

The Effect of Fraternity Moratoriums on Alcohol Offenses and Sexual Assaults

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Abstract

I exploit variation in timing from 44 temporary university-wide halts on all fraternity activity with alcohol (*moratoriums*) across 37 universities over 2014-2019. I construct a novel data set, merging incident-level crime logs from university police departments to provide the first causal estimates of the effect of moratoriums on reports of alcohol offenses and sexual assaults. In particular, I find robust evidence that moratoriums decrease alcohol offenses by 26%. Additionally, I find suggestive evidence that moratoriums decrease reports of sexual assault on the weekends by 29%. However, I do not find evidence of long-term changes once the moratorium is lifted.

JEL Codes: I12, I28, K42

Please note that an Online Appendix is included.

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Data Replication Statement: This paper uses a combination of publicly available data from multiple sources. The raw data can be obtained through Clery Act/Freedom of Information requests at each unique university. Additionally, I am willing to assist in data requests for replication (michaeltopper@ucsb.edu). All replication files, including the master data, are available at github.com/michaeltopper1/Fraternities.

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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% of fraternity alumni agreed that they would join their fraternity again 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 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, becoming a common policy used by school administrators. However, studying this topic is challenging for several reasons; moratorium dates are difficult to find/confirm and there does not exist a centralized data source for university-specific crime with enough detail to enable causal 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 the incidence 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). 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 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 (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 incidents 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 of 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 the literature shows that college partying increases daily reports of rape and alcohol offenses when using football game variation (Lindo, Siminski, and Swensen 2018), this study more directly focuses on a policy tool that reduces college partying. I later analyze in Section 7.A whether moratoriums have mitigating effects on college partying behavior when coinciding with college football game-days, although I find no clear evidence in support of this. Second, this paper contributes to an emerging body of economic work relating to the effectiveness of university policy, and more specifically, fraternity policy. Although university policies such as academic probation (Lindo, Swensen, and Waddell 2013) and financial aid (Dynarski 2003) have been found to be effective in improving GPA and recruiting students respectively, there are only two studies as of this writing that analyze fraternity-targeted policies—both of which study the effects of deferring fraternity recruitment from freshman to sophomore year (De Donato and Thomas 2017; Even and Smith 2020). Moratoriums, in contrast, alter a university’s party culture instantly, since unaffiliated undergraduates also attend fraternity parties (Harford, Wechsler, and Seibring 2002). However, as discussed in Section 5.D, the moratorium effects diminish following the first month of implementation, making them ill-suited as a long-term solution for mitigating excessive partying. Currently, only one related study has examined the relationship between fraternities and university crime (Raghav and Diette 2022), although this study focuses on how the size of a fraternity population affects campus crime rather than the effect of a typical fraternity policy. I explore a similar idea in Online Appendix D which shows suggestive evidence that uni-

versities with higher shares of fraternity members exhibit larger moratorium effects. 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](#)), emergency room visits ([Francesconi and James 2019](#)), and adolescent brain development ([Silveri 2012](#)), 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

A 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 non-members ([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, other research finds that membership causes students to select into easier courses and complete fewer course credits ([Even and Smith 2020](#)). 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 engage 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.

B 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 scope 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 1 shows the start and end dates of each moratorium over time. Moratoriums in the sample can range from as short as six calendar-days to as long as 848 calendar-days.⁴ Additionally, moratoriums are generally implemented due to triggering events (see Online Appendix Table E1). 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 2 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, moratoriums are enacted across the US. Figure 3 shows the locations of the 37 universities in the sample (see Section 3.A for further details on sample construction). Most universities are located in the Midwest and South, although there are several universities from both the West and East Coast.

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 2 shows that IFC-implemented moratoriums are less frequent (17) than university-implemented moratoriums (27), and Section 7.B explores 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).

A 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. Online Appendix Table E2 lists all the universities included with their corresponding moratorium dates. 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.⁷ 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 dates 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 Logs are 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 Online Appendix Figure E1 for an example). Moreover, the Daily Crime Logs contain the universe of incidents that are reported to (or by) the university-specific police department. Hence, each of the incidents listed in these documents represents incidents that occurred on or near university property.⁹

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, and therefore, only one university is missing data from a complete calendar-year.¹⁰ 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 (Bernat et al. 2014). While the CSS data includes similar information as the Daily Crime Logs, the CSS data is aggregated to the calendar-year which makes the effect of moratoriums difficult to study given their short-lived nature. See Online Appendix Table E3 for more details on the advantages of the Daily Crime Logs.

University characteristics such as total enrollment, student demographics, and academic calen-

dars 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.

B 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. 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. 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 1 shows the corresponding words, phrases, and abbreviations used to match each incident description to its corresponding offense. To demonstrate the accuracy of this process, Online Appendix Table E2 shows the 15 most frequent descriptions matched to each offense.

C Descriptive Statistics

Table 2 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. The majority of the student body are undergraduates and 61% of them are white. Graduation rates vary widely among schools and there is substantial variation in the selectivity of each university. For instance, graduation rates and the fraction of students admitted range between 39% to 95% and 14% to 94% respectively. Moreover, IFC fraternities, on average, represent a small fraction of the undergraduate enrollment with approximately 5% of students possessing membership to an IFC fraternity. Although IFC members represent a small number of students, the universities in the sample are representative of schools with an active Fraternity and Sorority Life as shown in Online Appendix Figure E3.

Panel B displays 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.

Last, Panel C describes characteristics of the 44 moratoriums in the sample. On average, each university experiences approximately one moratorium, although some universities experience up to three. Furthermore, the moratoriums persist for an average of 64 academic-calendar days, with significant variation in the length of the moratoriums. Specifically, 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, or approximately 1.5 months.

4 Empirical Strategy

A 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:

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

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 . $InMoratorium_{u,t}$ is an indicator variable equal to one when university u is experiencing 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 to account for serial correlation within each 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, 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. Furthermore, holiday controls¹¹ are included

since there may be less student activity on holidays and academic year controls are included due to differences in fraternity rules and guidelines between academic-years. Lastly, while not shown in Equation 1, I also control for football game-days 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).¹² 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, academic-years, and football game-days.

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 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.A, including these fixed effects produce 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.

B Identification Assumptions

To estimate Equation 1 and interpret β as a casual effect of fraternity moratoriums, there are four 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, the triggering event is not changing student behavior, 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 can have 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 Sieglöcher (n.d.); I generalize a classic dummy variable event study to accommodate multiple moratoriums within a university and classify the event-time (period 0) as the entire moratorium period. Therefore, period 0 represents all moratorium days within a university.

Figures 4 and 5 show the results of the ‘multiple event’ event study which demonstrate that there is little suggestive evidence that crime is declining 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. Fourteen-day periods are chosen instead of seven-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 Sieglöcher (n.d.). The errorbars indicate 95% confidence intervals and the number of periods before and after the moratorium are chosen to give approximately a median moratorium length of days (46). 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 10% level.¹³ 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,¹⁴ thereby 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 evidence of a decreasing crime trend prior to a moratorium.

To test the second assumption that moratoriums do not change policing or incident 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 (*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.¹⁵ 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 3 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.¹⁶ 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 assaults. 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.

To evaluate the third assumption that the triggering event is not changing student behavior, I perform heterogeneity analysis in Section 6.B and analyze the effect of a moratorium by each triggering event. As discussed further in Section 6.B, I find that the main results are driven by moratoriums triggered by fraternity deaths. While it is plausible that the shock of a death, rather than a moratorium, contributes to behavior changes in students, I construct a sample of 15 universities that experience a fraternity-related death but no moratorium, and apply a 64-day treatment period (the average length of a moratorium) starting with the day of the death to test whether the shock of death alone affects student behavior. In doing so, I find little evidence that alcohol offenses or sexual assaults decrease due to the shock of a death—neither outcome shows a statistically significant decrease during the 64-day period following a death.

For the final assumption that moratoriums have no lasting effects, I conduct two series of analyses which are further discussed in Section 5.C. 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 one-week lead and one-week lag to identify whether the effects of moratoriums disappear instantly once a moratorium is lifted. Additionally, I conduct an F-test on the four 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.

C 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’Haultfœuille 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’Haultfœuille 2020; Borusyak, Jaravel, and Spiess 2022). While this paper’s research design is not a typical TWFE design since the treatment can occur multiple times, each treatment has a different length, and the preferred specification uses interacted 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 experimen-

tal design, I conduct two different series of analyses which yield consistent results with the main findings: one that analyzes a typical TWFE design in this setting using university and day-by-month-by-year fixed effects which has no negative weights (see Online Appendix B), and another that includes 14 never-treated universities to potentially reduce the occurrence of negative weights while maintaining the preferred specification (see Section 5.A).

5 Results

In this section, the estimated causal effects of a fraternity moratorium on alcohol offenses and sexual assaults are reported using OLS. Figure 6 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.

A Main Results

Table 4 reports that fraternity moratoriums result in significantly fewer alcohol offenses on university campuses and provides suggestive evidence of decreases in sexual assaults. In Column 1, the baseline specification from Equation 1 is shown. This specification includes day of the week, holiday, semester, football game-day, and academic-year fixed effects. In Columns 2 and 3, increasingly flexible fixed effects are added, with Column 2 being the preferred specification as noted in Section 4.A. Panel A shows that alcohol offenses decrease during moratorium days compared to non-moratorium days. On average, moratorium days experience between 26% and 28% fewer alcohol offenses compared to an average academic-calendar day as indicated by the point estimates in the first three columns. These estimates are statistically significant in all three specifications, emphasizing the impact of moratoriums on reducing alcohol offenses on campus. Although the point estimates on alcohol offenses are robust, the estimates on sexual assaults do not reach statistical significance in each specification. Additionally, the magnitude varies consid-

erably, with sexual assaults showing a 14% to 20% reduction from the mean across the different estimations.

The effects of moratoriums, as shown in Table 4, Columns 4 and 5, are driven by weekends (Friday-Sunday), which aligns with the literature that most college partying occurs on weekends (Lindo, Siminski, and Swensen 2018). Columns 4 and 5 of Table 4 show the preferred specification (Column 2) separated by weekends and weekdays. During the weekends, alcohol offenses decrease by 28% relative to an average academic-calendar weekend as shown in Panel A. However, weekdays show no statistically significant decreases. Similarly, Panel B shows that sexual assaults also decrease more on the weekends than weekdays with a 29% decrease in sexual assaults relative to an average academic-calendar weekend. This decrease in sexual assaults is significant at the 10% level.

