably, Spohn and Horney’s (1992) comparative analysis of six urban jurisdictions did not find differences in reporting rates between strong-reform and weak-reform jurisdictions (1992).

FACTORS AFFECTING RAPE REPORTING

It is not just rates of reporting that deserve scrutiny; however; it is also important to examine whether extralegal factors associated
with rape reporting have changed. An ancillary goal of rape reform was to remove the stigma attached to certain types of rape such as those committed by acquaintances or those in which no additional injury occurred. As Estrich (1987) noted, these types of rapes were not considered “real rapes” and were not treated seriously by either the public or the criminal justice system. In developing her argument, Estrich cited the fact that rapes committed by strangers (one type of aggravated rape) are more likely to be reported than are rapes committed by acquaintances, most of which would be classified as simple rape. Indeed, studies using crime data gathered from the late 1970s to the mid-1980s have concluded that women raped by strangers were significantly more likely to report compared to women raped by acquaintances (Feldman-Summers & Ashworth, 1981; Greenberg & Ruback, 1992; Lizotte, 1985; Smith & Nelson, 1976; L. S. Williams, 1984).

Consistent with Estrich’s (1987) assertion, situational danger has also been found to affect reporting. Smith and Nelson (1976) found that victim reporting varied directly with the amount of danger experienced by the victim during her rape and that reporting was more likely if the rapist committed additional crimes at the time of the attack. Similarly, Greenberg and Ruback (1992) found that women were more likely to report their rapes if degrading acts were inflicted. Other indicators of rape seriousness, such as weapon use (Amir, 1971; Bachman, 1998; LaFree, 1980; Lizotte & Wolfson, 1981) and the level of physical injury (Bachman, 1993, 1998; Holmstrom & Burgess, 1978; LaFree, 1980; Lizotte, 1985; Lizotte & Wolfson, 1981), have also been found to increase the likelihood of reporting. In fact, Lizotte (1985) found that although the seriousness of the incident was predictive of crime reporting in general, serious injury had an even greater effect on the likelihood of reporting in rape cases than in cases of nonsexual assault.

Other factors not directly related to the simple-aggravated rape dichotomy have also been found to be associated with rape reporting. Lizotte (1985) found that both married rape victims and highly educated victims were more likely to report. Greenberg and Ruback (1992) reported that women were also more likely to report their rape if the attack occurred outdoors and if the rapist...
was African American. Social distance factors have also been found to be important. Smith and Nelson (1976) found that nonreporting was more likely if the victim and her rapist were of equal age and social class. Similarly, Lizotte (1985) reported that the more familiar the victim was with the offender, the less likely she was to report the sexual assault to the police.

Most of these studies, however, have not examined whether there have been changes in the impact of these extralegal factors on reporting since the institution of rape reform, and none has examined whether simple rapes are more likely to be reported now than prior to reforms. The only relevant variable that has been examined is acquaintance between the victim and offender. Bachman (1993), using rape data from the NCVS from 1987 to 1990, found that the victim-offender relationship did not affect the likelihood of reporting. Pollard (1995) and Ruback (1993) questioned this finding on both statistical and conceptual grounds. Ruback (1993) argued that this research simply demonstrated that acquaintance was a less important factor than it once was and that it was “premature to say that the victim-offender relationship does not matter” (p. 278). Because Bachman did not compare rape reporting pre- and postreform, however, we cannot conclude that there has actually been a significant change across time in the effect of the victim-offender relationship on rape reporting.

Thus, it is still unknown whether rapes were more likely to be reported in the period after reforms than before or whether the gap between the reporting of simple and aggravated rapes has been attenuated. In addressing these questions, this study attempts to avoid some of the problems associated with prior studies of rape reporting. First, most of the studies were conducted in the 1970s and 1980s (e.g., Berger et al., 1994; Bridges, 1991; Caringella-McDonald, 1985; Lizotte, 1985; Marsh et al., 1982). It is possible, however, that the effects of rape reform on reporting would not be seen so quickly after their enactment, as changes in reporting are contingent upon changes in societal perceptions—either about rape itself or about the way in which the crime of rape is treated in the criminal justice system. In this way, the effects of reforms on rape reporting may be either direct or indirect. Reforms may directly affect reporting only when victims become aware of the reforms and make decisions about reporting based upon this
knowledge. A more likely scenario is that reforms have an indirect effect on reporting by contributing to a change in the way the public views rape and rape victims, as was a goal of many feminist reformers (e.g., Bachman & Paternoster, 1993; Bryden & Lengnick, 1997; Largen, 1988). Whether victims are more likely to report as a result of direct knowledge of the reforms or because of changes in the way society views rape victims, we should not expect increases in reporting to be instantaneous with reforms. Bachman and Paternoster (1993) suggested that this might be the case, noting that attitudes about rape and victimization may have changed in the past 15 years thus translating into significant operational changes in the criminal justice system as well as in the way victims respond to the crime.

