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Violence Against Women 2012 18: 223

DOI: 10.1177/1077801212440158

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Violence Against Women
18(2) 223–244

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DOI: 10.1177/1077801212440158

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Rebecca Campbell¹, Debra Patterson²,
and Deborah Bybee¹

Abstract

Most sexual assaults are never reported to law enforcement, and even among reported cases, most will never be successfully prosecuted. This reality has been a long-standing source of frustration for survivors, victim advocates, as well as members of the criminal justice system. To address this problem, communities throughout the United States have implemented multidisciplinary response interventions to improve post-assault care for victims and increase reporting and prosecution rates. One such model is the Sexual Assault Nurse Examiner (SANE) program, whereby specially trained nurses (rather than hospital emergency department [ED] physicians) provide comprehensive psychological, medical, and forensic services for sexual assault victims. The purpose of this study was to examine whether adult sexual assault cases were more likely to be investigated and prosecuted after the implementation of a SANE program within a large Midwestern county. A quasi-experimental design was used to compare criminal justice system case progression pre-SANE to post-SANE. Results from longitudinal multilevel ordinal regression modeling revealed that case progression through the criminal justice system significantly increased pre- to post-SANE: more cases reached the “final” stages of prosecution (i.e., conviction at trial and/or guilty plea bargains) post-SANE. These findings are robust after accounting for changes in operation at the focal county prosecutors’ office and seasonal variation in rape reporting. Implications for policy and practice are discussed.

¹Michigan State University, East Lansing, MI, USA

²Wayne State University, Detroit, MI, USA

Corresponding Author:

Rebecca Campbell, Department of Psychology, Michigan State University, 127 C Psychology Building,
East Lansing, MI 48824-1116, USA

Email: rmc@msu.edu

Keywords

Sexual assault, rape, Sexual Assault Nurse Examiner (SANE) programs, prosecution, criminal justice system

Sexual assault is a pervasive social problem that has been linked to multiple long-term negative outcomes, such as psychological distress, repeated sexual victimization, physical health problems, and difficulties in life functioning (Koss, Bailey, Yuan, Herrera, & Lichter, 2003; Kilpatrick, Amstadter, Resnick, & Ruggiero, 2007). Epidemiological data suggest that at least 17% of women will be sexually assaulted in their adult lifetimes (Tjaden, & Thoennes, 1998); however, most victims/survivors do not report to law enforcement (Bureau of Justice Statistics, 2007). Even when victims do contact the police, previous studies indicate that only 18% to 44% of assaults reported to law enforcement are referred to prosecutors for consideration of criminal charges; of those referred reports, prosecutors issue warrants in 46% to 72% of the cases (Bouffard, 2000; Campbell, Wasco, Ahrens, Sefl, & Barnes, 2001; Chandler & Torney, 1981; Crandall & Helitzer, 2003; Frazier & Haney, 1996; Galvin & Polk, 1983; LaFree, 1980; Spohn, Beichner, & Davis-Frenzel, 2001; Spohn & Horney, 1993). Overall, approximately 14% to 18% of all reported sexual assaults are prosecuted (see Lonsway & Archambault, 2012) for a more detailed discussion of the problem of case attrition).

To address these problems, communities throughout the United States have implemented multidisciplinary response interventions to improve post-assault care for victims and also increase reporting and prosecution rates (Campbell, Patterson, & Lichty, 2005). One such model is Sexual Assault Nurse Examiner (SANE) programs whereby specially trained nurses (rather than hospital emergency department [ED] physicians) provide comprehensive psychological, medical, and forensic services for sexual assault victims (Ledray, 1999; Littel, 2001). In addition, SANEs work with police and prosecutors in their communities for on-going case consultation and can testify as expert witnesses should a case go to trial. SANE programs are a vital resource to both sexual assault survivors and the legal community, which raises the question: Do SANE programs have an impact on prosecution rates in their communities? In this article, we will review the extant literature on SANE programs and the criminal justice system as well as present new data on how these interventions may contribute to increased prosecution for adult sexual assault cases.

When sexual assault victims/survivors seek professional help after an assault, they are most likely to be directed to the medical system, specifically hospital ED (Resnick et al., 2000). The survivor's body is a crime scene and due to the invasive nature of sexual assault, medical personnel, rather than a crime scene technician, is the appropriate professional to collect this type of evidence. Over the years, both researchers and rape victim advocates have noted numerous problems with this ED-based approach to post-assault health care and forensic collection (Campbell, 2008; Campbell & Bybee, 1997; Campbell & Martin, 2001; Martin, 2005). Many ED physicians are reluctant to perform rape exams (Martin, 2005), and most lack training in forensic evidence collection procedures (Littel, 2001). As a result, many rape kits collected by ED doctors are done so incorrectly and/or incompletely

(Littel, 2001; Sievers, Murphy, & Miller, 2003). In addition to problems with evidence quality, emerging research indicates that many rape victims are often retraumatized by post-assault ED exams, which may leave them feeling more depressed, anxious, blamed, and reluctant to seek further help (Campbell, 2005, 2006; Campbell et al., 2001; Campbell & Raja, 1999, 2005; Campbell et al., 1999). These negative experiences have the unintended effect of decreasing victims' willingness to participate in law enforcement investigations and legal prosecution (Campbell, 1998; Campbell & Raja, 2005). Practitioners from the legal, medical, and advocacy communities readily agreed that a new approach to post-assault care was needed, one that would attend to forensic legal issues as well as victims' psychological and medical needs.