Importantly, these findings persist across various robustness and sensitivity tests. First, given the non-negative count nature of the incident data and the sensitivity of OLS estimation to outliers, Online Appendix Table E4 reaffirms the results reported in Table 4 using Poisson estimation instead of OLS. Specifically, Poisson estimation shows a statistically significant 27% and 32% average reduction in alcohol offenses and sexual assaults on the weekends, respectively. Second, to ensure that the results are not driven by a single university, Online Appendix Figures E6 and E7 show the leave-one-out coefficient estimates for each offense. In particular, 37 unique regressions are estimated for each offense, omitting one university in each iteration—all which demonstrate similar findings to the main results. Third, due to the large variation in university size, the models in Table 4 are weighted by total enrollment in Online Appendix Table E5. The weighted estimations exhibit similar results to the unweighted models with alcohol offenses and sexual assaults decreasing by 29% and 32% on the weekends respectively, while the standard errors remain similar in magnitude. Finally, recall from Section 4.C 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, 14 additional universities that never underwent a moratorium in the period of analysis are included to potentially mitigate the negative weighting issue. This results in 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.¹⁷ The additional universities are selected if they are regarded as a Top 50 Greek Life school.¹⁸ Fourteen of these universities are already included in the sample due to experiencing a fraternity moratorium, further justifying the remaining 36 Top 50 Greek Life universities as a good counterfactual. However, only 14 of these universities are included in the sample while the remaining 22 are excluded since they are unable to provide Daily Crime Logs. Online Appendix Figures E8 and E9 show the effect of moratoriums when including these never-treated universities (see *Main Sample + Never Treated* rows). Overall, the results remain similar, with weekend decreases in alcohol offenses and sexual assaults of approximately 18% and 26% respectively.

B Are There Spillovers to Nearby Areas?

One potential caveat to the main results in Table 4 is that the reported decreases in alcohol offenses and sexual assaults may be 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 reliably covers 24 percent of the sample universities' neighboring police departments and includes only alcohol arrests rather than all incidents.¹⁹ 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, two sets of analyses are performed 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 to 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.²⁰

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 (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 5, alcohol offenses and sexual assaults have an insignificant and negative point estimate at nearby police departments, thereby showing little evidence of substantial spillovers. Reassuringly, the university-specific police departments continue to 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 disciplinary actions and arrests corresponding to liquor law violations in addition to reports of sexual assaults that occur in a calendar-year. The main advantage of 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.²¹ Therefore, on average, the Daily Crime Logs contain approximately 30% and 50% of the yearly alcohol offenses and sexual assaults reported in the CSS data respectively. 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 Online Appendix C for a more detailed discussion of the CSS data and the corresponding model.

Using the CSS data, there is evidence that moratoriums move drinking from fraternity houses to residence halls. Residence halls show a 0.270 *increase* in yearly alcohol offenses for each additional moratorium day in a calendar-year. Interestingly, this is accompanied by a 0.033 *decrease* in yearly 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.

C Do Moratoriums Have Long-run Effects?

Although moratoriums clearly impact student behavior when implemented, I find no evidence showing that moratoriums provide long-run impacts. In this subsection, I perform two analyses to demonstrate this: first, I conduct an F-test on the lagged coefficients in the event study specification shown in Section A, and second, I extend the preferred specification from Table 4 with an indicator for the one-week before and one-week after a moratorium.

Table 6 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 4 and 5. In addition, p-values from joint F-tests on the four lagged coefficients are reported. The p-values for both alcohol offenses and sexual assaults are above the 10% level of significance, 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, there is potential that longer moratoriums may induce more behavior change than relatively shorter ones. To study this possible heterogeneity, I supplement the analysis above by splitting the sample into 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).²² Panel B of Table 6 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 effect, therefore showing that the length of a moratorium may be crucial to the overall effectiveness (see Section 5.D).

Last, Figure 7 reports the estimates from the second analysis which extends the specification in Column 2 of Table 4 with an indicator variable for the one-week after and one-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 following a moratorium. This pattern persists when restricting the sample to weekends where the effects of the moratorium are most prominent.

D Are Moratoriums Effective Across the Entire Duration?

Although moratoriums can reduce alcohol offenses, it is likely that the reductions are not consistent throughout the enforcement period. For instance, students may find alternative ways to party or enforcement may become less strict as the moratoriums continues. Therefore, it is crucial to understand both when and how long a moratorium is most effective, as this can aid school administrators in making informed decisions about future moratorium lengths.

To understand the progression of a moratorium's effectiveness, I split the $InMoratorium_{u,t}$ treatment variable into weekly bins for the first nine weeks of a moratorium and pool the remaining weeks into one bin (Moratorium Weeks 10+) as shown in Panel A of Figures 8 and 9.²³ This

amounts to 10 unique coefficients, each identifying the effect of the moratorium in the corresponding week. However, since moratorium lengths differ by university, each point estimate is identified by a different number of schools as shown in parenthesis on the x-axis. For example, the coefficient identifying the effect of a moratorium in Week 3 is identified by 33 universities that have a moratorium that reach the three-week length. Note that if a university has, for instance, a 22-day moratorium, this moratorium will contribute only one day to the identification of the Moratorium Week 4 coefficient.

Panel A of Figures 8 and 9, exhibit evidence that moratoriums are most effective in the first five weeks. In Panel A of Figure 8, alcohol offenses show statistically significant declines at the 5% level in weeks one, two, and five of a moratorium. The effectiveness appears to trend upward after the fifth week, thereby suggesting that moratorium effectiveness may diminish over time. Similarly, sexual assaults show statistically significant declines in weeks one and three in Panel A of Figure 9, while the effects appear to fade in later weeks.

Although Panel A illustrates the by-week effect, it is possible that the significant declines in the first five weeks are driven by universities that have short moratoriums. To ensure that the trends are consistent across universities, I re-estimate the coefficients using only universities that have moratoriums longer than nine weeks in Panel B of Figures 8 and 9. In each figure, Panel B shows similar trends to Panel A, although less precise due to the loss of power. The results suggest that long moratoriums exhibit the strongest effects during the initial weeks of implementation, and similarly, the effects diminish after approximately five weeks.

6 Heterogeneity

A 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, past literature finds that party schools exhibit two times the increase in reports of rape on football game days than non-party schools

(Lindo, Siminski, and Swensen 2018). To examine this possibility, I use Niche.com’s Top Party Schools in America list.²⁴ 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. 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.

As shown in Table 7, universities defined as party schools have higher averages of alcohol offenses assaults relative to non-party schools. In particular, non-party schools experience approximately 52% less alcohol offenses on average. These differences are similar when excluding moratorium days (53%), although both party schools and non-party schools have relatively similar levels of sexual assault.

Table 7 also shows that party schools exhibit larger decreases in alcohol offenses than non-party schools during moratoriums. 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, thereby suggesting that the effects of the moratorium are driven by schools that have a stronger party culture.

Similarly, Online Appendix D explores whether universities that have relatively more *fraternity* life—defined by the fraction of undergraduate students enrolled in an IFC—display larger effects. By supplementing the preferred specification with an interaction between the $InMoratorium_{u,t}$ treatment variable and the fraction of undergraduates in an IFC fraternity, there is evidence that universities with more fraternity life show larger declines in alcohol offenses during moratoriums. Although statistically insignificant, the point estimates are negative, thereby hinting at the possibility that moratoriums are more effective when a higher fraction of students are directly affected by its guidelines.

B Does the Triggering Event for a Moratorium Matter?

As described in Section 2.B, moratoriums can be the result of a fraternity-related death, a prominent sexual assault, or a behavior violation. Given the differing salience of these events, it is possible that a triggering event affects a moratorium's effectiveness. As an illustration, a death may be more salient than a behavior violation, resulting in a behavior shock to the college campus. Moreover, both deaths and sexual assaults are exceptionally undesirable results of risky behavior—a moratorium may seem more justified under these outcomes rather than following an instance of hazing.

Figure 10 reports that moratoriums have a stronger impact when triggered by a death or sexual assault, rather than a behavior violation. Specifically, alcohol offenses decrease notably when a fraternity-related death is the trigger. To confirm that this effect is caused by the moratorium rather than the triggering death, I analyze data from 15 additional universities that had a fraternity-related death in the sample period, but did not have a moratorium.²⁵ Hence, these supplemental universities serve as a control group to observe the effect of a fraternity-related death without the influence of a moratorium. I exclusively analyze data from these 15 universities that did not have a moratorium by creating a 64-day binary treatment variable (i.e., the average length of a moratorium) beginning with the date of the death. Next, I estimate the preferred specification using the 64-day period after the death instead of the $InMoratorium_{u,t}$ treatment variable. Panel C of Online Appendix Figures E8 and E9 show that there is little evidence of declines in alcohol offenses or sexual assaults following a fraternity-related death without a moratorium. The point estimates for alcohol offenses are consistently positive, while both offenses exhibit statistically insignificant estimates at the 10% level. To increase precision, I supplement this analysis in Panel D of Online Appendix Figures E8 and E9 by including the 14 never-treated schools that are used in Section 5.3 as never-treated controls. This amounts to 29 universities, 15 of which undergo the effect of a death, and 14 of which receive no such treatment. As shown, the point estimates remain consistent across both of these analyses and the statistical significance does not change. Taken together, there is little evidence suggesting that a fraternity-related death contributes to the decreases shown in alcohol

and sexual assault offenses during a moratorium. Instead, this points to the possibility that students may more seriously abide by the moratorium guidelines when the triggering event is a death.

Additionally, Figure 10 shows significant decreases in sexual assaults when a triggering event involves either a sexual assault or behavior violation. However, the persistent shortcomings of estimating effects on sexual assaults, such as the under-reporting issue, may be exacerbated in this analysis since these estimates are based on a small subset of universities (19 universities for behavior violations and 10 for sexual assaults).²⁶ Consequently, although the results indicate evidence of decreases in sexual assaults, this evidence is mostly speculative under the data limitations.

