

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 future income, fraternities are also a source of alcohol and partying for underage students. In this paper, I exploit the variation in timing from 44 temporary university-wide halts on all fraternity activity with alcohol (moratoriums) across 37 universities over a six-year period (2014-2019). I construct a novel data set, merging unique incidence-level crime logs from university police departments to provide the first causal estimates of the effect of moratoriums on campus-wide reports of alcohol and sexual assault offenses. In particular, I find strong and robust evidence that fraternity moratoriums decrease alcohol violations 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). Many have 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, 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 2019¹ 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. Since 2010, 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 for casual inference. Despite this lack of research, administrators continue to use moratoriums as a disciplinary action on their 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

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

Sweeney 2006). The inclusion of legal-age drinkers and large social events allows for easy 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. 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 to potentially riskier places off-campus where behavior is less regulated. As a result, the net effect of moratoriums is 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 and sexual assault offenses. 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 together two particularly unique data sources: university-specific Daily Crime Logs, which contain the universe of all reported incidences of crime 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 decreases are concentrated only when a moratorium is in place suggesting that there are no persistent effects once a moratorium is lifted. In particular, the immediate and subsequent weeks following a moratorium show no evidence of alcohol offenses or sexual assaults below their expected levels when compared to both a 14-day

period before a moratorium occurred and all days without a moratorium.

A key distinction of this work is that I am able to closely link changes in student behavior to a campus-wide policy that affects college partying. While other work such as [Lindo, Siminski, and Swensen \(2018\)](#) find that college partying increases daily reports of rape and alcohol offenses using college football game variation, moratoriums represent an understudied policy lever that university officials can use to reduce campus-wide partying. Therefore, this study provides further evidence that stronger sanctions on alcohol decrease alcohol-related incidents in college-aged individuals similar to [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.

More broadly, this paper adds to the literature in two areas. First, this paper contributes to a small 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 the effect of fraternity population on campus crime rather than fraternity policy. Second, this paper adds to the literature relating to the health effects of alcohol on college-aged individuals which include increases in mortality ([Carpenter and Dobkin 2009](#)), emergency room visits ([Francesconi and James 2019](#)), and crime ([Carpenter and Dobkin 2015](#)), in addition to 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 results. Section ?? explores the differences in effectiveness between different types of moratoriums. Section ??

analyzes possible mechanisms and implications. Section ?? 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, membership has been found to decrease GPA ([De Donato and Thomas 2017](#); [Even and Smith 2020](#)) and fraternity members spend approximately two more hours per-week partying than nonmembers ([Routon and Walker 2014](#)) and binge drink on approximately two additional days per-month ([DeSimone 2007](#)). Additionally, [Even and Smith \(2020\)](#) find that membership causes students to select into easier courses and complete less 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, whom frequently interact with fraternity men, are four times more likely to be a victim 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, it is the IFC fraternities that are restricted by a moratorium 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 differ 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 few as six calendar-days, or as long as 848 calendar-days.⁴ Additionally, moratoriums are the generally implemented because of a triggering event (see Figure ??). This event 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 by a fraternity-related death, and six are unspecified. As alluded to in the introduction,

²A chapter, or 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 is a catchall term for hazing, rule violations, offensive behavior, and other disorderly conduct that results in a moratorium.

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) in addition to their fraction of total enrollment belonging to an IFC fraternity. Most universities are located in the mid-west 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 had such 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 ?? examines the heterogeneous effects involving this difference in oversight.

3 Data

The main analysis uses data from a variety of sources. In particular, I construct a novel data set that links incidence-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

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.

Figure ?? shows a list of all 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 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 incidences are collected from Daily Crime Logs maintained by the 37 university's police departments resulting in approximately 500,000 distinct reports. The Daily Crime Log is an incidence-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 by (or to) the university-specific police department. Hence, each of the incidences listed in these

⁷Miami University is excluded due to being unable to verify the end-date of their moratorium. Pennsylvania State University is excluded because they would 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 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.

logs represent incidences 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 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 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

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 contain 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.

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.

are small changes in academic calendars year-to-year, a seven-day window is added to each start and end date of each 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 incidences, each university police department describes their incidences 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—incidences 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 and sexual assault offenses.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, 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

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

incident description to its corresponding offense. Importantly, each of these phrases are only portions 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 amount of offenses in each category since there are instances in incidence 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% 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-25000 enrolled students per-academic-calendar day. Therefore, the average amount of alcohol offenses per-25000 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 6 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

The goal of this paper is to identify the average causal effect of fraternity moratoriums on alcohol and sexual assault offenses across universities that experience moratoriums. In a naive analysis, this would amount to taking a difference of means for moratorium days and non-moratorium days across all universities for each offense. However, there are several issues with identifying such difference of means as a causal effect. First, university police departments each vary considerably in the frequency of reporting offenses. This is the result of differing policing tactics, the departments' available resources—such as number of officers per-student—and the overall composition of students at the university. For instance, a police department that oversees a university with a reputation for partying may police differently than a police department that rarely encounters college partying. Second, frequencies of offenses vary depending on the day of the week and the time of year. As an example, alcohol offenses most commonly occur on Fridays, Saturdays and Sundays, and fraternity recruitment is typically in the fall semester. A simple difference of means fails to account for each of these systematic differences between universities, days of the week, and semesters.

