

The Effect of Fraternity Moratoriums on Alcohol Offenses and Sexual Assaults

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Abstract

Over 800 universities in the United States have fraternities. While there are positive effects of membership such as increased graduation rates and higher future income, fraternities are also a source of alcohol and partying for students. In this paper, I exploit the variation in timing from 44 temporary university-wide halts on all fraternity activity with alcohol (henceforth, *moratoriums*) across 37 universities over a six-year period (2014-2019). I construct a novel data set, merging unique incident-level crime logs from university police departments to provide the first causal estimates of the effect of moratoriums on campus-wide reports of alcohol offenses and sexual assaults. In particular, I find strong and robust evidence that fraternity moratoriums decrease alcohol offenses campus-wide by 26%. This effect is driven by decreases in weekend reports, consistent with the timing of most college partying. Additionally, I find suggestive evidence that moratoriums decrease reports of sexual assault on the weekends by 29%. However, while moratoriums clearly impact student behavior when implemented, I do not find evidence of long-term changes once the moratorium is lifted.

1 Introduction

Over 800 universities in the United States have fraternities (Hechinger 2017). Existing literature has documented benefits of membership which include higher future income (Mara, Davis, and Schmidt 2018) and significantly more hours spent participating in community service and volunteering (Hayek et al. 2002; Asel, Seifert, and Pascarella 2009). Moreover, according to a Gallup survey in 2021, over 80 percent of fraternity alumni agreed that they would join their fraternity if they were to redo their college experience.

Despite these benefits, fraternity membership has been associated with risky behaviors. In particular, at least one hazing-related death has occurred each year in the US between the years 2000 and 2019,¹ and studies have found that fraternity members binge drink and party more frequently than their non-member peers (DeSimone 2007; Routon and Walker 2014). While universities have regularly banned specific misbehaving fraternities from their campuses, the past decade popularized a new policy tool called *moratoriums*—campus-wide halts on fraternity social events with alcohol—as a way to change member behavior.

This paper is the first to estimate the causal effects of moratoriums on campus-wide police reports of alcohol offenses and sexual assaults. Between 2010 and 2019, over 50 moratoriums have been enacted across university campuses, thus becoming a common policy used by school administrators. However, no prior research has investigated this topic; moratorium dates are difficult to find/confirm and there does not exist a centralized data source for university-specific crime with fine enough detail to enable casual inference. Despite the lack of research surrounding the efficacy of moratoriums, administrators continue to implement moratoriums as a disciplinary action on fraternities.

Nonetheless, how these moratoriums affect student behavior, and thus on-campus crime, is theoretically unclear. On one hand, prohibiting alcohol from fraternity social events may reduce incidences of crime. Fraternities are a source of alcohol for underage drinking, as fraternities are typically a mix of lower and upperclassmen (Armstrong, Hamilton, and Sweeney 2006). The inclusion of legal-age drinkers and large social events facilitates access to alcohol for underage students. Given that the literature has documented that alcohol causes higher prevalence of crimes such as assaults and alcohol offenses (Carpenter and Dobkin 2015), road accidents and arrests (Francesconi and James 2019), and reports of rape (Zimmerman and Benson 2007; Lindo, Siminski, and Swensen 2018), prohibiting such events could reduce the incidence of on-campus crime—especially for underage students. On the other hand, moratoriums may have the opposite effect. Without alcohol-fueled fraternity parties, students may substitute away from consuming alcohol at fraternity houses

¹This is based on the online repository of hazing deaths from journalist Hank Nuwer. See here: <https://www.hanknuwer.com/hazing-destroying-young-lives/>

to potentially riskier places off-campus where behavior is less regulated by the university. As a result, the net effect of moratoriums remains ambiguous.

In this paper, I estimate the causal effect of 44 fraternity moratoriums across 37 universities over a six-year period (2014-2019) on university police reports of alcohol offenses and sexual assaults. I use a difference-in-differences identification strategy, leveraging the variation in timing of moratoriums. Intuitively, I compare academic-calendar days (e.g., excluding summer and winter breaks) with a moratorium to academic-calendar days without a moratorium while accounting for expected differences across days of the week and different times of the year. I construct a novel data set, merging two particularly unique data sources: university-specific Daily Crime Logs, which contain the universe of all incidences of crime reported to the university police at the incident-level, and moratorium start and end dates obtained through school newspapers and public records requests.

Using these data, I find that moratoriums significantly decrease alcohol offenses campus-wide by 26%. This effect is driven by weekends (Fridays-Sundays) when college partying is most frequent and is robust across various specifications, estimation methods, and sensitivity tests. Furthermore, I find suggestive evidence that reports of sexual assaults decrease by 29% on the weekends. Both of these declines are concentrated only when a moratorium is in place, therefore suggesting that there are no persistent effects once a moratorium is lifted. In particular, the immediate and subsequent weeks following a moratorium show little evidence that alcohol offenses or sexual assaults significantly decline and this is consistent across moratoriums of different lengths.

A key distinction of this work is that I am able to closely link changes in student behavior to a campus-wide policy that directly affects college partying. As a result, this study provides further evidence that stronger sanctions on alcohol decrease the number alcohol-related incidents in college-aged individuals, consistent with [Liang and Huang \(2008\)](#) who study zero-tolerance drunk driving laws. However, unlike state or federal laws, moratoriums are unique in that university officials have the power to enact them immediately and indefinitely. This makes moratoriums an appealing policy tool as university officials can implement them at times when they see fit. Moratoriums therefore represent an understudied policy lever that university officials can readily use to reduce campus-wide partying, which in-turn, may affect alcohol and sexual assault incidence.

More broadly, this paper adds to the literature in several bodies of work, the first of which is the effect of college partying. While [Lindo, Siminski, and Swensen \(2018\)](#) find that college partying increases daily reports of rape and alcohol offenses using college football game variation, this study more directly focuses on a policy tool that reduces college partying. As a consequence, I later discuss in [Section 7](#) that moratoriums may have mitigating effects

on college partying behavior when coinciding with college football game-days. Second, this paper contributes to an emerging body of economic work relating to fraternity policy. [De Donato and Thomas \(2017\)](#) and [Even and Smith \(2020\)](#) each study the effects of deferring fraternity recruitment from freshman to sophomore year. However, these works focus on academic outcomes rather than university crime. As of this writing, only one related work has focused on fraternities and crime ([Raghav and Diette 2021](#)), although this study focuses on how the size of a fraternity population effects campus crime rather than the effect of a typical fraternity policy. Last, this paper adds to the literature relating to the effects of alcohol on college-aged individuals which include health effects, such as increases in mortality ([Carpenter and Dobkin 2009](#)) and emergency room visits ([Francesconi and James 2019](#)), and behavioral effects, such as increases in crime ([Carpenter and Dobkin 2015](#)) and hindering academic performance ([Carrell, Hoekstra, and West 2011](#); [Ha and Smith 2019](#)).

