

Concealing Campus Sexual Assault: An Empirical Examination

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This study tests whether there is substantial undercounting of sexual assault by universities. It compares the sexual assault data submitted by universities while being audited for Clery Act violations with the data from years before and after such audits. If schools report higher rates of sexual assault during times of higher regulatory scrutiny (audits), then that result would support the conclusion that universities are failing to accurately tally incidents of sexual assault during other time periods. The study finds that university reports of sexual assault increase by approximately 44% during the audit period. After the audit is completed, the reported sexual assault rates drop to levels statistically indistinguishable from the preaudit time frame. The results are consistent with the hypothesis that the ordinary practice of universities is to undercount incidents of sexual assault. Only during periods in which schools are audited do they appear to offer a more complete picture of sexual assault levels on campus. Further, the data indicate that the audits have no long-term effect on the reported levels of sexual assault, as those crime rates return to previous levels after the audit is completed. This last finding is supported even in instances when fines are issued for noncompliance. The study tests for a similar result with the tracked crimes of aggravated assault, robbery, and burglary, but reported crimes show no statistically significant differences before, during, or after audits. The results of the study point toward 2 broader conclusions directly relevant to policymaking in this area. First, greater financial and personnel resources should be allocated commensurate with the severity of the problem and not based solely on university reports of sexual assault levels. Second, the frequency of auditing should be increased, and statutorily capped fines should be raised to deter transgressors from continuing to undercount sexual violence. The Campus Accountability and Safety Act, presently before Congress, provides an important step in that direction.

Keywords: rape, sexual assault, education, universities, law

In early 2014, President Barack Obama directed the national spotlight toward sexual violence at universities¹ (Calmes, 2014). Unfortunately, there remain serious holes in our understanding of the nature and magnitude of campus sexual assault that inhibit effective policy formulation (Bialik, 2014). In particular, it has proven difficult to reliably and validly determine the number of reported sexual assaults on university campuses. This is because of uncertainty as to whether higher education institutions are accurately disclosing their sexual assault statistics and because of apparent inconsistencies with survey and municipal police data. As a result of differing methods, conflicting definitions, and other vagaries of the comparable data sources, there is an open issue as to whether the university-provided and other data are simply measuring different types of incidents and/or reporting levels. This study attempts to address this concern

through an empirical analysis of Clery Act² data submitted by schools before, during, and after audits by the U.S. Department of Education (DoE).

This study seeks to test whether there is substantial undercounting of sexual assault by universities by examining statistical patterns in data submitted by universities. To that end, the study compares the sexual assault, aggravated assault, robbery, and burglary data from periods during DoE audits for Clery Act violations with the data from years before and after such audits. On the basis of differences in before-, during-, and after-investigation sexual assault statistics, the study aims to determine whether there is significant undercounting by universities. If schools report higher rates of sexual assault during times of higher regulatory scrutiny (audits), particularly in comparison with other crimes, then that result would support the conclusion that universities are failing to accurately tally incidents of sexual assault during other time periods.

This article is dedicated to Andrew Taslitz, who taught me so much about the law of sexual violence and teaching. He was taken from us too soon. I thank Lindsey Collins, Christopher Drahoszal, and David L. Schwartz for their comments and assistance.

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¹ For simplicity, in this article, I refer to colleges and universities in the United States as *universities*. The use of that shorthand does not imply the omission of colleges from this article or the underlying study that it describes. However, because the study sample includes larger schools, the term *university* is the most appropriate.

² The full name of what is commonly known as the Clery Act is the Jeanne Clery Disclosure of Campus Security Policy and Campus Crime Statistics Act.

Clergy Act

On November 8, 1990, President George H. W. Bush signed into law the Clergy Act. The Act requires that, among other mandates, higher education institutions submit yearly data to the DoE regarding designated crimes on campuses (Clergy Act, 1990; Clergy Act Regulations, 2014). In particular, universities must provide tallies in the following categories: murder/nonnegligent manslaughter; negligent manslaughter; sex offenses—forcible; sex offenses—nonforcible; robbery; aggravated assault; burglary; motor vehicle theft; and arson. This study is primarily concerned with the category designated as “sex offenses—forcible.” Those crimes are referred to as *sexual assaults* for purposes of this study.

The differentiation between *forcible* and *nonforcible* sex offenses in the Clergy Act is likely misleading to those familiar with rape and sexual assault law. Of primary interest, the Clergy Act includes as “*forcible*” crimes incidents in which either the defendant uses force *or* the sex act is nonconsensual. In contrast, criminal statutes treat force, when an element of the crime, as a requirement *in addition* to nonconsent. Under the Clergy Act, forcible sex offenses include the following types of incidents:

A. **Forcible Rape**—The carnal knowledge of a person, forcibly and/or against that person’s will; or not forcibly or against the person’s will where the victim is incapable of giving consent because of his or her temporary or permanent mental or physical incapacity (or because of his or her youth).

