Preventing college sexual assault in California

A state level policy evaluation using individual school data

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Abstract

In 2014 California was the first state to introduce legislation that requires schools in the state to establish procedures, in line with contemporary recommendations of the Whitehouse, to prevent sexual assaults in colleges. A fixed effects model is used to compare the developments of sexual assault rates in California with the rates in states that considered similar policies and by using synthetic control methods. Estimates from this paper suggest that the reported number of sexual assaults would have been somewhere between 24 and 69 % higher if the policy had not been introduced. The policy will most likely tend to increase reporting in California for a given level of sex assault incidents and the effectiveness of the policy might, therefore, be understated.

I show that the recent 2014 state legislation of California concerned with preventing sexual violence in colleges had a large impact in reducing the number of reported forcible sex offenses following its implementation. The state-level nature of the policy and novel data enables the estimation of the policy's effect by comparing the post-policy sexual assault rates in California with that of other states in a school-level fixed effects model. For this purpose, California is compared to states in which similar policies were proposed but not passed, and to the control groups obtained from synthetic control methods.

Recent surveys reporting that one in five women experience sexual assault in college and the succeeding media coverage have put pressure on policy makers and post-secondary educational institution to act on the contemporary issues of sexual violence in college (DiJulio, Norton et. al. 2015). At the federal level actions, that require the educational institutions to report sexual assault incidents have been taken and recommendations about procedures to prevent sexual assaults, and for supporting the victims, outlined. 29 states introduced or enacted policies concerned with sexual violence in college during the 2015 legislative sessions (Morse and Sponsler et. al., 2015).

According to the US Department of Justice (CSA, 2007), the annual victim costs of rape is the highest of all types of crimes at $127 billion followed by assaults at $97 billion. This means that rape victims annually endure higher costs in relation to their assaults than e.g. what the federal government spent on education and science in 2015 (OMB, 2015). A 2010 paper by DeLisi and Matt, et al. finds that on average rape costs the victim roughly $118,000 and the associated justice costs are $7,200. These high costs are the result, not only of immediate negative consequences for the victims but also due to the fact that sexual assaults to a large extent alternate subsequent outcomes in the life of those individuals. Four out of five victims of rape suffer from chronic illness either physical or psychological and 50 % is forced to quit or leave their jobs within the first year of the assault (Krebs, Christopher P., et al 2007; Ellis et. al. 1981). Victim costs of sexual violence are to a large extent driven by poor handling of these criminal cases both in the educational institutions and the justice system. A study 2006 by Campell finds that victims who receive legal counseling following sexual assault experienced less trauma and re-victimization in the medical and legal system. The presence of a criminal advocate in the legal and medical proceedings of rape significantly decrease long-term psychological distress, self-blame, guilt and depression, all of which might negatively affect subsequent outcomes of the victim. In the event that policies aimed at reducing sexual assault prove ineffective, the role of e.g. procedural policies in the educational system might still be very large because of the nature of victim costs.

The scope of this paper is to evaluate the pioneering Californian 2014 Senate Bill No. 967 which is concerned with preventing sexual violence in college. Toward this end I estimate a school fixed effects model using the reported number of sexual assaults of each school as the variable of interest. As control groups, I use first the states in which similar legislation was proposed but died, and I choose two other viable control group using synthetic control methods. This is done to prevent differences in time trends to bias the results. The Californian Bill has been formative of legislation passed subsequently in a range of other states, and therefore evidence of preventative effects can reasonably be generalized.

Prevalence of sexual assaults and the patterns of reporting

The literature on college sexual assault is large and has received increasing attention in recent years. Studies of the effectiveness of policies that try to reduce sexual assault, however, are rare because of

the difficulty of measuring the extent of the problem; sexual assaults are notoriously underreported (DeMatteo, 2015).

Studies consistently estimate that approximately 18 to 20 % of women are victims of sexual assault during college (National Insitute of Justice, 2008). This study focuses on what is called forcible sex offenses which, in colleges, are mainly incidents of rape. Here the estimates vary more widely because they're typically based on smaller surveys and because the definition of what constitute rape differ. Another reason is that female college students are sometimes asked if they ever experienced this type of assault whilst in other cases, they're asked if they experienced it while in college. Multiple survey studies have found that around 5 % of women in college has been victims of forcible rape (Mohler-Kuo et. al. 2004).

When it comes to the students reporting sexual assaults, Langton and Sinozich (2014) estimate that college students report sexual assault in 20 % of the cases whereas non-students of the same age report it 32 % of the time. Another study finds that 14 % of victims of rape report the incidents and that only 2 % of victims that were intoxicated at the time of the assault reported it (Sampson, 2002). This often attributed to the fact that victims know the perpetrator in 90% of cases (Fisher, Cullen, & Turner, 2000).

Because the policy being evaluated here is recent and data for the school year of 2015 was published in march of 2017 there are no similar studies of the effectiveness of this specific type of prevention policy. Federal level policies tend to be harder to measure the effect of, whereas state policies are often hard to compare. Studies in this field have most commonly concerned with grasping the extent of sexual assaults in college or measuring different risk factors.

However, there are new studies that have important implication for the results provided by this analysis, because they shed light on some of the behavioral patterns of students and schools with respect to the issue of sexual assault prevention. One study shows that female enrollment drops in response to a Universiy being under investigation under the Title IX Act. This law provides college student the posibility of filing a complaint about the way the University has handled an incident of sexual assault thereby potentially starting an investigation by Department of Education's Office of Civil Rights (E. Marcotte, 2016). Investigations are followed by a 16-22 % drop in female enrollment, show that female college students take risks of sexual assault into account when choosing between colleges. This in turn, means that Universities have incentives to underreport the level of sexual assault, or more likely, to discourage students from reporting them. Therefore, when the data show a high level of sexual assaults, it might be indicative of a climate that encourages reporting.

In relation to that, another study finds that an audit of the annually reported crime statistics (and the categorization of crime types etc.) increases the reported sexual assaults by up to 44 % temporarily, before falling back to its pre-audit level (Yung, 2015). There were no similar increases in other types of crimes which are thought be consistent with the hypothesis that it is ordinary practice for universities to underreport sexual assaults.