This paper proceeds as follows: Section 2 discusses the background on fraternities and moratoriums. Section 3 describes the construction of the data. Section 4 describes the empirical strategy used to estimate causal effects. Section 5 presents the main results. Section 6 explores the differences in effectiveness between different types of schools and moratoriums. Section 7 analyzes possible implications. Section 8 concludes.

2 Fraternities in the US

2.1 Fraternity Demographics and Oversight

On average, fraternities consist of students from families of higher-than-average educational attainment and income; they are predominantly white, and prior research has linked fraternity membership to positive outcomes such as increases in graduation rates ([Routon and Walker 2014](#)), future income ([Mara, Davis, and Schmidt 2018](#)), and social capital formation ([Mara, Davis, and Schmidt 2018](#)). On the other hand, fraternity members spend approximately two more hours per-week partying than nonmembers ([Routon and Walker 2014](#)), binge drink on approximately two additional days per-month ([DeSimone 2007](#)), and membership has been found to decrease GPA ([De Donato and Thomas 2017](#); [Even and Smith 2020](#)). Additionally, [Even and Smith \(2020\)](#) find that membership causes students to select into easier courses and complete fewer course credits. While not causal, there is also survey evidence that fraternity members are more accepting of sexual violence than nonmembers ([Seabrook 2019](#)) and that sorority women, who frequently interact with fraternity men, are four times more likely to be victims of sexual assault than nonmembers ([Minow and Einolf 2009](#)).

This paper focuses on the Interfraternity Council (IFC) fraternities which are a type of social fraternity. These fraternities are the most common at universities and differ from professional, academic, or service fraternities. IFC fraternities participate in philanthropy and professional development, and according to their creed, they “exist to promote the shared interests and values of our member fraternities: leadership, service, brotherhood and scholarship” (Hechinger 2017). Importantly, IFC fraternities are the fraternities that are restricted by moratoriums in the sample.

Each IFC fraternity chapter² has three sources of oversight: the chapter national headquarters, the parent university, and the parent university’s own IFC council—a group of student representatives from each recognized IFC fraternity chapter whom regularly meet with university staff to discuss rules/boundaries. Failure to abide by the rules outlined by these overseers’ policies can result in a fraternity being unrecognized by the university which is costly—a fraternity relies on the university for new students to recruit.

2.2 Moratoriums

A moratorium is defined as a temporary ban on social events with alcohol for IFC fraternities.³ This can include the cancellation of new member recruitment, philanthropy activities, tailgates, or third party vendor events, although the breadth of restrictions differs by university. For example, some universities may allow philanthropy events provided no alcohol is present. Importantly, moratoriums differ from individual chapter suspensions. While universities may temporarily suspend individual fraternity chapters each year, moratoriums apply to all IFC fraternities. Moreover, the timing and length of a moratorium varies substantially. Figure ?? shows the start and end dates of each moratorium over time. Moratoriums in the sample can last as little as six calendar-days, or as long as 848 calendar-days.⁴ Additionally, moratoriums are generally implemented due to triggering events (see Figure ??). These events can be a prominent sexual assault allegation, a fraternity-related death (usually due to alcohol poisoning), or an extreme behavior violation.⁵ Figure ?? shows the distribution of the triggering events: 19 are triggered by behavior violations, 10 by sexual assaults, nine

²A chapter, otherwise known as a *house*, is a unique fraternity. A fraternity can have many chapters across the US, with usually one per-school.

³This is the minimum requirement for a moratorium in this paper. Some universities ban alcohol at social events for all IFC fraternities in addition to the rest of their Fraternity and Sorority Life. However, IFC fraternities are generally the main focus.

⁴Note the distinction between calendar-days and academic-calendar days. A calendar day represent the entire calendar, whereas an academic-calendar represents only the fall/spring semesters of the university school year.

⁵A behavior violation refers to hazing, rule violations, offensive behavior, and other disorderly conduct that results in a moratorium.

by a fraternity-related death, and six are unspecified. As alluded to in the introduction, moratoriums are enacted across the US. Figure ?? shows the locations of the 37 universities in the sample (see Section 3 for further details on sample construction) alongside the corresponding fraction of total enrollment in an IFC fraternity. Most universities are located in the Midwest and South, although there are several universities from both the West and East Coast. Note that the fraction of total enrollment belonging to an IFC is incomplete as only 33 of the 37 (89%) universities provided this information.

Moratoriums can be implemented by two sources of jurisdiction: the university or the IFC council.⁶ When a moratorium is implemented by the university, the university sets the guidelines that fraternities must abide by during the moratorium. On the other hand, an IFC-implemented moratorium is student-enforced. This means that the IFC council is responsible for producing both the guidelines and oversight of the moratorium. Figure ?? shows that IFC-implemented moratoriums are less frequent (17) than university-implemented moratoriums (27), and Section 7 examines the potential differences in oversight.

3 Data

The main analysis uses data from a variety of sources. In particular, I construct a novel data set that links incident-level crime from university police departments to fraternity moratorium dates and university characteristics over a six-year period (2014-2019).

3.1 Sample Construction

The 37 universities in the sample have a combined 44 moratoriums in the sample period (2014-2019). These moratoriums represent any moratorium that match the following criteria: first, the moratorium must prohibit alcohol from all fraternity social events campus-wide, and second, the moratorium must be identifiable by Google/Lexis Nexis searches. Appendix Figure ?? lists all of the universities included with their corresponding moratorium dates. Importantly, these do not represent the universe of fraternity moratoriums that occurred from 2014-2019. In particular, there are six schools that are known to have experienced a moratorium in this time frame, but are excluded due to data issues or their definition of a moratorium.⁷ While there is a possibility that the sample period contains more moratoriums

⁶Note that the fraternity's chapter headquarters cannot impose a moratorium. Since chapter headquarters are unique to a fraternity chapter, they only have jurisdiction over one specific fraternity.