B. **Forcible Sodomy**—Oral or anal sexual intercourse with another person, forcibly and/or against that person’s will; or not forcibly against the person’s will where the victim is incapable of giving consent because of his or her youth or because of his or her temporary or permanent mental or physical incapacity.

C. **Sexual Assault With An Object**—The use of an object or instrument to unlawfully penetrate, however slightly, the genital or anal opening of the body of another person, forcibly and/or against that person’s will; or not forcibly or against the person’s will where the victim is incapable of giving consent because of his or her youth or because of his or her temporary or permanent mental or physical incapacity.

D. **Forcible Fondling**—The touching of the private body parts of another person for the purpose of sexual gratification, forcibly and/or against that person’s will; or, not forcibly or against the person’s will where the victim is incapable of giving consent because of his or her youth or because of his or her temporary or permanent mental incapacity. (Clergy Act Regulations, 2014)

Notably, tabulations of sexual assaults under the Clergy Act include numerous events that are not defined as “rape” in any American jurisdiction. The subcategory forcible fondling is particularly significant in that regard, as unwanted sexual touching, without penetration, is included in the incident counts. In contrast, nonforcible sex offenses are limited to incest and statutory rape (Clergy Act Regulations, 2014). Unsurprisingly, because of the rarity of incest and the norm of university students being above the state age of consent, forcible sex offenses far outnumber nonforcible ones using the Clergy Act definitions.

University Reporting of Campus Crime

On or before every October 1, universities are required to submit Clergy Act reports that include crime statistics for the previous full calendar year to the DoE and to make those reports publically available (Clergy Act Regulations, 2014). To ensure the accuracy of reported crime statistics, the DoE engages in periodic audits of college and university crime statistics and reporting policies.³ The ordinary in-person audit only takes 2 or 3 days, but wrangling over the findings of the investigators can last for years.

The in-person audit begins with entrance interviews with people responsible for campus security and discipline. The federal auditors then review the university crime log, incident reports, and all other documents relevant to campus crime during a specific time period. Universities are obligated to provide unlimited access to such records and information sources. Auditors may also conduct interviews with students, faculty, and staff on the basis of the review of documents. Amalgamating all of the documentary and interview information, the DoE auditors issue a set of findings about the university’s compliance with Clergy Act requirements.

After issuance of the initial findings, assuming there is evidence of at least one violation, universities have a choice as how to proceed. They may simply acknowledge the error(s) and commit to better future performance. Normally, though, schools issue formal responses denying wrongdoing. This triggers follow-up reviews of the auditor findings at the DoE. These appellate processes rely on the documents and interviews made available through the previously completed audits. The subsequent reviews can reject, accept, or partially reject the responses of universities.

On the basis of the results of the audits and any subsequent review, the Clergy Act authorizes the DoE to level fines against institutions that are noncompliant with the requirements of the Act. Presently, the statutory cap for Clergy Act fines is \$35,000 per violation. In some cases, the university transgressions are so severe that the DoE needs to monitor required policy change implementation until the institution has fully complied with the Clergy Act.

Previous Research and Data

Diane Moyer, the Legal Director of the Pennsylvania Coalition Against Rape, succinctly summarized the belief among people in the field that universities are not providing accurate information about incidents of sexual assault: “This will sound counterintuitive, but I actually tell parents to send their kids to the college or university with the highest number of sexual assaults reported through the Clergy Act, because these schools are probably most aware of the campus sexual assault problems” (Police Executive Research Forum, 2012). There are at least two reasons, on the basis of comparisons with other data, to suspect that universities are undercounting incidents of sexual assault. However, as discussed later, there is not currently, on the basis of prior research, solid or definitive evidence to support that hypothesis.

First, the reported rates of sexual assault on university campuses are far less than would be expected on the basis of incidents of rape reported by municipal police. The rates of sexual assault for

³ Audits performed without a filed complaint are ordinarily timed to correspond with quality assurance reports in the same jurisdiction by the Federal Bureau of Investigation for Uniform Crime Report data.

schools in the study sample under Clery Act reporting versus the rates of forcible rape reported to the Federal Bureau of Investigation as part of the Uniform Crime Reports (UCR; Uniform Crime Reporting Statistics, 2010) are shown in Figure 1.

At first blush, the data would appear relatively consistent, particularly after the increase in sexual assaults reported by universities after 2009. However, the differences in the UCR and Clery Act definitions of rape and sexual assault are substantial. As a result, it is expected that universities will report incidents of sexual assault at a far higher rate than police departments do through the UCR program. The UCR definition, during the study period, is limited to (a) forcible (b) vaginal penetration of (c) women. In contrast, the Clery Act does not require force be used, includes other forms of penetration and nonpenetrative acts, and includes male victims. There are also strong reasons to believe the police departments have been substantially undercounting rapes during the study period (Yung, 2014) which would indicate that the actual UCR rate should be much higher. As a result, Figure 1 illustrates that universities are reporting sexual assault rates far lower than expected on the basis of noncampus data.