7 Discussion

A Do Moratoriums Mitigate the Effects of Football Games?

It is well-documented in the literature that college football games cause higher rates of alcohol offenses and rape (Rees and Schnepel 2009; Lindo, Siminski, and Swensen 2018). 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 11 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 is larger on home games rather than away games which is consistent with Lindo, Siminski, and Swensen (2018) and Rees and Schnepel (2009). Furthermore, Figure 11 also shows the combined effect of a game day and a moratorium. In each of these estimations, the point estimates are similar to 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 morato-

riums. As a robustness check, I broaden the definition of game-days to game-weekends in Online Appendix Figure E10. Although this nearly triples the amount of identifying variation, the results are consistent.²⁷ 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 the undesirable effects of football game-days.

B Who Should Enforce Moratoriums?

Recall from Section 2.B 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 maintain their community life. As such, further disciplinary measures by the IFC-council directly affect the council members themselves, thus creating a system that may incentivize IFC council members to look away from the moratorium guidelines.

In Table 8, the coefficient estimates on alcohol offenses show 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, in Panel B, 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 the number of alcohol offenses point to the university administration as the more effective enforcement body rather than the fraternity members themselves.

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. Importantly, there is not substantial evidence that moratoriums displace 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 are a welfare-improving policy. On one hand, moratoriums cause decreases in alcohol offenses. If these decreases are the result of a displacement effect, these offenses may be occurring in relatively safer areas (residence halls) as speculated in Section 5.B. Furthermore, moratoriums may help 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 are effective

during the first month of 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 seek 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, more research is needed to understand the benefits and downfalls of this practice. Specifically, 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.

9 References

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Notes

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

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.

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.

⁷See Online Appendix A for a discussion of known universities that experienced moratoriums but are excluded.

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 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.

⁹Sometimes, 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.

¹⁰See Online Appendix Section B for more details.

¹¹Holiday 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.

¹²Information 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.

¹³As a measure of robustness, an alternative event-study is estimated using 46-day periods before and after a moratorium in Figures E4 and E5. Each of these figures fails to show evidence of a decreasing or increasing pre-period trend.

¹⁴This statistic is based on 13 of the 15 universities in which I have data on date of the triggering event.

¹⁵Only 32 of the 37 universities had data for the date occurred of their incidents. Hence, this test only reflects a

subset of the sample.

¹⁶Literature such as Sahay (2021) use a 3-day lag when applying this test.

¹⁷I 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.

¹⁸Notably, 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.

¹⁹In 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.

²⁰The 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

²¹Similarly, if a student tells a school counselor of a sexual assault, that sexual assault may not necessarily be reported to the university police and thus not appear in the Daily Crime Logs. However, this is mandated to be included in the CSS data.

²²Note 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%)

²³Note that nine weeks is approximately the average length of a moratorium.

²⁴I use Niche.com over the Princeton Review since the Princeton Review no longer posts their party school rankings. For more details on the methodology see: <https://www.niche.com/about/methodology/top-party-schools/>.

²⁵These universities were found using Hank Nuwer’s repository of hazing-related deaths in the US: <https://www.hanknuwer.com/hazing-deaths/>.

²⁶Survey evidence shows that nearly 80% of sexual assaults go unreported. This 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

²⁷Not all game-days occur on a weekend, so the expanding the definition to a game-day weekend does not quite triple the number.

10 Tables

Table 1: Words and Phrases used to Pattern Match on Offenses of Interest

Outcome	Words to Match
Alcohol Offense	alcohol, dwi, intox, drink, dui, drunk, liquor, driving under the influence, dip, abcc, underage, dwi, underage, pula, owi, mip, under age, beer, wine, booze, minor in possession, ovi
Sexual Assault	sex, rape, fondling, fondle

Note:

The second column represents a portion of an incident's description to pattern match on. Words for alcohol violations and sexual assaults are found by reading each university's dataset for common words within incident descriptions. For example, the word 'sex' will match on 'sexual assault' and 'sex offense' since 'sex' appears in each of these descriptions. Notably, this method likely undercounts the true number of violations in each police department's Daily Crime Log due to spelling errors. As a demonstration, the word 'alcohol' may be written as 'aclohol' which this matching process will not include. Some notable abbreviations include the following:

- 'dwi' is an abbreviation for 'driving while intoxicated'.
- 'dip' is an abbreviation for 'drunk in public'.
- 'abcc' is an abbreviation for 'alcohol beverage control comission'.
- 'pula' is an abbreviation for 'possession under legal age'.
- 'owi' is an abbreviation for 'operating while intoxicated'.
- 'mip' is an abbreviation for 'minor in possesion'.
- 'ovi' is an abbreviation for 'operating vehicle intoxicated'.

Table 2: Summary Statistics of the Universities in the Sample

	Mean	SD	Median	Min	Max
Panel A: University Characteristics					
Total Enrollment	29,074	14,423	28,718	3,127	69,402
Total Undergraduate Enrollment	22,417	11,878	22,309	2,571	59,371
Fraction Asian	0.07	0.08	0.04	0.01	0.36
Fraction Black	0.07	0.04	0.06	0.01	0.20
Fraction Hispanic	0.13	0.14	0.07	0.02	0.68
Fraction White	0.61	0.18	0.67	0.08	0.83
Graduation Rate	70.33	13.78	70.00	39.00	95.00
SAT Math 75th Percentile	655.79	69.11	650.00	480.00	790.00
SAT Reading 75th Percentile	641.26	54.25	640.00	490.00	760.00
Fraction Admitted	0.60	0.21	0.61	0.14	0.94
Fraction Private	0.13	0.34	0.00	0.00	1.00
Fraction IFC Fraternity ^a	0.052	0.025	0.049	0.011	0.113
Panel B: Daily Crime Log Offenses					
Alcohol Offense	0.46	1.23	0.00	0.00	31.68
Sexual Assault	0.05	0.30	0.00	0.00	15.99
Panel C: Moratorium Characteristics					
Number of Moratoriums per-University	1.36	0.61	1.00	1.00	3.00
Length of Moratoriums	64.07	80.90	45.50	6.00	541.00
<i>Total Number of Universities</i>	<i>37</i>				

Note:

Offenses are per-25000 students enrolled per-academic calendar day. Length of moratorium statistics are in academic-calendar days. Number of moratoriums refers to number of moratoriums only within the 2014-2019 time period. Some schools may or may not have had moratoriums in periods before or after the time period of analysis. Only a subset of races were chosen, and hence, the fractions do not sum to 1 in the table. SAT Math 75th Percentile and SAT Reading 75th Percentile correspond to the 75th percentile SAT score for an admitted student. A perfect score is 800, while an average score is approximately 500. Fraction Private refers to the fraction of universities that are private universities.

^a This is defined as the number of IFC members divided by the total undergraduate enrollment. However, in the case of four universities, counts had to be obtained from year 2022 due to lack of data availability within departments. Note that IFC fraternity populations do not change substantially year-to-year.

Table 3: Effect of Moratoriums on Changes in Reporting (OLS)

	Reporting Lag			
	More than 1-Day Lag (1)	More than 3-Day Lag (2)	More than 7-Day Lag (3)	More than 14-day Lag (4)
<i>Panel A: Proportion of Alcohol Offenses Reported with Lag</i>				
In Moratorium	0.002 (0.002)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
Observations	48026	48026	48026	48026
Mean of Dependent Variable	0.003	0.002	0.001	0.001
<i>Panel B: Proportion of Sexual Assaults Reported with Lag</i>				
In Moratorium	-0.001 (0.004)	-0.003 (0.004)	-0.001 (0.004)	0.000 (0.003)
Observations	48026	48026	48026	48026
Mean of Dependent Variable	0.017	0.014	0.011	0.001

Note:

Standard errors are clustered by university. Panels A and B are OLS regressions of proportions of alcohol offenses and sexual assaults reported with a reporting lag. A reporting lag is defined as an offense that was reported more than one (Column 1), three (Column 2), seven (Column 3), or 14 (Column 4) days after it occurred. 32 of the 37 universities have information on date occurred. Specification is the preferred specification which includes day of week, holiday, football game-day, semester, and university-by-academic-year fixed effects. See Table 4 Column 2 for more details on the preferred specification.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effect of Moratoriums on Alcohol Offenses and Sexual Assaults (OLS)

	Specification (2)			
	(1)	(2)	(3)	Weekends (4) Weekdays (5)
<i>Panel A: Alcohol Offenses</i>				
In Moratorium	-0.125** (0.047)	-0.123** (0.051)	-0.131*** (0.046)	-0.238** (0.106)
Observations	55115	55115	55115	31472
Mean of Dependent Variable	0.464	0.464	0.464	0.190
Wild Bootstrap P-Value	0.004	0.010	0.006	0.179
<i>Panel B: Sexual Assaults</i>				
In Moratorium	-0.009** (0.004)	-0.010 (0.006)	-0.007 (0.006)	-0.017* (0.010)
Observations	55115	55115	55115	31472
Mean of Dependent Variable	0.049	0.049	0.049	0.042
Wild Bootstrap P-Value	0.014	0.149	0.246	0.518
FE: Holiday	X	X	X	X
FE: Game Day	X	X	X	X
FE: Semester (Spring/Fall)	X	X		X
FE: University	X			
FE: Academic Year	X			
FE: University by Academic Year		X		X
FE: University by Academic Year by Semester			X	

Note:

Estimates are obtained using OLS. Standard errors shown in parenthesis are clustered by university (37 clusters) and each offense is defined as per-25000 enrolled students. P-values from 1000 wild cluster bootstrap iterations are shown for the In Moratorium coefficient as suggested by Cameron, Gelbach, and Miller (2008) in cases with a small number of clusters (typically lower than 30). This analysis is near, but not below this threshold. Game Day controls consist of university football games within each university. Weekends include Friday-Sunday while Weekdays include Monday-Thursday. Column 2 is the preferred specification due to the flexibility of the fixed effects and the conservativeness of the estimates. Significance stars correspond to clustered standard errors.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effect of Moratoriums in Local Police Departments Compared to University Police Departments (OLS)

