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The Effect of Changing the Military's Sexual Assault Laws on Law Enforcement Investigative Findings in the U.S. Army

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Objective: In 2007, Congress changed the military's sexual assault laws as part of an effort to improve sexual assault case processing. This study looked at the U.S. Army law enforcement investigative finding for every sexual assault reported to the Army from 2004 through June 2012, along with every nonsexual assault. Our objective was to measure whether the legal intervention affected the investigative findings made by Army law enforcement officers in sexual assault cases (penetrative, nonpenetrative, and combined) as compared to assault cases (aggravated, simple, and combined). ***Hypotheses:*** We hypothesized that we would not find evidence that the legal intervention affected the rate of sexual assault cases labeled as "founded" by Army law enforcement, such that for the best-fitting time-series models, any difference in the residuals of the means before and after the intervention would not be statistically significant. ***Method:*** We received data from the U.S. Army on all sexual assaults and nonsexual assaults from 2004 through June 2012. The data comprised 47,058 observations. We used time-series analysis with autoregressive integrated moving average modeling. The variable tracked over time was the ratio of the proportion of founded sexual assault cases to the proportion of founded nonsexual assault cases. We then conducted *t* tests of the means of the residuals before and after the legal intervention. ***Results:*** The difference in the means of the residuals before and after the intervention was not statistically significant for combined sexual assaults versus combined assaults, penetrative sexual assaults versus aggravated assaults, or nonpenetrative sexual assaults versus simple assaults. ***Conclusions:*** This reform to sexual assault laws does not appear to have affected sexual assault case processing by U.S. Army law enforcement.

Public Significance Statement
This study used time-series analysis to see if the 2007 change to the military's sexual assault statute affected how law enforcement processed sexual assault cases and did not find evidence that the legal change had an effect. Legislatures seeking to improve sexual assault case processing may need to modify the mistake of fact defense and focus on the training and certification of law enforcement officers to help prevent rape myths from entering the legal system.

Keywords: public policy, crime reporting, military investigations, rape myths

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In the 1970s, many legislatures began reforming the laws and procedural rules related to sexual assault. These reforms were meant to address several issues, to include holding more offenders accountable for their crimes (Bachman & Paternoster, 1993). Although the U.S. military kept pace with the procedural and evidentiary reforms, it did not keep pace with reforms to the substantive law. The common law definition of rape that was in the Uniform Code of Military Justice (UCMJ) when it was passed in 1950 remained law through the start of the next century. Frustrated with the military's response to the problem of sexual assault within its ranks, Congress replaced that statute with a reform model that took effect on October 1, 2007. The new statute focused on the force used by the offender and removed "without consent" as an affirmative legal element. Now, the government did not have to prove lack of consent in its case-in-chief.

That intervention is the subject of this quasi-experimental study. With data obtained from the U.S. Army and using time-series analysis, we look for evidence that this intervention had an effect on the investigative decisions made by Army law enforcement. Learning if this change affected case processing is important. If it did not, Congress may need to revisit the substantive law to make further reforms or look to other areas, such as the training and

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certification of law enforcement officers, to improve case processing. Further, many states still have common law statutes, and legislatures looking to reform those laws need to know if those changes will achieve the desired effects.

The Rationale for Reform Efforts

Reformers were motivated to reform the law for complementary reasons. One was to hold more offenders accountable, and another was to change societal perceptions of rape and rape victims that are shaped by rape myths (Bachman & Paternoster, 1993). Rape myths are attitudes and beliefs about rape that are generally false, are widely and persistently held, and justify male aggression against women (Lonsway & Fitzgerald, 1994). Rape myths include beliefs that only deviant men rape, that men cannot control their sexual urges, that women who were raped wanted it or deserved it, that women lie about rape, that no harm was done, or that certain events do not qualify as "real" rape (Payne et al., 1999). The image of a "real" rape case involves a stranger who uses force and surprise to overwhelm the victim.

According to reformers, if law enforcement officials and prosecutors hold these beliefs, then they might drop the "consent defense" cases (Bryden & Lengnick, 1997). Consent defense cases are those in which an offender can plausibly say that the victim consented, such as when the offender and victim were friends, dates, coworkers, classmates, intimate partners, or the like. Consistent with this argument, research has shown that sexual assaults that look like "real" rape are more likely to make it through the legal system. These include cases in which the victim is physically injured or a weapon is used, the evidence against the suspect is strong (where the strength of the evidence is often measured by the victim's willingness to participate and the availability of other witnesses and forensic evidence), or the victim did not engage in risky behavior (Beichner & Spohn, 2012; O'Neal, 2019). In these circumstances, the victim looks like a "real" victim and appears more credible (Page, 2008; Spohn et al., 2001). When these factors are absent or when the victim engaged in risky behavior, made inconsistent statements, or has memory problems, then law enforcement and prosecutors may think that the victim is not credible (Campbell et al., 2015; Cuevas et al., 2018; O'Neal, 2019). Law enforcement and prosecutors then might not pursue the case (Maddox et al., 2012; Quinlan, 2016).

Reformers recognized that the common law legitimized and incorporated these beliefs into procedural rules, evidentiary rules, and the substantive law (Caringella, 2009). For example, procedural rules required prompt complaints, evidentiary rules required the victim's testimony to be corroborated and allowed opinion and reputation testimony about the victim's character for chastity, and the substantive law contained spousal exceptions so that husbands could not be convicted of raping their wives. Almost all jurisdictions, including the military, have reformed these rules and enacted rape shield laws. Rape shield laws prevent the defense from introducing evidence of the victim's previous sexual acts or sexual predisposition unless certain conditions are met.

Reformers also noted that under the common law, the legal elements of the crime of rape incorporated rape myths (Estrich, 1986). The elements of common law rape are penile–vaginal intercourse by a man of a woman, without her consent, and by force. Because consent is written into the statute, the government has the burden of producing evidence during its case-in-chief of the victim's mental state. This directs attention during direct and cross-examination on areas influenced by rape myths: Did she want it and so really did consent? Is she lying because she needs to explain away infidelity, or has regrets, or is a woman scorned? Under the common law, the focus is on the victim's mental state, not the offender's mental state.