The methods applied in this paper are common in econometrics. Using standardized methods is especially appropriate in this case since studies of this particular policy have not been possible before now.

If other policy estimates were available (estimates of the effect of older statues) it is unlikely that comparisons would be meaningful given that the scope of the policies often varies substantially across time and states. Although the policy evaluated in this paper is coherent with the guidelines of a Whitehouse commitee, and therefore more comparable, it is still unlikely that positive results

can be directly carried over to other states.

Changing the standard of consent

In 2014 California enacted Senate Bill No. 967 in response to high reported levels of sexual assaults on college campuses. The bill requires all post-secondary institutions, in order to receive state funding for student financial aid, to adopt policies and protocols concerning sexual violence including affirmative consent standards in determining whether consent was given by the complainant. Affirmative consent means conscious and voluntary agreement and changes the "no means no" logic with a "yes means yes" standard. Consent from both parties must be ongoing and lack of protest or resistance is not considered consenting. This in effect, invalidates excuses for alleged lacking affirmative consent under circumstances in which the accused knew or should have known, that the complainant was unable to consent because of e.g. being asleep or unconscious, incapacitated under the influence of alcohol or drugs etc. (Senate Bill No. 967, 2014). This is of particular importance because it has often been used in defense of the accused.

Institutions are also required to ensure the protection of privacy (confidentiality) of the individuals involved, and protocols for personnel initial response to reports of an incident, preliminary victim interviews and follow-up interviews. Another important aspect of the policy which might have preventive effects are required procedures for confidential reporting of incidents by victims and third parties. The Bill also states that,

> "In order to receive state funds ... postsecondary institutions shall implement ... a comprehensive prevention program that includes a range of prevention strategies, including, but not limited to, empowerment programming for victim prevention, awareness raising campaigns, primary prevention, bystander intervention, and risk reduction. Outreach programs shall be provided to make students aware of the institution's policy on sexual assault, domestic violence, dating violence, and stalking. At a minimum, an outreach program shall include a process for contacting and informing the student body, campus organizations, athletic programs, and student groups about the institution's overall sexual assault policy, the practical implications of an affirmative consent standard, and the rights and responsibilities of students under the policy."

The bill has been criticized, amongst other things, of shifting the burden of evidence to the accused which contrasts common judicial practice. However, the focus of this paper is only to estimate whether the bill as a whole has had quantifiable success in reducing the number of reported incidents of sexual violence in the state of California.

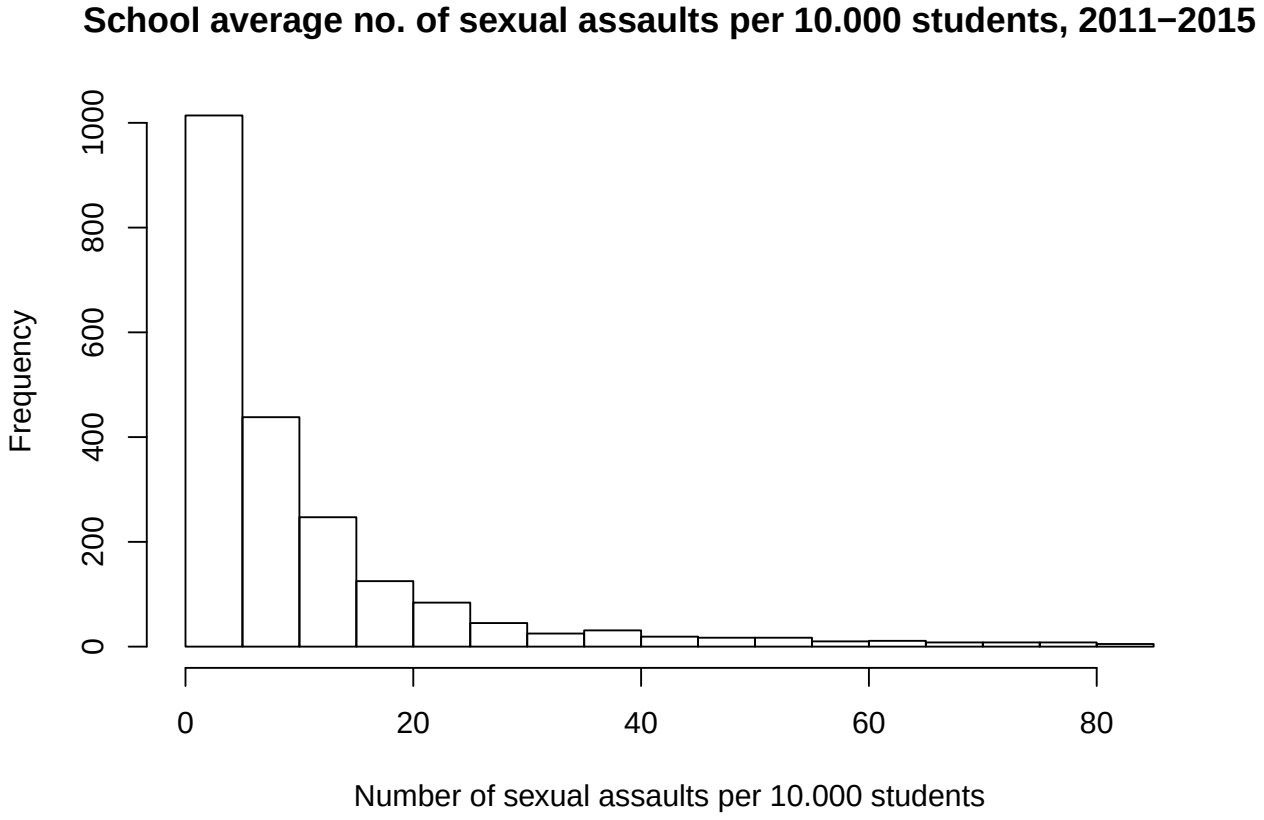
Data

Reporting incidents of sexual assault is mandatory for post-secondary educational institutions that receive federal funding for student financial aid under the Clery Act of 1990. The board of each institution reports the number and types of incidents for each campus of the institution to the Department of Education which publishes this data on their website for Campus Safety and Security. I aggregate crimes within each year from campus- to the institutional level and add together different types of forcible sexual offenses: forcible rape, forcible fondling, sexual assault with an object and forcible sodomy. Crimes in this category are typically more severe than the

crimes categorized under the violence against women act. To avoid confusion and mixing of many different types of crime incidents the latter is not part of this analysis.