	Nearby Police Departments			University Police Departments		
	All Days (1)	Weekends (2)	Weekdays (3)	All Days (4)	Weekends (5)	Weekdays (6)
<i>Panel A: Alcohol Offenses</i>						
In Moratorium	-0.156 (0.130)	-0.201 (0.206)	-0.126 (0.114)	-0.320* (0.141)	-0.714** (0.290)	-0.029 (0.040)
Observations	13764	5898	7866	13743	5889	7854
Mean of Dependent Variable	1.225	1.930	0.696	0.754	1.403	0.267
<i>Panel B: Sexual Assaults</i>						
In Moratorium	-0.025 (0.016)	-0.011 (0.017)	-0.035 (0.021)	-0.003 (0.017)	-0.013 (0.029)	0.004 (0.013)
Observations	13764	5898	7866	13743	5889	7854
Mean of Dependent Variable	0.478	0.522	0.446	0.055	0.071	0.043
FE: Day of Week	X	X	X	X	X	X
FE: Holiday	X	X	X	X	X	X
FE: Game Day	X	X	X	X	X	X
FE: Semester (Spring/Fall)	X	X	X	X	X	X
FE: Agency by Academic Year	X	X	X			
FE: University by Academic Year				X	X	X

Note:

The columns under Nearby Police Departments use the NIBRS data which pertains to police departments that are closest to the university. University Police Departments uses the Daily Crime Log data set which contains only university-specific police departments. Only 9 local police departments in the NIBRS data consistently report in the sample period. This table represents the comparison of alcohol offenses and sexual assaults per-25000 enrolled students at the nine local police departments and the corresponding nine universities. Standard errors are clustered by agency for NIBRS data and by university for Daily Crime Log data.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Absence of Long-Run Effects of Moratoriums Split by Moratorium Length (OLS)

	Dependent Variable	
	Alcohol Offenses (1)	Sexual Assaults (2)
<i>Panel A: Full Sample</i>		
<i>Estimates from Figures 4 and 5</i>		
In Moratorium	-0.137** (0.059)	-0.015 (0.010)
Observations	55115	55115
F-test P-value of Lags	0.158	0.102
<i>Panel B: Quantiles by Moratorium Length</i>		
<i>Moratorium Length: 1st Quantile</i>		
In Moratorium	0.062 (0.036)	-0.015 (0.021)
Observations	22503	22503
F-test P-value of Lags	0.459	0.070
<i>Moratorium Length: 2nd Quantile</i>		
In Moratorium	-0.238** (0.097)	-0.021 (0.012)
Observations	19241	19241
F-test P-value of Lags	0.552	0.408
<i>Moratorium Length: 3rd Quantile</i>		
In Moratorium	-0.128 (0.087)	-0.007 (0.015)
Observations	22653	22653
F-test P-value of Lags	0.203	0.128

Note:

Point estimates of In Moratorium reflect the time 0 for the ‘multiple event’ event studies similar to Figures 4 and 5 with four leads and four lags of 14-day bins. Each offense is defined as per-25,000 enrolled students. Standard errors are clustered at the university level. All periods are normalized by the 14-day period before the moratorium. Panel A represents the same coefficient estimates as Figures 4 and 5, while Panels B, C, and D represent subsets of the sample split by three quantiles. The three quantiles represent the 33rd, 66th, and 100th percentile of a moratorium length which correspond to [0-32], [33-59], and [60-541] academic calendar days of a moratorium respectively. Hence, if a university has a moratorium that lasts 30 academic calendar days, then it is included in Panel A. P-values are reported from joint F-test of the four lags. Fixed effects include day of the week, holiday, semester number, football game-day, and university-by-academic-year.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Effect of Moratoriums on Alcohol Offenses and Sexual Assault by Party School (OLS)

	School Type		
	All Schools (1)	Party Schools (2)	Non-Party Schools (3)
<i>Panel A: Alcohol Offenses</i>			
In Moratorium	-0.123** (0.051)	-0.223** (0.101)	-0.053 (0.034)
Observations	55115	23980	31135
Mean of Dependent Variable	0.464	0.658	0.314
Non-Moratorium Mean	0.461	0.661	0.312
<i>Panel B: Sexual Assaults</i>			
In Moratorium	-0.010 (0.006)	-0.008 (0.007)	-0.011 (0.010)
Observations	55115	23980	31135
Mean of Dependent Variable	0.049	0.045	0.052
Non-Moratorium Mean	0.049	0.045	0.052

Note:

Standard errors are clustered by university and each offense is defined as per-25000 enrolled students. The column All Schools represents the preferred specification (i.e., Column 2) from the main results table which includes day of the week, football game-day, semester number, and university-by-academic-year fixed effects. A party school classification is determined from Niche.com's list of top partying schools. A university in the top 50 is considered a party school which amounts to 16 of the 37 universities.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Effect of Moratoriums Imposed by the University vs. the IFC (OLS)

	Days of the Week		
	All Days (1)	Weekends (2)	Weekdays (3)
<i>Panel A: University-Enacted Moratoriums</i>			
<i>Alcohol Offense</i>			
In Moratorium	-0.132* (0.065)	-0.252* (0.136)	-0.041 (0.035)
Observations	55115	23643	31472
<i>Sexual Assault</i>			
In Moratorium	-0.010 (0.008)	-0.019 (0.013)	-0.003 (0.007)
Observations	55115	23643	31472
<i>Panel B: IFC-Enacted Moratoriums</i>			
<i>Alcohol Offense</i>			
In Moratorium	-0.101 (0.082)	-0.197 (0.166)	-0.030 (0.026)
Observations	55115	23643	31472
<i>Sexual Assault</i>			
In Moratorium	-0.010 (0.010)	-0.014 (0.010)	-0.007 (0.012)
Observations	55115	23643	31472

Note:

Standard errors clustered by university. In Panel A, the In Moratorium is interacted with an indicator variable equal to one if the moratorium was enacted by a university. In Panel B, In Moratorium is interacted with an indicator variable equal to one if the moratorium was enacted by the IFC. Controls follow the preferred specification from Column 2 in the main results table with day of week, holiday, semester, football game-day, and university by academic year fixed effects. Panel A shows the effects of a moratorium when a moratorium is imposed by the university. University-imposed moratoriums represent 27/44 (61%) of the moratoriums. Panel B shows the effects of a moratorium when the IFC council imposes the moratorium. This is a student-lead initiative. IFC-imposed moratoriums represent 17/44 (39%) of the moratoriums in the sample. Weekends represent Fridays through Sundays while Weekdays represent Mondays through Thursdays.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

11 Figures

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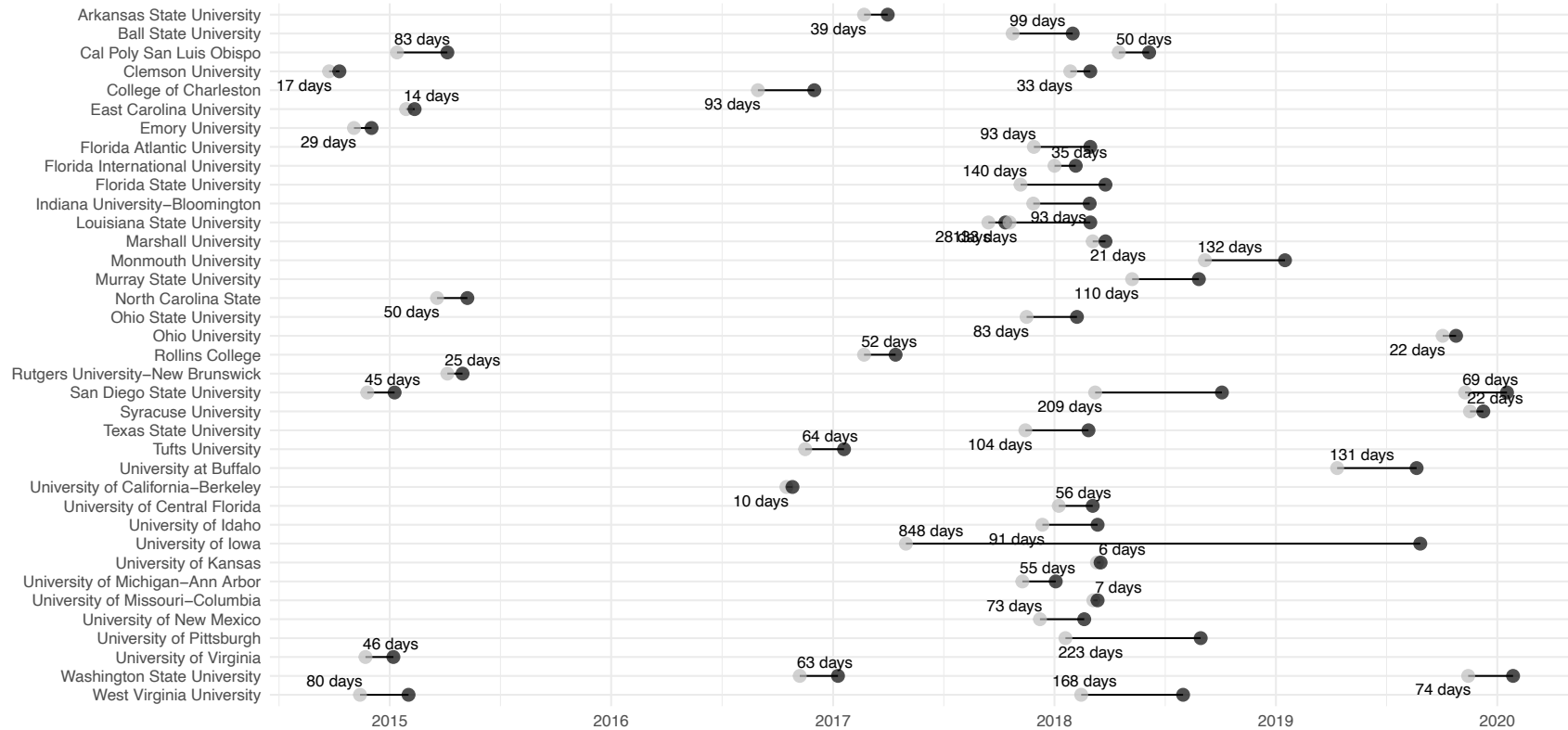


Figure 1: Distribution of Moratoriums Across the Sample Period for all Universities

Note: The sample period starts in 2014 and ends on the last day of 2019. The lengths of the moratoriums in this graph represent calendar-day lengths, not academic-calendar day lengths. Universities experience one to three moratoriums in the sample period. Note that the two moratoriums that end in the year 2020 end in January at the beginning of the semester. This short period in 2020 is not included in the sample.