In response, SANE programs were created in the 1970s by the nursing profession, in collaboration with rape crisis centers/victim advocacy organizations, and rapidly proliferated across the nation during the 1990s (Department of Justice, 2004; Ledray, 1999; Littel, 2001). These programs were designed to circumvent problems with traditional hospital ED care by having specially trained nurses, rather than doctors, provide first-response care, first-response care to sexual assault victims/survivors. SANE programs are staffed by registered nurses or nurse practitioners who have completed a minimum of 40 hr of classroom training and 40 to 96 hr of clinical training (Department of Justice, 2006; Ledray, 1997, 1999). Most SANE programs are hospital based (e.g., ED; 75% to 90%), but some are located in community settings (10% to 25%; e.g., clinics or rape crisis centers; Campbell et al., 2005; Logan, Cole, & Capillo, 2007). Nearly all programs serve adolescents and adults, and approximately half serve pediatric victims/survivors (International Association of Forensic Nurses, 2012).

SANE programs strive "to minimize the physical and psychological trauma to the victim and maximize the probability of collection and preserving physical evidence for potential use in the legal system" (Young et al., 1992, p. 878). To address victims/survivors' psychological needs, SANEs focus on treating victims with dignity and respect to ensure that they are not retraumatized by the exam (Campbell, Patterson, Adams, Diegel, & Coats, 2008). Many SANE programs work with their local rape crisis centers so victim advocates can provide emotional support (Hatmaker, Pinholster, & Saye, 2002; Littel, 2001; Taylor, 2002). This delineation of roles is critical because most states have confidentiality laws that protect victim's communications with sexual assault advocates (American Bar Association Commission on Domestic Violence, 2007). By contrast, SANEs may have to testify in court about their communications with survivors. To attend to victims/survivors' physical health needs, SANEs treat victims' injuries, offer emergency contraception for those at risk of becoming pregnant, and provide prophylactic antibiotics to treat STIs that may have been contracted in the assault (Campbell et al., 2006; Ledray, 1999).

For the forensic evidence collection itself, most SANE programs use specialized equipment, such as a colposcope, which is a noninvasive, lighted magnifying instrument used for examining the anogenital area for the detection of microlacerations, bruises, and other injuries (Voelker, 1996). A camera is attached to the colposcope to document anogenital injuries (Lang, 1999). Toluidine blue dye can also be used for trauma identification by enhancing the visualization of microlacerations (Ledray, 1999). The forensic evidence

collected by the SANEs is typically sent to the state crime lab for analysis. If a case is prosecuted, SANEs may provide expert witness testimony (Campbell et al., 2007; Ledray & Barry, 1998).

SANEs provide extensive post-assault services for rape victims/survivors, but truly comprehensive care involves the efforts of multiple service providers. Many SANE programs today operate as part of multidisciplinary response teams (e.g., Sexual Assault Response Teams [SARTs]) or coordinated community response initiatives (Hutson, 2002; Littel, 2001). Recognizing the importance of collaboration, some states require all SANE programs who apply for state funding to have a multidisciplinary team to oversee the implementation (Littel, 2001). Many SANE programs continue to work closely with the members of the multidisciplinary team after implementation to review cases and verify that victims/survivors received comprehensive care (Littel, 2001). Some SANE programs also offer formalized multidisciplinary trainings on sexual assault that address strategies for working effectively with survivors, why injuries may or may not be present, and how forensic evidence can be used in law enforcement investigations and prosecution (Littel, 2001; Stone, Henson, & McLaren, 2006).

SANEs provide law enforcement personnel and prosecutors with valuable resources, including but not limited to state-of-the-art medical forensic evidence, so it is reasonable to ask whether this intervention model has a positive impact on prosecution rates. Several case studies suggest that SANE programs increase arrest and prosecution rates (Aiken & Speck, 1995; Arndt, 1988; Cornell, 1998; Hutson, 2002; Ledray, 1992; Littel, 2001; O'Brien, 1996; Solola, Scott, Severs, & Howell, 1983). Case study designs are often used in evaluations of new interventions (Rossi, Lipsey, & Freeman, 2004) and are useful for providing rich descriptive information about programs and identifying outcomes (Stake, 2005). However, it is difficult to determine whether the effects documented in case study research (e.g., increased prosecution) can be attributed to the focal intervention because this methodology does not include comparison groups or other methodological controls that permit causal inferences. To date, only two studies have used more rigorous research designs to evaluate whether SANE programs increase prosecution.

First, Crandall and Helitzer (2003) used a quasi-experimental pre-post design to compare prosecution rates in a New Mexico jurisdiction 2 years before to 3 years after the implementation of a SANE program. Their results indicated that significantly more victims/survivors treated in the SANE program reported to the police than before the SANE program was launched in this community (72% vs. 50%) and significantly more victims/survivors had evidence collection kits taken (88% vs. 30%). Police filed more charges of sexual assault post-SANE as compared with pre-SANE (7.0 charges/perpetrator vs. 5.4). The conviction rate for charged SANE cases was also significantly higher (69% vs. 57%), resulting in longer average sentences (5.1 vs. 1.2 years).

These results are quite promising, but it is important to note that this New Mexico community may be somewhat atypical: the pre-SANE conviction rates were substantially higher than published reports (38% vs. 17% typically) and post-SANE rates were higher still, which raises the question whether such effects are possible in communities with lower starting conviction rates. In addition, it was unclear the extent to which the pre- and

post-SANE cases were comparable (e.g., same jurisdictions/law enforcement agencies, similar kinds of sexual assault cases), which is critical for the methodological rigor of quasi-experimental designs (Shadish, Cook, & Campbell, 2002). If the pre- and post-cases differ in multiple ways, not just whether they were processed before or after the implementation of the SANE program, it is not possible to draw causal inferences. These issues were not directly addressed in the Crandall and Helitzer project, so whether the increased rates can be attributed to the SANE program is unresolved.