Second, many studies have examined only a single state or jurisdiction, which precludes any general conclusions about the nationwide impact of the reforms. Finally, much of the previous research has been limited by the available data, which has been drawn primarily from the NCVS and official reports. Studies employing the NCVS have been criticized for the way rape victims are identified. In critiquing Bachman (1993), Ruback (1993) and Pollard (1995) pointed out that individuals who reported their rapes to the police would be more likely to inform the NCVS interviewers that they had been raped than women who had not reported to the police. This bias could serve to inflate the percentage of acquaintance rapes that were reported to police. Reflecting upon societal stigma and decisions concerning what actually constitutes rape, they argued that victims of acquaintance rapes who do not report their rape are more likely to believe or convince themselves that they were not actually victims of rape thus leading to unreliable data (Koss, 1992; Pollard, 1995; Ruback, 1993).

On the other hand, studies that use official rape reports (e.g., UCR) are suspect because they can only measure whether the total number of rape reports has increased; they cannot determine whether the rate of reporting has increased, because official records contain no measure of the number of rapes that are not reported. In this way, it is quite possible that reports to the police could increase because of an actual increase in crime and not because of any increase in the rate at which crimes are reported. In fact, some researchers have used changes in UCR rates as evidence of actual
changes in rates of rape commission (e.g., Baron & Straus, 1989) thereby suggesting that an increase in official reports of rape is not a reliable indicator of increases in the rate of rape reporting. This is a limitation faced by all studies that use official reports of crime to examine changes in reporting (e.g., Berger et al., 1994; Marsh et al., 1982; Spohn & Horney, 1992).

In an attempt to overcome these data limitations, we address the effects of rape reform on reporting using data from the NVAW. Because the NVAW Survey employs a representative sample of women and asks about both reported and unreported rape, it is a more accurate measure of changes in reporting than data drawn from police records. We also avoid some of the problems with underreporting associated with the NCVS. Although the NCVS was redesigned in 1992 to ask more directly about rape victimization, research indicates that significant underreporting remains a problem. Fisher, Cullen, and Turner (2000) found that women participating in the National College Women Sexual Victimization Survey who were asked behaviorally specific questions reported a rape prevalence rate 11 times higher than another group of participants whose rape screening questions were drawn from the redesigned NCVS. Because the NVAW Survey uses behaviorally specific questions virtually identical to those used in the National College Women Sexual Victimization Survey, the problems of underreporting associated with the NCVS are less likely to occur in the NVAW Survey. Also, the NVAW Survey is not described to participants as a crime survey, as is the NCVS. Thus, a woman could acknowledge having experienced sexual violence without identifying as a crime victim. This would increase the likelihood that women who did not report to police would reveal sexual violence to the interviewer.

Our work also goes further than previous research by expanding the central question. We assert that any measure of the success of rape reform in changing reporting behavior must examine not only changes in the likelihood of rape being reported pre- and postreform, but also must address changes in the effects of pertinent extralegal factors on rape reporting (simple vs. aggravated rape). Here we investigate both of these issues in multivariate models.
METHOD

DATA

Data from the NVAW Survey were analyzed (Tjaden & Thoennes, 1999). The NVAW Survey is a national telephone survey jointly supported by the Centers for Disease Control and Prevention and the National Institute of Justice. Participants were chosen through random-digit dialing of residential telephone numbers. To ensure that a nationally representative sample was obtained, the sample was stratified by region, as defined by the U.S. Census Bureau (Tjaden & Thoennes, 1999). Once contact was made with a household resident, eligible adults were identified (those at least 18 years of age). In households in which more than one adult was eligible, the adult with the most recent birthday (day and month) was asked to participate. Thus, there was only one respondent per household. Both men and women were eligible for participation, although the current study analyzes data only from the female participants. The participation rate for women was 72.1% (n = 8,000). Because this was a telephone-based survey, individuals without telephones, those residing in group facilities or institutions, and homeless persons are not represented.

