Thank you for the opportunity to revise and resubmit this journal submission. This revision contains updates which significantly strengthen the paper's assumptions, results, and analyses. In this revision, the paper now contains a more convincing test to credibly argue that the effect of moratoriums is not driven by triggering events such as a death. In addition, weighted regressions are shown to verify the main results, moratorium dynamics are analyzed in the new Section 5.4, and heterogeneity analysis leveraging fraternity member population is newly discussed in Appendix D. Clarity has also been enhanced, as the paper now provides more context surrounding the sample universities, expands upon the missing details regarding the Campus Safety and Security Data, and more directly connects to the literature surrounding university policies and adolescent health. Despite these important additions, the length of the paper has grown minimally, as I have condensed the data collection/cleaning sections by over 500 words and moved minute details to the new Appendix A: Details on Data Collection. Please note that I have created sections and subsections to help organize this response. I provide all figures and tables at the end of this document so that they can be linked to my responses to the comments and suggestions. Section 0 responds to the suggestion from the editor. Section 1 includes responses to all comments and suggestions provided by Referee #1, Section 2 responds to comments and suggestions from Referee #2, Section 3 responses to the report provided by Referee #3, and Section 4 responds to suggestions from Referee #4. Note that all comments and suggestions from the referees are in boxes.

0 Response to the Editor

0.1 Suggestion for Improvement

Your analysis is a straight-up policy evaluation, which is fine and the JHR has published a number of papers of this type. Still, in their letters to me, all the reviewers raise the question whether this is an important enough question to warrant publication in the JHR. They fall on both sides of the divide, about equally split. R2 has perhaps the strongest concerns along those lines, which are outlined also in their comments to you. I believe it is an important question and goes to the core issue of whether university policies have any bite. However, the fact that all reviewers have the same concern signals to me that the importance of the question and the implications from your analysis could be emphasized more strongly and perhaps positioned in a somewhat wider literature.

Thank you for your helpful insight on how to better phrase the core question of this paper. To emphasize the importance, I have more firmly placed the paper in a literature surrounding the effectiveness of university policies including academic probation, financial aid, and deferred recruitment—a fraternity/sorority-related policy. I now underscore the contrast between moratoriums and these policies, as moratoriums can be quickly implemented and distinctly alter a university's social environment given that students unaffiliated with fraternities may still attend fraternity parties. In addition, I briefly describe two new findings in the introduction to address whether this university policy has any bite; first, that moratorium effects diminish after a month of implementation, and second, that there is suggestive evidence that schools with a higher share of fraternity life exhibit stronger moratorium effects. The added information improves the understanding of the results as it helps school administrators determine the appropriate duration of a moratorium, or whether a moratorium is even suitable given their fraternity-affiliated population. In addition, to position this paper in a somewhat broader literature, I have expanded the literature review to include a citation on the health effects of alcohol on adolescent brain development. By mentioning this, I hope to show readers that there could be implications beyond the decreases in alcohol offenses and sexual assaults that are explicitly analyzed in the paper. I have highlighted these points in the introduction, beginning in paragraph eight (page 3), which now reads:

> ...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 5.4, 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 2021), 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 Appendix D which shows suggestive evidence that universities with higher shares of fraternity members exhibit larger moratorium effects...

1 Response to comments from Referee #1

1.1 Suggestion for Improvement

> In the section about spillover analysis using Campus Safety and Security (CSS) data, the author has not specified if they used arrests for liquor law violations, disciplinary actions for liquor law violations, or some (weighted) average of both. Under Clery Act 1990, both the number of arrests and the number of disciplinary actions in residence halls (and elsewhere) are collected and reported by colleges and universities. However, the author has failed to specify which of the two they have used in this section of the paper. Not only should the author clearly specify which of these two variables is being used for the analysis in this section, but I would suggest that the author run separate analyses using each of the two variables (the number of arrests and disciplinary actions due to liquor law violations in residence halls) so that the reader can get some additional insights.