Second, the surveys of university students and the general population are in sharp contrast to the Clery Act reports. For example, the Campus Sexual Assault (CSA) Study (Krebs, Lindquist, Warner, Fisher, & Martin, 2007) found—on the basis of surveys at two large, public universities—that approximately one in five women were victims of sexual assault. Among the general public, the Centers for Disease Control and Prevention (CDC) survey of sexual assaults found that 19.7% of men and women are sexually assaulted (CDC, 2012). In contrast, the Clery Act data in the study indicate that 0.02% of students are sexually assaulted in a given year. Even adjusting that over a 5-year enrollment period, the survey and Clery Act data are in sharp contrast. As with the UCR, the CDC, the CSA, and the Clery Act use differing definitions of sexual assault, making reconciliation of the results difficult. Beyond those definitional inconsistencies, one major difference between the Clery Act, CSA, and CDC data is that the CDC and CSA include unreported sexual assaults. Nonetheless, the underreporting rate of sexual assault needed to explain the discrepancy would far exceed any level that had ever been observed.

The definitional and other differences between the Clery Act, UCR, CSA, and CDC data make any inference of university undercounting based on the data difficult to support with certainty. Further, the data from the different sources could be reconciled

with two other viable theories. First, it could simply be that university campuses are far safer than noncampus environments. This would account for the UCR and CDC data's higher rates of sexual assault and rape. Second, it might be that university students report sexual assault at a rate much lower than the general population. That contention would explain why campus survey data, which include unreported sexual assaults, indicate a higher rate of sexual assault than would be expected. As a result of the shortcomings in applying external data sources, this study uses the data provided by universities to determine whether undercounting of sexual assault is occurring. This should address whether the two alternate hypotheses (safe universities and higher underreporting) can effectively explain the differences with the CDC, CSA, and UCR data results.

Data Sources and Coding

The study uses two major sources of data: submitted Clery Act crime statistics (crime data; U.S. Department of Education, 2014) and documents detailing DoE audits of universities (audit data; (Federal Student Aid, 2014)). For the crime data, the study is limited temporally to reports for the years 2001–2012 and to 4-year schools with at least 10,000 students and on-campus housing throughout that period. The time limitation corresponds to all of the years for which data are available (U.S. Department of Education, 2014). The student minimum is adopted primarily because the larger schools in the study have established on-campus housing and are not online or commuter universities (where sexual assault is difficult or impossible to track).⁴ Further, smaller schools regularly create statistical problems with floor effects because of numerous years with zero reported sexual assaults.

The audit data consist of posted documents of different types related to various stages of each audit. They are gathered from an online repository (Federal Student Aid, 2014) made available by the DoE for each school in the crime data that was audited or investigated during the study period. The beginning and end of an investigation is coded by using dates in the posted documents. The investigation start date is identified as the first day of the in-person audit or the filed complaint, if the complaint triggers the audit. These dates are chosen because they correspond to the moments when institutions are made aware of the nature of potential Clery Act violations. The end date for each investigation is determined to be the date that a fine letter is issued or, if no fine is assessed, the date of the last available document pertaining to the investigation.

As a result of the different trajectories of the investigations, the time frame for each can vary, in the audit data, from a period less than 1 year to, in one unusual case, almost 9 years. Notably, investigations begin and end at various dates throughout a year. To determine if a given calendar year of crime data is before, during, or after the investigation, a consistent cutoff point needs to be assigned. As previously noted, each school is required to submit its Clery Act report on October 1 of the year following the data in the

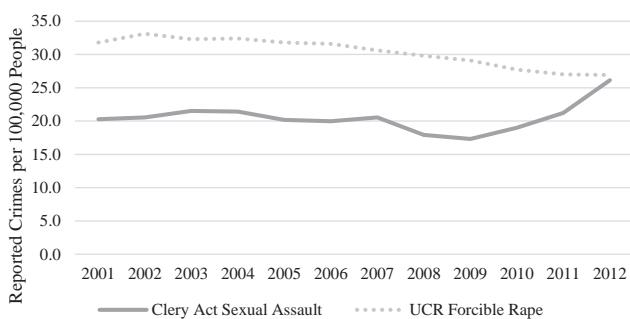


Figure 1. Reported Clery Act on-campus sexual assault and Uniform Crime Reports (UCR) forcible rape rates.