A goal of the NVAW Survey was to determine lifetime prevalence rates of violent victimizations including sexual assault, physical assault, stalking, and intimate partner violence. Respondents were asked behaviorally specific questions to determine whether they had experienced specific forms of violence. If victimization was reported, the interviewer asked respondents detailed questions about the incident and the perpetrator, and a separate report was filed for each incident. In the case of multiple victimizations by the same perpetrator, women were asked these same questions about the most recent victimization by this perpetrator.

For purposes of this research, we are interested in incidents of completed and attempted rape perpetrated by men against women. Incidents of both forced vaginal, anal, or oral penetration (rape) and attempts at such forced penetration (attempted rape) are included in our analyses. We also include only rapes and...
attempted rapes occurring to women who were at least 16 years of age at the time of victimization. We exclude childhood sexual assaults, because issues facing minors in reporting violence are likely to be different from the issues facing adults; thus, it would be inappropriate to include youths and adults in the same analysis. Also, rape law reform focused primarily on forcible rape statutes, which are often separate from child molestation statutes. Therefore, an inclusion of child sexual assault cases could obscure potential differences pre- and postreform.

Including only one reported victimization per study participant further reduced our sample. As previously noted, the NVAW Survey allows victims of sexual violence to identify multiple incidents of victimization, and a separate file is created for each incident. It is inappropriate in most regression procedures to include multiple cases from a single individual, however, as this violates the assumption of uncorrelated errors. To address this problem, we are including only the first incident of rape or attempted rape reported by a participant (occurring after the age of 16). Therefore, the unit of analysis is the rape incident, and there is only one rape incident included per participant. Our final sample is composed of 824 incidents of completed or attempted rape.

MEASURES

The dependant variable was a dichotomous measure indicating whether the assault was reported to police. Following previous research (e.g., Bachman, 1998; Bryden & Lengnick, 1997) we coded the reported variable as 1 if the victim indicated that the incident had been reported to law enforcement either by the victim or any other individual. Of the assaults that were reported, the victim reported to police in approximately three quarters of the cases. Friends or relatives of the victim reported most of the other assaults. A total of 15% of the victimizations were reported.

To examine whether the likelihood of a rape being reported has increased since the passage of rape reform legislation, we constructed two dummy variables for the year in which the assault occurred. Variables for the early reform period (1975-1989) and the modern reform period (1990-1996) were included in the analysis with the prereform period (before 1975) as the left-out,
comparison category. We chose 1974 as the cutoff for the prereform period, because it was not until 1974 that the first rape reform statute was passed (in Michigan). The period from 1975 until 1989 was characterized by ongoing changes in both statutory reform and reform by case law with virtually every state enacting some form of statutory reform by the mid-1980s (Berger et al., 1994). Thus, we labeled this period as early reform.8 One hundred and eighty-nine incidents occurred from 1990 until 1996. Four hundred and twenty-four incidents occurred between 1975 and 1989. The remaining 211 assaults took place before 1975.

The variable *aggravated* distinguished between simple and aggravated rapes using the criteria established by Estrich (1987) and followed by Horney and Spohn (1996). A rape incident was classified as aggravated if a stranger perpetrated the assault, there were multiple assailants, if a weapon was used, or if an injury occurred (e.g., burns, internal injuries, broken bones, sprains, chipped teeth, and other wounds). Forty-two percent of the assaults were aggravated (*n* = 342).

A number of control variables were included that have been found in previous research to affect rape reporting. The variable *completed* was created to distinguish between completed and attempted rapes. Participants were asked a number of behaviorally specific questions to ascertain whether they had been sexually victimized. For example, one question asked,

> Regardless of how long ago it happened, has a man or boy ever made you have sex by using force or threatening to hurt you or someone close to you? Just so there is no mistake, by sex we mean putting a penis in your vagina.

Other questions address forced penetration of the vagina by objects other than the penis as well as the forced penetration of the mouth and anus by either a penis or other objects including fingers. The acts described in these questions meet legal definitions of rape or forced sodomy in most states, and participants who indicated being victimized in such a way were coded 1 for completed rape. Participants were also asked whether an unsuccessful attempt at forced vaginal, anal, or oral penetration was made. Incidents in which women indicated an unsuccessful attempt were coded 0 for attempted rape. In our sample, there were 331 rape attempts (40%) and 493 completed rapes (60%).
Two alcohol/drug use variables were included. Victim drug/alcohol use was coded 1 if the participant indicated that she was under the influence of drugs and/or alcohol at the time of the assault. Twenty percent of victims reported substance use at the time of the rape. Victims were also asked whether the perpetrator was using drugs or alcohol at the time of the incident. When victims indicated that the perpetrator was using drugs, alcohol, or both, the perpetrator drug/alcohol variable was coded 1. In 55% of included cases, the victim reported that the assailant was using alcohol and/or drugs.