This is an excellent suggestion as the delineation between disciplinary actions and arrests is important for interpretation, clarity, and replication purposes. In the reviewed draft, the Campus Safety and Security (CSS) analysis uses disciplinary actions for liquor law violations and excludes arrests. As suggested, I have extended the analysis to include both disciplinary actions and arrests for liquor law violations in Appendix C and updated the final two paragraphs of Section 5.2 with additional commentary. Both of these extensions are further explained in the following paragraphs.

In Appendix Table C1, I delineate between disciplinary actions and arrests in the CSS data for alcohol offenses by adding Columns 4 and 5 which analyze the effect of moratoriums on arrests for all reports in the CSS and residence hall incidents respectively. Columns 4 and 5 show that there is little evidence of arrests changing at universities, regardless of the location. In particular, for each additional moratorium day in a calendar year, alcohol arrests per-25000 enrolled students do not exhibit any statistically significant decrease and the point estimates are relatively small when compared to disciplinary actions. This is consistent with the literature that campus police do not commonly arrest students for underage drinking (Bernat et al. 2014). Hence, while there is evidence that moratoriums affect student alcohol behavior, there is little evidence that moratoriums affect arrests for alcohol violations. In light of these findings, I have updated the final paragraph of Appendix C to reflect this analysis:

> However, there is little evidence of an effect on liquor law arrests as shown in Columns 4 and 5—consistent with the literature that campus police do not typically

> arrest students for alcohol violations (Bernat et al. 2014). As discussed in Section 5.2, this supports the notion that if moratoriums displace alcohol-fueled behavior, they displace it to *less* risky areas whereby behavior can more easily be intervened before it becomes dangerous.

Given the lack of significance and small point estimates in the arrest columns, I update the final two paragraphs of Section 5.2 to address the delineation between arrests and disciplinary actions and save the additional analysis of arrests for Appendix C. However, I am happy to move the analysis to the main paper upon the editor's request. The following is an excerpt from the updated Section 5.2 in the main paper:

> 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 extension continues in the final paragraph where I more directly assert the findings:

> 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 disciplinary actions of 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 assault reports.

Finally, the text of Appendix C more closely describes the CSS data used and the corresponding results. As noted at the beginning of the document, the appendices are included at the end of this document for convenience.

2 Response to comments from Referee #2

2.1 Suggestion #1: Weighting

> I'm not sure the results are strong enough. Qualitatively, the results are unsurprising - banning activities with involve alcohol will likely reduce crimes which involve alcohol. Quantitatively, the results are imprecise, and hence difficult to interpret confidently. The point estimates seem very large, alcohol related crimes reduced by 26% despite the fraction of the students enrolled in IFC fraternities being less than 5%. I'm not dismissing this magnitude as unfeasible - especially if the author could show some more supplementary evidence on the likely share of such crimes that are associated with fraternities. However, the CIs are wide, and do not rule out very small effects, so it is not clear whether or not the effects are actually 'large'. Of course these issues are true to a greater extent for the more imprecise results for sexual assault.
>
> Two suggestions which may improve precision: To weight by total enrollment (or perhaps undergraduate enrollment if you feel that is more appropriate). Larger schools should have less residual variation, so weighting by size should reduce standard errors. The variance in school size should be large enough for this to make a difference.

Thank you for the thoughtful suggestion on how to tighten/strengthen the main results of the paper. First, I agree that despite the large point estimates, the standard errors do not rule out small effects. To reconcile this, I have deleted all language where I describe the effects as large. However, as pointed out, these estimates are not unfeasible. Although the data cannot delineate whether an incident is associated with fraternities, non-members are also likely affected by moratoriums as fraternity parties are a substantial source of partying for all undergraduates (Harford, Wechsler, and Seibring 2002). Moreover, the magnitude (29% on weekends) for the decreases in sexual assaults are in-line with the increases (28%) found due to intensifying partying in Lindo, Siminski, and Swensen (2018).

Second, as recommended, I have explored weighting by total enrollment to tighten the main results. Unfortunately, the weights do not tighten the results to a significant degree. However, the results remain extremely consistent with their unweighted counterpart. For ease of comparison between the weighted and unweighted results, I have included a figure that is unique to this cover letter in Appendix Figure F1. In this figure, I show the point estimates and 95% confidence intervals for each specification (unweighted and weighted) from the main results table (Table 4). The figure demonstrates that the results are largely similar with minimal gains in precision across most specifications. However, to maintain transparency, I have included a table with the weighted main results in Table E5. In addition, I have also alluded to these weighted estimations when discussing the main results in paragraph three of Section 5.1 which reads:

> Third, due to the large variation in university size, the models in Table 4 are weighted by total enrollment in 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.