In the second study, Nugent-Borakove and colleagues (2006) collected case data from victims who received SANE-SART interventions (in a New Jersey county and a Kansas county, combined), a SANE-only intervention (in a Massachusetts county), and those who received no SANE or SART intervention (from the same New Jersey, Kansas, and Massachusetts counties). In the SANE-SART intervention cases, more types of evidence (e.g., sexual assault medical forensic kits, DNA, clothing, other fibers, and photographs) were collected than in the other two groups. However, with respect to DNA evidence specifically, SANE-SART cases had DNA collection only 37% of the time, as compared with 97% of the time in SANE-only interventions. The SANE-SART intervention cases were more likely to result in arrest, charges being filed, and case convictions, but in multivariate analyses, the effect for intervention type became nonsignificant when victim participation and victim/offender relationship were controlled. Victim participation was highest in the SANE-SART intervention group, but victim participation was significant in its own right, meaning that higher levels of victim participation—irrespective of the intervention approach the victim received or did not receive—predicted case convictions. Victim participation was lowest in the SANE-only approach, but the underlying reasons for these effects were not examined.

The findings from this study suggest that the *absence* of SANE and/or SART interventions was clearly associated with less successful legal case outcomes, but the results were less clear regarding the relative utility of the SANE-SART or SANE-only intervention. It is difficult to ascertain the extent to which the three groups in fact received different interventions (i.e., were mutually exclusive). For example, it appears that one site yielded both SANE-SART cases and non-SANE-SART cases. Usually when a community/county implements a SANE, SART, or SANE-SART, it becomes “the” model within the community and all or virtually all cases are treated within that intervention approach. It is unclear whether there were multiple cities or hospitals within this site that used different interventions, or whether the cases reflect different years within the focal county whereby older cases were non-SANE-SART and more recent cases were SANE-SART. Either way, it does not appear that this study accounted for within site heterogeneity, within or across site comparisons, or effects over time (i.e., case year) making it impossible to know how different state laws, jurisdictional variability, and local community contexts may have affected the obtained results; as such, the findings need to be interpreted with caution.

The few SANE studies that have used comparative research designs suggest these interventions may increase sexual assault prosecution rates, but there are lingering questions regarding the extent to which the increased rates were directly attributable to the intervention. The purpose of this project was to build on this prior work and examine whether adult

sexual assault cases in a Midwestern community were more likely to be investigated and prosecuted after the implementation of a SANE program. We used a quasi-experimental, nonequivalent comparison group cohort design (Shadish, Cook, & Campbell, 2002) to compare criminal justice system outcomes for adult sexual assault cases treated in county hospitals 5 years prior to the implementation of the SANE program to cases treated in the focal SANE program during its first seven years of operation. A quasi-experimental design was necessary because it was not feasible to randomly assign victims to receive exams at either the SANE program or a hospital. Once this county's SANE program was created, all hospitals in the focal county referred their cases to the SANE program (which also precluded collecting a concurrent comparison sample). It is methodologically preferable to draw the comparison sample from the same community (i.e., cases in focal county before the SANE program was implemented rather than cases from another county during the same period of time that the SANE program was operational) as such a design minimizes threats to internal validity (e.g., history, selection). In addition to these feasibility issues, a quasi-experimental design was selected due to ethical concerns about randomly assigning sexual assault victims to a non-SANE condition for medical care. Previous research has indicated that SANE programs are significantly more likely than traditional hospital EDs to provide vital services such as screening and prophylaxis for STIs and emergency contraception (Campbell et al., 2006; Ciancone, Wilson, Collette, & Gerson, 2000; Logan et al., 2007). As such, the methodological gains of a pure experimental design do not outweigh the potential risks to survivors' health.

In this study, we also reconceptualized how to assess the impact of SANE programs on criminal justice case outcomes. Prior research has tended to focus on discrete outcomes, most typically prosecutorial charging rates, but sexual assault prosecution is a complex, multistage process involving both law enforcement personnel as well as prosecutors. The emerging literature on SANEs suggests that there may be positive effects in the earlier stages of law enforcement investigation, and therefore, we defined our outcome variable as *case progression through the criminal justice system*. Some cases are never referred by police to the prosecutors, others are referred but never warranted by prosecutors, some are warranted but later dropped, and ultimately, some are charged and resolved through trials or plea bargains. In this study, we examined changes over time in the number of cases that moved through these different stages of system progression—how many reached what outcome, and did this change after the implementation of the SANE program? Are there more cases moving further through the criminal justice system post-SANE? It is hypothesized that there would be a significant increase in adult sexual assault case progression post-SANE.

Method

Research Setting

The setting for this study was a geographically diverse county in the Midwest with a population of 829,453 that included urban, suburban, and rural areas. In 1997, a multidisciplinary community task force was formed to address the problems of low reporting and

conviction rates for sexual assault cases, inadequate forensic evidence collection, and victim-blaming treatment by hospital ED personnel. The task force determined that a SANE program was needed in the community, and the local rape crisis center was selected as the host for the program because of their 10-year history of providing comprehensive services to sexual assault victims. This group decided that the SANE program should be community based (rather than hospital based), and facilities were established in a medical office building that is easily accessible by public transportation. The program space consists of separate interview and exam rooms, a separate waiting area for family and friends, and a private bathroom with shower. The facility is also equipped with state-of-the-art medical forensic equipment not generally available at hospitals. In September 1999, the focal SANE program opened with established agreements from all hospitals and law enforcement agencies in the county to transfer sexual assault victims to the program for evidence collection. In the event a survivor needed urgent medical care, the SANE program nurses would be permitted to conduct the exams on-site in every county hospital.