Because previous research has found that attacks occurring out of doors are more likely to be reported (Greenberg & Ruback, 1992), a variable for location of assault was included. Outside was coded 1 if the assault took place at any out-of-doors location including a street, alley, parking lot, car, park, rural area, lake, dock, beach, or pool. If the assault occurred inside any public or private building, the outside variable was coded 0.

Two demographic variables were included that have been found in previous studies to be linked to rape reporting and/or reporting of other serious violent crimes. Education has been positively linked to reporting of rape (Lizotte, 1985). Therefore, a measure for college was included and coded 1 if the victim reported having attended college (63%). Hindelang and Davis (1977) found that minority victims of rape were more likely to report than were White victims. Other studies have found, however, that African American women were less likely to report rape than were White women (e.g., Greenberg & Ruback, 1992). To account for racial differences in reporting, we included a dichotomous measure for race (African American = 1). Finally, we also included a control variable for victim’s age in years at the time of the assault.

RESULTS

CHANGES IN THE LIKELIHOOD OF REPORTING

The first question asks whether rapes committed after the passage of reform statutes were more likely to be reported than those committed before the reforms. To address this question, a series of multivariate logistic regression models were constructed. The
first model contains only the control variable, whereas the second model adds the aggravated rape variable. The third model also contains the dummy-coded variables for time (see Table 1).

As seen in the first model, the control variables contributed relatively little to the prediction of reporting. The model was statistically significant, $\chi^2 = 14.74, p < .05$, although the Nagelkerke $R^2$ value of .03 indicates that this set of variables is only weakly associated with reporting. The only significant variable was for victim drug use, which indicates that, controlling for all other variables in the equation, victims who were using drugs and/or alcohol at the time of the assault were less likely to report to police than were victims who were not using drugs or alcohol. When the variable for aggravated rape was added (see Table 1, Model 2), the explanatory power of the model increases significantly, $-2 \log \text{likelihood } \chi^2(1, n = 824) = 93.21, p < .001$. The odds ratio associated with the aggravated rape variable (Exp $\beta = 7.38$) indicates that women who were victims of aggravated rape were more than seven times more likely to report than were victims of simple rape.

The addition of the two dummy-coded variables for year of the assault also resulted in a significantly better fitting model, $-2 \log \text{likelihood } \chi^2(2, n = 824) = 9.74, p < .01$. Most importantly, the variable for the modern reform period was statistically significant, indicating that rapes occurring during the modern reform period (1990-1996) were more likely to be reported than rapes occurring prior to reforms (see Table 1, Model 3). A rape occurring in the modern reform period was 88% more likely to be reported than a rape occurring prior to 1975 (Exp $\beta = 1.88, p < .05$). A sexual assault that occurred in the middle reform period (1975-1989), however, was no more likely to be reported than one occurring before the enactment of reforms (Exp $\beta = 0.91, p = .71$).

In the full model, the variable for aggravated rape remains significant ($p < .001$). This indicates that aggravated rapes were significantly more likely to be reported than were simple rapes even when taking into account the effects exerted by the time variables. The additive model cannot tell us, however, whether the variable for aggravated rape was more predictive of reporting prior to reforms than in the periods following reforms. In other words, has the gap between the reporting of simple and aggravated rape narrowed over time? Answering this question requires the inclusion of an interaction term, as detailed below.
TABLE 1
Main Effects Models Predicting Rape Reporting (N = 824)