⁷Miami University is excluded due to being unable to verify the end-date of their moratorium. Pennsylvania State University is excluded because they did not digitally release their Daily Crime Logs. University of Texas at Arlington is excluded because the crime logs are scanned images that can not be read reliably by any computer software. Cal State Northridge is excluded because it is unclear whether the moratorium

from other universities, the documents provided from various fraternity associations and conversations with experts in the field suggest that the sample covers the large majority. Furthermore, each moratorium’s start and end dates are obtained through public records requests (20%), conversations with Fraternity and Sorority Life advisers (11%), and school newspaper articles (68%). All start and end date are verified by at least one of these sources.⁸

Daily reports of incidents are parsed from Daily Crime Logs maintained by the 37 universities’ police departments resulting in approximately 500,000 distinct reports. The Daily Crime Log is an incident-level source of information; each crime log contains the date occurred, date reported, time occurred, time reported, a short summary of the incident, the general location of the incident, and a distinct case number (see Appendix Figure ?? for an example).⁹ The Daily Crime Log contains the universe of incidents that are reported to (or by) the university-specific police department. Hence, each of the incidents listed in these logs represent incidents that occurred on or nearby university property.¹⁰

There are two main advantages of the Daily Crimes Logs over readily available crime data sources such as the National Incidence-Based Reporting System (NIBRS), Uniform Crime Reporting System (UCR), and the Campus Safety and Security Data (CSS). First, each university police department is mandated under the Clery Act to maintain and make available a Daily Crime Log. Crime logs must be kept for seven years, although this mandate is subject to each university’s interpretation.¹¹ Hence, only one university is missing data from a complete calendar-year.¹² Second, the Daily Crime Logs contain all daily incidences of alcohol offenses and sexual assaults reported to or by the police—the primary outcomes

includes a ban on alcohol. University of North Florida is excluded because of a discrepancy between public records information and newspaper articles: newspaper articles claim there is a moratorium beginning 12/4/17, but the public records department says this is untrue. The University of Vermont is excluded due to issues with the reliability of the data—crimes often are reported to have occurred in large intervals of days (or months) for nearly 40% of the data provided which is not suitable for the daily-level analysis in this paper. There may exist other universities that experienced a moratorium, but may not have had any sort of news coverage—these are also excluded from the sample.

⁸There is one exception to this which is the first moratorium at San Diego State University. While the start date has been verified by a newspaper article, the exact end date is a little ambiguous. However, evidence shows that the moratorium ended before the start of the 2015 spring semester, and hence, this is the date used in the analysis. The newspaper article showing this evidence can be seen here: https://newscenter.sdsu.edu/sdsu_newscenter/news_story.aspx?sid=75357.

⁹While the date occurred is technically mandated under the Clery Act to include each of these categories, only 32 of the 37 universities contained the date occurred. However, these five schools contained the date reported. I use the date reported in lieu of the date occurred when the date occurred is missing.

¹⁰Sometimes, university police may respond to calls slightly outside of university property. Based on conversations with university police, this is usually when a student is involved.

¹¹For instance, if a crime log from 2014 is requested in year 2021, most police departments will have this information as it falls within 7 years. However, some police departments may consider seven-years to be inclusive of their current year, and hence, may only retain records for 2015-2021.

¹²Rollins College is missing data from 2014. North Carolina State University is also missing data, although their missing data spans from January 2014-August 2014.

used in the main analysis. This is a major advantage as the UCR does not contain alcohol offenses and the NIBRS only contains alcohol violations that end in arrests. Since not all violations of underage drinking at universities result in arrests, the NIBRS data would under-report the prevalence of alcohol misuse. While the CSS data includes similar information as the Daily Crime Logs,¹³ the CSS data is aggregated to the calendar-year which makes the effect of moratoriums difficult to study given their short-lived nature. See Appendix Table ?? for more details on the advantages of the Daily Crime Logs.

University characteristics such as total enrollment, student demographics, and academic calendars are obtained through the Integrated Postsecondary Education Data System (IPEDS) or directly from the corresponding university. However, not all academic calendars for each year in the sample are available. Therefore, only the most current academic calendar found on a university’s website is utilized. To define the start of a semester, the first day of instruction is used while the finalized grade date is used to denote the end of a semester. Since there are small changes in academic calendars year-to-year, a seven-day window is subtracted from each start date and added to each end date of every semester. For instance, if a semester begins on August 20th and ends on December 16th, the sample period will be August 13th to December 23rd.

3.2 Matching and Harmonization

One of the challenges of using the Daily Crime Logs is their uniqueness to each university. While all crime logs contain daily reports of incidents, each university police department describes their incidents differently. For example, Indiana University’s crime log describes driving under the influence as “driving under the influence” while Cal Poly San Luis Obispo’s describes this as “dui.” As such, there is a lack of harmonization between the crime logs—incidents do not have a standardized way of being reported between university police departments. To mitigate this issue, I use regular expressions to match on typical words, phrases, and abbreviations seen in each crime log for descriptions relating to alcohol offenses and sexual assaults.¹⁴ For each offense, I use the following definitions for matching the incident descriptions:

- **Alcohol Offense** - Any incident description that refers to a public intoxication, underage drinking, or drinking in an unlawful manner. For instance, public drunkenness,

¹³There are important differences between these two sources. The CSS provides data on liquor violations that occur in residence halls that may not be reported to the police and therefore not appear in the Daily Crime Logs. Hence, an aggregated Daily Crime Log should not (and will not) match the CSS exactly.

¹⁴In particular, I found all unique descriptions of incidences in each Daily Crime Log, and then independently analyzed which descriptions matched to each offense.

a minor in possession, and driving while intoxicated refer to each of these definitions respectively.

- **Sexual Assault** - Any incident description that refers to a sexual assault or sex crime including rape and fondling. This corresponds to the types of sex crimes that are reported in the CSS data: rape, statutory rape, incest, and fondling. However, incest sex crimes are omitted as these are infrequent and less likely to be associated between college students.

Table ?? shows the corresponding words, phrases, and abbreviations used to match each incident description to its corresponding offense. Importantly, each of these phrases is only a portion of an incident’s description. For instance, the word “sex” is used as a word to match on sexual assaults. The advantage to this method is that the word “sex” will be matched to descriptions such as “sexual assault” or “sex offense” since the word “sex” appears in each of these descriptions. While this is an imperfect method, it is conservative—it is likely that this method is under-counting the true number of offenses in each category since there are instances in incident descriptions where words are misspelled (e.g., “aclohol” vs. “alcohol”). Appendix Table ?? shows the results of this matching process with the most 15 most frequent descriptions matched to each offense.