2.2 Suggestion #2: Leverage IFC Variation

> To interact the treatment variable with the share of students who are in IFC fraternities. Perhaps I'm missing something, but I would expect the effect size to be essentially proportional to the proportion in IFC fraternities. To be clear, I'm suggesting replacing Moratorium (in equation 1) with Moratorium * Fraction_IFC.

This is a great suggestion, also proposed by Referee #4. Prompted by this, I have collected the additional data needed for this analysis as the data was previously missing four universities' IFC population numbers. I have updated Section 6.1 with a short discussion and have added an additional Appendix D which more thoroughly describes the analysis. As discussed in the excerpts copied below, the results give suggestive evidence that higher proportions of IFC members at a university result in larger moratorium effects. However, these results are low powered with relatively large confidence intervals. To reconcile these results, I have conducted additional analysis which illuminate why these results are not as strong as intuition might suggest. I show that the IFC population numbers are a weak proxy for a school with an active fraternity/sorority life by plotting universities' IFC fraction against their Niche.com Colleges with the Best Greek Life ranking. The following paragraphs describe both the recommended analysis, and the additional analysis.

First, as mentioned above, I have updated Section 6.1 with a short discussion on the results of the suggested analysis in paragraph four. This paragraph also links the reader to a more thorough discussion of this analysis in the new Appendix D. The contents of the new Appendix D are copied below:

> In this appendix, I analyze whether universities with a higher fraction of undergraduates belonging to IFC fraternities exhibit larger effects during a moratorium.

Each university in the sample has a different share of its student population belonging to IFC fraternities. Recall from Table 2 that the fraction of undergraduate students with IFC membership can range from 1% to as high as 11%. Presumably, a moratorium has a greater effect on student behavior when the restrictions apply to a greater share of students.

To conduct this analysis, I supplement the preferred specification with an interaction of $InMoratorium_{u,t}$[1] and $FractionIFC_u$, where $FractionIFC_u$ is the earliest recorded count of IFC fraternity members over 2014-2019 at university u, divided by the undergraduate enrollment, and centered at its mean. I use the earliest count of IFC members for two reasons; first, to avoid the potential issue of declines in IFC membership after a moratorium due to permanent suspensions of specific IFC chapters, and second, many universities do not maintain records of IFC numbers for every year in the sample period. However, in the universities that do supply complete records, I do not find substantial semester-to-semester changes in IFC populations.[2] Therefore, an early one-year measure of the IFC population is a good approximation for the other corresponding years. In effect, the interaction of $InMoratorium_{u,t}$ and $FractionIFC_u$ creates a measure of moratorium intensity—universities with a higher fraction of IFC members receive a more intense treatment than universities with lower shares.

Table D1 provides suggestive evidence that moratoriums with a higher fraction of student enrollment belonging to an IFC fraternity result in larger decreases in alcohol offenses and sexual assaults during a moratorium period. In Panel A, the point estimates for the interaction term show patterns consistent with the main findings in the paper—the effects are negative with the strongest effects are observed on the weekends when partying is more frequent. Similarly, in Column 1 of Panel B, the interaction term coefficient shows suggestive evidence that moratoriums in universities with a higher share of IFC members exhibit larger decreases of sexual assaults. However, none of the interaction coefficients presented in either panel are significant, indicating only a suggestive relationship between the share of IFC members and the impact of moratoriums.

The results of Table D1 may appear surprisingly inconclusive given the expectation that universities with a higher share of fraternity members exhibit larger effects.

[1]Note that I have changed the nomenclature of the main equation from reading $Moratorium_{u,t}$ to $InMoratorium_{u,t}$. This was done to align better with the tables/figures.

[2]West Virginia University is an exception to this. Their official IFC count decreased by over 60 percent in years following the moratorium.