Next, under the common law, lack of consent is not enough for rape. A woman could plainly say "no," but if the man did not apply a certain level of force in addition to that refusal, no rape occurred. Force is then defined by how much the woman resists (in some jurisdictions, utmost resistance; in others, such reasonable measures of resistance as required by the circumstances; Caringella, 2009; Estrich, 1986). Note, if the government does not introduce evidence that the victim fought back, then whether the victim did not consent is of no consequence—the government would still not be able to prove the elements of rape. Her "no" without more is treated by the common law as "yes, unless she fights."

Under the common law, even if the prosecution proves that the victim did not consent, the defendant can still attack this element by claiming that he honestly and reasonably believed that she consented, which is known as a *mistake of fact defense*. Moreover, through the definition of force, the common law already defined when it is reasonable for a man to be mistaken: If a woman does not fight back, then it is reasonable for a man to be mistaken because she has not adequately communicated "no" (Estrich, 1986). In the mistake of fact defense, the defendant has the initial burden of producing evidence that he honestly and reasonably believed that the victim consented. If such a defense is raised, then the government has the burden to disprove it beyond a reasonable doubt.

The Definitional Intricacy of the Substantive Law Reforms

Reforms related to the legal definitions of sexual assault have not been universally adopted. About one-third of U.S. jurisdictions have retained the common law definitions (while expanding the definitions of penetration and dropping the male-on-female requirement), but about two-thirds have adopted reform models (Schulhofer & Murphy, 2017). The differences between the pre- and post-intervention laws are intricate, and because this study attempts to measure the impact of those differences, they require close examination.

The reform models tend to define consent the same way. Instead of defining consent by how much the victim resists, the reforms define consent such that it is a freely given agreement to the sexual conduct (Schulhofer & Murphy, 2017). Where before the law presumed that a victim consented until she fought back, now the law presumes that the victim has not consented until she says so by her words or actions.

The reforms tend to fit into three models: the force-centric model; a variation of that model; the assault-plus model; and the consent-centric model. The main difference in the reforms is the choice to drop the consent element or to drop the force element. In the force-centric model, the legislature drops the consent element and focuses on the force used by the offender, which is how most jurisdictions draft regular assault statutes. Further, force is no longer defined in terms of the victim's resistance. In this model, the prosecution no longer has the burden of producing evidence in its case-in-chief of

the victim's lack of consent, so the focus shifts to what the offender has done. However, the defense can still allege that the victim consented (if the victim consented, there was no force), and once the defense does, the government has to prove lack of consent beyond a reasonable doubt. As a result, the reform affects only who has the burden of producing evidence about consent (now, the defense) and when it will be raised (now, in the defense's case-in-chief rather than in the government's). Further, in this reform model, the defense can still raise the mistake of fact defense as to consent.

A variation of the force-centric model is the assault-plus model. This model starts with assault as the baseline offense and then adds sexual contact as an aggravator to the underlying assault. This model differs from the force-centric model by naming convention only, where the name change serves to focus attention on the assaultive nature of the crime.

The consent-centric model drops the force element from the primary offense. A penetrative act without consent is sufficient for a baseline conviction. From there, degrees of force or other aggravating conditions increase the severity of the crime. In this model, the legislature has decided that the public expression about the value of a woman's sexual autonomy outweighs the benefit of forcing the prosecution to focus on the offender's conduct in its case-in-chief. Once the government introduces evidence that she said "no" through words or actions, it can rest. The defense can still attack the government's burden of proof by offering evidence that the victim consented, and the defense can raise the mistake of fact defense as to consent. If the government tries to prove force to get a conviction of a more serious offense, then this model is very similar to the common law for that more serious offense.

To sum up, the differences between the models are slight. The definitions of consent are generally the same. The difference between the common law and the force-centric model is just timing. In the common law, the government must present evidence of consent in its case-in-chief. In the force-centric model, the defense raises it in its case-in-chief. In both, the burden of proof is still on the government. The difference between the common law and the consent-centric model is that the government can get a conviction for the baseline offense without having to prove force.

Further, both the common law and the reform models contain normative words. Normative words include *reasonable*, *should*, *fair*, *due*, *called for by the circumstances*, *sufficient*, *necessary*, *foreseeable*, and *offensive*. The law does not define these terms in meaningful ways. Instead, law enforcement officials, prosecutors, judges, and jurors are expected to decide when these words are satisfied by using their life experiences and values. In all of the models, consent (and whether the victim is telling the truth about lack of consent) is central, and jury instructions commonly tell jurors to evaluate the *reasonableness* of the witness' testimony. When deciding whether someone is telling the truth, legal actors rely on beliefs about how people behave so they can spot where the witness is testifying consistently with those expectations (and so is telling the truth) or inconsistently (and so is mistaken or telling a lie; Devine, 2012). Further, in all of the models, the mistake of fact defense (raising an honest and *reasonable* mistake) is available, and the inquiry into whether the defendant could reasonably interpret the situation for consent is central. In the reform models, the credibility and behavior of the victim are still on trial, and rape myths may shape how legal actors judge that credibility and behavior.

Measuring Reform Effects

There is still no clear answer on whether legal reforms affect case processing by law enforcement officers. Marsh et al. (1982) looked at rape reporting and processing statistics in Michigan (a force-centric model) for 1972 through 1978. Using time-series analysis that compared trends in rape processing with those in murder, aggravated assault, and robbery, they found no relationship between an upward arrest trend and the change in the law. Horney and Spohn (1991) looked at data from multiple jurisdictions (Detroit, Chicago, Philadelphia, Atlanta, Houston, and Washington, DC) that had varying degrees of reform on various dates. Using time-series analysis, they found that in Michigan, the change in law appeared to have a positive effect on the ratio of indictments to reported cases, but rape reforms did not appear to have any effect in the other jurisdictions. Horney and Spohn (1996) then looked at the reforms in 1974 to the Michigan system using randomly selected files from Detroit for 1970 through 1984. They used a before-and-after design and found that more simple rape cases were bound over for trial after the reform than before.