⁴ Statistics for off-campus sexual assault are particularly problematic. Schools do not gather such data directly. Instead, regulations require a “good faith” effort to acquire such information from local police. Unfortunately, the data do not distinguish between when the police report zero incidents and when the police provide no data at all. In both instances, a zero value is submitted. The vast majority of schools submit zero for off-campus sexual assault in a given year.

report. As a result, an investigation is designated in this study as underway if, for the year for which the data were being prepared, the beginning date is before July 1. That determination is based on the assumption that a school could not substantially alter its lengthy Clery Act report too close to the submission deadline. Nonetheless, the study was repeated using August 1, September 1, and October 1 as cutoff dates with no significant difference in results.⁵

In the crime data, there are 269 universities. The incident totals for each university are based on only on student reports of sexual assault and not any subsequent adjudication of guilt. Of those 269 schools, the DoE audited 31 (listed in the Appendix) during the study period. For each of the 31 schools, the study codes each year of crime data based on whether it was before, during, or after an investigation. The study also codes for the following additional variables for each audited school: whether the audit was begun because of a complaint or to correspond in time with an FBI investigation in the same jurisdiction, whether the DoE found that the school had undercounted sexual assaults, and whether a fine was assessed or settlement reached.

To reduce the influence of unobserved variables, such as social or economic factors affecting crime rates during specific years, it is helpful to put all of the crime data on the same scale. As shown in Figure 2, the rate of sexual assault on campus was not consistent during the study period (shown with statistics for aggravated assault and robbery for comparison).

As Figure 2 illustrates, sexual assault rates, like those of aggravated assault and robbery,⁶ have fairly consistent slopes until 2009. At that point, sexual assault rates change from a slow decline to a rapid increase (particularly from 2011 to 2012). Regardless of the reason, the 2009 shift in direction for sexual assault rates must be addressed in analyzing the crime data. As a result, the study computes a normalized sexual assault, aggravated assault, robbery, and burglary rate for every school during each year as a percentage of the overall average for each crime in the crime data during that year. So, for example, the sexual assault rate in 2012 for Boston College was 34.2 per 100,000 students. The national average of the data sample during that same year was 26.1. The normalized sexual assault rate is thus 131.0% (34.2/26.1). This same technique was previously used in a similar study analyzing rape and murder data submitted by municipalities (Yung, 2014). These computed normalized sexual assault rates are used for the regression and other statistical analysis in this study. The distribution of the normalized school-year sexual assault rates is contained in Figure 3.

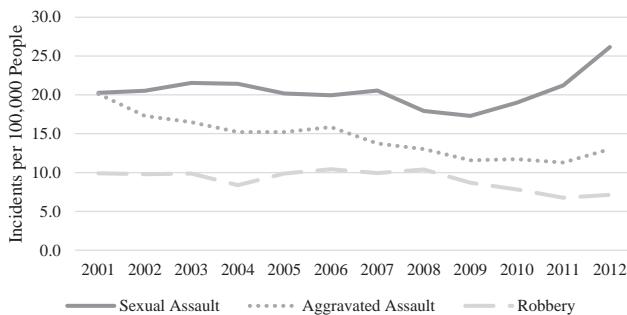


Figure 2. Reported on-campus sexual assault, aggravated assault, and robbery rates.

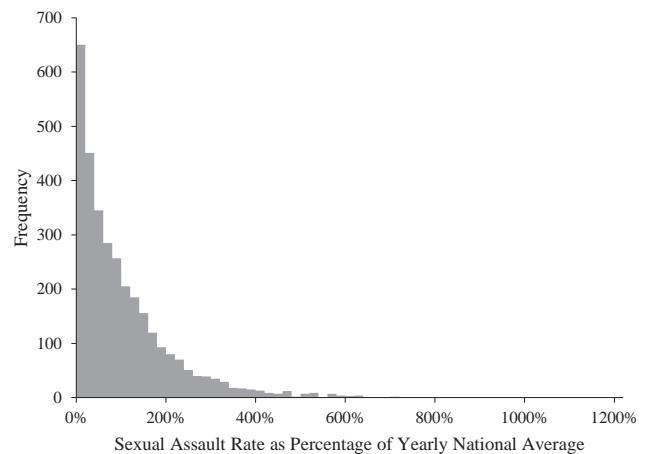


Figure 3. Distribution of reported sexual assault rates.

As the underlying nature of sexual assault data is based on counts, the distribution of the percentage rates is unsurprising. The rates follow a general Poisson distribution, with the possibility of some overdispersion. As a result, the regression analysis has to use the appropriate tools on the basis of the distribution of the normalized sexual assault rates.

Method and Results

The study was focused on testing a singular hypothesis: universities substantially undercount incidents of sexual assault on campuses in their Clery Act submissions. To test that claim, a counterfactual baseline needed to be established. That is, the study had to have a means of determining when a school was accurately (at least relatively so) reporting sexual assault.