> One possible reason for these inconclusive results is that the share of fraternity members is a noisy indicator for a fraternity-related activity—schools with a small share of fraternity life may have chapters that are particularly active, or vice-versa. To demonstrate this, I plot each university's undergraduate IFC fraction against its Niche.com Colleges with the Best Greek Life ranking. The ranking, based on survey responses from Niche.com users, ranges from 1-300, and 32 out of the 37 universities in the sample are ranked in the top 300. For the remaining five schools, I assign a ranking between 301-305. Figure D1 shows the inverse relationship between these two measures: as the Greek Life ranking increases, the fraction of undergraduates in an IFC fraternity generally decreases. This likely contributes to the negative point estimates in the previous analysis. However, this relationship is noisy, and the slope is not statistically different from zero at the 5% level. This may explain why the previous analysis only provided suggestive rather than clear evidence.

Last, note that the Niche.com Colleges with Best Greek Life rankings list is similar to the Niche.com Top Party Schools in America list which I analyze in Section 6.1, Table 7. Specifically, 16 of the highest 18 ranked Greek Life universities in the sample are also defined as party schools.

3 Response to comments from Referee #3

3.1 Major Comment 1: Effect of a Death

In reading the manuscript, I was initially concerned that the author was not going to address the critical assumption that a triggering event that may lead to a moratorium does not also lead students to change their behavior in ways that would reduce alcohol offenses or sexual assaults - the outcomes of interest. It is plausible that the death of a peer as a result of risky behavior at a fraternity would lead students at the university to at least temporarily change their behavior in ways that would reduce alcohol offenses and sexual assaults. This concern would seem to merit discussion in section 4.2 "Identification Assumptions". While the author addresses the concern that the timing of fraternity moratoriums are as-good-as-random, it would seem equally important that the triggering event is not influencing both existence of a moratorium and the outcomes of interest. This analysis, that was critical for me to view the estimated effects as credibly causal, comes at the very end of the paper in section 6.2. This section "Does the Triggering Event for a Moratorium Matter?" primarily focuses on whether it matters if the triggering event is "...the result of a fraternity-related death, a prominent sexual assault, or a behavior violation." Finally, the author gets to the critical statement on page 24 "To ensure that this is the effect of the moratorium rather than the triggering death, Appendix Figure C7 shows the preferred specification restricted to only the universities that experienced a fraternity-related death with an additional 15 universities that experience a fraternity-related death in the same period, but did not undergo a moratorium." These additional universities and their experiences with a fraternity-related death but no moratorium provide a valuable comparison group. I believe the author needs to clarify the estimation equation used in this figure - I am assuming the author did what I would hope, but found the text and note in the figure less than clear. I also believe the author needs to put a greater spotlight on this issue and more clearly present the results, even if it weakens the findings. The results would still be important. However, as a college administrator considering a moratorium, I want to clearly understand the best estimate for how much I would be expected to reduce alcohol violations and sexual assaults by implementing a moratorium after a fraternity-related death of a student - beyond any expected reduction resulting from just the triggering event. The estimated impact of moratoriums on sexual assaults, in Figure C8, are not even explicitly mentioned in the text for this specification and sample.

Thank you for the excellent comment. I agree that it is indeed plausible that the effect of a death—rather than the moratorium itself—could be contributing to the reductions in alcohol offenses that this paper shows and needs greater emphasis for the main results to be interpreted as causal. To address this concern, I make two major refinements in the paper. First, as suggested, I have highlighted this concern in Section 4.2 by including it as an additional assumption of the model (see page 12). I have also included a more detailed discussion of this assumption in paragraph 6 of Section 4.2, which directly responds to this concern. Second, I conduct a new and more convincing test for showing that this assumption is plausible based on a suggestion from Referee #4. In this test, I utilize 15 universities that undergo a fraternity-related death during the sample period, but do not undergo a moratorium. Using these 15 universities, I assign a 64-day treatment period starting with the date of the death and find little evidence that alcohol offenses or sexual assaults are affected by a fraternity-related death in the absence of a moratorium. This test is distinct from the previous test in the original draft that initially utilized these 15 universities as never-treated control units with the main sample and more directly tests the effect of a death without a moratorium. Considering the results of this stronger test, I have removed the old test, as this new analysis provides clearer evidence that the event of a death does not contribute to decreases found in the main results. To present these results, I have revised the Appendix Figures E8 and E9 to clearly convey the findings and distinguish between the samples.