Analyzing the 1983 reforms to the Canadian system (an assault-plus model that includes consent as an element), Roberts and Gebotys (1992) looked at nationwide data for 1979 through 1988. Using time-series analysis, they reported that unfounded rates did not change (where *unfounded* meant that a crime did not take place), and the change in rates for clearance by charge (meaning the police charged an offender) was matched by changes in nonsexual assault cases. Schissel (1996) also used nationwide data from Canada for 1969 through 1991 to evaluate the reforms. After running time-series regression models, Schissel concluded that the change had little impact on the prosecution of sexual assault cases.

Looking at the state of the research, many have concluded that legal reforms have been ineffective: The laws change, but the attitudes of those who execute the laws do not (Corrigan, 2013; Frohmann & Mertz, 1994). Ajzenstadt and Steinberg (2001) noted that the effectiveness of any legal reform depends on how that law is enforced and that there is considerable room within the law for a law enforcement official's values and norms to enter. If those values and norms remain unchanged, then the formal legal change will not affect case processing.

The Military's Legal Intervention

Before October 1, 2007, the military's sexual assault scheme included common law rape, sodomy, and indecent assault. The elements of rape (Article 120, UCMJ) were the same as those found in the common law. Although the rape statute only included penetration of the vagina by the penis, the forcible sodomy statute (Article 125, UCMJ) covered other penetrative offenses. The elements of sodomy were "unnatural carnal copulation" with another person, done by force or without the consent of the other person. Because of the "or," this crime could operate as a force-centric or consent-centric offense. "Unnatural carnal copulation" included placing a person's sexual organ into the mouth or anus of another or taking into the mouth or anus the sexual organ of another. The mistake of fact defense was available.

Nonpenetrative sexual assaults were criminalized as "indecent assaults" under UCMJ Article 134, with these listed elements: an assault of a person, not the spouse; with the intent to gratify the lust

or sexual desires of the offender; and the conduct was to the prejudice of good order and discipline in the armed forces or of a nature to bring discredit upon the armed forces. This was an assault-plus offense. Nonconsent was not an explicit element of the offense, but the defense could raise the element of consent, and issues related to consent were generally treated the same as they were with rape, with the mistake of fact defense available.

This overall scheme focused attention on the victim's behaviors rather than the offender's behaviors, so Congress replaced it on October 1, 2007, with a force-centric scheme, roughly based on Michigan's statute. Now, rape was a penetrative sexual act with a high degree of force or aggravating factor, aggravated sexual assault was a penetrative sexual act with a lower degree of force or aggravating factor, aggravated sexual contact was a nonpenetrative act with a high degree of force or aggravating factor, and abusive sexual contact was a nonpenetrative act with a lower degree of force or aggravating factor. This scheme also included wrongful sexual contact, which was nonpenetrative sexual touching without permission and fit within the consent-centric model. The definition of a penetrative act was broadened from the common law to include contact between the penis and vulva and the penetration, however slight, of the genital opening of another by a hand or finger or by any object, if done with an intent to abuse, humiliate, harass, or degrade any person or to arouse or gratify the sexual desire of any person. Forcible sodomy remained on the books and continued to cover the penetration of the mouth or anus.

If an offense occurred before October 1, 2007, it had to be processed under that legal scheme. If an offense occurred on or after October 1, 2007, it had to be processed under the new scheme. The full definitions of each offense, before and after the change, are available in the *Manual for Courts-Martial* (U.S. Department of Defense, 2016).

As with the civilian reforms, on close inspection, the differences between the models are slight. Consent was not an express element of the reformed offenses (other than wrongful sexual contact), but the defense could still raise the element of consent, and the mistake of fact defense was available. The credibility of the offender and the victim were still central. Importantly, the reformed statute included numerous normative words: An offender's threat or action had to *overcome or prevent another's resistance*; restraint had to be *sufficient* such that the other person could not escape the sexual conduct; a threat had to be of *sufficient* consequence to cause a *reasonable* fear; touching had to be *offensive* (Article 120, UCMJ). Because the differences between the common law model and the reformed model are slight, and because both models contain similar normative words, it is not likely that the reform affected case processing.

Investigative Findings

The focus of this study is the investigative finding made by Army law enforcement. If an offense is reported to law enforcement, law enforcement officers should investigate and then label the case. In the civilian system, the labels are *founded*, meaning probable cause exists to make an arrest, or *unfounded*, meaning that a thorough investigation determined the report to be false or baseless, and meaning no crime occurred (U.S. Department of Justice, 2004). The label reflects law enforcement's belief about the strength of the case, and suspects in unfounded cases are not arrested or forwarded for prosecution.

In the Army, law enforcement officials use the labels *founded*, *insufficient evidence*, and *unfounded*. The Army defines *founded* as indicating probable cause supported by corroborating evidence (U.S. Department of the Army, 2016); *insufficient evidence* denotes investigators' inability to determine whether or not an offense occurred or to establish probable cause (Ham, 1998; U.S. Department of the Army, 1994); and *unfounded* indicates that a report was determined through investigation to be false or baseless, meaning no crime occurred (U.S. Department of Defense, 2010a). The Army's formal use of the term *insufficient evidence* is not legally significant. Both the *unfounded* and *insufficient evidence* labels indicate that military law enforcement officers do not think that the important legal threshold of probable cause was met.

Overview of Present Study and Hypothesis

This is a quasi-experimental study that used time-series analysis to measure the effect of a legal interruption (the 2007 change in the military's sexual assault law) on the investigative findings made by Army law enforcement officers in sexual assault cases. This study looks at the law enforcement investigative finding for every sexual assault that was reported and recorded in the U.S. Army from 2004 through June 2012, along with a control group consisting of every nonsexual assault during that same period. We were able to divide the sexual assaults into penetrative and nonpenetrative and the assaults into aggravated and simple.