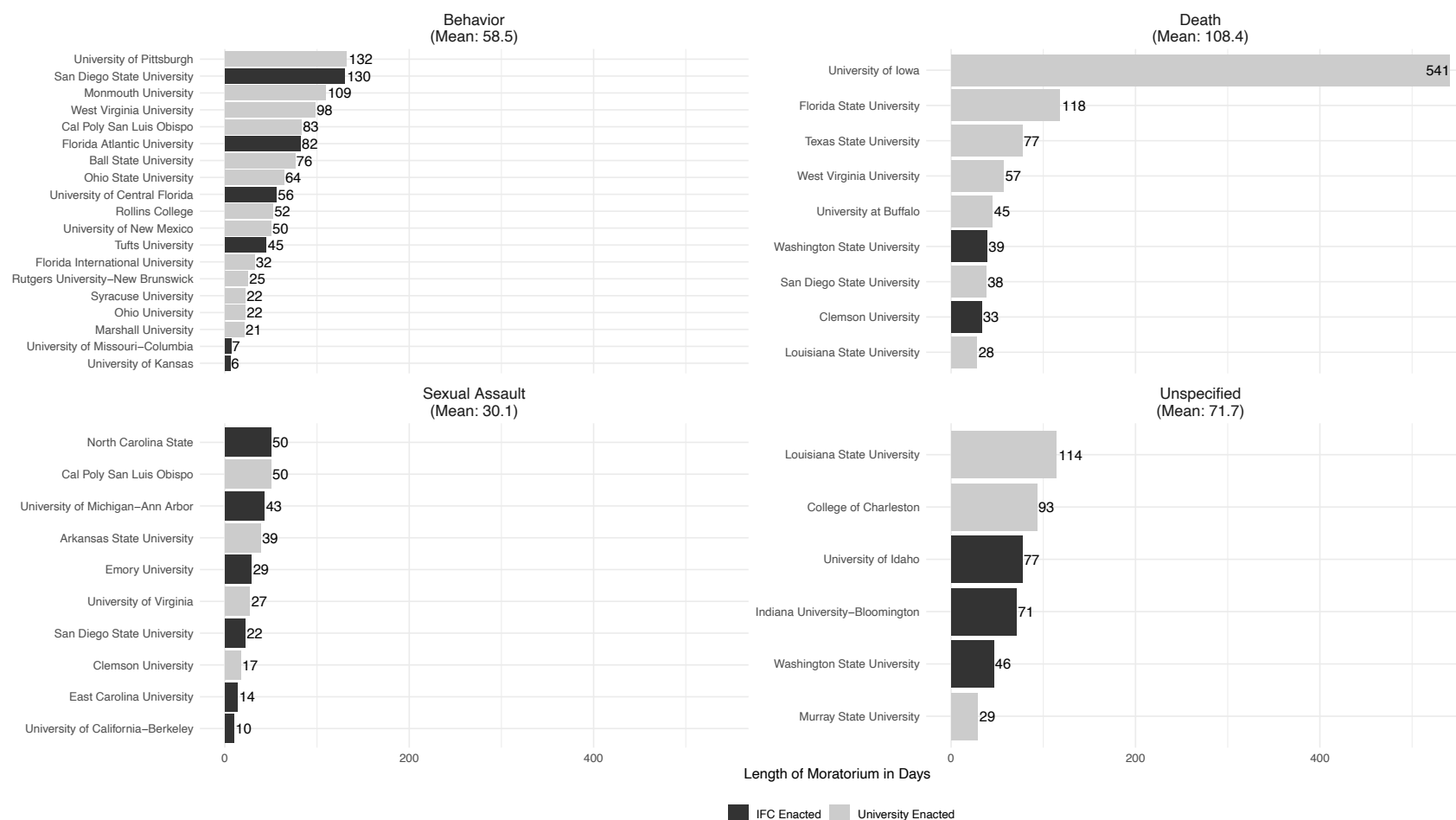


Figure 2: Number of Academic Calendar Days in Each Moratorium by Triggering Event

Note: Lengths of moratoriums represent academic calendar days. Therefore, the lengths of moratoriums differ from Figure 1. Grey shaded regions represent a moratorium that was imposed by the university, while black shaded regions represent moratoriums that were imposed by the IFC. Each of the four categories represents the event that triggered a moratorium. Behavior violations is a catchall term for hazing, rule violations, offensive behavior, and other disorderly conduct. Death relates to a fraternity-related death that triggered a moratorium. Sexual assaults relate to a sexual assault case that triggered a moratorium. Lastly, the Unspecified category represents all moratoriums in which the moratorium triggering event is unknown or unclearly defined.



Figure 3: Locations of the Universities Included in the Sample

Note: There are a total of 37 universities in the sample, five of which are private universities. Data on both geographic location and private/public entity are obtained from the Integrated Postsecondary Education Data System (IPEDS).

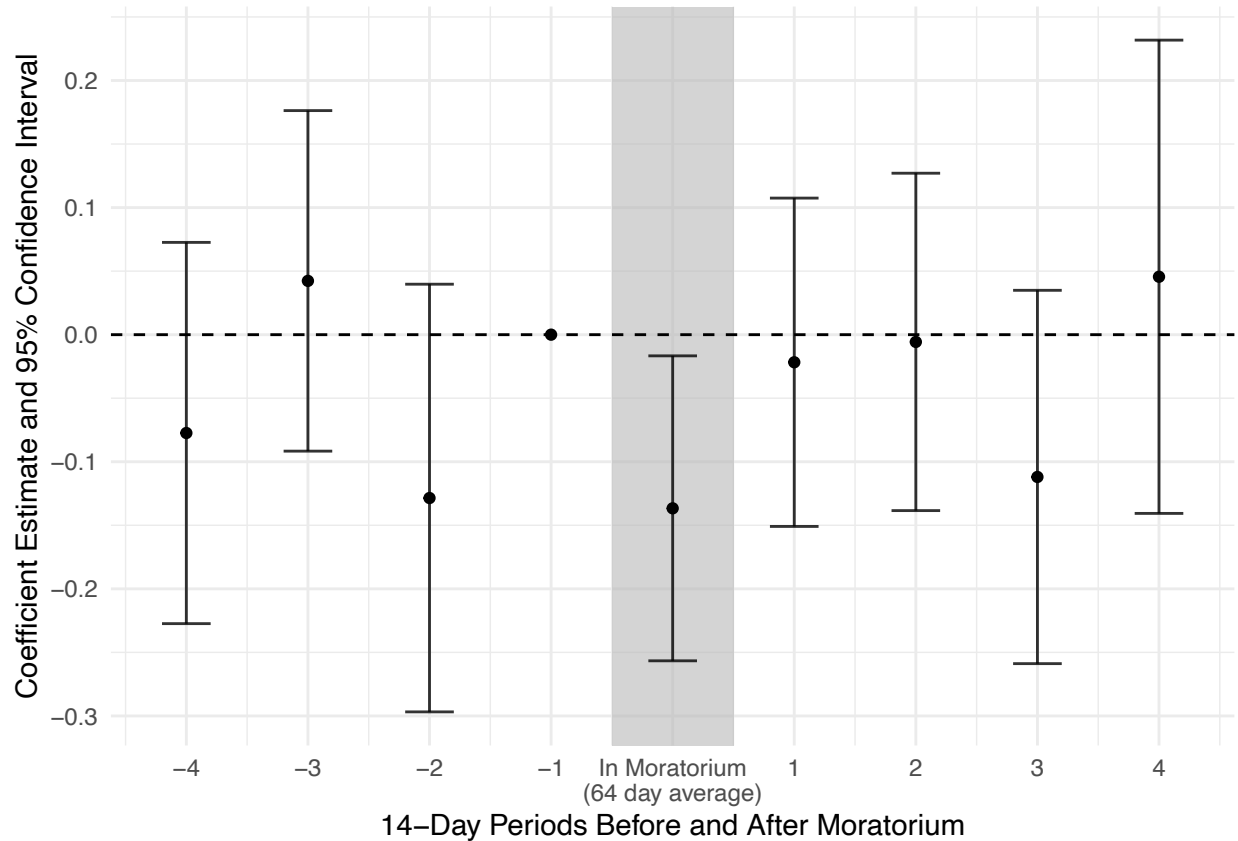


Figure 4: Event Study for Alcohol Offenses

Note: The shaded area point estimate represents an entire moratorium period for each university. Hence, the shaded area point estimate has varying amounts of days within based on the university. For instance, Arkansas State University had a 39-day moratorium and therefore their shaded area point estimate would be identified by the 39 moratorium days. Point estimates not within the shaded region are 14-day periods. Number of days within a period are chosen to give approximately a median-length (46 days) moratorium on each side of the shaded area. All periods are normalized by the 14-day period before the moratorium. Alcohol offenses are defined as alcohol offenses per-25000 enrolled students. Controls include holiday, spring semester, day of the week, football game-days, and university-by-academic-year. Standard errors clustered by university. All errorbars represent 95% confidence intervals. A joint-hypothesis F-test that each of the leading periods are zero shows that the p-value is 0.27 which is statistically insignificant.