SANE services are accessible 24-7 through the rape crisis center's 24-hour crisis line. A team comprised of a nurse and a victim advocate is dispatched to the site by request of law enforcement, hospital, or the survivor. This team provides comprehensive medical treatment, crisis intervention, and follow-up services. The programs' clientele is predominately female (97%) and White (68%; 25% African American, 1% Latina, and 6% Other), which is consistent with the racial/ethnic composition of this county. This program is consistent with emerging national-level data on SANE program characteristics with respect to size, staffing, number of patients served, services provided, and training/supervision of nurses (Campbell et al., 2005; International Association of Forensic Nurses, 2012; Logan et al., 2007).

Police data were collected from the five largest law enforcement agencies in this county. Departments 1 and 5 each have one designated detective to handle reported sexual assault cases. By contrast, Department 2 does not have designated personnel for sexual violence crimes; all detectives are responsible for responding to a full variety of crimes. Departments 3 and 4 have semispecialized units: one has a family crime division, which includes domestic violence, child abuse, and sexual violence crimes; the other has a crimes against people unit, which includes all nonproperty crimes.

The prosecutor's office in the focal county has a specialized sex crimes unit that consists of five assistant prosecutors who handle all case decisions, including the decision to warrant cases. Vertical prosecution, whereby a case is assigned to a prosecutor who handles the case until final disposition, is only provided to victims under the age of 13. Therefore, adult victims may work with multiple prosecutors. For example, Prosecutor A could make the decision to warrant the case, Prosecutor B could handle the case at the preliminary hearing, and Prosecutor C could handle the case at the trial. In some instances, prosecutors outside of the sex crimes unit may handle the case after the warranting decision is made.

Research Design

This study used a quasi-experimental, nonequivalent comparison group cohort design (Shadish, Cook, & Campbell, 2002) to compare criminal justice system outcomes for adult

sexual assault cases treated in county hospitals 5 years prior to the implementation of the SANE to cases treated in the focal SANE program during its first 7 years of operation. Shadish and colleagues noted that the cohort design is a rigorous quasi-experimental design that permits making causal inferences provided that the two groups compared are as equivalent as possible (see below) and that alternative threats to internal validity have been reasonably ruled out.

Sample

Adult sexual assault cases treated in county hospitals 5 years prior to the implementation of the SANE program (January 1994 to August 1999; the comparison group) were compared to cases treated in the focal SANE program during its first 7 years of operation (September 1999 to December 2005; the intervention group). In quasi-experimental designs, it is essential that the groups to be compared are as equivalent as possible (Shadish et al., 2002). As such, cases were sampled that fit the following criteria: (a) the victim was age 18 or older; (b) the victim was assaulted within the focal county; (c) the case was investigated by one of the five largest police departments in the county; (d) a complete medical forensic exam was conducted by either county hospital personnel (comparison group) or the SANE program (intervention group); and (e) the exam results were analyzed by the state crime lab for DNA evidence.

For the comparison group, adult sexual assault case outcome records were requested through the Freedom of Information Act from the five largest police departments in the focal county. These law enforcement agencies processed 171 adult sexual assault cases from January 1994 to August 1999 that met the study's eligibility criteria. Fifteen of these cases were eliminated from the sample (seven victims were charged with false reporting and in eight stranger rape cases, the offenders were never able to be identified), yielding a final sample of $N = 156$ cases. To assess the reliability of the sampling procedures, 30% of the records were randomly selected and reviewed by a second research assistant to determine if the same cases were selected for inclusion based on the five criteria for eligibility (100% agreement).

Information about the intervention sample was collected from SANE program records. The program served $N = 146$ victims between September 1999 and December 2005 who met the study's five eligibility criteria. Nine cases were eliminated from the intervention sample (one victim was charged with false reporting, two victims recanted their police reports, in two of the stranger rape cases the offenders were never able to be identified making prosecution impossible, and in four cases records were missing regarding final prosecutorial outcome), yielding a final sample size of $N = 137$. To assess sampling reliability, 30% of the SANE records were randomly selected and reviewed by a second research assistant to determine if the same cases were selected based on the five eligibility criteria (100% agreement).

Procedures

For all sampled cases, complaint numbers and date of assault were recorded to search the prosecutors' databases for case outcome data. For cases that were warranted by the prosecutors,

the accuracy of the database was checked against the police records for 30% of the cases to ensure that both sources of information stated that the case had been warranted (100% agreement). For cases not warranted by the prosecutors, police records were checked to clarify whether the case was referred by police to the prosecutors but was not warranted, or whether it was never referred by law enforcement (100% agreement between prosecutor database and police records for nonwarranted cases). Complaint numbers and date of assault were also submitted to the state crime lab, which provided data as to whether the kit findings were positive, negative, or inconclusive for DNA evidence.

In addition to collecting case outcome data and DNA analysis findings, we anticipated being able to obtain medical forensic exam, assault characteristics, and victim and offender demographic information for both the intervention and comparison group cases. These data were available for the intervention cases from the SANE report, and for the comparison group, we requested copies of the hospital medical forensic exam records from the police files to gather this information (this documentation is not available directly from the hospitals due to Health Insurance Portability and Accountability Act [HIPAA] restrictions). Unfortunately, the hospital reports were almost always missing from the police records for the comparison group cases (and the existing materials in the file rarely provided complete information). This was unanticipated because key informant interviews conducted prior to data collection indicated that the records *should* be in the police files. In subsequent interviews, we inquired about the reasons why these records were missing. Legal personnel stated that the information contained in the pre-SANE hospital reports was often not useful to the investigation, so most likely, law enforcement officers did not request and/or retain them. Copies of the full police investigation reports (not just case outcome information) were not available from all departments—the records we were trying to request were simply too dated to have been retained in their archives. Without medical forensic, assault, offender, and victim data available for the comparison group cases, the variables modeled in the current study (described below) focus on case outcome as predicted by the law enforcement agency that handled the case, DNA findings, and county-level factors, including prosecutor elections and the emergence of the focal SANE program.