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th></th>
<th>Model 2</th>
<th></th>
<th>Model 3</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>β</td>
<td>SE</td>
<td>Exp β</td>
<td>β</td>
<td>SE</td>
<td>Exp β</td>
</tr>
<tr>
<td>Victim African American</td>
<td>0.17</td>
<td>0.29</td>
<td>1.19</td>
<td>0.39</td>
<td>0.32</td>
<td>1.47</td>
</tr>
<tr>
<td>Age at time of assault</td>
<td>0.02</td>
<td>0.01</td>
<td>1.02</td>
<td>0.02</td>
<td>0.01*</td>
<td>1.02</td>
</tr>
<tr>
<td>Attended college</td>
<td>-0.09</td>
<td>0.19</td>
<td>0.92</td>
<td>-0.19</td>
<td>0.21</td>
<td>0.83</td>
</tr>
<tr>
<td>Assaulted outside</td>
<td>0.21</td>
<td>0.25</td>
<td>1.23</td>
<td>0.11</td>
<td>0.27</td>
<td>1.12</td>
</tr>
<tr>
<td>Perpetrator drugs/alcohol</td>
<td>0.29</td>
<td>0.20</td>
<td>1.33</td>
<td>0.16</td>
<td>0.21</td>
<td>1.12</td>
</tr>
<tr>
<td>Victim drugs/alcohol</td>
<td>-0.75</td>
<td>0.29*</td>
<td>0.47</td>
<td>-0.66</td>
<td>0.30*</td>
<td>0.52</td>
</tr>
<tr>
<td>Completed rape</td>
<td>0.16</td>
<td>0.20</td>
<td>1.17</td>
<td>0.25</td>
<td>0.21</td>
<td>0.78</td>
</tr>
<tr>
<td>Aggravated rape</td>
<td>2.00</td>
<td>0.23***</td>
<td>7.38</td>
<td>1.99</td>
<td>0.23***</td>
<td>7.28</td>
</tr>
<tr>
<td>Early reform (1975-1989)</td>
<td>0.63</td>
<td>0.29*</td>
<td>1.88</td>
<td>0.63</td>
<td>0.29*</td>
<td>1.88</td>
</tr>
<tr>
<td>Modern reform (1990-present)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.63</td>
<td>0.29*</td>
</tr>
<tr>
<td>Constant</td>
<td>-2.21</td>
<td>0.35***</td>
<td>0.11</td>
<td>-3.11</td>
<td>0.41***</td>
<td>0.04</td>
</tr>
<tr>
<td>-2 log-likelihood</td>
<td>736.31</td>
<td></td>
<td>643.30</td>
<td></td>
<td>633.36</td>
<td></td>
</tr>
<tr>
<td>Χ²</td>
<td>14.74*</td>
<td></td>
<td>107.95***</td>
<td></td>
<td>117.68***</td>
<td></td>
</tr>
</tbody>
</table>

*p < .05, **p < .01, ***p < .001.
CHANGES IN THE LIKELIHOOD OF REPORTING SIMPLE VERSUS AGGRAVATED RAPE

To examine whether rape type (simple vs. aggravated) had a greater effect on reporting after the institution of reforms than before, it was necessary to interact rape type with both time period measures. These two interaction terms are included in the analysis presented in Table 2. Neither term was significant thus indicating that the effects of rape type on the likelihood of reporting in the prereform period (before 1975) were not significantly different from the effects of rape type on the likelihood of reporting for either the early reform (1975-1989) or the modern reform (1990-1996) eras.

DISCUSSION

These results indicate that although some changes have occurred in the reporting of rape across time, not all goals related to reporting have been realized. Importantly, there have been changes in the likelihood of rape reporting over time. According to the regression analyses, a rape that occurred in the modern reform era (1990-1996) was significantly more likely to be reported than one that occurred in the prereform period (before 1975). The size of the odds ratio indicates that this effect is substantively significant.

Reforms have been unsuccessful, however, in altering the effects of rape type on reporting. Overall, aggravated rapes were significantly more likely to be reported than simple rapes. These effects were relatively stable across the three time periods; the interaction terms revealed no significant differences in the effects of rape type on reporting between the prereform era and either period after reform. Thus, there is no statistical evidence to suggest that the gap in reporting between simple and aggravated rapes has narrowed.

RECONCILING FINDINGS WITH PREVIOUS RESEARCH

These findings are consistent with previous research suggesting that reforms have been only partially successful in changing
reporting behavior. Because this study asks somewhat different questions and uses a different data source, however, the findings reported here complement previous research by helping to draw a more complete picture of the effects of reforms on reporting.

Earlier studies using official data reported that rape reforms had little effect on the likelihood of reporting (Berger et al., 1994; Spohn & Horney, 1992). As previously discussed, research using only official data is limited, because it is impossible to determine if the number of reported rapes has increased because of an increase in reporting rates or an increase in the actual number of rapes being committed. As a result, a decrease in the number of rapes committed would obscure any changes in reporting across time. Also, Spohn and Horney (1992) examined rape reporting only through 1984. Our results indicate that significant changes in reporting rates could not have been detected until after this period.