When doing before-and-after studies of abrupt interventions, one problem is that even if a change is detected after the intervention, the abrupt intervention may not have caused the change; instead, other external, underlying, longer term trends may have caused the change (Lopez Bernal et al., 2018). The percentage of consent defense cases (those most likely to be influenced by rape myths) may have gone up or down over time. Or, increased public attention on sexual assault issues might have made criminal justice officials more alert to these issues, and that may have led to observed changes in processing (Horney & Spohn, 1991). Increased law enforcement training, personnel, and budgets could lead to better investigations. For the military, several policy changes were undertaken during this period of this study that may have impacted how Army law enforcement treated cases. The Department of Defense began specialized training in sexual assault investigations in 2009 (U.S. Department of Defense, 2010b). Further, starting in 2010, Army law enforcement began using specialized interview techniques with victims, although training on those techniques did not become mandatory until 2014 (Cuevas et al., 2018; U.S. Department of Defense, 2014). In addition, starting in 2009, the Army established a program of special victims prosecutors. These specially trained prosecutors would interact with Army law enforcement during the investigation of the cases (U.S. Department of Defense, 2010b). These policy changes should not have had abrupt impacts; instead, they should have had longer-term impacts as the policies were implemented over time.

If the intervention had an effect, we should expect an abrupt and permanent increase in founded sexual assault cases as compared to the control groups, once longer term trends are accounted for by the time-series models. Prior research does not provide much guidance for forming a hypothesis, so we formed our hypothesis based on the

analysis of the legal elements. Because the differences between the common law model and the reformed model are slight, and because both models contain similar normative words, we hypothesized that we would not find evidence that the legal intervention affected the founding decisions made by Army law enforcement. Thus, for the best-fitting time-series models for the three different comparisons (sexual assaults combined vs. nonsexual assaults combined, penetrative sexual assaults vs. aggravated assaults, and nonpenetrative sexual assaults vs. simple assaults), any difference in the means of the residuals before and after the intervention would not be statistically significant.

Method

Data

We sought data from the U.S. Army through a request under the Freedom of Information Act (FOIA) for every sexual assault and nonsexual assault committed by a known military offender[1] on an adult victim that was reported and recorded in the Army from January 1, 2004, through June 30, 2012. Congress modified the sexual assault statute somewhat, to take effect on June 28, 2012, so we excluded the last half of 2012 from the study. The data set came from information entered into the Army's Centralized Operations Police Suite Military Police reporting system. The Army collects the data from the military police report using Department of the Army Form 3975. This record captures the initial report of the incident. Army law enforcement investigators then report their findings (founded, insufficient evidence, and unfounded) to this database (U.S. Department of Defense, 2011).

The data include only unrestricted reports of sexual assault. Since at least June 30, 2005, the Department of Defense has allowed victims to make restricted reports, which give them access to victim services but are not transmitted to law enforcement or the chain of command (U.S. Department of Defense, 2005). Restricted reports are not included in military police reports or in this data set. To get a sense of scale, in fiscal year 2011, there were 2,439 unrestricted reports in which either the victim or the offender was in the military, compared to 753 restricted reports (U.S. Department of Defense, 2012).

The data have 47,058 observations. The data include the following values: unique case file number; report date; subject rank; investigative office;[2] offense code that is published in an Army regulation (U.S. Department of the Army, 2007); and an abstract, short, legal title of the offense. Some unique case files had multiple offenses, including both sexual and nonsexual assaults. These could have been from the same event or from discrete events over time. We kept all of these observations and analyzed each discrete offense code. Almost all of the offenders (44,867, or 95%) were junior to midlevel enlisted service members (E1–E6).[3] Beyond the title of the offense, the data did not include any concrete details about the crime. Citing a FOIA exemption, the Army did not disclose any information on the subjects or victims, so there are no other potential explanatory variables such as the race, sex, or age of either the offender or the victim. Other Department of Defense reports show that the offenders are almost always men, and the victims are overwhelmingly women. For fiscal year 2007, for example, within the unrestricted reports, the offenders were 99% men, and the victims were 88% women (U.S. Department of Defense, 2008). Although the population of interest is known military offenders, the victims in the data could be either civilian or military. The data are described in Table 1.

Analytic Strategy

To address underlying long-term trends, time-series analysis of an abrupt intervention is widely used to isolate the impact of policy interventions (Bouffard & Askew, 2019; Maurelli & Ronan, 2013; Ren et al., 2015). Time-series analysis is an analytical technique that is used to examine whether there was a statistically significant trend before and after an intervention (Tabachnick & Fidel, 2019). In time-series analysis, the statistical model "pre-whitens" the data by accounting for variation that is dependent on prior observations and is likely due to long-term, ongoing processes that also occurred during the observed period (Bouffard & Askew, 2019; Cochran et al., 1994). Once the data are pre-whitened, researchers can analyze the residuals within the model before and after the date of the intervention to see if the intervention affected those residuals. Here, the legal intervention occurred on October 1, 2007.

In this study, we also use a comparison group (nonsexual assault cases) to help control for general changes in law enforcement practices or views that may have affected all crimes, including sexual assault (Bouffard & Askew, 2019), and to help control for other abrupt interventions that may have occurred at or near the same time as the change in the military's substantive sexual assault law (Lopez Bernal et al., 2018). Assault cases were chosen as the comparison cases because (a) previous research (Carpenter, in press) showed that in the Army, assaults (including sexual assaults) comprise the vast majority of violent offenses (92% for sexual assaults and assaults, compared to 8% for murder, manslaughter, and robbery combined); (b) the contribution of these other offenses would be subsumed by the data from the assaults; (c) trends in case

[1] The data included a small number of civilian offenders. Because those offenders would not fall under military jurisdiction for prosecution, those cases would be transferred to civilian investigators and prosecuted under civilian statutes, not the military statute that is the subject of this study. Therefore, we excluded this population. The data also included unidentified offenders (n = 6,455). Army law enforcement treated this population differently: The founding rates for all offenses (assaults and sexual assaults) were much lower for the unidentified offenders than for the identified offenders (17%–40%). This caused us to question the reliability and validity of those data. Possibly, this is because Army law enforcement does not use *cleared by exceptional means*, which is a category used by civilian law enforcement to clear a case when an offender cannot be arrested. Army law enforcement might be incorrectly using *insufficient evidence* and *unfounded* as proxies for *cleared by exceptional means*. Because this population was being treated differently, we excluded these observations.