To test the validity of university-reported crime data under the Clery Act, the study focused on values reported in three separate time frames: before, during, and after DoE audits. The study posits that an increase in the sexual assault rate during an audit is indicative of undercounting, because the heightened scrutiny increases compliance in reporting. The investigation of Pennsylvania State University in the wake of the Jerry Sandusky scandal provides an extreme example supporting the study method. After the story about Sandusky broke, an intensive internal investigation was performed, in part related to Clery Act noncompliance by the university. That sexual assault reports at Penn State increased an unbelievable 138% from 2010 to 2012 is illustrated in Figure 4.

Because there is a lag between when incidents occur and when the Clery Act annual report is issued, the 2010 data would have

⁵ For either an August 1 or September 1 cutoff date, sexual assault rates increased by 43% instead of 44% from the before to during investigation periods ($p < .001$ when controlling for other variables as described later). For an October 1 cutoff date, sexual assault rates increased by 45% instead of 44% from the before to during investigation periods ($p < .001$ when controlling for other variables, as described later).

⁶ Burglary, although discussed later in the study, is omitted from Figure 2 because it occurs at a far higher rate than the three other crimes (ranging from 101.9 to 200.3 per 100,000 people during the study period). Inclusion of burglary would render differences between the other three crimes impossible to discern in graphical form.

been submitted by October 1, 2011, when the school was under substantial regulatory scrutiny (consistent with the belief that the university had been undercounting incidents of sexual assault). The idea that federal investigation or auditing increases compliance is also warranted by general research about administrative regulation (May & Wood, 2003) and, as will become clear, the data in this study. However, contrary explanations for observed changes in sexual assault rates are also discussed later.

Regression analysis was used to assess whether the three time periods (before, during, and after audits) were correlated with changes in reported normalized sexual assault rates at universities. Initially, negative binomial regression was used to assess statistical relationships between sexual assault rates and the dependent variables. The selection of the method was based on the Poisson distribution, with some overdispersion of the sexual assault rates. Nonetheless, because the overdispersion was slight, the analysis was repeated with ordinary Poisson regression with no appreciable difference in results.⁷

The negative binomial regression indicated a statistically significant relationship between reported sexual assault rates and whether a school was being audited by the DoE ($p = .004$).⁸ The correlation existed even when controlling for whether the DoE found errors in the sexual assault data, assessed a fine, or reached a financial settlement or the audit began as a result of a complaint ($p < .001$).⁹ Notably, there was no statistically significant relationship found between the before- and after-investigation periods ($p = .808$).

Schools were estimated to increase sexual assault reports so that the rate of sexual assault was 44% higher during the investigation (vs. before) and almost identical after the investigation (vs. before).¹⁰ The marginal effects of an investigation on the sexual assault rate are shown in Figure 5.¹¹

As Figure 5 illustrates, in the preinvestigation period, the audited schools had an estimated rate of 137% versus the overall sample average. This higher than average rate is expected because some, but not all, of the audited schools were investigated because of complaints related to high levels of sexual assault on campus. The estimated sexual assault rate climbed to 198% of the sample average throughout the audit period. And once the investigation was concluded, the estimated sexual assault rate dropped to 141%, a value statistically indistinguishable from the before-investigation sexual assault rate.

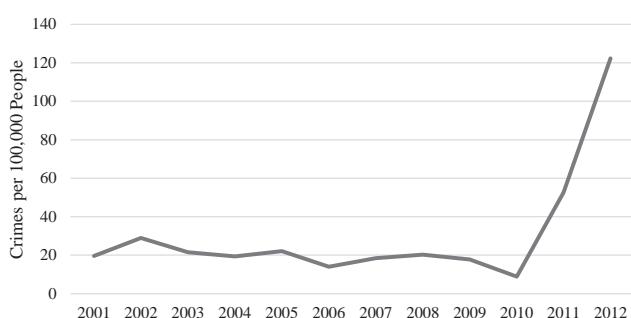


Figure 4. Reported sexual assault rates at Pennsylvania State University from 2001 to 2012.

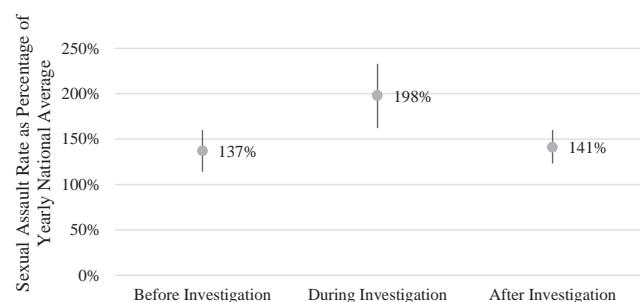


Figure 5. Reported sexual assault rates before, during, and after U.S. Department of Education investigation, with 95% confidence intervals.