The resulting longitudinal panel data set has observations for all post-secondary educational institutions in the US which receive state funding (N=6.278), and contains variables describing the number of enrolled students, the gender composition and the sexual assault rate (number of sex offenses in proportion to the number of students). Finally, the panel is restricted by removing the smallest of schools with less than 130 students (N=4.880) and balanced so that each institution has an observation in each year from 2011-2014 (N=4.265). The size and balancing restrictions reduced the number of observations considerably, which can pose a problem if schools that did report in all years, and smaller schools, differ systematically from the schools that did not. This would lead to a sample that is unrepresentative of the general school population. The very small schools/institutions are excluded because they are likely to be quite different in nature compared to an ordinary college and to avoid outliers, i.e. arbitrarily high sexual assault rates.

The key variable of interest is the sexual assault rate which describes the number of reported sexual offenses for every 10.000 students. Table 1 shows that the mean sexual assault rate is 6.8 meaning that, on average, there is 6.8 reported incident for every 10.000 students in the US whereas California has a lower average rate of 3.8. The median, on the other hand, shows that most schools didn't report any incidents of sexual assault, in fact in most years only around 30 % of schools reported to have any (see table in appendix A). Problems of underreporting will be discussed at greater length in the discussion section. The below histogram illustrates the distribution, where all observations of zero reports have been excluded and the upper 99th percentile has been removed:



Note: Distributions of average rate of sexual assaults reports for each school 2011-2015. For illustrational purposes the histogram excludes all schools that didnt report any sexual assault in the years 2011-2015, and

has also been cut off at the 99th percentile.

The distribution of the SA-rate variable is very right-skewed, as it has a long thin tail because of few high observations which increase the variance although almost all of the mass of the distribution is found to the left of the mean.

Table 1 also indicates that the median school has roughly 1.550 students and that variation in school size is indeed big. The minimum of 131 student number is due to my restriction on school size which excludes the smallest of schools (1st percentile) to avoid noise in the SA-rate.

Table 1: Descriptive statistics

Variable	Statistic	Other states	California
Sexual assault	Mean	6.82	3.83
	Median	0	0
	Minimum	0	0
	Maximum	1105.99	484.02
	Standard deviation	27.50	18.11
Private school	Mean	0.59	0.64
	Median	1	1
	Minimum	0	0
	Maximum	1	1
	Standard deviation	0.49	0.48
No. students	Mean	4397.22	6011.27
	Median	1551	1357
	Minimum	131	131
	Maximum	100272	43401
	Standard deviation	7646.96	8698.57
4-year college	Mean	0.55	0.46
	Median	1	0
	Minimum	0	0
	Maximum	1	1
	Standard deviation	0.5	0.5

The share of private schools is 5 percentage points higher in California than the rest of the US, and there are fewer 4-year college institutions as can be seen from the table above.

There are two primary reasons that the reported number of incidents in the DoE data that is used in this project is too low. The first reason is that victims of sexual offenses typically doesn't report incidents; one study finds that of women experiencing forcible rape only 14 % reported it to law enforcement or academic authorities. Victims who were intoxicated only reported the assault in 2 % of the cases (Sampson, 2002). The lack of reporting is typically attributed to the fact the victims are acquainted with the perpetrator in somewhere between 75-90 % of the cases of college sexual assault (Karjane et al., 2005; Krebs et al., 2009). Other explanations include that victims fear that the evidence is not substantial for a ruling or fear of public exposure of the trial. Evidence also suggest that college students report less than non-college students; 4/5 victims of sexual violence (a broader definition) does not report the incidents whereas 2/3 non-students did not report (DoJ, 2014). The other source of underreporting stems from the fact that the data is collected

and reported by the Universities. Universities have an incentive to understate the severity of the problem because it can be a factor in deciding between colleges for aspiring college students and their parents. The Campus safety and security web site is foremostly meant for exactly this purpose. This hypothesis is backed up by the evidence referred to in the introduction, suggesting that female enrollment drops considerably in response to a college being subject to (title IX) investigation of having handled cases of sexual assault poorly (Marcotte, 2016). Another piece of evidence of underreporting at the centralized (school) level is that schools who has their reports audited for Clery Act violations increase the reported number of sexual assaults temporarily at the time of the audit (Yung, 2015).

Identificaiton: Estimating the policy effect using a fixed effects model

By using a fixed effects model I try to account for school-level heterogeneity e.g. characteristics that are time-invariant and possibly confounding - that is, both affecting the rate of sexual assault and other explanatory variables of the estimated model. To illustrate, we might suspect that the geographical placement of schools has an effect on the level of sexual violence, or that persistent culture and norms exist in different schools which cause level differences in sexual assault.

In the particular context of sexual violence in college, campuses it seems plausible that a large part of the variation in the rate of assault is to be attributed, not to the few explanatory variables of the estimated model, but rather, to a variety of factors many of which are constant over time. Including school-fixed effects is a way of accounting for this type of heterogeneity, which makes it more likely that we estimate the causal effect of the policy of interest. If the school-level effects are not random but correlated with for instance the introduction of a policy, a pooled estimation would not only be inconsistent but also suffer from omitted variable bias.