[2] The data included more than 240 investigative offices. A few large installations had enough observations to model, but most of the offices did not. Further, we could not consolidate the offices such that the result would validly measure a variable of interest. Therefore, we did not model the investigative offices.

[3] We broke the ranks into *junior enlisted* (E1–E4), *middle enlisted* (E5–E6), *high enlisted* (E7–E9), *junior officer* (O1–O3, W1–W2), and *senior officer* (O4–O10, W3–W5). There were no meaningful differences between the rank categories and the investigative findings. There were not enough high enlisted, junior officer, or senior officer cases to model over time to see if the legal intervention affected the processing of service members of different ranks differently. Junior enlisted and middle enlisted made up almost all of the data set.

Table 1
Investigative Findings by Case Category, Before and After the Legal Intervention

Investigative finding by case category	January 2004 through September 2007		October 2007 through June 2012	
	N	(%)	*N*	(%)
Sexual assaults (combined)				
Founded	2,963	75	8,509	83
Insufficient evidence	537	13	421	4
Unfounded	461	12	1,334	13
Penetrative sexual assaults				
Founded	1,403	67	4,960	79
Insufficient evidence	362	17	314	5
Unfounded	319	15	982	16
Nonpenetrative sexual assaults				
Founded	1,560	83	3,549	88
Insufficient evidence	175	9	107	3
Unfounded	142	8	352	9
Assaults (combined)				
Founded	13,061	99	19,078	97
Insufficient evidence	59	<1	216	1
Unfounded	128	1	291	1
Aggravated assaults				
Founded	1,821	97	3,260	97
Insufficient evidence	22	1	13	<1
Unfounded	33	2	74	2
Simple assaults				
Founded	11,240	99	15,818	97
Insufficient evidence	37	<1	203	1
Unfounded	95	1	217	1

Note. Percentages may not equal 100 due to rounding. The legal intervention took place on October 1, 2007.

processing that were not unique to the change in sexual assault law would likely be revealed in the large volume of assault cases; and (d) creating models for murder, manslaughter, and robbery would defeat parsimony in the research design. We used these cases to control for external trends in crime processing, not to serve as a basis for counterfactual reasoning.

We compared sexual assault to assault, where *sexual assault* included penetrative and nonpenetrative sexual assaults and *assaults* included aggravated and simple assaults. We also made more refined comparisons. We compared penetrative sexual assaults to aggravated assaults and nonpenetrative assaults to simple assaults to control for external trends that may have affected more serious offenses differently than less serious offenses or vice versa. For the period before October 1, 2007, *penetrative sexual assault* included rape and sodomy, and *nonpenetrative sexual assault* included indecent assault. For October 1, 2007, and beyond, *penetrative sexual assault* included rape and aggravated sexual assault, and *nonpenetrative sexual assault* included aggravated sexual contact, abusive sexual contact, and wrongful sexual contact. For both periods, *aggravated assault* included assaults carried out with a dangerous weapon, assaults in which grievous bodily harm was intentionally inflicted, and maiming. *Simple assault* included demonstrations of violence that create a reasonable fear of bodily harm and assault consummated by battery that does not involve a dangerous weapon or the intentional infliction of grievous bodily harm.

We standardized the sexual assaults (combined, penetrative, and nonpenetrative) and assaults (combined, aggravated, and simple) by creating ratios of the percentage of the founded cases of sexual assault cases to the founded cases of nonsexual assault cases for each period (Huckle et al., 2020; Males, 2007). If the change in the law had the desired effect of increasing the founding rates for sexual assault cases, that ratio should move to the value of 1 as the sexual assault founding rates moved closer to the assault founding rates, accounting for external factors that might cause the same change in trends for both. We converted the data to a monthly format with a total of 102 months. We then tracked those ratios over the 102 months using time-series analysis.

Results

Simple Before-and-After Comparisons

Looking first at simple before-and-after comparisons, it appeared that more sexual assaults (combined, penetrative, and nonpenetrative) were founded after the intervention, and this increase appeared to come from reduced use of the label *insufficient evidence* (see Table 1). Next, we introduced the control groups by creating ratios of the percentage of founded sexual assault cases to the percentage founded assault cases, the percentage of founded penetrative sexual assaults to the percentage of founded aggravated assaults, and the percentage of founded nonpenetrative sexual assaults to the percentage of founded simple assaults. We conducted two-tailed *t* tests of the ratios before and after the intervention and display the results in Table 2. We display the ratios over time in Figures 1–3, respectively. (The vertical line in the figures represents October 1, 2007.) The ratio of founded sex assault cases to founded assault cases appears to have increased after the legal intervention. However, none of these comparisons can tell us statistically whether the observed changes were due to this abrupt intervention or instead were due to longer term, ongoing processes (Bouffard & Askew, 2019). For that, we turn to time-series modeling.

Time-Series Models

We modeled the ratios of the percentage of founded sexual assault cases to the percentage founded assault cases, penetrative sexual assault cases to aggravated assault cases, and nonpenetrative sexual assault cases to simple assault cases.[4] We checked each time-series model with the diagnostics necessary to confirm that the model assumptions were not violated. We used partial autocorrelation (PACF) for the ratio of the percentages of founded cases for each group comparison to identify lags in the plots. We developed time-series predictive models such as autoregression, moving average (MA), autoregressive moving average, and autoregressive integrated moving average (ARIMA). We used automatic selection of ARIMA function in R to identify the best model fit, using PACF and model fit statistics (Akaike information criterion [AIC], Bayesian information criterion, and corrected AIC) as criteria. The best-fitting predictive models for each group comparison were sexual assault versus assault, ARIMA (0, 1, 1), MA = −0.643, *SE* = 0.094; penetrative sexual assault versus aggravated assault, ARIMA (0, 1, 1),

[4] We were not able to do time-series analysis of the *insufficient evidence* label. There were not enough observations over time within each month to allow for robust statistics. Further, the control groups had essentially no insufficient-evidence observations, so we would not have been able to control for external long-term trends or concurrent abrupt interruptions.