Measures

The dependent variable for this study was case outcome, which we assessed as an ordinal variable to capture case progression through the criminal justice system: 1 = *not referred by the police for prosecution*; 2 = *referred to the prosecutor but not warranted for prosecution*; 3 = *warranted by the prosecutor but later dropped or acquitted*; and 4 = *guilty plea or conviction*. We recognize that the third ordinal category, warranted by the prosecutor but later dropped or acquitted, groups together two seemingly different scenarios: cases that did go to trial but ended without conviction and those in which the prosecutor initially began proceeding, but later dropped the case. Yet, in both situations, prosecutors invested effort in the case and pursued prosecution, but in the end, there was no conviction. From that point of view, it made sense to combine these kinds of cases into the third ordinal level (and indeed, the test of ordinality, described below, empirically supported this decision). We also acknowledge that the fourth ordinal category, guilty plea or trial conviction, combines

two strikingly different scenarios under which defender guilt is determined. We did not have sufficient sample size to consider these outcomes separately.

Five predictor variables were examined: (a) the law enforcement agency that handled the case (five departments, dummy coded into four variables; Department 5 was the omitted comparison because it had the lowest levels of case progression through the system); (b) whether the medical forensic exam DNA findings were positive (1 = *yes*; 0 = *no*); (c) time/month in which case was processed over the 12 focal years (see below for more discussion on how time was modeled); (d) whether the case was handled during the prosecutor reelection year (1 = *yes*; 0 = *no*); and (e) whether the case was pre-SANE (0 = *comparison group*) or post-SANE (1 = *intervention group*).

Analytic Plan

Cases spanned 12 years (5 before and 7 after the implementation of the SANE program), and it is likely that cases handled near the same time were likely to have been influenced by shared historical circumstances other than the SANE program. To reflect these shared historical influences, cases were grouped for analysis by the month in which the case was processed. The extent of shared variance in the dependent variable appeared to be modest, with the intraclass correlation coefficient (ICC) = .02 indicating that 2% of the variance among case outcomes could be explained by month-to-month fluctuations. Grouping cases by month allowed examination of the data for time trends and possible seasonal effects that should be reflected in the analysis. Graphical inspection showed a small and nonsignificant upward trend in the extent of progression through the system across all the months. However, within each calendar year, there was a marked downward trend in level of system case progression from January through December. This trend was further identified as primarily a “December effect,” in which cases processed in December reached lower levels of progression through the system compared with cases presenting during the other months of the year (Somers’s $d = -.26, p < .05$). Cases processed in December were less likely to be referred for prosecution (29.5% vs. 55.4%) and less likely to result in a conviction or plea bargain (11.8% vs. 27.2%). However, fewer cases were processed in December relative to other months (18 vs. 25), so these specific comparisons should be interpreted with caution.

To reflect both the grouping of cases by month and the ordinal nature of the dependent variable, multilevel ordinal regression was used to analyze the impact of SANE program implementation on case progression through the system (Hedeker & Gibbons, 1996). Ordinal regression analyzes the cumulative probability that a case will exceed each of several thresholds, or observed levels of the ordinal outcome variable, as a function of the predictor variables included in the analysis. Multilevel ordinal regression is an extension that incorporates the shared influence of predictor variables that affect groups of cases, along with variables that exert independent effects on individual cases. Like all multilevel analyses, multilevel ordinal regression produces standard errors that are appropriate for testing the influence of group-level predictor variables, reflecting their shared effects on individual cases nested within the same group.

Ordinal regression assumes that odds are proportional, namely that the effect of a predictor variable is the same across the thresholds or levels of the ordinal dependent variable (e.g., that SANE has the same effect on convictions/pleas vs. lesser outcomes as on referrals for prosecution vs. nonreferrals). Although in theory the validity of this assumption could be tested, the number of cases was not sufficient for a definitive test across all levels of the dependent variable. Similarly, it was not possible to estimate a proposed extension of multilevel ordinal regression that does not make the proportional odds assumption (Hedeker & Mermelstein, 1998).

The current analysis reflected two levels of data: individual cases (Level 1), which were nested within months (Level 2). The 293 individual cases at Level 1 were nested within 126 months at Level 2. (The Level 2 N was 126 rather than the possible 144, because no cases were processed in each of 18 months scattered across the interval). Modeling time at a higher level of analysis is somewhat unusual in multilevel analysis, but this strategy is cited by Goldstein (2003) as appropriate for analyzing traditional time series data in which cases are nested within units of time. The effect of the continuous passage of time was examined to determine whether there was an identifiable upward, downward, or more complex trend over the months in the progression of cases through the justice system. To facilitate interpretation, months were numbered sequentially, centered so that 0 reflected the initial month of 1999, the year in which the SANE program was implemented. Linear, quadratic, and cubic effects of time were tested to identify any underlying trends in case progression. No effects approaching significance were found, either unconditional or conditional on the effects of other predictor variables, so continuous time trends were omitted from further analysis.

Two predictor variables were modeled at Level 1: the law enforcement agency that investigated each individual case (to adjust for confounding between time and police department effects due to anticipated unevenness in month-to-month fluctuation of case distributions across police departments) and whether the medical forensic exam was positive for DNA findings. The remaining predictor variables were analyzed at Level 2 and characterized the month in which each case was processed: the seasonal "December effect" (i.e., whether case was processed in the month of December, dichotomously coded, 0/1); whether case was processed during the prosecution election year; and whether the case was handled pre- or post-SANE.

Analyses were conducted with hierarchical linear modeling 6.04 software (Raudenbush, Bryk, Cheong, & Congdon, 2004), using the hierarchical generalized linear model with a logit link function to characterize an ordinal dependent variable. Restricted maximum likelihood was used for estimation. To reduce the influence of nonnormality, robust standard errors were used to compute confidence intervals; however, results were virtually identical using robust or nonrobust estimation. Random intercept models were estimated, but variance estimates were consistently nonsignificant ($p > .5$), so intercept variances were fixed at 0 for all final models.