3.3 Descriptive Statistics

Table ?? summarizes the characteristics of the 37 universities and their corresponding distribution of offenses and fraternity moratoriums. Panel A shows descriptive statistics of the universities’ demographics. On average, the universities are large with total enrollment exceeding 29,000. Undergraduates are the majority population with 61% of the student population being white. Graduation rates vary substantially between schools and there is particularly large variation in the selectivity of each university. For instance, graduation rates and the fraction of students admitted range between 39-95 percent and 14-94 percent respectively. Moreover, IFC fraternities represent a small fraction of the total enrollment with approximately 5% of students belonging to an IFC fraternity on average. Panel B shows summary statistics of the primary outcome measures: reports of alcohol offenses and sexual assaults. Each of these outcomes are measured as per-25,000 enrolled students per-academic-calendar day. Therefore, the average amount of alcohol offenses per-25,000 enrolled students in an academic-calendar day is approximately one-half. Lastly, Panel C describes characteristics of the 44 moratoriums in the sample. On average, each university undergoes approximately one moratorium, although universities can experience up to three. Furthermore, the moratoriums persist for an average of 64 academic-calendar days. Notably,

there is significant variation in the length of the moratoriums. In particular, the minimum length of a moratorium is only six academic-calendar days while the maximum is 541. Due to this large range, it is important to note that a median moratorium lasts for approximately 46 academic-calendar days (approximately 1.5 months).

4 Empirical Strategy

4.1 Empirical Approach

In order to estimate the average causal effect of fraternity moratoriums on alcohol and sexual assault offenses, I estimate the following baseline difference-in-differences specification using OLS:

$$Y_{u,t} = \beta \text{Moratorium}_{u,t} + \gamma_u + \lambda \mathbb{X}_t + \epsilon_{u,t} \quad (1)$$

where $Y_{u,t}$ is an outcome of alcohol offenses or sexual assaults per-25000 enrolled students per academic-calendar day at university u in time t . $\text{Moratorium}_{u,t}$ is an indicator variable equal to one when university u is undergoing a moratorium at time t , γ_u is a university-specific fixed effect, \mathbb{X}_t is a vector of time-varying controls that are shared across universities, and $\epsilon_{u,t}$ is the error term. The standard errors are clustered by university (Bertrand, Duflo, and Mullainathan 2004). In essence, Equation 1 is comparing moratorium days to non-moratorium days within universities that have experienced, or will experience a moratorium while accounting for expected differences across universities and time.

Including university-specific fixed effects (γ_u) in the baseline model accounts for systematic differences between a university’s police department, the corresponding student demographic they are policing, and overall fixed differences in incident prevalence. For instance, university police departments may have systematic differences in the frequency of reporting due to the corresponding party-culture of their university or their own policing practices/resources, which in-turn, may lead to stronger or weaker enforcement of student drinking. Hence, including university-specific fixed effects ensures that moratorium days are compared to non-moratorium days while adjusting for these expected differences in universities. Moreover, \mathbb{X}_t includes day of the week, semester type (spring/fall), holiday, football game-day, and academic-year controls. Day of the week controls are included to address day-to-day fluctuations, while semester controls are included to adjust for activities that vary across the year such as fraternity recruitment. Additionally, football game-day controls are included to account for the increases in both alcohol offenses and rapes that college foot-

ball games cause (Rees and Schnepel 2009; Lindo, Siminski, and Swensen 2018).¹⁵ Lastly, holiday controls¹⁶ are included since there may be less student activity on holidays and academic year controls are included due to differences between fraternity rules and guidelines between academic-years. Taken together, the corresponding interpretation of the parameter of interest, β , is the average difference in offense $Y_{u,t}$ on moratorium days relative to non-moratorium days, conditional on the expected differences between universities, days of the week, holidays, semesters, football game-days, and academic-years.

I expand on the baseline model in Equation 1 using several other specifications to allow for more flexibility in controlling for differences between universities’ academic years. In particular, I progressively add both university-by-academic-year and university-by-academic-year-by-semester fixed effects to allow for several different comparisons within the model. The inclusion of university-by-academic year fixed effects allow for comparisons within university academic years while university-by-academic-year-by-semester fixed effects allows for comparisons within a semester during a university’s academic year. While university-by-academic-year-by-semester fixed effects are most flexible, a large fraction (33%) of moratoriums span across multiple academic-year-semesters which leads to a small number of comparisons within each university-academic-year-semester. Moreover, as shown later in Section 5, including these fixed effects produces less conservative results than the inclusion of university-by-academic-year fixed effects. Because of this, the preferred specification utilizes university-by-academic-year fixed effects, although I show that the results are similar across all empirical approaches. Hence, unless otherwise noted, all analyses in this paper utilize the preferred specification which include university-by-academic-year fixed effects.

4.2 Identification Assumptions

To estimate Equation 1 and interpret β as a casual effect of fraternity moratoriums, there are three main assumptions that need to be satisfied: the timing of fraternity moratoriums is as-good-as-random, there are no changes in reporting/policing during a moratorium, and moratoriums have no lasting effects.

To address the first assumption of as-good-as-random timing, I estimate a ‘multiple event’ event study to identify any trends prior to a moratorium. Given that each university can experience multiple moratoriums and each moratorium is a different length, a staggered adoption event-study design is not appropriate. Therefore, to estimate the event study,

¹⁵Information on football game dates and locations are found using sports-reference.com and espn.com. In total, 34 of the 37 universities in the sample that have football teams resulting in over 2000 football games, 89 of which coincide with a moratorium.

¹⁶Holiday controls include indicators for Veterans Day, Thanksgiving, Labor Day, Halloween, and MLK Day. Christmas/New Years/July 4th are not included since no university’s academic-calendar contains them.

I follow the guidelines outlined in [Schmidheiny and Siegloch \(2020\)](#); I generalize a classic dummy variable event study to accommodate multiple moratoriums within a university and classify the event-time (i.e., period 0) as the entire moratorium period. Therefore, period 0 represents all moratorium days within a university.