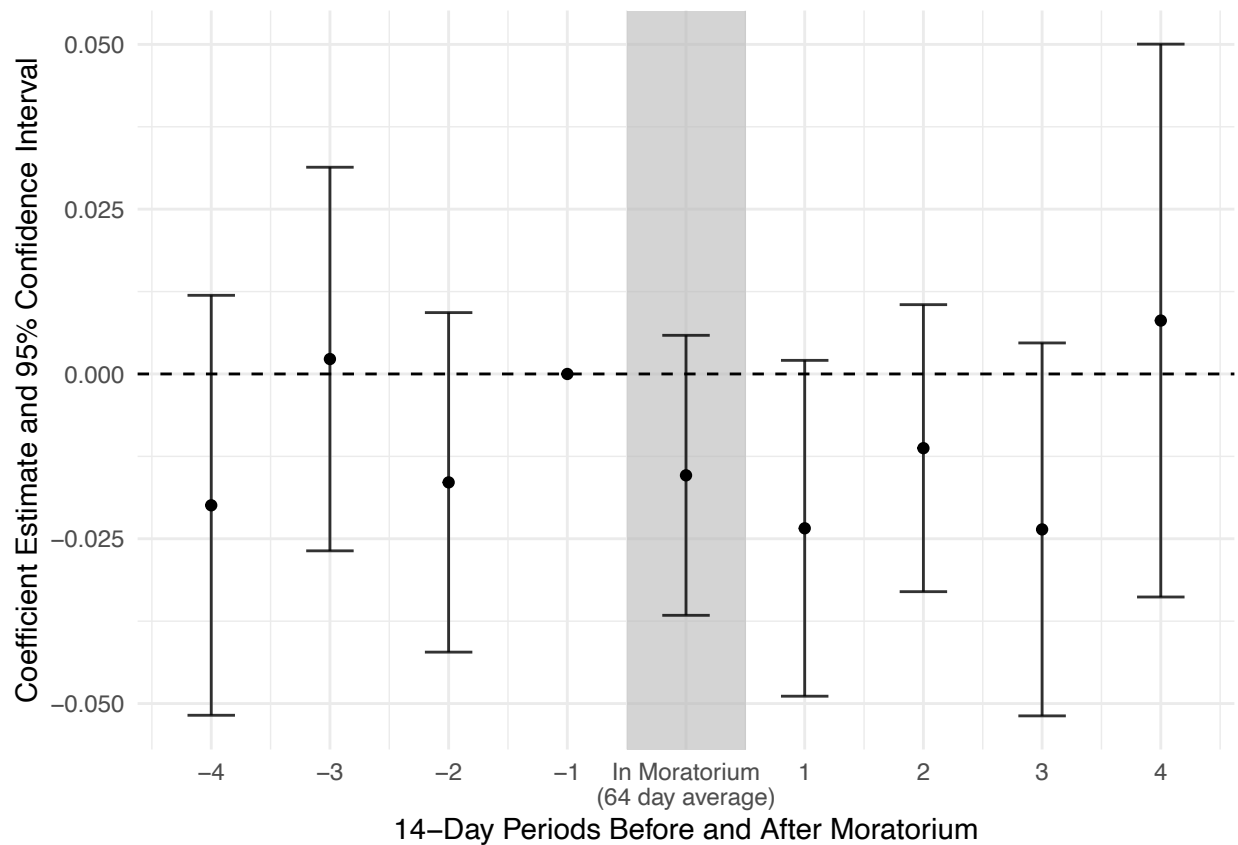


Figure 5: Event Study for Sexual Assault Offenses

Note: The shaded area point estimate represents an entire moratorium period for each university. Hence, the shaded area point estimate has varying amounts of days within based on the university. For instance, Arkansas State University had a 39-day moratorium and therefore their shaded area point estimate would be identified by the 39 moratorium days. Point estimates not within the shaded region are 14-day periods. Number of days within a period are chosen to give approximately a median-length (46 days) moratorium on each side of the shaded area. All periods are normalized by the 14-day period before the moratorium. Sexual assault offenses are defined as sexual assaults per-25000 enrolled students. Controls include holiday, spring semester, day of the week, football game-day, and university-by-academic-year. Standard errors clustered by university. All errorbars represent 95% confidence intervals. A joint-hypothesis F-test that each of the leading periods are zero shows that the p-value is 0.54 which is statistically insignificant.

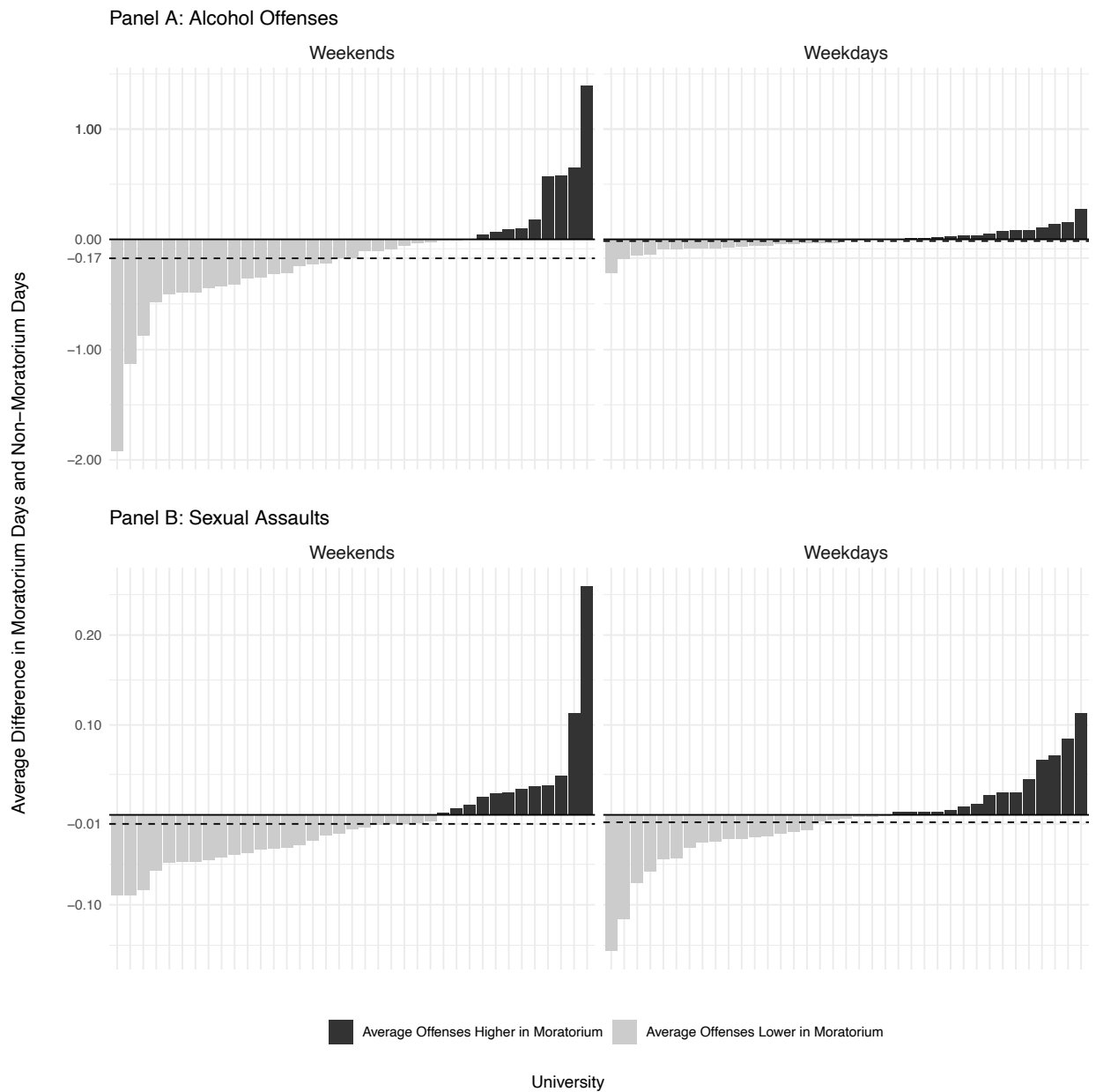


Figure 6: Difference in Average Offenses on Moratorium Days and Non-Moratorium Days
Note: The y-axis represents the average difference in offenses per-25000 enrolled students on moratorium days and non-moratorium days for each university. Negative y-axis values indicate that average offenses were lower on moratorium days than non-moratorium days. The x-axis denotes a unique university. The solid black line on the y-axis is 0, while the dashed black line denotes the average of the entire distribution.