Results

Table 1 presents a summary of case progression outcomes pre-SANE to post-SANE. There was a decrease in the number of cases police did not refer for prosecution post-SANE

Table 1. Case Progression Outcomes Before and After SANE Implementation

Outcome	Pre-SANE	Post-SANE
Not referred by police for prosecution	49%	43%
Referred to prosecutor, but not warranted for prosecution	17%	15%
Warranted by prosecutor, but dropped or trial acquit	10%	13%
Guilty plea or trial conviction	24%	29%

(i.e., more cases were referred for prosecution post-SANE). Similarly, after the implementation of the SANE program, there was a slight decrease in the number of cases that were referred to the prosecutors, but were not warranted for prosecution. There was also a slight increase post-SANE in the number of cases that were warranted by the prosecutors, but were later dropped or acquitted. Although these case outcomes may be undesirable, this does reflect investment and effort on the part of prosecutors. Finally, there was an increase in the number of cases that ended in guilty plea or conviction after the implementation of the SANE program. The vast majority of cases in this highest ordinal level were resolved by guilty plea bargains (80% were pleas; 20% were trial convictions). Plea bargains were no more common post-SANE than pre-SANE. Overall, this pattern of decreases and increases is consistent with the hypothesis that the SANE program positively affected case progression outcomes. Multilevel ordinal modeling was used to determine whether these increases and decreases were statistically significant.

Results of the multilevel ordinal regression are summarized in Table 2. The first block of effects describes the expected cumulative probabilities of justice system outcomes at the three thresholds of the ordinal dependent variable, adjusting for the influence of other predictor variables. The log odds of conviction/guilty plea versus other outcomes (i.e., warranted but dropped/acquitted, referred but not warranted, and not referred) was -2.06 , which translates to a cumulative odds ratio (*OR*) of 0.13. This indicates that a pre-SANE case from Department 5 (the omitted comparison law enforcement agency), processed in a month other than December and in a nonelection year (i.e., a case with scores of 0 on all predictor variables) was approximately one-sixth as likely ($OR = 0.13$) to attain a conviction/plea relative to other outcomes (i.e., warranted but dropped/acquitted, referred but not warranted, or not referred for prosecution). The log odds of a case with the same characteristics (e.g., pre-SANE, Department 5, non-December, nonelection year) being warranted versus not warranted was 0.59, which translates to a cumulative *OR* of 1.81. A case with these characteristics was 80% more likely to be warranted (and either convicted or dropped/acquitted) than not warranted. Finally, the log odds of a similar case being referred for prosecution versus not referred was 1.31, cumulative *OR* of 3.71. This indicates that a case with the same characteristics was three and a half times as likely to be referred for prosecution versus not referred.

The second block of Table 2 shows the relative influence of the Level 1 predictor variables. The law enforcement agency used as the omitted comparison (Department 5) had the lowest levels of progression through the system; all other police departments had comparatively higher levels. The highest was Department 1, where a case was 4.79 times as likely to reach a higher level outcome, in comparison with a similar case in Department 5.

Table 2. Multilevel Ordinal Regression Results

	Log odds	Robust SE	Odds ratio	T (df = 283)	p	Robust CI
Ordinal effect						
Threshold 1— Convicted/plead versus warranted and dropped/ acquitted, referred, or not referred	-2.06	0.25	0.13	-7.43	.001	[0.07, 0.22]
Threshold 2— Convicted/plead or warranted and dropped/acquitted versus not warranted	0.59	0.09	1.81	6.19	.001	[1.50, 2.19]
Threshold 3— Convicted/plead or warranted and dropped/acquitted or referred versus not referred	1.31	0.13	3.71	10.03	.001	[2.87, 4.80]
Level 1						
Law enforcement agency 1 versus 5	1.57	0.48	4.79	3.26	.01	[1.86, 12.33]
Law enforcement agency 2 versus 5	1.07	0.32	2.96	3.93	.001	[1.58, 5.56]
Law enforcement agency 3 versus 5	0.43	0.28	1.54	1.52	.13	[0.88, 2.69]
Law enforcement agency 4 versus 5	0.70	0.39	1.95	1.71	.09	[0.91, 4.21]
DNA positive	0.62	0.23	1.86	2.78	.01	[1.20, 2.90]
Level 2						
Seasonality (December vs. other months)	-1.08	0.55	0.34	-1.97	.05	[0.12, 1.00]
Prosecutor election year versus nonelection year	-1.13	0.54	0.32	-2.07	.04	[0.11, 0.95]
SANE (post-SANE implementation vs. pre-SANE)	0.53	0.23	1.71	2.32	.02	[1.09, 2.69]

Note: The between-month variance of a random intercept model was not significant ($p > .5$), so in this final model, intercept variances were fixed at 0.

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

Department 2 was second highest—2.96 times as likely to reach a higher level outcome, Department 4 was next—1.95 times as likely, and Department 3 was closest to Department 5, which was 1.54 times as likely to attain a higher level outcome. All but the Department 3

police department had significantly higher expected case outcome levels, in comparison with Department 5. Cases that had positive DNA evidence were 1.86 times as likely to reach a higher level outcome.

The final block of Table 2 shows the average influence of the Level 2 predictor variables. The December seasonality effect had a marginally significant impact on the odds that a case would progress to a higher level outcome; cases processed in December were about one-third ($OR = 0.34$) as likely to reach a higher outcome, compared with cases processed in other months ($p = .05$). Similarly, cases processed during a prosecutor election year were about one third ($OR = 0.32$) as likely to reach a higher outcome, in comparison with cases processed in nonelection years ($p < .05$). Finally, after adjusting for all other effects, cases processed post-SANE were approximately 70% more likely ($OR = 1.71$) to attain a higher level of outcome, compared with cases processed pre-SANE ($p < .05$).