Table 2
Analysis of Ratio of Proportions of Founded Cases Before and After the Legal Intervention

Case category	January 2004 through September 2007	October 2007 through June 2012	*t* (*df*)	*p*	Cohen's *d*	95% CI
SA:A	.76	.85	−8.87 (100)	<.001	1.77	[1.30, 2.21]
PSA:AA	.69	.81	−7.62 (69.5)	<.001	1.60	[1.14, 2.04]
NPSA:SPLA	.84	.91	−5.39 (100)	<.001	1.07	[0.65, 1.48]

Note. For the SA:A and NPSA:SPLA ratios, variances were equal, and a *t* test was used. For the PSA:AA ratio, variances were not equal, and Welch's *t* test was used. The legal intervention took place on October 1, 2007. CI = confidence internal; SA = sexual assault cases combined; A = assault cases combined; PSA = penetrative sexual assaults; AA = aggravated assaults; NPSA = nonpenetrative sexual assaults; SPLA = simple assaults.

MA = −0.648, *SE* = 0.092; and nonpenetrative sexual assault versus simple assault, ARIMA (0, 1, 1), MA = −0.825, *SE* = 0.057.

We used the augmented Dickey–Fuller *t* test on the residuals for each respective best-fitting model to find if the series had a unit root (i.e., a test of stationarity). For the three case category comparisons, residuals were stationary. We also used the Ljung–Box test (a test of independence among the residuals) to determine whether autocorrelations existed in the time series. For all three best-fitted models, the residuals were independent. We split the residuals of each model into two groups, before and after the intervention, and used Levene's test and Bartlett's test to assess the equality of variances for the residuals of both case categories for each of the three models. For the sexual assault (combined) versus assaults (combined) model and the nonpenetrative sexual assault versus simple assault model, the variances between the case categories were equal before and after the change. For the penetrative sexual assault versus aggravated assault model, the variances were unequal.

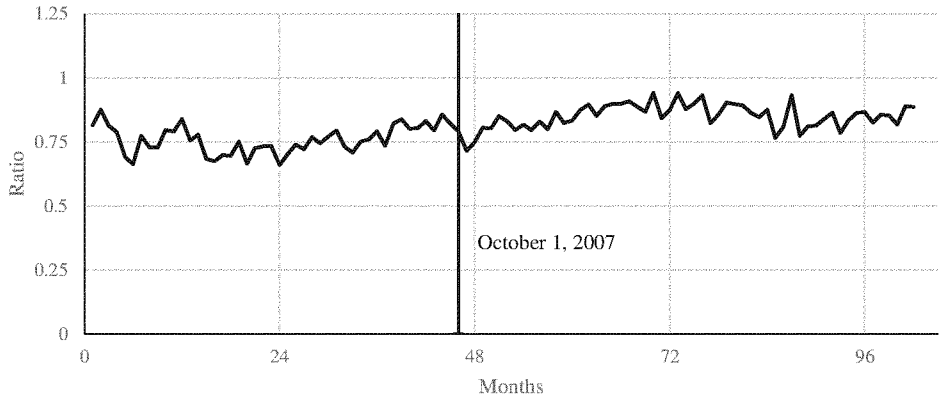
With the data now pre-whitened, we analyzed what was left. We used *t* tests to determine whether the means of the residuals before the legal intervention were significantly different than the means of the residuals after the intervention. Because the penetrative sexual assault versus aggravated assault model had unequal variance, we used Welch's *t* test for this model. The results are displayed in Table 3. For all three models, because the *p* value is greater than .0167 (corrected α), the differences in the means of the residuals before and after the modification of the law are not statistically significant. We then conducted a post hoc power analysis to identify the ratio and residual differences necessary to reject the null, and the methods we used had sufficient power to detect even a single-percentage-point change in the original case founding ratios (after autocorrelative effects were removed).

Discussion

The results support our hypothesis. For the three models, there was not a statistically significant difference in the means of the residuals before and after the change in the law. Thus, we did not find evidence that suggests that the law affected Army law enforcement's labeling of these cases. This finding is consistent with previous research that has not detected a processing change following a legal intervention in sexual assault cases and is consistent with the conclusions drawn by others that sexual assault legal reforms have been ineffective because the laws change but the attitudes of those who execute the laws do not (Ajzenstadt & Steinberg, 2001; Corrigan, 2013; Frohmann & Mertz, 1994). Here, this could be because the legal reform was modest, and the reformed law contained normative words that could allow for the entry of rape myths into the problem-solving process.

The means ratios did increase for sexual assaults (combined), penetrative sexual assaults, and nonpenetrative sexual assaults, meaning more sexual assaults were founded relative to assaults. The time-series analysis suggests that other longer term trends—possibly other policy changes related to training or resource allocation—are the likely reason for the increase. The gains in this period were modest,

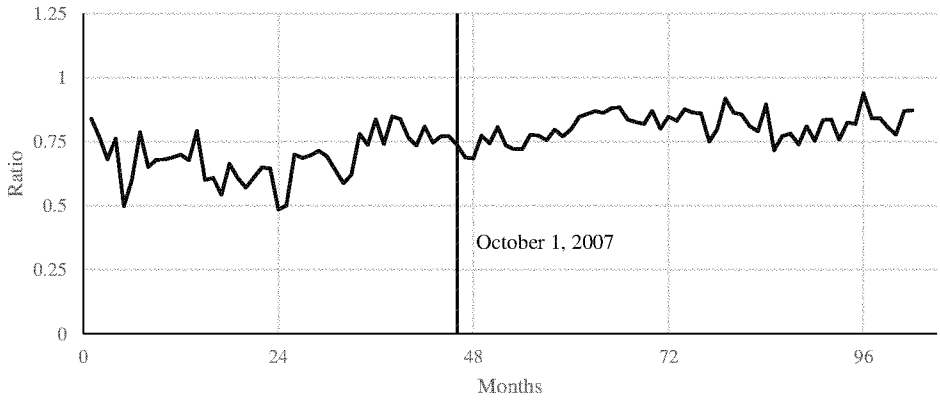
Figure 1
Ratio of Proportions of Founded Sexual Assault Cases (Combined) to Assault Cases (Combined)



Note. Sexual assault cases (combined) consist of penetrative and nonpenetrative sexual assault cases. Assault cases (combined) consist of aggravated and simple assaults.