Below, I have copied the updated Section 4.2 paragraph 6 which describes the added assumption and Section 6.2 paragraph 2 which tests this assumption for your convenience:

> (Section 4.2 paragraph 6: added assumption) To evaluate the third assumption that the triggering event is not changing student behavior, I perform heterogeneity analysis in Section 6.2 and analyze the effect of a moratorium by each triggering event. As discussed further in Section 6.2, 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.
>
> (Section 6.2 paragraph 2: testing of assumption) 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.[3] 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 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 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.

3.2 Major Comment 2: Progression of a Moratorium

> The author also suggests that the finding might inform administrators of the potential benefit of making a moratorium permanent. To address this question and to provide further evidence that the moratorium is causing the change and not the triggering event, I believe the author should also consider further examination of the variation in the length of the moratoriums. The author does explore the average daily effect of moratoriums across three lengths (Table 6). I was rather surprised to find the shortest moratoriums

[3] These universities were found using Hank Nuwer's repository of hazing-related deaths in the US: https://www.hanknuwer.com/hazing-deaths/.

to have no effect (and a positive point estimate on alcohol offenses). Presumably none of these short moratoriums were due to a student death? As a reader, I was certainly left wondering if this was due to the type of triggering event. What I was hoping to see was an analysis that explicitly examined whether there are heterogenous impacts of the moratorium by how long it has been in effect. Over time, does the effect diminish within the moratorium? My understanding of the analysis is that the author explores different periods after the conclusion of a moratorium, but not different time periods within the moratorium. In Table 6, I don't know if the large negative impact on alcohol offenses are occurring evenly across the moratorium or primarily during the first 30 days. For policy implications, it would be incredibly valuable to understand when the reductions of alcohol offenses occur within the moratorium. This would inform both the optimal length of a moratorium and whether we would expect any benefit from a permanent ban on alcohol at these events.

I agree that this suggestion gives valuable information for school administrators to more effectively plan their moratorium length. I have added an additional section, Section 5.4, which describes the analysis to address this suggestion. I have copied the contents of Section 5.4 below for convenience:

> 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.[4] 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

[4]Note that nine weeks is approximately the average length of a moratorium.

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.

3.3 Major Comment 3: Update Table 7

To better interpret the results in Table 7, it would be helpful to know about the relative mean number of alcohol offenses at party schools versus non-party schools outside of a moratorium. It is possible for the reader to calculate this between the text in the second paragraph in section 6.1 and the information on Table 7, but it would be easier if it was provided. It would also highlight for the reader that party schools (as determined by the external rankings) do have more alcohol offenses per-25000 students than non-party schools.

Thank you for this comment as it will certainly help readers better understand the level of alcohol offenses/sexual assaults at party schools relative to non-party schools outside of a moratorium. I have added an additional paragraph in Section 6.1 and have updated Table 7 with the non-moratorium means to help highlight the fact that party schools have more alcohol offenses per 25000 students than non-party schools. I have included the paragraph below for convenience:

As shown in Table 7, universities defined as party schools exhibit higher averages of alcohol offenses assaults relative to non-party schools. In particular, non-party schools experience approximately 49% less alcohol offenses on average. These differences are similar when excluding moratorium days (47%), although both party schools and non-party schools have relatively similar levels of sexual assaults.

4 Response to comments from Referee #4

4.1 Major Suggestion 1: Leverage IFC Variation

> Currently, the main specification treats all moratoriums the same. However, presumably a moratorium at a school with 1.3% of the body in IFC fraternities is less impacted by a moratorium than one with 10.2% IFC membership. Can you provide heterogeneity analysis that probes this difference in intensity of treatment? This could be accomplished by interacting moratorium with share IFC or looking at quartiles of share IFC etc.