An effort was made to test cross-level interactions to examine the possibility that SANE effects might differ by police department. None of the interactions was significantly different from zero, so these effects were removed from the final model. However, power for these tests was minimal, so the tests for these interactions should not be taken as definitive. Analyses were rerun omitting cases handled in the initial year of SANE implementation (1999), to ensure that effects were not dependent on the pattern of case outcome seen in this influential year. The concern was that high community visibility associated with the new program may have exerted such a strong effect that it could obscure a waning impact of SANE in successive years. Results of analyses omitting cases handled in the initial year were very similar, indicating that the results were not driven by initial implementation effects.

Discussion

The goal of this study was to evaluate whether the implementation of the focal SANE program affected criminal justice system case progression for reported adult sexual assaults. Most prior research on the impact of SANEs in the legal system has used case study designs, which is common in evaluations of new interventions (Rossi et al., 2004). However, case studies rarely allow for causal inferences, and prior quasi-experimental projects had methodological limitations that limited the strength of their conclusions. Therefore, our aim in this project was to build on this prior work by using a quasi-experimental cohort design to determine whether criminal justice system case progression improved after the implementation of the SANE program, and whether such effects (if found) could be reasonably attributed to the efforts of the SANE program.

The sampling design for this study was constructed such that the cases in the pre-SANE comparison group were equivalent to the post-SANE intervention group along several important dimensions: all victims were adult women assaulted in the focal county, their cases were reported to the same law enforcement agencies, all had a complete medical forensic exam, and the results of which were analyzed by the state crime lab. The key difference, which was the very crux of this study, was whether the medical forensic exam was conducted by the non-SANE hospital personnel or the SANE program forensic nurses. The

results from the multilevel ordinal regression models indicated that there was a significant increase in criminal justice system case progression pre-SANE to post-SANE. More sexual assault cases were moving further through the system, reaching higher levels of case disposition (i.e., plea bargains and trials) after the implementation of the SANE program as compared with before SANE. Case progression was higher for the SANE cases, and this effect was significant after accounting for department-level and county-level effects in this community over time.

In addition to our finding that post-SANE cases had significantly higher case progression, our results indicated that the particular law enforcement agency investigating the case was a significant predictor of case outcome. The five police departments in this study were not consistent in their processing of cases: one was significantly less likely to refer cases to the prosecutor. However, even in that individual department, there was still an increase from pre- to post-SANE. This department was by no means the most resource strapped as they did have one designated detective for sexual assault cases (but they did not have a semispecialized unit either). However, the departments with specialized investigation units did not have consistently higher referral/case progression. Other effects in the multilevel model demonstrated the importance of organizational-level resources. We uncovered a seasonal effect such that cases processed in month of December were significantly less likely to reach higher levels of case disposition outcome. It seems probable that less investigational time was put into cases due to holiday vacations. Time to invest in cases also appears relevant at the prosecutorial level. During prosecution election years, cases were also somewhat less likely to progress to higher levels ($p = .05$). These effects highlight the importance of organizational-level factors, but do not resolve the issue of whether specialized detective units are instrumental in advancing sexual assault cases for prosecution. From a policy perspective, these findings suggest that increased advocacy may be necessary on behalf of survivors whose cases are processed during times of lower organizational resources. In addition, institutional advocacy by rape crisis centers with law enforcement agencies may be needed to draw attention to this problem and develop proactive strategies to prevent these decreases.

As expected, positive DNA evidence was also a significant predictor of higher case progression. It has been suggested that DNA evidence is particularly important in sexual violence crimes because it provides “hard” evidence to support survivors’ accounts (Arndt, 1988; Littel, 2001). Although we were not able to capture the details of how the DNA evidence was used by law enforcement in their investigation or by prosecutors in plea bargains or trials, its presence was clearly influential. Unfortunately, we did not have sufficient statistical power to test an interaction effect between DNA and SANE intervention to see if positive DNA was more likely in the post-SANE era, which merits further investigation. Nevertheless, DNA evidence cannot be useful to the investigation and prosecution of a case if it is never collected, which highlights the importance of having highly trained medical professionals, such as SANEs, collect, store, and maintain the chain of forensic evidence to ensure that DNA samples can be obtained. The new federal Violence Against Women Act (VAWA) provisions for forensic exams (i.e., no cost to victims, effective January, 2009) are an important new policy development consistent with these research

findings. Sexual assault survivors need to have information about the availability of medical forensic exam services. A multidisciplinary approach whereby legal and mental health professions refer survivors for a medical forensic exam is key. Taking this issue one step further, it is equally imperative that law enforcement submit the evidence kits to the crime lab for analysis, and that there are adequate resources for crime labs as DNA evidence appears to be instrumental in case processing outcomes.

However promising the results of this study may be, it is important to consider the methodological limitations of this project and their impact on the strength of the conclusions that can be drawn from these data. As noted previously, the sample size for this study was relatively small, which precluded testing interaction effects or estimating multilevel ordinal regression models that do not make the proportional odds assumption. Replication studies with larger sample sizes are sorely needed. But the more critical limitation to consider is whether the observed increases in case processing—significant in spite of a small size—can be reasonably attributed to the implementation of the SANE program. Shadish et al. (2002) outlined multiple threats to internal validity in quasi-experimental designs, two of which are highly relevant in the context of this project: history and selection (and by extension, interactions between selection and history).