Figure 2

Ratio of Proportions of Founded Penetrative Sexual Assault Cases to Aggravated Assault Cases



though, and throughout the observed period, there was a significant gap between the rate of founded sexual assaults and the rate of founded nonsexual assaults. Research using more recent data suggests that this gap still exists and may have widened (Carpenter, in press). If changing norms and values brought about by improved training account for the increase in the means ratio observed from 2004 to 2012, that effort appears to have stalled.

Limitations

We limit our results to saying that the models did not detect evidence that the legal intervention had an effect, rather than saying that there was no effect. Although our study has strengths (we analyzed an entire population rather than a sample, and our power analysis indicated that our models could detect small differences in the residuals before and after the legal intervention), and although other researchers using similar methods have found effects from legal interventions (Cunningham et al., 2015; Delcher et al., 2015; Friedman et al., 2007; Males, 2007; Maurelli & Ronan, 2013; Ren et al., 2015), there is a possibility that our models might not have been able to detect an effect when there was one.

This study is limited by the nonexperimental design and a data set with limited variables, and so only provides evidence that is consistent with there being no causal relationship between the intervention and case processing. Of two variables that we did have—rank and installation—we did not have enough observations to model whether the legal intervention impacted some ranks and some installations but not others. We were also not able to model the insufficient-evidence label. The data in Table 1 show that Army law enforcement used that label with less frequency after the intervention, but we do not know if that was due to the legal intervention or to some other abrupt intervention or long-term trend. In addition, we used data from the U.S. Army, and the results do not support an inference that the change in law did not affect case processing in the other services. That said, the Army is the largest service. To get a sense of scale, in fiscal year 2009, the Army had 1,658 unrestricted reports, the Navy had 451, the Marine Corps had 338, and the Air Force had 311 (U.S. Department of Defense, 2010b).

We do not know what percentage of these cases were consent defense cases (more likely to be affected by rape myths) versus stranger rape cases (less likely to be affected). If most of these cases were stranger rape cases, that might mask the effect that the legal intervention had on the processing of consent defense cases. Although we do not know the percentage in this data set, the Defense Advisory Committee on Investigation, Prosecution, and Defense of Sexual Assault in the Armed Forces (2020) reviewed and

Figure 3

Ratio of Proportions of Founded Nonpenetrative Sexual Assault Cases to Simple Assault Cases

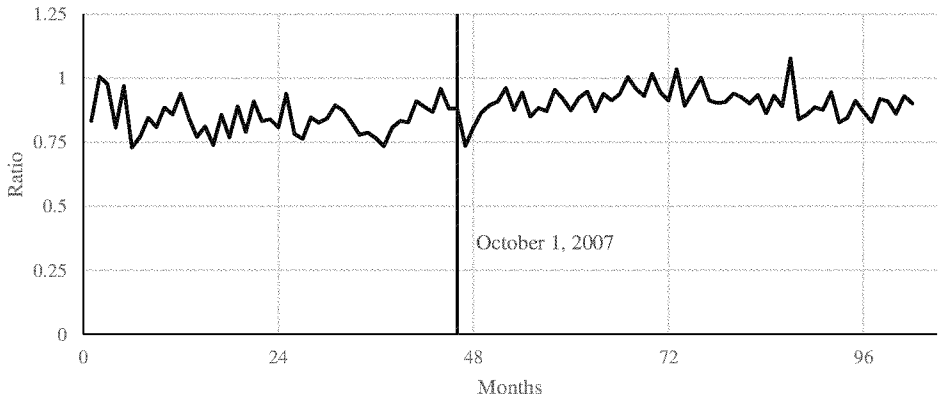


Table 3
Analysis of the Means of the Residuals Before and After the Legal Intervention

Model	January 2004 through September 2007	October 2007 through June 2012	*t* (*df*)	*p*	Cohen's *d*	95% CI
SA versus A	0.00008	0.00214	−0.234 (100)	.82	0.05	[−0.34, 0.44]
PSA versus AA	−0.00225	0.00378	−0.436 (68.3)	.66	0.09	[−0.30, 0.48]
NPSA versus SPLA	−0.00230	0.00268	−0.409 (100)	.68	0.08	[−0.31, 0.47]

Note. αs (0.05) were adjusted using Bonferroni correction due to the multiple comparison of equality of means. Welch's *t* test was used for the PSA versus AA comparison. CI = confidence interval; SA = sexual assault; A = assault; PSA = penetrative sexual assault; AA = aggravated assault; NPSA = nonpenetrative sexual assault; SPLA = simple assault.

coded the investigative files from the penetrative sexual assault cases for fiscal year 2017. In the Army, 10% were stranger cases, whereas 85% were consent defense cases (31% former or current spouse or intimate partner; 41% friend, acquaintance, or first date; 13% coworker, classmate, roommate, subordinate or superior, recruit or recruiter). The remainder were other or unknown. Consistent with 85% of the cases being consent-defense cases, 86% of the victims reported no force used and 87% reported no injuries, and for those who reported injuries, bruising and redness accounted for almost all of the injuries.