Interestingly, when the same analysis was performed for three other crimes (aggravated assault, robbery and burglary),¹² no similarities in results were found. The estimated changes in reported crime rates for sexual assault, aggravated assault, robbery, and burglary before, during, and after audits are shown in Figure 6.

Unlike for sexual assault, negative binomial regression results showed no observed statistically significant differences for aggravated assault ($p = .195$ between the before and during periods, and $p = .082$ between the before and after periods), robbery ($p = .452$ between the before and during periods, and $p = .887$ between the before and after periods), or burglary ($p = .669$ between the before and during periods, and $p = .699$ between the before and after periods) during the three studied time frames. As illustrated in Figure 6, the marked increase from the before- to during-audit periods was only found for sexual assault. Similarly, the significant decrease from the during- to after-audit periods was only observed for sexual assault.

The results are consistent with the hypothesis that the ordinary practice of universities is to undercount incidents of sexual assault. Only during periods in which schools are audited do they appear to offer a more complete picture of sexual assault levels on campus. Further, the data indicate that the DoE has no long-term effect on the reported levels of sexual assault, as those crime rates returned to previous levels after an audit was completed. This last finding was supported even in instances when the DoE issued fines for noncompliance. Nonetheless, it is important to consider other possible explanations for the findings.

⁷ Using a Poisson regression, these are the results when controlling for all relevant variables: $p < .001$, pseudo $R^2 = .042$; $n = 372$; log likelihood = -543.9349 . The marginal effect of moving from the before- to during-audit period was a 45% (instead of 44%) increase in sexual assault rates.

⁸ Pseudo $R^2 = .010$; $n = 372$; log likelihood = -561.0217 .

⁹ Pseudo $R^2 = .040$; $n = 372$; log likelihood = -543.9744 .

¹⁰ The estimate is based on the incident-rate ratio option in Stata 12 for the negative binomial regression.

¹¹ These results were obtained using the *margins* command in Stata 12.

¹² The study omits analysis of other crimes tracked under the Clery Act (murder/nonnegligent manslaughter; negligent manslaughter; sex offenses—nonforcible; motor vehicle theft; and arson) because such crimes are so rarely reported. In each case, the average number of reported incidents per school per year was approximately zero. As a result, any observed changes on a school-by-school basis would not be statistically comparable to changes in sexual assault rates.

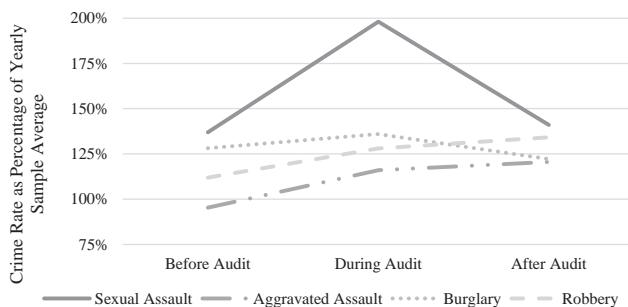


Figure 6. Reported crime rates before, during, and after U.S. Department of Education audits.

Discussion

The results of the study are alarming. During audits, universities submit sexual assault incident reports that are an estimated 44% higher than prior submissions. When the investigation is complete, reported rates of sexual assault return to levels prior to intervention by the DoE. This result is consistent with the contention that schools are undercounting incidents of sexual assault and only accurately (at least relatively) tallying on-campus sexual violence when under heightened federal government scrutiny. Further, the findings hold true even when controlling for whether the audit is commenced as a result of a filed complaint, when the DoE identifies errors in the submitted sexual assault statistics, or when a fine is issued or a settlement reached.

To explain why universities might undercount sexual assault and not other crimes, it is important to consider belief systems and incentive structures in reporting crime data. If they mirror patterns of society at large, individuals working at universities would be expected to have conscious motivations and unconscious beliefs that might lead them to undercount on-campus incidents of sexual violence (Cole & Smith, 2008; Yung, 2014). Widespread adoption of “rape myths” and exaggerated belief in false reporting are the prime culprits in such pervasive hostility to sexual assault complaints. Campus police and administrators might exhibit the same cultural attitudes hostile to rape and sexual assault complaints that have been found in the general population and law enforcement (Bouffard, 2000; Fisher, 1993; Human Rights Watch, 2013; Jordan, 2004; Lonsway & Archambault, 2012; Schulhofer, 1998; Spohn & Tellis, 2010–2011; Taslitz, 1999; Walker & Katz, 2008).

Further, employees responsible for tabulating and submitting crime statistics might have professional incentives to report lower levels of sexual assaults to further career goals and preserve their institution’s reputation (Cole & Smith, 2008; Yung, 2014). High reported rates of sexual violence typically undermine the assessed job performance of individuals responsible for addressing such crimes. However, consistently low sexual assault numbers can result in promotions or other forms of career advancement.