Studies that utilized victimization data, however, found slight increases in reporting postreform (Bachman & Paternoster, 1993; Bryden & Lengnick, 1997). Our findings are consistent with this research, although the effect sizes found in our data appear larger than those in other studies. These other studies, however, used data from the NCVS, which has been shown to dramatically

<table>
<thead>
<tr>
<th></th>
<th>β</th>
<th>SE</th>
<th>Exp β</th>
</tr>
</thead>
<tbody>
<tr>
<td>Victim African American</td>
<td>0.28</td>
<td>0.33</td>
<td>1.32</td>
</tr>
<tr>
<td>Age at time of assault</td>
<td>0.02</td>
<td>0.01</td>
<td>1.02</td>
</tr>
<tr>
<td>Attended college</td>
<td>-0.18</td>
<td>0.21</td>
<td>0.83</td>
</tr>
<tr>
<td>Assaulted outside</td>
<td>0.19</td>
<td>0.27</td>
<td>1.21</td>
</tr>
<tr>
<td>Perpetrator drugs/alcohol</td>
<td>0.19</td>
<td>0.22</td>
<td>1.21</td>
</tr>
<tr>
<td>Victim drugs/alcohol</td>
<td>-0.70</td>
<td>0.31</td>
<td>0.50</td>
</tr>
<tr>
<td>Completed rape</td>
<td>-0.22</td>
<td>0.22</td>
<td>0.81</td>
</tr>
<tr>
<td>Aggravated rape</td>
<td>2.08</td>
<td>0.47</td>
<td>7.97</td>
</tr>
<tr>
<td>Early reform (1975-1989)</td>
<td>0.09</td>
<td>0.48</td>
<td>1.09</td>
</tr>
<tr>
<td>Modern reform (1990-present)</td>
<td>0.57</td>
<td>0.52</td>
<td>1.77</td>
</tr>
<tr>
<td>Early Reform × Aggravated Rape</td>
<td>-0.27</td>
<td>0.57</td>
<td>0.77</td>
</tr>
<tr>
<td>Modern Reform × Aggravated Rape</td>
<td>0.09</td>
<td>0.61</td>
<td>1.09</td>
</tr>
<tr>
<td>Constant</td>
<td>-3.21</td>
<td>0.53</td>
<td>0.04</td>
</tr>
<tr>
<td>-2 log-likelihood</td>
<td>632.85</td>
<td></td>
<td></td>
</tr>
<tr>
<td>χ²</td>
<td>118.20***</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*p < .05. ***p < .001.
underestimate rape victimizations (Fisher et al., 2000; see also Koss, 1992), with some suggesting that those assaults not reported to police are even more susceptible to underestimation because women who do not report the crime to police often do not identify themselves as victims thereby making them less likely to report to an interviewer (Ruback, 1993). Because the NVAW Survey uses behaviorally specific questions and does not make specific reference to crime or victimization, a woman may report an incident of sexual assault to the interviewer without actually identifying herself as a crime victim. As a result, rape data in the NVAW Survey are likely to be more accurate than rape data in the NCVS. The fact that the NVAW Survey classifies women as rape victims without the women themselves acknowledging their victimization also widens the scope of the analysis of reporting, because it allows women to be included who may not be reporting because they do not identify their victimization as a crime. Because increasing awareness of rape as a crime, however, was one goal of rape reform, it is important to include the universe of rape victims and not only those who have acknowledged their victimization. Including these unacknowledged rape victims also lends further credibility to our significant findings for differences in reporting across time.

Our general findings concerning simple rape and reporting are also consistent with previous research. Although no other study to our knowledge has specifically examined the effects of simple versus aggravated rape on reporting, others have considered separately whether certain aspects of rape that would distinguish an assault as either simple or aggravated affect reporting. Following Estrich (1987), we coded all stranger rapes as aggravated, as well as any in which weapons were used or injury occurred. A great deal of research finds that weapon use (Amir, 1971; Bachman, 1998; Lafree, 1980; Lizotte & Wolfson, 1981) and the degree of physical injury (Bachman, 1993, 1998; Holmstrom & Burgess, 1978; LaFree, 1980; Lizotte, 1985; Lizotte & Wolfson, 1981) are predictive of reporting. Assaults in which the offender has no prior acquaintance with the victim are also considered aggravated, and consistent with the results reported here, other studies find that victims are more likely to report sexual violence when assaulted by strangers (Feldman-Summers & Ashworth, 1981; Greenberg &

What has not been previously examined, however, is whether simple rapes were more likely to be reported in recent times than before reforms or even immediately after their passage. Although Bachman (1993) did not examine data from the prereform era, she did find that in the period from 1987 to 1990, the prior relationship between the victim and offender was not significantly related to reporting. The prior relationship between the victim and offender, of course, does not perfectly distinguish between simple and aggravated rapes, because acquaintance rapes involving weapons, injury, or multiple assailants would be considered aggravated. Nonetheless, Bachman’s finding that acquaintance rapes were no less likely to be reported in the late 1980s than were stranger rapes suggests that the distinction between aggravated and simple rape may also have little predictive power postreform given that most acquaintance rapes take place without weapons and additional injury (Burt, 1991). Our findings, however, indicate not only that the distinction between aggravated and simple rape remains an important predictor of reporting but also that the impact of this variable did not differ significantly across time.