If we try to estimate the model without considering that school-level effects may be correlated with explanatory variable, estimates of β will generally be biased. Here I use a least-square dummy variables approach which yields the same results as standard FE estimator, i.e. the *within estimator* in which you cancel out the individual effects by utilizing repeated observations to subtract averages. Instead, including $N-1$ dummy variables captures the school-specific effects along with the effects of the time-invariant variables and allows for a different slope for each school. This also eliminates the likely source of bias that stems from time-invariant omitted variables. A downside to this approach is that effects of time-invariant variables cannot be estimated because they are 'merged' with the school-fixed effects; adding them to the estimated equation would cause perfect collinearity between the time-invariant variable and the dummies. As usual, to arrive at a causal interpretation of any estimated parameters from the below equation it has to be the case that $u_{i,t}$ is mean independent of those variables. The estimated model can be formally writting:

$$SA_{i,t} = \Omega policy_{s,t} + X_{i,t}^{'}\beta + school_i + year_t + u_{i,t}$$

Where $SA_{i,t}$ is the reported number of sexual assaults per 10.000 students in school i at time t. $X_{i,t}^{'}$ is vector of time-varying explanatory variables, $School_i$ are the school-specific effects. The key parameter of interest is Ω because the dummy variable $policy_{s,t}$ takes the value of 1 for the state of California in 2014 and 2015. Subscript s indicates that the policy only varies at the state level. Besides the school-fixed effects, time fixed effects have also been assumed to be part of the model. These are modeled by including a dummy variable for each year. The parameters of these dummies

will then be an estimate of the independent level of sexual assault in the reference school (the one with no dummy variable) in the reference year.

The idiosyncratic error term $u_{i,t}$ is assumed to be mean independent of the explanatory variables and fixed effects. The assumption is key to obtaining parameter estimates that have causal interpretation.

$$E[u_{i,t}|policy_{s,t} + X'_{i,t}, school_i, year_t] = 0$$

Note that the assumption is less restrictive than the standard assumption of mean-independence of OLS regression. The fixed effects are included exactly because the assumption which is so critical to causal interpretation is more likely to hold.

Estimating the equation above using standard OLS inference requires imposing assumptions which must be vindicated. Other than standard OLS assumptions of e.g. homogeneity and the likelihood that the mean-independence assumption is justifiable, the proceeding analysis uses clustering of standard errors at the state level to account for the likely dependence of error terms within a given state and error terms being correlated within schools across time (serial correlation).

Although it seems unlikely that school specific effects would be random (not correlated with explanatory variables or the policy), I tested whether this was in fact the case, or if $E[school_i|X_{i,t}, policy_{s,t}, year_t] \neq 0$. If the school effects are in fact not correlated with the explanatory variables, we can instead implement a random-effects model which would be a more efficient estimator and allow for the possibility of estimating the influence of time-invariant variables. Because of likely heterogeneity and the introduction of time-fixed effects, the assumption cannot be tested by the commonly used Hausmann test. Whether school-specific effects are unrelated to the regressors is instead tested using an auxiliary regression from Wooldridge (2010, p. 332), which adds the averages of the variables in X_i to the equation above. The school individual effects were correlated with explanatory variables and throughout the analysis all models are therefore fixed effects models.

The panel structure of the data allows us to take advantage of the time dimension as a way of eliminating time-fixed omitted variables and thereby reduce biases. In the specified model, a justification of the above assumption would be to say that nothing time-varying (like media coverage or political upheaval) particular to California paved the way for the policy or that no other time-varying factors influencing sexual assault changed specifically in California at the time of the policies introduction. This is commonly referred to as the assumption of parallel trends. To better be able to justify this assumption the sample is limited to select control groups. Before discussing the viability of different control groups; first a word on the relationship between the true number of sexual assaults and the reported numbers.

Interpretation of the results

The data on sexual assault is as described previously heavily underreported. Since it is not possible to obtain data on the true numbers of forcible sex offenses, results from this analysis are in terms of reports. The nature of the above model, however, measures relative changes in the reports. Effectively, this means that as long as the likelihood of an incident being reported doesn't change (a

constant relationship between reports and actual incidents), the relative changes to reports can be interpreted as relative changes to assaults.

One of the most likely sources of bias in the results of this analysis stems from the likelihood that the policy does in fact increase reporting when introduced in California, irrespectively of the number of actual incidents. Remember that the policy requires schools to raise awareness and establish procedures for confidential reporting. This will cause a downward bias in the estimate and can lead to a type 2 error. That is, increased reporting could potentially crowd out any decrease in the true number of incidents.

Searching for a proper control group

I first tested nearby West coast states (Oregon, Washington) as a possible control group because they're geographically close together and might be influenced similarly by political events as well as being economically integrated. Trends of pre-treatment reported sexual assault incidents, however, were very different from that of California and these two states, therefore constitute a poor control group (graph in appendix C).

A more viable treatment group is the states in which policies aimed a preventing sexual assault was proposed (policies with similar scope as that of Californias) but died in the process. The average rate of reported sexual assault for this group can be seen from figure 1. There seems to be a consistent relationship between the trends prior to the introduction of the policy in California. In this case, the assumption of no unobserved or omitted time-varying variables seems more likely to hold and thus provides greater confidence in the estimated coefficient of the policy variable.

If for example, we suspect the policy to be introduced in reaction to e.g. increased attention to the issue (like media coverage), we might account for some of this type of heterogeneity by comparing California to states that tried to implement similar legislation, and thus might have been subject to the same political pressure. The states where legislation died were Arizona, Connecticut, Iowa, Kansas, Maryland, Minnesota, Missouri, North Carolina and West Virginia.