The public nature of Clery Act crime statistics also gives extra incentives for universities to undercount sexual assault. The Clery Act requires universities to supply sexual assault crime data both to students and the DoE. The DoE has recently set up a Web site aggregating the data from all higher education institutions regulated under the Clery Act (U.S. Department of Education, 2014). Some prospective students and their parents use the crime data as

part of the decision-making process in selecting universities to attend (Police Executive Research Forum, 2012). As a result, reporting high sexual assault rates can be viewed as a detriment to an institution’s goal of recruiting quality students. Consequently, higher education institutions might have an incentive to downplay particularly salient crime statistics. For example, if a school stands out as having a high rate of sexual assault versus peer schools, it risks attracting fewer students and suffering long-term reputational damage.

Administrators also have personal professional incentives to avoid scandals associated with high crime levels. If a university is perceived as having a rampant sexual assault problem, students or advocacy organizations might file a complaint, leading to a Title IX investigation on the basis of gender discrimination (Wilson, 2014). Such investigations are performed by the U.S. Department of Justice and carry the risk of much larger fines and more significant public relations problems than do Clery Act audits (Wilson, 2014). A Title IX investigation would not ordinarily be based on mishandling of aggravated assault, robbery, or burglary complaints, because those crimes are not similarly gendered. Thus, no similar professional incentives would warrant undercounting those crimes. Further, the attention given to sexual assault at higher education institutions is greater than that given to other crimes such as burglary and aggravated assault, as demonstrated by the Obama administration’s focus on the former without mention of the latter crimes.

Alternative Inferences

Although university undercounting because of conscious motives or unconscious beliefs is one possible inference that can be drawn from the study findings, other hypotheses are worth considering. Primarily, it might be that publicity from an audit increases victim reporting and/or that the audit timing coincides with an increase in sexual assaults.

A viable argument could be made that the announcement of an audit itself is triggering an increase in reports by students. Such effects have been observed in the nonuniversity context when law enforcement prioritizes targeting of certain crimes. For this explanation to be viable, though, there would have to be some publicity surrounding the Clery Act audit. Otherwise, students would have no reason to increase reporting. A review of news articles in LexisNexis pertaining to Clery Act audits before the Freeh Report (2014), which brought more attention to the statute in 2009, illustrates the unlikelihood of an increased reporting effect.¹³ Only 17 unique articles were found, and only six of those pertained to schools in the sample (five regarding Eastern Michigan University and one concerning schools in the University of California system). Unlike recent Title IX investigations, it appears that the media and public are rarely made aware of Clery Act audits. Further, the varying lengths of investigations and the returns to previous levels of sexual assault reporting are contrary to any increased victim reporting effect. Also of note, a statistically significant increase in aggravated assault, burglary, and robbery

¹³ The search was performed in the “All English Language News” database with the following search term: “Clery Act w/50 audit and date bef 1/1/2009.” The database includes national, regional, local, and some university newspapers; magazines; and other periodicals.

reporting would also be expected from publicity surrounding an audit but was not observed in the study.

Another explanation for the findings would be that the spike in sexual assault rate during the investigation was correlated with an unobserved variable that was also the basis for the audit. To address this concern, the study coded for whether the audit was triggered by a complaint or a coincident FBI investigation of UCR data in the same jurisdiction. If the FBI investigation was based on genuine concerns about sexual violence in the jurisdiction (that also affected campuses), that might explain the increase in reported sexual assaults.

However, this theory is, ultimately, not a good fit for the data. In particular, because of the reporting lag, the data being audited by the FBI and the DoE are not the data during which the spike occurred. That is, if an audit was scheduled in May of 2005, the school would still be preparing 2004 data. The DoE could only be looking at data prior to 2003. And yet, if the 2003 data triggered the FBI investigation, it is unclear why the 2004 data would show a marked increase for a reason other than undercounting. Further, this alternative explanation does not incorporate reasons why the sexual assault rates would decline to preinvestigation levels after an audit is completed. Also of possible significance, to fit the data, the unobserved variable in question would have to correlate with increased sexual assault rates but not those of other studied crimes.

Limitations

There are some limitations in the data and methods that need to be noted and discussed. The data sample was limited in at least four ways that could cause representativeness problems when applying the findings to all 4-year universities.

First, the study focuses exclusively on on-campus sexual assault. The study design was necessitated by the unreliability of the off-campus sexual assault data, but this may have ramifications for study significance. It might be warranted to conclude that the same factors driving undercounting of on-campus sexual assault would have a similar effect off campus. However, the off-campus incidents are often filtered through separate departments, primarily municipal police, than are on-campus sexual assaults. As a result, inferring a similar undercounting of off-campus incidents on the basis of this study is likely not supported. However, for on-campus incidents, the differences in results for sexual assault versus other crimes provides support for the hypothesis that sexual assault is particularly suppressed in official school-reported data. If similar undercounting were occurring for off-campus sexual assault, the gap between the survey results, to the degree such results are comparable, could be further explained.