Figures ?? and ?? show the results of the ‘multiple event’ event study which demonstrate that there is little suggestive evidence that crime is already decreasing prior to a moratorium. Recall that the shaded area (period 0) represents an entire moratorium period while each lead and lag represents a 14-day period prior to, or proceeding a moratorium (normalized by the 14-day period immediately proceeding a moratorium). 14-day periods are chosen in lieu of 7-day periods to allow for more precise point estimates. Five periods before and after are estimated, but only four are included as the fifth lead and lag are binned endpoints as described in [Schmidheiny and Siegloch \(2020\)](#). The errorbars represent 95% confidence intervals while the number of periods before/after the moratorium period are chosen to give approximately a median moratorium length of days (46) before and after the moratorium period. In each figure, there is little visual evidence of a downward or upward trend prior to a moratorium. This is reinforced with statistically insignificant F-test showing that the three pre-periods are jointly zero at the five and 10 percent level.¹⁷ Moreover, the results of this analysis are intuitive; moratoriums are caused by triggering events in which typical behavior is taken “too far” and are usually enacted within three days following such event,¹⁸ thus giving little reason to expect anticipatory effects. In addition, according to an online repository of fraternity-related deaths from journalist Hank Nuwer, there are 19 universities that experienced a fraternity-related death but *did not* undergo a moratorium in the sample period which suggests that fraternity members may not expect a moratorium even when experiencing a particularly salient act of misconduct. Taken together, there is little suggestive evidence of a decreasing crime trend prior to a moratorium.

To test the second assumption that moratoriums do not change policing or offense reporting, I conduct an indirect test which shows no significant change between reporting behavior. In particular, I test whether there are significant differences between the time incidents occur and the time they are reported during a moratorium (i.e. *reporting lags*). This test is motivated by the notion that reporting lags may be due to factors such as police force staffing or the willingness of students to report. To perform this test, I construct the proportion of offenses that are reported with a lag on a given day for each offense.¹⁹ An offense is defined

¹⁷As a measure of robustness, an alternative event-study is estimated using 46-day periods before and after a moratorium in Figures ?? and ?. Each of these figures fails to show evidence of a decreasing or increasing pre-period trend.

¹⁸This statistic is based on 13 of the 15 universities in which I have data on date of the triggering event.

¹⁹Only 32 of the 37 universities had data for the date occurred of their incidents. Hence, this test only reflects a subset of the sample.

as reported with a lag if the date the incident occurred is not equal to the date the offense was reported.

Table ?? shows that there is no significant change between the proportion of crimes reported with a lag during a moratorium. As a measure of robustness, I change the definition of a lag to reflect a difference of one, three, seven, and 14 days between the date occurred and date reported.²⁰ Panel A shows that roughly 0.3% of alcohol offenses are reported with a one-day lag, and the change during a moratorium is insignificant. Similarly, Panel B shows no difference in lagged reporting for sexual assault offenses. While sexual assaults have a higher proportion of reports that are reported with a lag (1.7%), the change during a moratorium is also insignificant.

For the final assumption, I conduct two series of analysis which are further discussed in Section 5 to show that moratoriums have no lasting effects. Note that Equation 1 implicitly assumes that student behavior changes only during moratoriums and that this behavior change does not persist over time. To address this, I supplement the preferred specification with a week lead and week lag to identify whether the effects of moratoriums instantaneously disappear once a moratorium is lifted. Moreover, I conduct an F-test on the 4 post periods in the ‘multiple event’ event study and show that there is no significant post trend. These results further justify the use of already-treated universities (i.e., universities that have already experienced a moratorium) as a reasonable control group—a common critique of the difference-in-differences estimator with variation in treatment timing (Goodman-Bacon 2021). Given that moratoriums show no lasting effects, an academic-calendar day without a moratorium is a good counterfactual for an academic-calendar day with a moratorium.

4.3 Sample Challenges and Difference-in-Differences Literature

Several recent journal articles have found that using OLS in a two-way-fixed-effects (TWFE) difference-in-differences design can cause issues with the coefficient estimates in the presence of heterogeneous treatment effects between groups over time (Chaisemartin and D’Haultfoeulle 2020; Sun and Abraham 2021; Athey and Imbens 2022). In particular, the coefficient on the explanatory variable for treatment is a weighted sum of average treatment effects where some of the weights may be negative. This negative weighting occurs when the two-way-fixed-effects estimator uses treated observations as controls (Goodman-Bacon 2021; Chaisemartin and D’Haultfoeulle 2020; Borusyak, Jaravel, and Spiess 2022). While this paper’s research design is not a typical TWFE design since treatment is not staggered, the treatment can occur multiple times, and the preferred specification uses interacted

²⁰Literature such as Sahay (2021) use a 3-day lag when applying this test.

group and time fixed effects, there remains a possibility that the negative weights issue could extend to the preferred model used in this paper due to the exclusion of never-treated units. Since the new estimators proposed by [Callaway and Sant’Anna \(2021\)](#) and [Chaisemartin and D’Haultfœuille \(2020\)](#) are not suitable for this experimental design, I conduct two different series of analyses which yield consistent results with my main findings: one that analyzes a typical TWFE design in this setting using university and day-by-month-by-year fixed effects (see Appendix ??), and another that includes 14 never-treated universities to potentially reduce the occurrence of negative weights (see Section 5).

5 Results

In this section, the estimated causal effects of a fraternity moratorium on alcohol offenses and sexual assaults are reported using OLS. Figure ?? serves as a preview of the main results and plots the distribution of differences between the number of offenses per-25000 enrolled students on moratorium days and non-moratorium days. On average, most universities observe fewer alcohol offenses and sexual assaults on moratorium days as displayed by the dashed line.

5.1 Main Results

Table ?? reports that fraternity moratoriums lead to substantially fewer alcohol offenses across university campuses and provides suggestive evidence of decreases in sexual assaults. Column (1) shows the baseline specification from Equation 1. This baseline specification includes day of the week, holiday, semester, football game-day, and academic-year fixed effects. Moreover, columns (2) and (3) show the results of adding increasingly flexible fixed effects, although recall from Section 4 that column (2) is the preferred specification. In Panel A, alcohol offenses decrease during moratorium days relative to non-moratorium days in the academic-calendar. Across the three specifications, an average moratorium day exhibits between 26 to 28 percent fewer alcohol offenses in comparison to an average academic-calendar day as reflected in the point estimates. These estimates are statistically significant across all specifications, reemphasizing that moratoriums decrease campus-wide alcohol offenses. Although the point estimates on alcohol offenses are robust, the estimates on sexual assaults fail to achieve statistical significance across each specification. Additionally, the magnitude of each specification varies considerably; sexual assaults show a 14-20 percent reduction from the mean across each estimation.