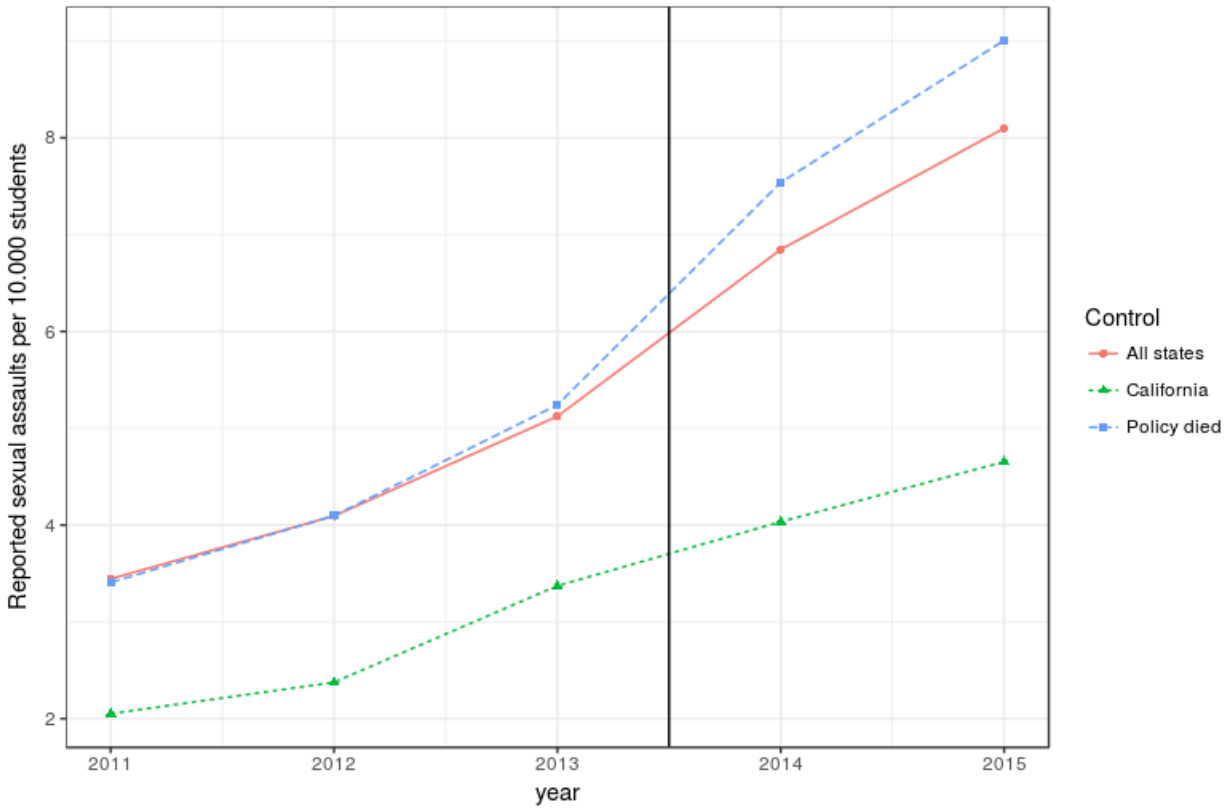


Figure 1 illustrates trends in reported sexual assault for California compared to the other 49 states in the data and to the control group of states in which the legislation died: Arizona, Conneticut, Iowa, Kansas, Maryland, Minnesota, Missouri, North Carolina and West Virginia. The vertical line represents the introduction of the policy.

From figure 1 it seems reasonable that the assumption of parallel trends is justified when comparing California to the rest of the US states and even more likely to hold when using the 'Policy died' control group. The graph also shows that in the most recent years this group experienced a larger increase in reported sexual assaults than the nation as a whole. Therefore the most simple comparison to all states might understate the impact of the policy.

The 'Policy died' control group can be thought of as being robust to changes in the political climate, public opinion and other things that might induce the policymakers to propose legislation of this type. However, California is, after all, different from these states in many respects some of which might influence the organization and functioning of the post-secondary schooling system and in turn sexual assault rates. If, for instance, two states are in different stages of a business cycle it might affect the composition of enrolling college students. In the proceeding section, I attempt to construct a control unit which is more similar to California with respect to general macro economic variables.

Constructing a comparison state

Synthetic control methods can be used as a way of constructing a control unit for the state of California by combining several other observed entities (states). The idea here is to make a state that closely resembles California on the available data parameters. This procedure attempts to

minimize the difference between the explanatory variables of the treated unit and all of the control units assigning weights to each variable and to each control unit. Here, the method is applied using two different specifications which result in two different units acting as controls for California. The first matches California on macroeconomic variables and lags of the sexual assault rate. The other uses only lags of the sexual assault rate.

The synthetic state that most closely resembles California when using Census macroeconomic indicators from a state level panel, turns out to be a combination of Texas, New York, and Nevada, with small weights on Rhodes Island and Massachusetts. The weights from the synthetic control procedure can be found in appendix B. The simpler synthetic control unit "Synthetic 2 (lags)" is obtained by only matching on the pre-policy levels of sexual assault, that is, lags of the dependent variable. This turns out to be a weighted combination of all but a few states. The latter synthetic control unit consisting of only lags of the dependent variable will by construction tend to have a good fit in the trend. The fit of the pre-policy trend of California is almost exact as can be seen from table 2 column 3.