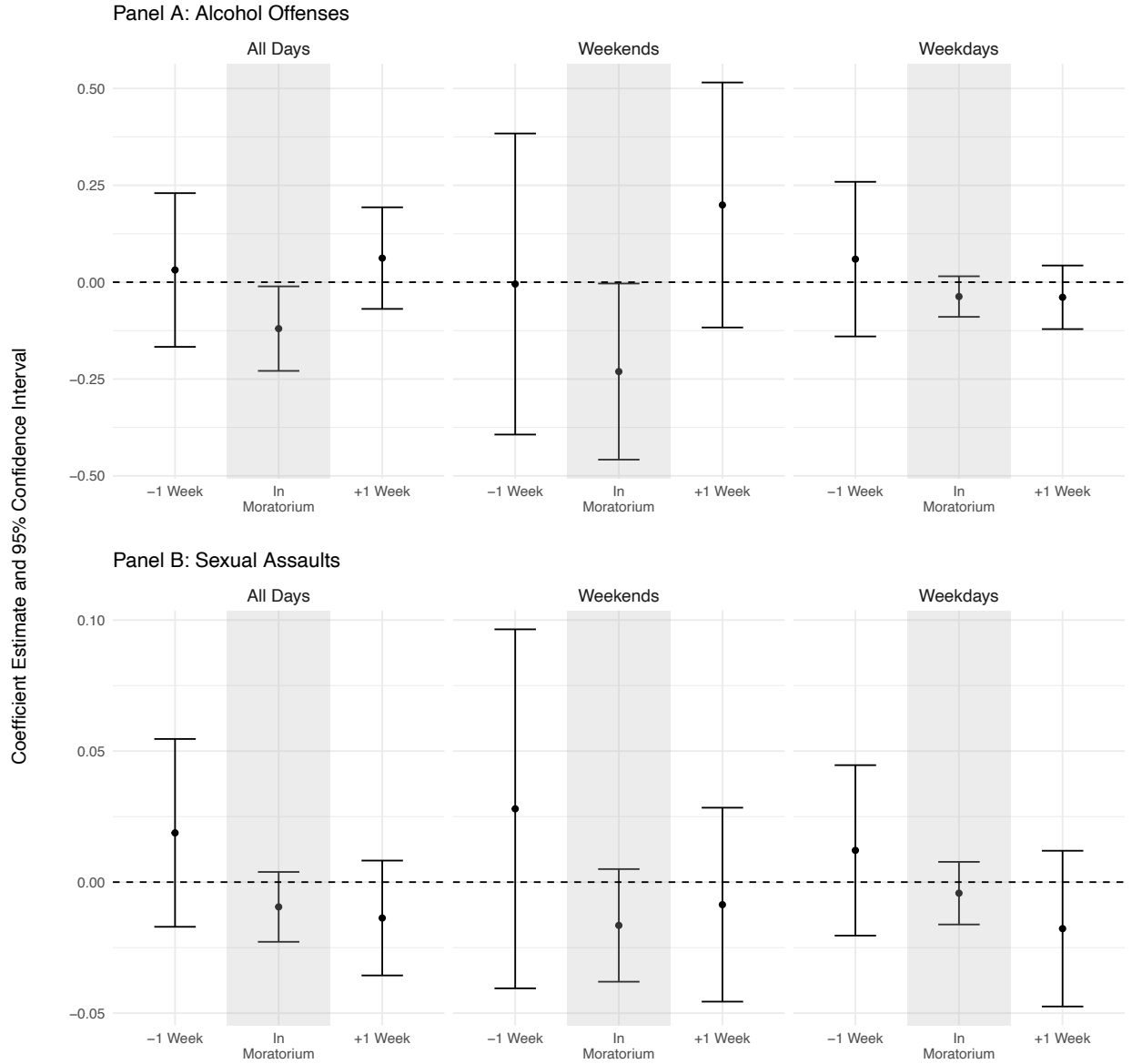


Figure 7: Coefficient Estimates Including a Week Before and Week After Indicator
Note: The x-axis represents three periods: the week before a moratorium, the moratorium itself, and the week after the moratorium. Indicators for week before and week after are added to Specification 2 from Table 4. Controls include holiday, spring semester, day of the week, football game-days, and university-by-academic-year. Standard errors are clustered by university. Weekends represent Fridays, Saturdays, and Sundays. Weekdays represent Mondays-Thursdays. Errorbars represent 95% confidence intervals.

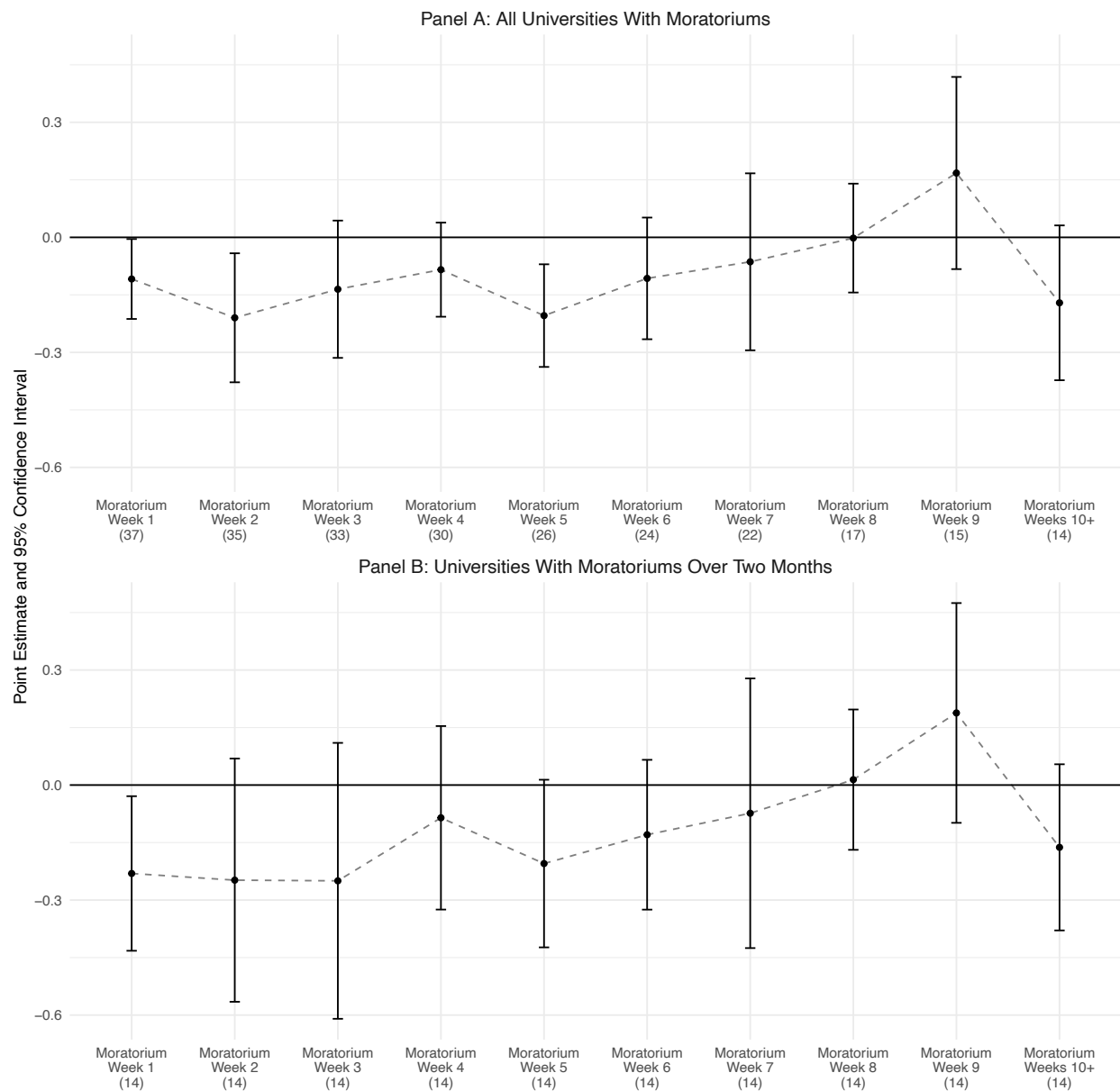


Figure 8: The Dynamics of a Moratorium (Alcohol Offenses)

Note: This figure shows how the effect of a moratorium progresses over time. Each point estimate represents a week within a moratorium, except Moratorium Weeks 10+, which pools moratoriums weeks 10 and above. The x-axis represents the week number the moratorium is currently in, while the parenthesis represents the number of universities that identify the point estimate. Recall that moratorium lengths differ across universities, and therefore some universities may not identify each weekly estimate. The y-axis represents the point estimates and 95% confidence intervals. Panel A estimates include all universities in the sample using the preferred specification, while Panel B estimates include only universities that have moratoriums over two-months long (approximately the average length of a moratorium). Standard errors are clustered by university, and controls include holiday, spring semester, day of the week, and university-by-academic-year.

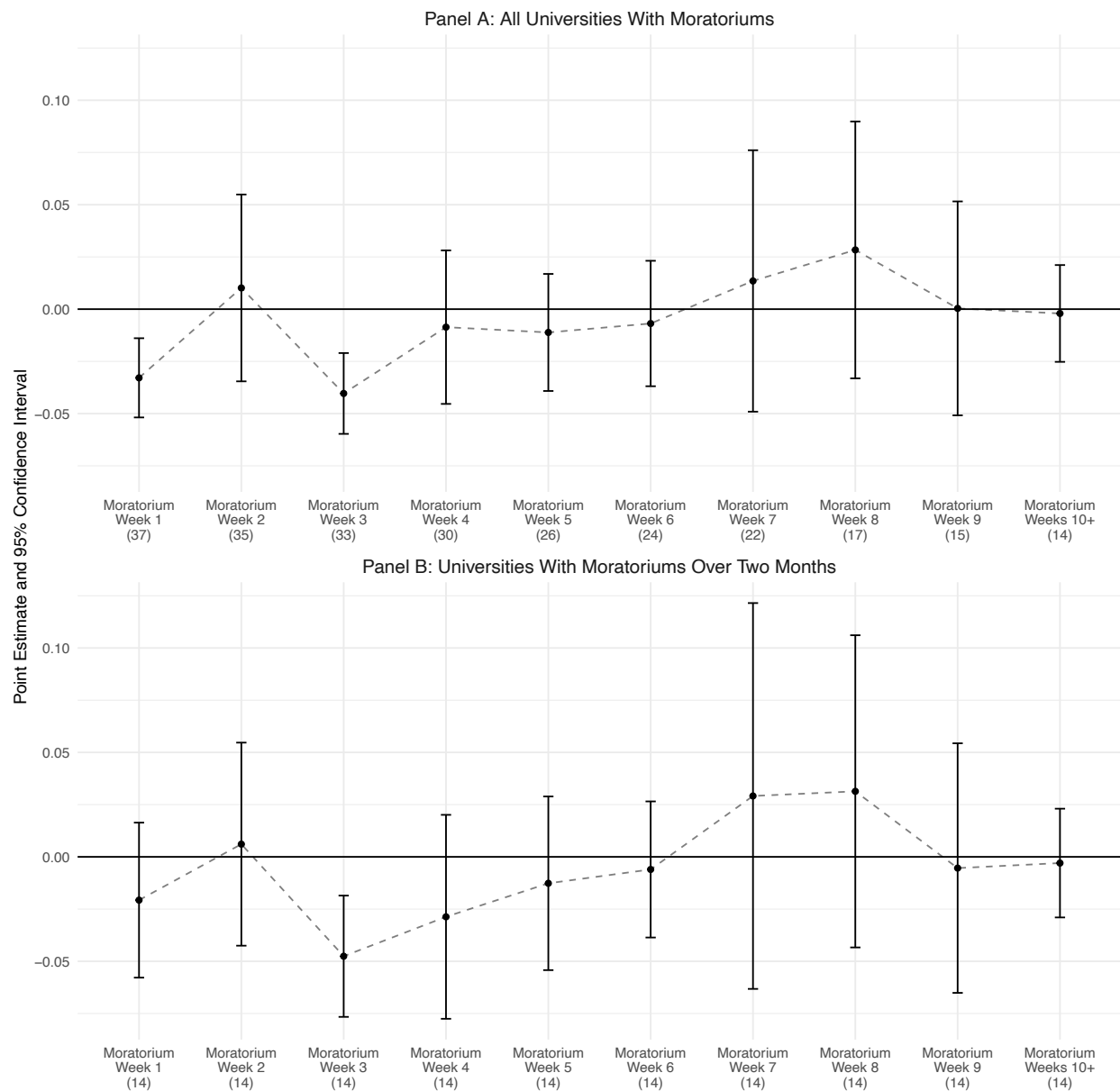


Figure 9: The Dynamics of a Moratorium (Sexual Assaults)

Note: This figure shows how the effect of a moratorium progresses over time. Each point estimate represents a week within a moratorium, except Moratorium Weeks 10+, which pools moratoriums weeks 10 and above. The x-axis represents the week number the moratorium is currently in, while the parenthesis represents the number of universities that identify the point estimate. Recall that moratorium lengths differ across universities, and therefore some universities may not identify each weekly estimate. The y-axis represents the point estimates and 95% confidence intervals. Panel A estimates include all universities in the sample using the preferred specification, while Panel B estimates include only universities that have moratoriums over two-months long (approximately the average length of a moratorium). Standard errors are clustered by university, and controls include holiday, spring semester, day of the week, and university-by-academic-year.