LIMITATIONS AND IMPLICATIONS FOR RAPE LAW REFORM

Before considering the meaning of these findings for evaluating the success of rape law reform, it is important to consider limitations associated with the analysis. One of the most significant issues is possible memory decay. Because the NVAW Survey does not employ a bounded survey design but instead seeks lifetime prevalence data, survey participants are asked to provide details about rapes that may have happened many years ago. As a result, there may be inaccuracies in respondent recollections, and these inaccuracies may increase over time. Underreporting may also occur in the NVAW Survey, although as previously discussed, the behaviorally specific screening questions reduce this risk. At the same time, underreporting may be exacerbated by cohort effects in that younger women may more readily identify forced sex than older women who were likely socialized to hold more conservative attitudes about women’s sexuality.
Although we cannot know the extent to which the issues discussed above actually affect the results reported here, we do know that most of the problems associated with the NVAW Survey are similar to problems of other victimization surveys. Because the NVAW Survey uses behaviorally specific screening questions that have been found to reduce underreporting, however, the NVAW Survey is likely to produce a more representative sample than many other surveys (e.g., NCVS). Furthermore, the NVAW Survey remains the best source of data currently available for analyzing changes in rape reporting, as it is the only nationally representative survey that includes detailed information about both reported and unreported rapes occurring pre- and postreform.

A more general consideration, however, is that it is impossible to determine whether the changes that we find in rape reporting are a direct result of the reforms or because of other societal changes occurring at the same time. For example, it is quite possible that the feminist movement, which pushed for rape reform, also brought about changes in the way women think about violence and their sexuality. The feminist movement stressed women’s right to control their bodies and their sexuality. This emphasis on ownership of one’s body has likely influenced the way the public views forced sexual contact with more women now identifying forced sexual contact as a criminal act. In this way, the effects of the rape reform movement may be largely symbolic.

Feminists involved in the reform movement, however, were strongly motivated by the symbolic goals of rape reform. Such groups were explicitly focused not only upon the measurable, instrumental outcomes but also upon the ability of the law to inform society about the gravity of sexual assaults (Bachman & Paternoster, 1993; Bryden & Lengnick, 1997). Symbolic goals focused on altering societal attitudes and providing visibility and legitimacy to oppressed groups’ goals and values (Berger et al., 1988). Although varying in intent, the effects of instrumental and symbolic goals of rape law reform were designed to be complementary, with alterations in society’s beliefs about “what rape ‘really is’ and whom rape ‘really victimizes’” expected to increase rape reporting (Bachman & Paternoster, 1993, p. 555). As a result, increases in reporting can be seen as an indication of the success of
rape reform, whether the success is measured instrumentally or symbolically.

One fact, however, still remains: The existing gap between aggravated rape and simple rape is a clear indication that reforms have not been completely successful. Not only did we find that aggravated rapes were consistently more likely to be reported than were simple rapes, but the importance of this classification in predicting reporting was no less important in postreform periods than in the period prior to reforms.

Reforms may have failed in improving the likelihood of reporting simple rape for a number of reasons. Rational choice perspectives argue that victims report when they perceive that the benefits associated with reporting are greater than the costs. Reform statutes were designed to reduce these costs by improving treatment of rape victims and increasing the benefits by improving the chances that reporting would result in a conviction. Reforms may have failed simply because they have not evoked enough changes in the criminal justice system to make the benefits greater than the costs. Indeed, rape victims continue to report dissatisfaction with the criminal justice system (e.g., Frazier & Haney, 1996; Konradi, 1997, 1999). Evidence presented by Horney and Spohn (1996) indicates, however, that there are few differences in criminal justice processing of simple versus aggravated rapes postreform thus suggesting that the potential benefits associated with reporting different types of rape are now roughly equivalent.