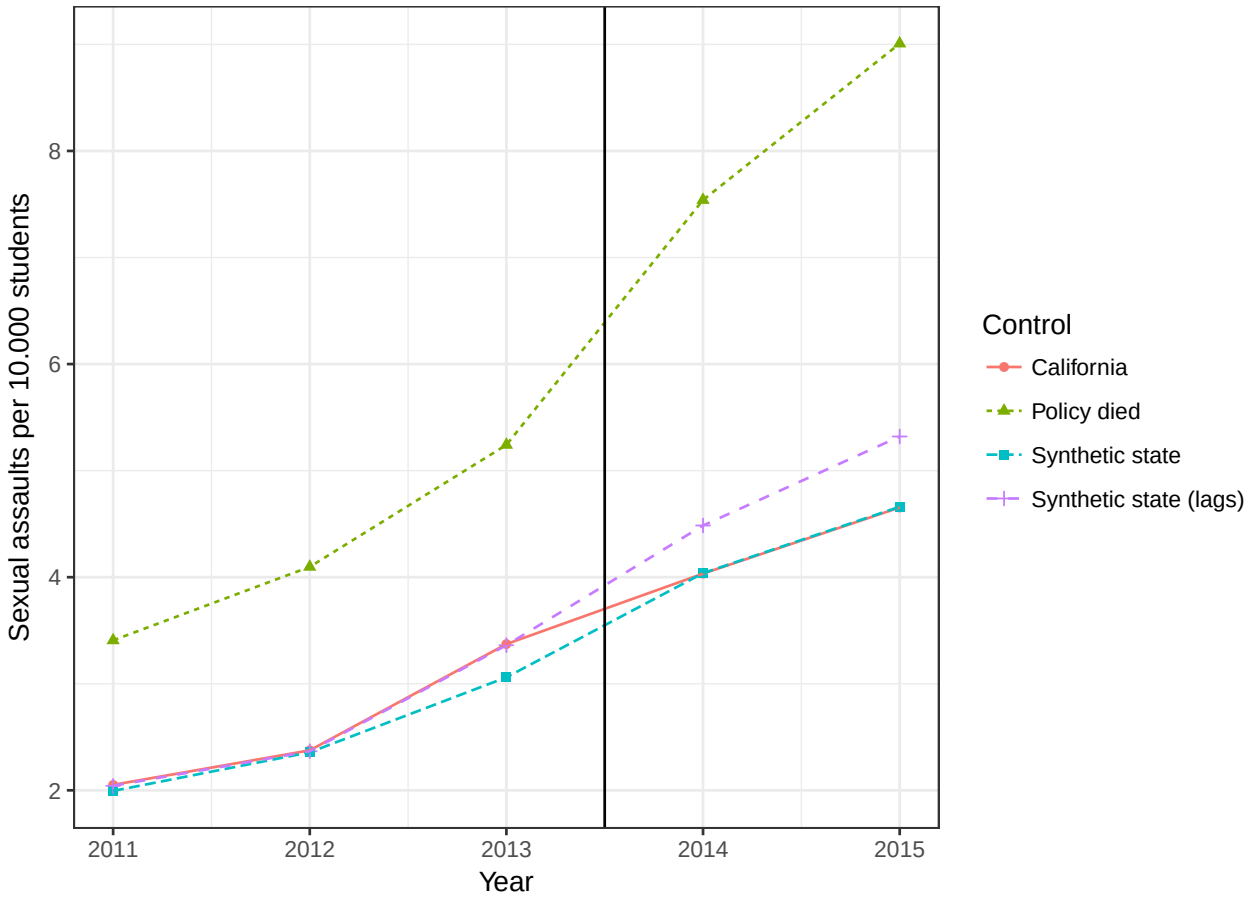
The former synthetic control unit (with macro economic variables) consists in a weighted combination of schools in the states of Texas, New York, and Nevada. To see how this control unit might be a useful control group, consider the averages in table 4. The synthetic unit has a larger population compared to the sample mean and a large fraction of democrats in the house, both of which are characteristics of California. The log of the population was taken to put less emphasis on Californias status as 'outlier' in this respect.

Table 2: Pre-policy covariates

	California	Synthetic	Synthetic 2 (lags)	Sample Mean
log(Population)	17.45	15.74		15.12
Fraction democrats (house)	0.67	0.62		0.45
Change in employment	-1.10	-0.86		-0.66
GDP per capita	0.06	0.05		0.05
Sexual assault 2011	2.05	1.99	2.38	4.42
Sexual assault change 2011-2012	0.32	0.36	0.32	0.38
Sexual assault change 2012-2013	1.00	0.70	1.03	1.26
Highschool	0.82	0.85		0.88
Bachelors	0.31	0.28		0.29
Advanced degree	0.12	0.11		0.11

Table note: Means of macro economic variables from Census data for California, synthetic states and the whole sample. Source: UK Center for poverty research www.ukcpr.org/data

The changes in employment resemble California better than does the sample mean, and the number of people with high school as their highest attained educational level is slightly lower in this control group and California. The pre-policy rates of reported sexual assaults match the trend of California well as can be seen from the figure below. The "Synthetic (lags)" group is identical to California both in level and trend of reported sex offenses prior to the introduction of the policy.



The 'macro' synthetic control unit also show a very similar pattern in the trend, although it is not completely parallel. As a test, I plotted the reported sexual assault rates of 2014 against the state level macro variables used to construct a control unit. Unsurprisingly these variables are not highly correlated with the reported number of sexual assaults and the synthetic unit may not constitute a better control unit than the control group of states in which legislation died, or the weighted group of many states "Synthetic (lags)" which has a better fitting trend. From a qualitative perspective, however, it seems appropriate to compare California to Florida, New York, and Texas because these are large populous states with big metropolises etc. In this respect, it is harder to reason why the simple synthetically constructed state which uses lags of the dependent variable in matching is a good comparison group since it is just a weighted average of most of the US states.

The two alternative control groups proposed here serves as ways to further challenge the assumption of parallel trends which is vital to the estimation. The 'policy died' group and the macro variable synthetic control unit are both, for different reasons, interesting from a qualitative perspective. One has had something or someone pushing for a policy including affirmative consent, while the other more similar to California in terms of demographics, politics etc. The synthetic control group just including lags can be thought of more as a statistical construction since it is just a weighted combination of many different states which make a perfect trend and level match for California.

In the following section, I analyze the results from the outlined model first using the 'policy died' group as the control group before proceeding with the synthetic control units.

Results

The first column of table 2 suggests that the policy prevented an increase of 2.95 reports of sexual assault for every 10.000 students in both 2014 and 2015, using the states in which the proposed legislation died as the control group. In comparison using 'All states' as the control group yields a lower estimate of 2.08. The estimates are significant at the 5 and 1 % level respectively. In these two years post-secondary educational institutions of California reported 2.251 incidents of forcible sex offenses which are equivalent of 4.3 incidents for every 10.000 students. The large results from the 'Policy died' control group correspond to a 69 % increase in reporting, or that there would instead have been roughly 3.800 reported incidents throughout 2014 and 2015 (an increase of almost 1.550).

Table 3: Fixed effects regression of policy on rate of sexual assault

	Policy died	All states
Policy variable	-2.95 *	-2.08 **
	(1.42)	(0.54)
School fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
School controls	Yes	Yes
No. Obs	5552	21326
No. Schools	1110	4265
R-sq.	0.56	0.7

*Table note: Results obtained from a regression of a balanced panel with school data for 4 years (2011-2014). School controls include yearly data on each schools number of students and the gender composition. State level Cluster-robust standard errors are shown in parenthesizes. *,** indicates significance at the 5 and 1 % level respectively.*

The two models are identical, both containing school and year fixed effects, as well as controlling for changes to each schools total number of students and the share of males to females. When estimating the effect using all other states as the control group, the p-value of the estimate is smaller, even though the effect is less pronounced. This is due primarily to the much larger number of schools in the control group, which reduce the standard error of the estimate. The choice between the two constitutes the classical variance/bias trade-off since the 'policy died' control group is thought to more realistically fulfill the necessary mean-independence assumption.