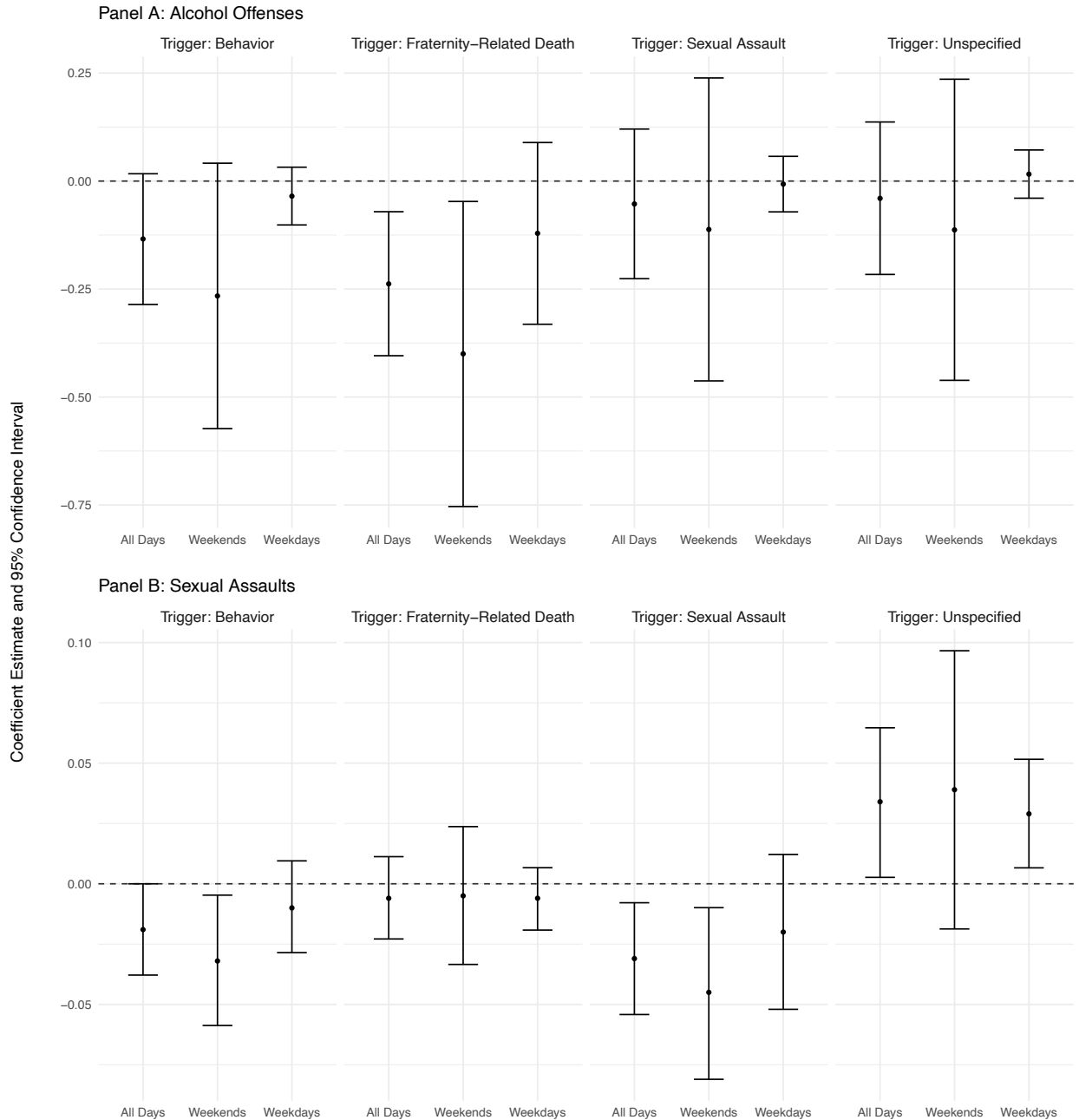


Figure 10: Heterogeneous Effects of Moratoriums by Triggering Event

Note: The x-axis represents three periods: the entire sample (All Days), weekends only, and weekdays only. Specification 2 (the preferred specification) from Table 4 is used in estimation. Each of the four categories represent the event that triggered a moratorium. A behavior violation refers to hazing, rule violations, offensive behavior, and other disorderly conduct. Death relates to a fraternity-related death that triggered a moratorium. Sexual assaults relate to a sexual assault case that triggered a moratorium. Lastly, the Unspecified category represents all moratoriums in which the moratorium triggering event is unknown or unclear. Errorbars represent 95% confidence intervals. Weekends represent Friday-Sunday, while Weekdays represent Monday-Thursday.

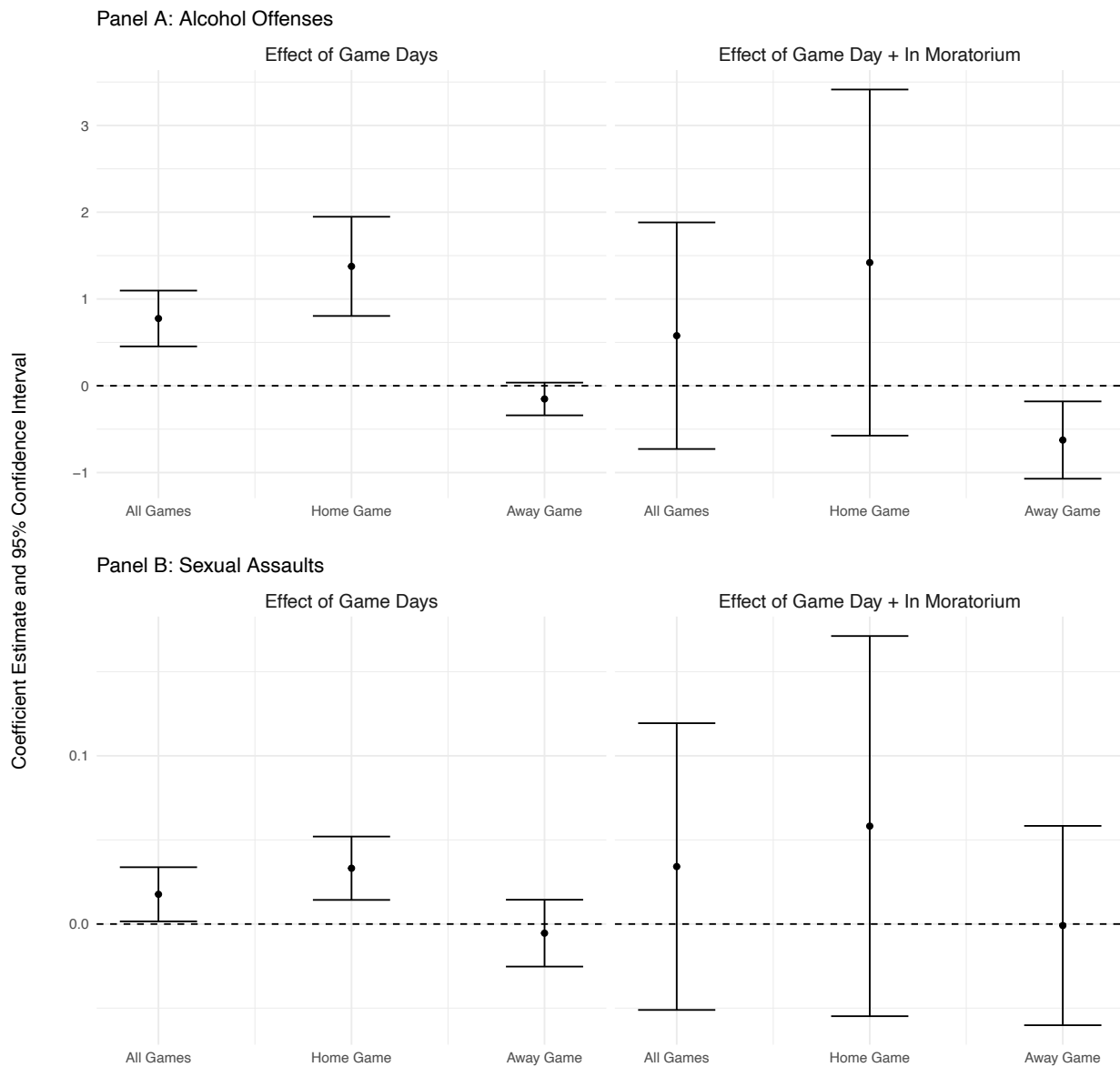


Figure 11: The Effect of Football Game-days With and Without Moratoriums
Note: Game days include all football games occurring in the sample period. 34 of the 37 universities have football teams and corresponding game days. The y-axis represents coefficient estimates. Errorbars represent 95% confidence intervals. Each panel is split into two effects: the first effect being the effect of only football game days on the outcome per-25000 enrolled students, and the second being the effect of a football game that occurs within a moratorium. The All Games category includes both home and away games. The effects of game days + moratorium is identified by 89 football games that coincide with moratoriums. Controls include holiday, spring semester, day of the week, and university-by-academic-year. Standard errors are clustered by university.