Including forcible sodomy in the study could be problematic if most of those cases were same gender assaults and bias against lesbian, gay, bisexual, transgender, queer or questioning, and other gender and sexually diverse (LGBTQ+) service members affected case processing. During the entire study period, the forcible sodomy statute was used for assaults on men and for assaults on women when penetration was oral or anal, and we could not split the male victims out from the female victims in the sodomy cases. The law that prevented LGBTQ+ service members from serving openly was repealed near the end of this study period, on September 20, 2011. If there was a disproportionate increase in reporting based on lifting that ban but there was additional bias when processing such cases, then that could mask other changes. However, a review of the Department of Defense's annual reports from fiscal year 2007 through 2012 showed that forcible sodomy cases in the Army were a low percentage of total sexual assault cases, ranging from 5% to 8% of all sexual assault cases (or roughly 90 a year). Males were victims in 32% to 66% of those cases, with large swings due to the low number of total cases. Almost no offenders were women, so there were almost no reported female-on-female assaults. Although the number of male-on-male assaults was small relative to the entire data set and the repeal was late in the study period, we do not know if the repeal masked other changes.

Implications for Future Research

We were able to analyze whether the legal intervention affected one processing point: the investigative finding. We do not know if it had any effect on downstream processing points, such as prosecutorial decisions, court-martial guilt phase outcomes, or sentencing phase outcomes. Time-series analysis of those decision points may show that the law had an impact, although all of those processing steps will have been influenced by the quality of the initial investigation. To start, Army law enforcement officials cannot technically drop a case—they must send every case forward for a prosecutorial decision. In 2011, the Department of Defense reformed the process so that the investigative reports that were forwarded for a disposition decision would no longer include the investigative findings within the reports. This was to ensure that those findings would not bias those who might make decisions on the case later. However, if military law enforcement officers think a case is weak, then they might compile weak files. When they forward those weak files, the official making the prosecutorial decision might deem these cases as weak and drop them, and military judges and panel members who sit in judgment may acquit or give light sentences.

We also do not know if the change in law affected victim reporting. The rates of both sexual assault reporting and nonsexual assault reporting increased significantly during this period in a linear fashion, with no obvious increase after the intervention. This research question may not be appropriate for the specific time-series analysis used in this study. Theoretically, researchers should not be able to detect an abrupt change in victim reporting related to the legal intervention. Victims are unlikely to know about the change or the date of the change. Instead, we should expect a longer term process. First, law enforcement officers, prosecutors, judges, and jurors would need to change their behavior based on the change in the law. Then, there would need to be a feedback loop such that victims would learn that the legal system was now operating in a more victim-friendly manner. The dynamics that influence victim reporting behavior are significantly different than the dynamics that are central to this study (those that influence law enforcement investigative finding behavior), and learning why both sexual assault and nonsexual assault reporting increased during this study period may help policy makers reduce barriers to reporting.

Implications for Policy

The 2007 changes to the substantive law do not appear to have had the reformers' desired effect, and the opportunity for further reform to the substantive law appears to be limited. Congress cannot reform one of the elements of the law that invites the use of rape myths—the element of consent and the related inquiry into the victim's credibility. All Congress can do is assign who has the burden of raising the element. However, Congress can reform another area: the mistake of fact defense.

In the 2007 reform, Congress attempted to reform the military's mistake of fact defense but did so by modifying the burden of production required by the defendant to raise the defense, increasing it from "some" evidence to a "preponderance of the evidence." Because of a court ruling related to that change in the burden of production standard (United States v. Prather, 69 M.J. 338, 2011), Congress retreated from this reform in 2012. Importantly, in 2007, Congress did not try to modify the substance of the defense. Canada has taken that step, making the defense unavailable if the accused's

mistaken belief is unreasonable due to intoxication, reckless conduct, willful blindness, or failure to take reasonable steps to determine if the victim was consenting. This reform returns the focus to the offender's culpable behavior rather than the victim's behavior and removes examples of reckless or negligent offender conduct from legal protection. Congress should consider adopting this reform for the military.

Still, even this reform has its limits, and Congress may need to focus elsewhere. Congress should focus on improving military law enforcement's handling of sexual assault cases through intensive training and certifications. Congress should start with the basics. Military law enforcement still seems to have trouble with the fundamentals. The Defense Advisory Committee on Investigation, Prosecution, and Defense of Sexual Assault in the Armed Forces (2020) recently investigated a random sample of the unredacted case files of penetrative sexual assaults cases from fiscal year 2017. The committee found that there was still considerable confusion among law enforcement about the definitions of *probable cause* and *unfounded*, and that military law enforcement was conducting rigid, formalistic investigations. Ten years after the change in the law, military law enforcement was still struggling with the mechanics of running an investigation.

Even with solid mechanics, if legal officials use rape myths when processing these problems, then those belief systems can enter the legal process through the normative words that are in the law. Those beliefs still need to be addressed. Horney and Spohn (1991) noted that increased national attention on rape case attrition might make criminal justice officials more alert to these issues and that this could lead to changes in processing. Possibly, the increased attention by Congress and the Department of Defense to the military's sexual assault problem could have brought about some changes in belief systems within military law enforcement, particularly when that attention coincided with the start of the #metoo movement. However, Army law enforcement still appears to be founding sexual assault cases at much lower rates than other crimes of violence. Using more recent data than were used in this study, Carpenter, in press, looked at Army law enforcement's processing of sexual assault cases compared to homicides, assaults, and robberies and found the same large differences between the founding rates for sexual assault cases compared to other cases that we report in Table 1. From 2008 through 2014, Army law enforcement officers founded 95% of the comparison cases (homicide, robbery, and assault) but only 76% of the sexual assault cases, and from July 1, 2015, through 2017, they founded 90% of the comparison cases but only 55% of the sexual assault cases. If social norms may be changing in society at large, those changing norms may not be influencing Army law enforcement officers.

Recent research demonstrates that if people are trained directly on their biases, then they can reduce the impact of those biases on their decision-making processes (Sellier et al., 2019). Congress began requiring specialized training for military law enforcement in 2009, but these low founding rates call into question the value of that training. Congress should closely review this training along with how law enforcement officers are certified for these positions. Investigators should be trained that their expectations about how offenders and victims behave in consent-defense cases could be inaccurate and are likely rooted in deeply held gender beliefs. Investigators should then be taught what social science has revealed about the behavioral patterns of offenders and victims and how to develop evidence that is consistent with those patterns. Changing the law does not appear to have affected case processing; however, changing the belief systems of those who process these cases might.

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