The effects of moratoriums are driven by the weekends (Friday-Sunday), consistent with

the literature that most college partying occurs on weekends rather than weekdays (Lindo, Siminski, and Swensen 2018). Table ?? shows the preferred specification from Table ?? separated by weekends and weekdays; the column *All Days* corresponds to the estimates of column (2) from Table ?. During the weekends, alcohol offenses decrease by 28% relative to an average academic-calendar weekend as shown in Panel A. Nevertheless, weekdays show no statistically significant decreases. Similar to alcohol offenses, the point estimates on sexual assaults show larger decreases on the weekend than on weekdays in Panel B. A weekend during a moratorium can expect 29% fewer sexual assaults relative to an average academic-calendar weekend.

Importantly, these results persist across a variety of robustness and sensitivity tests. First, given the non-negative count nature of the offense data and the sensitivity of OLS estimation to outliers, Appendix Tables ?? and ?? reaffirm the results reported in Tables ?? and ?? using poisson estimation in lieu of OLS. Specifically, poisson estimation shows a statistically significant 27 and 32 percent average reduction in alcohol offenses and sexual assaults on the weekends respectively. Second, to ensure that the results described above are not driven by a single university, Appendix Figures ?? and ?? show leave-one-out coefficient estimates for each offense. In particular, 37 unique regressions are estimated for each offense, omitting one university within each iteration—all which demonstrate similar findings to the main results. Finally, recall from Section 4 that negative weights occur in the difference-in-difference estimator when treated units are used as control groups. Given that the sample includes only treated universities, I include 14 additional universities in the sample that never underwent a moratorium in the period of analysis to potentially mitigate the negative weighting issue. This amounts to 51 universities for a total of approximately 75,000 academic calendar days. Each of the additional universities are chosen from the Colleges with the Best Greek Life list on Niche.com.²¹ Never-treated universities are selected if they are regarded as a Top 50 Greek Life school.²² Seventeen of these universities are already included in the sample due to experiencing a fraternity moratorium, further justifying the remaining 33 Top 50 Greek Life universities as a good counterfactual. However, only 14 of these universities are included in the sample while the remaining 17 are excluded since they are unable to provide Daily Crime Logs. Appendix Table ?? shows the effects of moratoriums when including these never-treated universities. Overall, the results remain similar, with weekend decreases in alcohol offenses and sexual assaults of approximately 18 percent and

²¹I use Niche.com since it is the top search result on Google when searching for the “best fraternity colleges”. The Princeton Review, notable for its annual list of party schools, does not have a list regarding fraternity life.

²²Notably, it is known that at least one university (Chico State) had a moratorium outside of the sample period (2013). This, however, only further validates the selection of the never-treated universities.

26 percent respectively.

5.2 Are There Spillovers to Nearby Areas?

One potential caveat to these results is that the observed decreases in alcohol offenses and sexual assaults shown in the Daily Crime Logs are being displaced to potentially riskier areas. For instance, while campus-wide alcohol is decreasing, it may be that fraternity members and other students are substituting their behaviors on-campus to off-campus areas that are less regulated. If this is true, the net effect of a moratorium may be worse than never implementing a moratorium. Unfortunately, there does not exist a perfect data source to explore such mechanism directly; the National Incidence-Based Reporting System (NIBRS) only reliably²³ covers 24 percent of the sample universities' neighboring police departments and includes only alcohol arrests rather than all incidents. Furthermore, the Campus Safety and Security (CSS) data, while containing all incidences of crime reported on university campuses, is aggregated to the yearly level.

Despite these challenges, I perform two sets of analyses using these data. First, to identify whether crime incidence is displaced into nearby areas, I use the NIBRS data to compare the reported incidence of crimes at nearby police departments with the crimes reported at university-specific police departments using the Daily Crime Logs. Nearby police departments are defined as police departments that serve the surrounding area, but are not affiliated directly with a university.²⁴ This results in a comparison of nine university police departments from the Daily Crime Logs and their corresponding nearby police departments from the NIBRS. To harmonize the NIBRS data with the Daily Crime Logs, I define each offense from the NIBRS as per-25000 enrolled students at the corresponding university and limit the panel to only academic-calendar days. Both alcohol offenses and sexual assaults are restricted to incidences involving college-aged individuals (i.e., 17-22), although the results are consistent when broadening the definition to include all ages. Moreover, I define sexual assaults in the NIBRS data to include fondling, rape, and sexual assault with an object to align with the definition using the Daily Crime Logs.

In both Panels A and B of Table ??, alcohol offenses and sexual assaults have an insignificant and negative point estimate at nearby police departments, thereby suggesting no substantial spillovers. Reassuringly, the university-specific police departments continue to

²³In this case, I consider a data source to be reliable if reporting of crime is consistent in the sample period. NIBRS features only nine schools that continually report data without large missing periods.

²⁴The neighboring police departments were identified using [Lindo, Siminski, and Swensen \(2018\)](#) public access data files in addition to Jacob Kaplan's NIBRS data tool available here: https://jacobdkaplan.com/nibrs.html#state=Colorado&agency=Denver%20Police%20Department&category=murder_nonnegligent_manslaughter&rate=false

show large and significant effects of the moratorium for alcohol offenses despite being a small subset of the main sample. These results give weight to the interpretation that moratoriums are decreasing the number of alcohol offenses on university campuses and students are not moving their risky behaviors to off-campus areas that are less regulated by the university.

As the second set of analysis, I analyze the CSS data to examine if students substitute partying at fraternity houses to different on-campus locations during moratoriums. The CSS data contains all violations of liquor, drug, and sexual assaults that occur in a calendar-year. The main advantage to using the CSS data is that it delineates between crimes that occur within a residence hall or a different on-campus location. Moreover, the CSS data includes liquor violations that may not have been reported to the university police (thus not in the Daily Crime Logs) if they were handled internally by university staff. For instance, if a liquor violation occurs in a residence hall, this citation will be absent from the Daily Crime Logs if it is handled only by the residence hall staff. However, recall that the biggest disadvantage to this data is the aggregation of all incidents to the calendar-year level. Since moratoriums can last for as few as six days and can continue through multiple calendar-years, this analysis should be taken only as speculative, not causal. See Appendix ?? for a more detailed discussion of the CSS data and the corresponding model.