Rape victims also often incur great personal costs when reporting simple rape, not the least of which is the risk of being ostracized by friends and family. It may be that the benefits obtained by reporting do not currently outweigh these personal costs. If so, then rape reform legislation will not be wholly effective until societal attitudes also change. This issue brings the focus back to the intersection of the instrumental and symbolic goals of reform. If victims of simple rape are less likely to report than victims of aggravated rape because of the ineffectiveness of reforms in altering the criminal justice process, then instrumental goals have not been met. If victims of simple rape are less likely to report than victims of aggravated rape because of the ineffectiveness of reforms in bringing about changes in societal perceptions of rape, then symbolic goals have not been met. In either case, the fact that simple rapes continue to be underreported relative to aggravated
rapes is evidence that not all goals of rape reform have been attained.

CONCLUSION AND DIRECTIONS FOR FUTURE RESEARCH

The study reported here used a nationally representative data set to examine changes in rape reporting since the institution of rape reform. Because we employed a national data set, however, we are addressing the effects of rape law reform in the aggregate. Thus, although this research addresses questions concerning the nationwide success of rape law reform, it also brings into view other questions that need to be addressed concerning rape law reform. For instance, are the findings reported here equally applicable across jurisdictions, or have some states been more successful than others in closing the reporting gap between simple and aggravated rape? If some states have seen a significant increase in the likelihood of reporting simple rape relative to aggravated rape, then what specific statutes might explain the increase? Have police and prosecutor policies that exist apart from statutory reforms affected the reporting gap? Although these questions lie outside our current frame, we hope that future research is able to address these issues in state- and/or jurisdictional-level analyses. Conducting such analyses would complement the data presented here as well as guide future reform efforts.

Ultimately, our examination of data pre- and postreform indicate that although there has been an increase in the overall likelihood of a rape being reported, aggravated rapes continue to be more likely to be reported than simple rapes. The importance of rape type in predicting reporting remained unchanged across pre- and postreform periods. Although we cannot determine whether changes in reporting are directly attributable to reform, the continuing gap between the reporting of aggravated and simple rape suggests an unfinished agenda for rape law reform.

NOTES

1. Some states redefined the charge previously termed rape as criminal sexual conduct, criminal sexual penetration, sexual battery, gross sexual imposition, or sexual abuse.
2. The Wisconsin Supreme Court in *Brown v. State* (1906) reversed a rape conviction on the grounds that the woman had not resisted enough, although she both struggled and screamed. The court’s reasoning was that “there must be the most vehement exercise of every physical means or faculty within the woman’s power to resist the penetration of her person, and this must be shown to persist until the offense is consummated” (p. 538).

3. Some states did retain a corroboration requirement for statutory rape (Kadish & Schulhofer, 1995).

4. Controversy surrounds these statistics, as the number of reported rapes per 100,000 declined somewhat from 42.8 in 1992 to 39.2 in 1994.

5. The comparison was made to ascertain the impact of the law reforms rather than factors affecting rape reporting trends, such as the increasing efficacy or punitiveness of the criminal justice system as a whole (Bachman & Paternoster, 1993).

6. Interestingly, Lizotte (1985) found that weapon use and the presence of multiple victims, however, increased the probability of reporting in nonsexual assaults, whereas no significant effects were found for the reporting of rape.

7. The participation rate was “the number of completed interviews, including those that were screened out as ineligible, divided by the total number of completed interviews, screened-out interviews, refusals, and terminated interviews” (Tjaden & Thoennes, 1999, p. 6).

8. These dummy-coded categories are also preferable to a continuous measure of time (such as the year in which the rape occurred), because it may be many years after the reforms that evidence of their effects on reporting can be seen. Rape reform can only affect reporting to the extent that the general public is aware of the reforms and trusts in their effectiveness. Thus, it is unlikely that rape reporting will increase in a linear fashion. As a result, the use of a continuous measure of time would be inappropriate.

9. The National Violence Against Women Survey does not distinguish between assaults occurring inside of a home and those occurring in the yard adjacent to the home. Therefore, assaults occurring in a residential yard are necessarily coded as inside.

REFERENCES


Jody Clay-Warner is an assistant professor of sociology at the University of Georgia where she conducts research on women’s responses to sexual violence. She also studies procedural justice in the workplace and in legal settings. Most recently, her work has appeared in *Violence and Victims* and *Social Psychology Quarterly*.

Callie Harbin Burt is a doctoral student in the Department of Sociology at the University of Georgia. In her M.A. thesis, she examined the mitigating effects of gender identity on adolescents’ reactions to strain.