Second, the study assumes that an audit is able to detect all missing reports of sexual assault. Because of the well-documented practice of municipal police not creating a written record of rape complaints (Jordan, 2004; Lonsway, & Archambault, 2012; Yung, 2014), it is possible, if a similar phenomenon occurs on campus, that this study understates the magnitude of missing sexual assaults. When no record of an incident report exists, there is no indication the auditors would know. Indeed, the change in reported sexual assaults shown in Figure 4 at Pennsylvania State University is far greater than the result found in this study. The result here also does not resolve the significant gap between the survey

results and Clery Act data for sexual assaults. Consequently, although the study does indicate a statistically significant level of undercounting, the actual rate of undercounting could be far higher.

Third, the findings are limited insofar as the audits occurred at different times during the study period. Some schools, such as Miami University of Ohio, had their audit completed earlier in the study period. In contrast, other schools, such as the University of Northern Iowa, had only a single year after their audit was completed. It is possible that, given the varying distributions of audit periods, the treatment of the audit periods as a homogenous group omits unobserved variables.

Fourth, it might be that the large schools studied have different reporting approaches than smaller institutions. There is, at present, no data to support or counter that claim. Nonetheless, the results would still hold for the 269 schools in the study sample.

Conclusion

The study results indicate that the sexual assault data supplied by schools is likely severely undercounting the number of reported incidents on campuses. As a result, policymakers, school administrators, campus police, municipal police, and the public are underestimating the actual severity of campus sexual assault. Further, depending on the stage in the investigation that the sexual assault is dismissed from official counts, universities might actually be short-circuiting investigations of sexual assaults, allowing serial offenders to prey on more victims. Such a pattern has been observed in several cities that have undercounted incidents of rape on a systemic basis (Yung, 2014). The moral implications and utilitarian effects of undercounting sexual assault at colleges and universities are substantial and warrant immediate policy changes.

The results of the study point toward two broader conclusions directly relevant to policymaking in this area. First, the magnitude of sexual violence on university campuses is likely worse than policymakers presently believe. The actual rate of sexual assault is likely at least an estimated 44% higher than the numbers that universities submit in compliance with the Clery Act. Consequently, greater financial and personnel resources should be allocated, commensurate with the severity of the problem. Second, the present mechanism of auditing, investigating, and punishing schools that violate the Clery Act requirements appears to be insufficient to deter misconduct in reporting sexual assault. The frequency of auditing should be increased and statutorily capped fines raised so as to deter transgressors from continuing to undercount sexual violence. The Campus Accountability and Safety Act, presently before Congress, provides an important step in that direction.

There are three relatively easy-to-implement mechanisms that could be put into effect to achieve greater accuracy in sexual assault incident counts from higher education institutions. First, as currently being considered in a bipartisan bill before Congress, the DoE should be authorized to issue much larger fines for Clery Act violations. Currently, the dollar cap on such fines does not serve as an adequate deterrent to crime undercounting. The Campus Accountability and Safety Act would increase maximum penalties for each violation from \$35,000 to \$150,000. It is possible that an even higher limit might be necessary to effectively deter undercounting. Second, the DoE should increase the frequency and number of

audits. Since 2001, the DoE has only performed 54 audits¹⁴ (Federal Student Aid, 2014), even though there are thousands of schools providing crime data on an annual basis under the Clery Act. This would require extra monetary and personnel resources for the DoE but would go a long way toward increasing the certainty of undercounting being detected and violators being punished. Third, schools with serious violations of Clery Act crime data reporting should be placed on a probation system that warrants greater punishment for future violations. This would help abate the current pattern of schools returning to apparent undercounting practices as soon as the DoE is no longer applying high levels of scrutiny as part of the audit process.

¹⁴ Some of those audits were excluded from the sample because they were performed on 4-year schools with fewer than 10,000 students, online schools, or schools that primarily grant 2-year degrees.

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Appendix

Audited Schools. The List Includes Some Universities Where There Were No Problems.

Schools audited during the study period with at least 10,000 students: California State University, Chico; California State University, Fullerton; California State University, Sacramento; Eastern Michigan University; Florida State University; Georgetown University; Louisiana State University; Miami University of Ohio; Ohio State University; Oklahoma State University; Oregon State University; University of Arkansas; University of California, Berkeley; University of California, Davis; University of California, Irvine; University of California, Los Angeles; University of California, Riverside; University of California, San Diego; Uni-

versity of California, Santa Barbara; University of Delaware; University of Michigan; University of North Dakota; University of Northern Iowa; University of Texas at Arlington; University of Utah; University of Vermont; University of Virginia; Virginia Polytechnic Institute and State University; Washington State University; West Virginia University; and Yale University.

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