Despite the shortcomings of the CSS data, there is evidence that moratoriums move drinking from fraternity houses to residence halls. Residence halls show a 28% *increase* in alcohol offenses relative to the mean when a proportion of a calendar-year is in a moratorium. Interestingly, this is accompanied by a large 82% *decrease* from the mean in residence hall sexual assaults. Although these results appear counterintuitive given the literature documents that alcohol offenses and sexual assaults tend to coincide (Lindo, Siminski, and Swensen 2018), these results point to the possibility that moratoriums cause a substitution effect of partying behavior; students substitute drinking at fraternity houses to residence halls. Residence halls, unlike fraternity houses, are far more regulated, contain university staff, and potentially have more sober bystanders to intervene if behavior appears to be escalating dangerously. Taken together, these results support the notion that if moratoriums displace dangerous alcohol-fueled behavior, they displace it to *less* risky areas.

5.3 Do Moratoriums Have Long-run Effects?

Although moratoriums clearly impact student behavior when implemented, there is not substantial evidence showing that moratoriums provide long-run impacts. In this subsection, I perform two series of analyses: first, I conduct an F-test on the lagged coefficients in the event study specification shown in Section 4, and second, I extend the preferred specification from Table ?? with an indicator for the week before and week after a moratorium.

Table ?? reports the results of the first set of analysis which fails to show significant evidence of long-run effects. Panel A includes results from the event study estimation shown in Figures ?? and ?. In addition, p-values from joint F-tests on the four lagged coefficients are reported. The p-value for both alcohol offenses and sexual assaults is not significant at the 5 or 10 percent level therefore showing little evidence that the effect of the moratorium persists in the four 14-day periods (56 total days) following a moratorium.

While the sample does not collectively exhibit long-run effects, I supplement this analysis by splitting the sample into three quantiles based on the length of a moratorium. Each quantile represents universities with a moratorium less than 32 academic-calendar days (quantile 1), between 33 and 59 academic-calendar days (quantile 2), and more than 60 academic-calendar days (quantile 3).²⁵ This test is motivated by the idea that longer moratoriums may induce more behavior change than relatively short ones. Panel B of Table ?? shows the p-values corresponding to the F-tests on the four lagged coefficients for both alcohol offenses and sexual assaults. Similar to Panel A, there is no statistical significance across each test. Interestingly, there does appear to be evidence that moratoriums with lengths between 33 and 59 days (quantile 2) have the largest instantaneous effects, therefore showing that the length of a moratorium may be crucial to the overall effectiveness.

Last, Figure ?? reports the estimates from the second analysis which extends specification (2) in Table ?? with an indicator variable for the week after and week before a moratorium. When considering the entire sample, each offense exhibits decreases that persist only during the moratorium period and instantaneously return to previous levels in the week directly following a moratorium. This pattern persists when restricting the sample to weekends where the effects of the moratorium are most prominent.

6 Heterogeneity

6.1 Do Party Schools Exhibit the Strongest Effects?

Universities that have a reputation for partying may be more impacted by the restrictions of moratoriums than universities that party less. For example, [Lindo, Siminski, and Swensen \(2018\)](#) find that party schools exhibit two times the increase in reports of rape on football game days than non-party schools. To examine this possibility, I use Niche.com's Top Party

²⁵Note that six universities have more than one moratorium and can therefore be included in multiple quantiles. This occurs for five of the six universities. However, this represents a small fraction within each quantile: quantile 1 (20%), quantile 2 (23%), and quantile 3 (26%)

Schools in America list.²⁶ The list assigns “party scene” scores based on criteria such as athletic department revenue, fraternity and sorority life statistics, access to bars, and student surveys.²⁷ Using this list, a university is defined as a party school if it appears in the top 50 rankings. This amounts to 16 of the 37 universities in the sample being classified as a party school.

Table ?? shows that party schools exhibit larger decreases in alcohol offenses than non-party schools. The point estimates in Panel A indicate that moratoriums decrease alcohol offenses on academic-calendar days by approximately 33% from the mean for party schools and 16% for non-party schools. Importantly, only the point estimates for party schools are statistically significant, therefore suggesting that the effects of the moratorium are driven by schools that have a stronger party culture.

6.2 Does the Reasoning for a Moratorium Matter?

As described in Section 2, moratoriums can be the result of a fraternity-related death, a prominent sexual assault, or a behavior violation. There is little reason to expect similar effects across these triggering events. As an illustration, a death may be more salient than a behavior violation,²⁸ resulting in a behavior shock to the college campus. Moreover, both deaths and sexual assaults are more objectively undesirable results of risky behavior—a moratorium may seem more justified under these outcomes rather than an instance of hazing.

Figure ?? reports that the effect of moratoriums is most prominent when a death or sexual assault is the corresponding trigger. In particular, alcohol offenses decline significantly when a fraternity-related death is the triggering event. To ensure that this is the effect of the moratorium rather than the triggering death, I separately analyze (although not shown) the sample restricting to only the universities that experienced a fraternity-related death with an additional 15 universities that experienced a fraternity-related death in the sample period, but *did not* undergo a moratorium.²⁹ Hence, the supplemental universities are those whose students experience the effects of a fraternity-related death, but do not experience a moratorium. Therefore, if the shock of death is the mechanism which produces decreases in alcohol offenses, then the effects of a moratorium on alcohol offenses would be insignificant when including these universities as a control group. However, with the inclusion of these

²⁶I use Niche.com over the Princeton Review since the Princeton Review no longer posts their party school rankings.

²⁷For more details on the methodology see: <https://www.niche.com/about/methodology/top-party-schools/>.

²⁸Recall that a behavior violation includes hazing, offensive behavior, rule violations, and other disorderly conduct.

²⁹These universities were found using Hank Nuwer’s repository of hazing-related deaths in the US: <https://www.hanknuwer.com/hazing-deaths/>.

universities, alcohol offenses maintain similar results—decreases in alcohol are significant at the 10% level.