To circumvent these issues, I estimate the following baseline difference-in-differences specification using OLS:

$$Y_{u,t} = \beta Moratorium_{u,t} + \gamma_u + \lambda X_t + \epsilon_{u,t} \quad (1)$$

In Equation 1, $Y_{u,t}$ is an outcome of alcohol offenses or sexual assaults per-25000 enrolled students per academic-calendar day at university u in time t . $Moratorium_{u,t}$ 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 are policing, and overall fixed differences in incident prevalence. As stated above, a police department may have systematic differences in the frequency of reporting due to the corresponding demographic of the university or their own policing practices. For example, some police departments may enforce policies against underage drinking stricter than others. 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. Moreover, football game-day controls are included to account for the increases in both alcohol offenses and rapes that college football 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-

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.

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 allows 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 analysis in this paper utilizes the preferred specification which utilizes university-by-academic-year fixed effects.

4.2 Identification Assumptions

In order for β to be interpreted as a casual effect of fraternity moratoriums, there are three main assumptions that need to be satisfied: the timing of fraternity moratoriums are 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, 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 (e.g., 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

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.

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 of no changes in policing or reporting of offenses between moratorium and non-moratorium days, I conduct an indirect test that shows that there is no significant change between reporting behavior. In particular, I test whether the time of occurrence to the time of incident reported changes during moratorium days. This test is motivated by the notion that the amount of time from an occurrence to an official report 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 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 from zero. 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 from zero.

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

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.

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

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 (e.g., 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 problematic issues with the coefficient estimates when there are heterogeneous treatment effects between groups over time (Chaisemartin and D’Haultfœuille 2020; Sun and Abraham 2021; Athey and Imbens 2022). In particular, the parameter of interest (e.g., the coefficient on the treatment variable) is a weighted sum of average treatment effects where some of the weights may be negative. This negative weighting issue can potentially lead to coefficient estimates switching signs. For instance, a negative-signed treatment effect implicating reductions may actually be positively-signed once corrected for the problematic negative weights. Additionally, negative weights occur when the two-way-fixed-effects estimator uses treated observations as controls (Goodman-Bacon 2021; Chaisemartin and D’Haultfœuille 2020; Borusyak, Jaravel, and Spiess 2022). While this paper’s research design is not a typical TWFE design since treatment can occur multiple times and the preferred specification uses interacted fixed effects, there maintains 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.

In light of these potential issues, I conduct two series of analysis. First, Appendix ?? analyzes a typical TWFE design in this setting using university and day-by-month-by-year fixed effects. I show that this design does not contain negative weights and that the coefficient estimates are consistent with the main results. Although this procedure is imperfect,

the new estimators proposed by [Callaway and Sant’Anna \(2021\)](#) and [Chaisemartin and D’Haultfoeuille \(2020\)](#) are not suitable for this research design since the specification is not staggered, treatment units are treated multiple times, and fixed effects are interacted. Second, as discussed in Section 5, I include 14 never-treated universities to potentially reduce the occurrence of negative weights. The results in this analysis are consistent with the main findings.

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 by plotting 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 less 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 lower alcohol offenses across university campuses while showing 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 results from progressively adding more 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 less alcohol offenses in comparison to an average academic-calendar day as shown in the point estimates. These estimates are statistically significant across

each specification, maintaining that moratoriums decrease campus-wide alcohol offenses. Although alcohol offenses are robust, sexual assaults fail to achieve statistical significance across each specification and the magnitude of each effect varies considerably; sexual assaults show a 14-20 percent reduction from the mean across the three specifications.

The effect of moratoriums are most evident during 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. On the other hand, weekdays show no statistically significant decreases. Likewise to alcohol, sexual assaults show larger decreases on the weekend in comparison to 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 are robust to 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 ?? show consistent results to Tables ?? and ?? using poisson estimation in lieu of OLS. Specifically, poisson estimation shows a statistically significant 27 and 32 percent average reduction for 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 total 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 However, 17 of these universities are already included in the sample due to experiencing a fraternity moratorium. As such, 14 of remaining 33 Top 50 Greek Life universities are able to provide Daily Crime Logs for the 2014-2019 period. Appendix Table ?? shows the effects of moratoriums when including these never-treated universities. Overall, the results remain similar, with weekend decreases of alcohol of approximately 18 percent reduction from the mean and sexual assault decreases at approximately 26 percent.

5.2 Potential Spillovers

5.3 Long-Run Effects

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 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.

6 References

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