First, history threats include events that occur during the course of the study (other than the independent variable), which may affect the outcome (e.g., policy change, new programs/resources become available). To assess for possible history threats, Shadish et al. (2002) recommend supplemental data collection (e.g., key informant interviews, archival data collection) to identify if and when pertinent policy changes or other historical shifts may have occurred. We conducted brief key informant interviews with $N = 8$ legal and medical system personnel prior to primary data collection, and an additional $N = 21$ in-depth qualitative interviews throughout the duration of the project to determine whether there had been other changes in case processing policies in this county since 1994 (see Campbell, Patterson, & Cabral, 2010 for more details).

These data indicated that all law enforcement agencies had experienced turnover in sexual assault investigators, but that such changes occurred both before and after the implementation of the SANE program. Similarly, all police departments made some modifications over time regarding internal practices/policies for sexual assault investigations, but such changes were minor relative to the major shift that occurred countywide with the implementation of the SANE program. An event many key informants highlighted as a likely historical threat was the change in elected county prosecutor. Prosecution election year was included in our analyses—and indeed it was significant, but the SANE intervention remained significant after accounting for this effect. With respect to historical changes in medical practices, the exam technology had not significantly changed from 1994 to the present, but it should be noted that hospitals were not using all available advanced practices. In addition, there were no significant changes in the way the state crime lab processes evidence kits, but DNA testing technology has progressed since 1994 such that testing can be done with much smaller samples. This difference could increase the rates of conclusive DNA evidence obtained in the intervention group. Although we did not have sufficient power to test interaction effects, univariate analyses indicated that positive DNA findings

were not more common in the post-SANE era. Furthermore, the effect of the SANE intervention was significant regardless of whether DNA findings were included in the statistical model. Regarding possible media effects, there had been high profile adult sexual cases in the focal county that have garnered substantial community attention, but such cases occurred both before and after the implementation of the SANE program. Although we cannot rule out all possible historical threats, we can account for the most significant events over time that may have affected case outcomes.

Second, selection threats refer to systematic differences between the intervention and comparison groups (other than whether they received the intervention). Experimental designs use random assignment to neutralize differences, but as noted previously, such designs have serious practical and ethical limitations in this context. Unfortunately, as we do not have victim, assault, and medical forensic evidence for the pre-SANE cases, it is impossible to rule out the possibility of a selection threat and/or selection-by-history interaction effect. The pre-SANE cases may have included disproportionately more stranger assaults and fewer intimate partner assaults given efforts throughout the 1990s to raise awareness about the prevalence of acquaintance rape and marital rape (Bachar & Koss, 2001). We were able to obtain the full police reports and all investigator records for pre-SANE cases in some departments, and among those records, the proportion of stranger assaults and marital assaults were comparable to the post-SANE cases within the same law enforcement agencies. Pre and post-SANE cases may have also differed in severity (e.g., injuries). In the key informant interviews, medical personnel indicated that adult sexual assault cases examined in hospitals pre-SANE were no more or less injurious than cases currently seen in the intervention SANE program. Even if a victim needed emergency medical care, hospitals have a SANE nurse to conduct the medical forensic exam at the hospital. In sum, neither the record review nor the key informant interviews provide particularly strong data that can rule out selection or selection interaction threats. As such, we cannot conclusively stipulate that the observed increases were caused by the implementation of the SANE program. However, we do contend that the pattern of findings in the quantitative analyses and key informant interviews is consistent with our hypothesis (see Campbell, Patterson, & Cabral, 2010 for more details), and that taken together, these data suggest the observed effects can be reasonably attributed to the SANE program.

The scope of this study was an in-depth examination of one SANE program, which is not a methodological limitation per se, but scope needs to be considered in context. There are more than 400 SANE programs in existence (and more emerging rapidly), and therefore, it would be wholly inappropriate to generalize the findings of this one evaluation to the success of SANE programs as an intervention model. Only a handful of SANE programs have been studied in any depth and with varying methodological rigor (e.g., Crandall & Helitzer, 2003; Ledray, 1992; Nugent-Borakove et al., 2006; Sievers et al., 2003), so it is unknown whether positive intervention effects are the exception or the norm. Previous research suggests there is substantial variability among SANE interventions with respect to program operation and philosophy (Patterson, Campbell, & Townsend, 2006) as well as organizational relationships with rape crisis centers and SARTs (Cole & Logan, 2008; Payne, 2007). It seems possible that some service models are more effective than others,

especially in terms of patient care and legal outcomes. As such, formative research is needed to explicate the heterogeneity of intervention models in existence, which can then inform multisite trials to examine the effectiveness of different kinds/types of SANE and SANE-SART programs. Such large-scale research on these interventions is warranted given the consistently positive findings emerging in this literature. SANE programs appear to be a promising agent in changing the long-standing problems of low prosecution, victim-blaming treatment, and the ineffectual community response to sexual assault.

Authors' Notes

The views represented in this article do not necessarily reflect those of the National Institute of Justice.

Declaration of Conflicting Interests

The authors declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

This research was supported by a grant from the National Institute of Justice (2005-WG-BX-0003) awarded to the first author.

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Bios

Rebecca Campbell, PhD is a Professor Psychology and Program Evaluation at Michigan State University. Her research focuses on violence against women, specifically sexual assault and how the legal, medical, and mental systems respond to the needs of rape survivors. Her current projects examine the role of sexual assault nurse examiner (SANE) programs in adult and adolescent sexual assault prosecution.

Debra Patterson, PhD is an Assistant Professor of Social Work at Wayne State University. Her research examines how the legal system responds to sexual assault cases, as well as, the impact of Sexual Assault Nurse Examiner (SANE) programs on legal outcomes and victims' emotional well-being.

Deborah Bybee, PhD is a professor in the Department of Psychology at Michigan State University. She specializes in research design and statistical methodology, focusing on the application of multivariate techniques to understand complex, real-world phenomena, especially those that involve change over time. Much of her research addresses violence against women.