Additionally, Figure ?? shows significant decreases in sexual assaults when a triggering event involves either a sexual assault or behavior violation. However, the shortcomings of the estimations underlying these results must be carefully considered. Specifically, sexual assaults are a significantly under-reported offense—survey evidence shows that nearly 80% of sexual assaults go unreported.³⁰ Due to this, sexual assaults are relatively rare in police reports, thus resulting in a small number of observations which hinders identification. In addition, these estimates are based on a small subset of universities (19 universities for behavior violations and 10 for sexual assaults). Overall, the results indicate evidence of decreases in sexual assaults, although this evidence is mostly speculative under these data limitations.

7 Discussion

7.1 Do Moratoriums Mitigate the Effects of Football Games?

Both Rees and Schnepel (2009) and Lindo, Siminski, and Swensen (2018) show that college football games cause higher instances of alcohol offenses and rape respectively. While football games cause negative outcomes, universities are reluctant to suspend football games—college football is popular among students and alumni in addition to being a major source of revenue. Therefore, finding an effective policy that can mitigate the detrimental effects of football games while maintaining the benefits is important for university administrators. This subsection analyzes whether moratoriums are the policy tool that can accomplish this.

Figure ?? shows that football game-days cause a significant increase in the number of alcohol offenses and sexual assaults. These effects are identified by 34 of 37 universities that have football teams in the sample, resulting in over 2000 football games. Each of these effects are largest on home games rather than away games which is consistent with Lindo, Siminski, and Swensen (2018) and Rees and Schnepel (2009). Furthermore, Figure ?? also shows the combined effect of a game day that occurs during a moratorium. In each of these estimations, the point estimates remain consistent with the effect of game-days only, although less precise. This may be caused by a lack of identifying variation—the estimates are identified by 89 occurrences of game days that coincide with moratoriums. As a robustness check, I broaden the definition of game-days to game-weekends in Appendix

³⁰This is based on statistics from the AAU Campus Climate Survey on Sexual Assault and Sexual Misconduct. See here: https://ira.virginia.edu/sites/ias.virginia.edu/files/University%20of%20Virginia_2015_climate_final_report.pdf

Figure ?? . Although this nearly triples the amount of identifying variation,³¹ the results are consistent. Considering these results, it is uncertain whether moratoriums mitigate the effects of game-days. On one hand, these results offer the possibility that fraternities are not an integral component to college partying on game-days—students can substitute away from fraternity parties to other alternatives such as tailgates. On the other hand, it may be that moratoriums restrict the amount of dangerous partying that occurs during football games and produce a safer environment. Since the estimates are imprecise, it is unclear whether moratoriums can act as an effective policy tool to mitigate alcohol offenses or sexual assaults on football game-days.

7.2 Who Should Enforce Moratoriums?

Recall from Section 2 that there are two sources of enactment/oversight for campus-wide moratoriums—the university itself and the university-specific IFC council. In the sample, 27 of the 44 (61%) moratoriums are enacted by a university. There is reason to suspect differences between these two sources of jurisdiction since IFC moratoriums may lack the incentive structure that university moratoriums have. For instance, a university can permanently suspend a fraternity chapter from its campus for failure to abide by moratorium guidelines which may damage the fraternity chapter’s membership and reputation. On the other hand, IFC councils have little incentive to permanently suspend or impose additional sanctions as fraternity chapters rely on each other to create a thriving social life and community. As such, further disciplinary measures by the IFC-council directly affects the council members themselves, thus creating a system that may incentivize IFC council members to look away from the moratorium guidelines.

In Table ?? alcohol offenses have suggestive evidence of a decline when a university imposes the moratorium as shown in Panel A. Consistent with the main results, the largest effects are on weekends rather than weekdays. However, the coefficient estimates for sexual assaults are insignificant across both university-imposed and IFC-enacted moratoriums, likely due to the infrequent reporting of sexual assaults. While there is no definitive evidence for differences in enforcement for sexual assaults, the significant declines in alcohol offenses points to the university administration as the more effective enforcement body rather than the fraternity members themselves.

³¹Not all game-days occur on a weekend, so the expanding the definition to a game-day weekend does not quite triple the number.

8 Conclusion

In this paper, I estimate the causal effect of temporary restrictions of fraternity social events with alcohol (*moratoriums*) on campus-wide reports of alcohol offenses and sexual assaults across 37 universities in the US. I construct a novel dataset which includes daily-level incident reports from each university police department. Using these data, I compare academic-calendar days with a moratorium to academic-calendar days without a moratorium while controlling for expected differences in the days of the week, holidays, semesters, academic years, football game-days, and universities. I find that moratoriums decrease the average reports of alcohol offenses on a given academic calendar day by approximately 26%. This result is most prominent on the weekends when partying is most frequent (28% reduction) while nonexistent on the weekdays, and importantly, shows no substantial evidence of displacing crime to nearby areas. Moreover, I find suggestive evidence of decreases in reports of sexual assaults on the weekends by 29%, although only significant at the 10% level. Notably, moratoriums show no lasting effects, and this result is consistent across moratoriums of shorter and longer lengths. Taken together, these results support the notion that moratoriums are only effective in temporarily reducing campus-wide crime.

Given that moratoriums are unable to create permanent changes in student behavior, it is unclear whether they should continue as active policy. On one hand, moratoriums may move college partying behavior to safer areas (residence halls) as speculated in Section ??, or may alleviate the detrimental health effects that alcohol causes in college students such as hindering academic performance and costly emergency room visits. On the other hand, moratoriums do not permanently change student behavior; while moratoriums have large effects during enforcement, moratoriums are an unproductive policy to systematically reduce college partying behavior. Hence, school administrators should understand that moratoriums are a transient solution and should therefore look for other methods to promote long-term change. One understudied possibility is the suspension of specific misbehaving fraternity chapters from universities rather than IFC moratoriums. Although this policy alleviates the criticism that moratoriums are punishing even well-behaving fraternities, it is unclear whether this truly propagates behavior change—members of a poor behaving fraternity may choose to substitute to a new fraternity and thereby negatively influence its members.

It is important to understand that this paper *does not* provide evidence advocating for the removal of fraternity life. Within this study, none of the universities removed fraternity life, only prohibited their social events with alcohol. Hence, this paper does not provide support for national movements such as “Abolish Greek Life”; recall that prior research has linked membership to many beneficial outcomes. However, this study *does* quantify the effect of

restricting fraternity events with alcohol, which in-turn, significantly decrease campus-wide reports of alcohol offenses and sexual assaults.

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