The number of student in a school and the male-to-female ratio did not significantly influence the rate of reports in the data. In one regression, the male-to-female ratio had a slightly negative impact on reported sexual assault rates, but the effect was too small to be of interest. I also tested the possibility that the policy had heterogenous impact on small and large schools (divided by the mean), but these estimates were not statistically different.

All standard errors shown in this paper are clustered at the state level. The clustering considers the possibility that error terms are not independent within a state. For instance, we might think that other legislation surrounding the educational system and colleges in California have a systematic influence on sexual assault, but that this is not accounted for by the explanatory variables. Standard errors increased when this intra state error term correlation was modeled thereby increasing

the uncertainty of the estimates. For example, the 'basic' OLS standard error of the 'All states' estimate is 0.76 with a p-value of 0.006 whereas the cluster robust standard error was 1.42 with a corresponding p-value of 0.038.

Results using synthetic control methods

Table 4 shows the estimated policy effects using the two synthetic control groups. The estimated impact is essentially identical in the two group, and indicates the policy prevented roughly 1 report of sexual assault per 10.000 students, which in turn indicates that the total number of reports in 2014 and 2015 would have been 24 % higher if not for the policy. However, only the macro variable synthetic control group provides a (borderline) significant estimate (p=0.045). The estimate of roughly 1 is still large since it constitutes a 24 % increase in reporting, but in comparison to the estimate obtained when using the 'policy died' control group it is small.

Note that the results are from a weighted OLS regression. Each school gets a weight depending on what state it is located in. The weights was obtained from the synthetic control procedure where and for example school in New York have higher weight than schools in Texas. The schools placed in California was all given a weight of 1 in the regression.

Table 4: Fixed effects regression: Synthetic control groups

	Synthetic state	Synthetic state (lags)
Policy variable	-1.07 *	-1.06
	(0.54)	(1.08)
School fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
School controls	Yes	Yes
No. Obs	5708	21136
No. Schools	1142	4227
R-sq.	0.72	0.61

*Table note: Results obtained from a regression of a balanced panel with school data for 4 years (2011-2014). 'Synthetic state' is a weighted average of Texas, New York and Nevada (see weights in appendix C). 'Synthetic state (lags)' is combination of many US states and was constructed to make the most accurate trend/level fit possible. State level Cluster-robust standard errors are shown in parenthesizes. *,** indicates significance at the 5 and 1 % level respectively.*

When using the control group, the identifying assumption of mean-independence seems more likely to be true. The condition is necessary to get an unbiased estimate. However, using a control group limits our sample thereby increasing the variance of the estimated effect. The fact that the estimates are of the same magnitude provides some merit to the hypothesis that the policy did, in fact, reduce the number of sexual assaults in California.

Discussion

The results of the analysis indicate that the policy prevented an increase in the reported number of sexual assault somewhere between 24-69 %.

Two aspects of the analysis require special attention as the validity of the presented results is most likely to be threatened by these. One is the low reported numbers of sexual assaults and the relationship between actual sexual assaults and *reported* sexual assaults. These are primarily of concern to the external validity of the analysis. The other important aspect is the likelihood that the parallel trends assumption to hold; this is critical primarily to the internal validity of the estimated policy effect.

As previously described there is a variety of reasons why students fail to report incidents of sexual assault, which notoriously makes any quantitative assessment of the extent of issues of college sexual assault difficult. In this respect, the methodology applied in this study is robust, because the policy effect can be estimated as long as the patterns of underreporting are not thought to be systematically different in California from the rest of the US. This is owed to the nature of the policy which is changed at the state level and thus allows us to investigate relative changes compared to other states.

It seems most likely that the policy would increase the number of incidents students report unambiguously for any given level of sexual assault since the policy focuses on e.g. confidential reporting and third-party reporting. This effect causes a downward bias in the estimated policy effect.

Schools also underreport the number of sexual offenses. The underreporting at this level could be due for example to a lack of resources or attention devoted to the issue of sexual assault. In that case, the policy can be expected to raise the number of reports in California for a given level of sexual assault incidents. If on the other hand schools underreport to avoid falling behind in the competition for students, the policy may theoretically incentivize more of that 'covering-up' behavior. It seems unlikely that the net effect on reports could be negative, and so school reporting patterns would also tend to bias the policy estimate downwards (California would experience an increase in reporting relative to other states irrespectively of the number of sexual assaults). This type of behavior would, however, change the relationship between the reported and the actual number of sexual assaults. For these reasons, the low level of reporting will likely tend to cause the estimated policy effect to be too low.

Several control groups are used in the analysis as the attempt to account for the time-varying factors that are not explicitly controlled for in the model. The assumption of common trends in the treatment and control group seemed plausible for all three control groups. The two different synthetic control groups have estimates equal in size indicating a prevented increase in reports of 24 %. The largest estimate of 69 % results from the 'policy died' group. Using all the US schools as the control group resulted in an estimate that is is roughly halfway between the two. The effect, in either case, is large as a percentage, but on the other hand the level of reporting is so low that it doesn't seem unreasonable that the reporting could increase by almost 70 % ; in that case the levels in the data would still be many times lower than what is typically found in survey data.
Given the already existing underreporting, and that the policy is expected to lead to increased reporting (for any given number of sexual assaults), makes a case that since we see a lower relative trend in California, forcible sex offenses did in fact decrease. It is hard to explain what could have caused most other states to experience much larger increases in the reported numbers but not

California.
One potential explanation that would skew the results in this way is the emergence of alcohol policies that seek to educate college students to drink more responsibly. This has become common practice in many states in recent years, including California. A connection between alcohol consumption and sex offenses is well documented (Krebs et al. 2009; Kilpatrick et al. 2007). If students respond to this by drinking less (more so in California than elsewhere) that will in itself reduce the number of sexual assaults. In this case, what could be happening is that all states, including California, has an increasing trend in reports of sexual assault but that the simultaneous decrease in alcohol-related sexual assaults in California makes the increase less pronounced. Developments like these would be wrongfully attributed to the policy in the methodological framework used in this paper. It could also be the case that California did a better job of reporting sexual assault incidents in the first place (true levels would also have to be lower) and that an overall attention to the issue caused other states to increase reporting more relative to California. The choice of control groups does, however, seem likely to avoid false conclusions because of a situation like this. At the same time the data didn't show evidence of better reporting/compliance in California.

On one hand, the 'policy died' control group might be similar in aspects such awareness on the issue or political movements putting pressure on politicians to introduce this particular type of policy. On the other hand, the synthetic state controlling for macro economic variables is more comparable to California in terms of size and are generally more liberal. The macro economic variables used to construct the synthetic control unit have the weakness of not being particularlay specific to the college population but the state as a whole. Having yearly state level data on the demographics of the college age population and e.g. youth unemployment would likely provide a better synthetic control group.

Attempts to evaluate the effectiveness of Bill No. 967 should compare its preventative effects with that of other possible initiatives. The costs of implementing and maintaining the minimum requirements of the policy are, of course, a burden on schools which will, in the end, be paid for by the students, but it seems a small one compared to the large prevention potential found in this analysis.

Conclusion

Estimates from this paper suggest that the reported number of sexual assaults would have been somewhere between 24 and 69 % higher if the policy had not been introduced. The policy will most likely tend to increase reporting in California for a given level of sex assault incidents and the effectiveness of the policy might, therefore, be understated. The fact that evidence of a decline in reporting is found in spite of this when using three different control groups, seems to favor the hypothesis that the policy successfully prevented sexual assaults on and around campuses in California.

California was the first state to introduce this type of policy in 2014. Since then 15 other states have proposed similar policies in many cases borrowing the language and framework from the Californian Bill. The results from this analysis are indicative of a large potential in increasing reporting and preventing sexual assaults in US colleges. If further research shows similar positive effects of these types of policies it is most likely in the interest of both states, schools, and college students to implement policies of this kind.

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Appendix A

Table of showing the number of missing observations by year.

Year	Missing obs.	Zeros	N	Share 0
2011	0	3105	4264	72.82
2012	0	3040	4265	71.28
2013	0	2989	4265	70.08
2014	0	2816	4266	66.01
2015	0	2780	4266	65.17

Appendix B (weights of Synthetic state)

Nevada: 0.521

New York: 0.347

Texas: 0.112

Rhodes Island: 0.019

Massachusetts 0.001

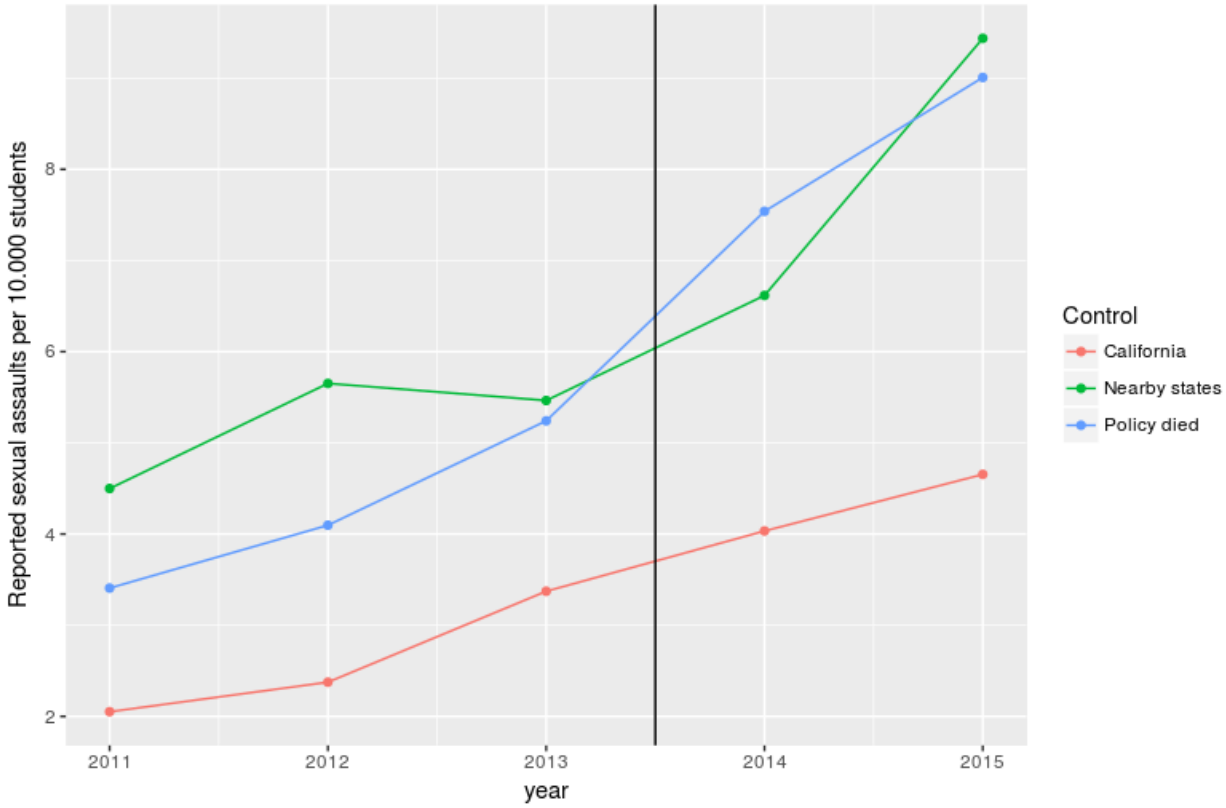

Figure 1: Neaby states control

Appendix C

The control group named 'nearby states' are Washington and Oregon.