Thank you for this thoughtful suggestion. Given that Referee #2 had the same suggestion, I copy my response in the following paragraphs for your convenience:

This is a great suggestion, also proposed by Referee #4. Prompted by this, I have collected the additional data needed for this analysis as the data was previously missing four universities' IFC population numbers. I have updated Section 6.1 with a short discussion and have added an additional Appendix D which more thoroughly describes the analysis. As discussed in the excerpts copied below, the results give suggestive evidence that higher proportions of IFC members at a university result in larger moratorium effects. However, these results are low powered with relatively large confidence intervals. To reconcile these results, I have conducted additional analysis which illuminate why these results are not as strong as intuition might suggest. I show that the IFC population numbers are a weak proxy for a school with an active fraternity/sorority life by plotting universities' IFC fraction against their Niche.com Colleges with the Best Greek Life ranking. The following paragraphs describe both the recommended analysis, and the additional analysis.

First, as mentioned above, I have updated Section 6.1 with a short discussion on the results of the suggested analysis in paragraph four. This paragraph also links the reader to a more thorough discussion of this analysis in the new Appendix D. The contents of the new Appendix D are copied below:

> In this appendix, I analyze whether universities with a higher fraction of undergraduates belonging to IFC fraternities exhibit larger effects during a moratorium. Each university in the sample has a different share of its student population belonging to IFC fraternities. Recall from Table 2 that the fraction of undergraduate students with IFC membership can range from 1% to as high as 11%. Presumably, a moratorium has a greater effect on student behavior when the restrictions apply to a greater share of students.

To conduct this analysis, I supplement the preferred specification with an interaction of $InMoratorium_{u,t}$[5] and $FractionIFC_u$, where $FractionIFC_u$ is the earliest recorded count of IFC fraternity members over 2014-2019 at university u, divided by the undergraduate enrollment, and centered at its mean. I use the earliest count of IFC members for two reasons; first, to avoid the potential issue of declines in IFC membership after a moratorium due to permanent suspensions of specific IFC chapters, and second, many universities do not maintain records of IFC numbers for every year in the sample period. However, in the universities that do supply complete records, I do not find substantial semester-to-semester changes in IFC populations.[6] Therefore, an early one-year measure of the IFC population is a good approximation for the other corresponding years. In effect, the interaction of $InMoratorium_{u,t}$ and $FractionIFC_u$ creates a measure of moratorium intensity—universities with a higher fraction of IFC members receive a more intense treatment than universities with lower shares.

Table D1 provides suggestive evidence that moratoriums with a higher fraction of student enrollment belonging to an IFC fraternity result in larger decreases in alcohol offenses and sexual assaults during a moratorium period. In Panel A, the point estimates for the interaction term show patterns consistent with the main findings in the paper—the effects are negative with the strongest effects are observed on the weekends when partying is more frequent. Similarly, in Column 1 of Panel B, the interaction term coefficient shows suggestive evidence that moratoriums in universities with a higher share of IFC members exhibit larger decreases of sexual assaults. However, none of the interaction coefficients presented in either panel are significant, indicating only a suggestive relationship between the share of IFC members and the impact of moratoriums.

The results of Table D1 may appear surprisingly inconclusive given the expectation that universities with a higher share of fraternity members exhibit larger effects. One possible reason for these inconclusive results is that the share of fraternity members is a noisy indicator for a fraternity-related activity—schools with a small share of fraternity life may have chapters that are particularly active, or vice-versa. To demonstrate this, I plot each university's undergraduate IFC fraction against its Niche.com Colleges with the Best Greek Life ranking. The ranking,

[5]Note that I have changed the nomenclature of the main equation from reading $Moratorium_{u,t}$ to $InMoratorium_{u,t}$. This was done to align better with the tables/figures.

[6]West Virginia University is an exception to this. Their official IFC count decreased by over 60 percent in years following the moratorium.

based on survey responses from Niche.com users, ranges from 1-300, and 32 out of the 37 universities in the sample are ranked in the top 300. For the remaining five schools, I assign a ranking between 301-305. Figure D1 shows the inverse relationship between these two measures: as the Greek Life ranking increases, the fraction of undergraduates in an IFC fraternity generally decreases. This likely contributes to the negative point estimates in the previous analysis. However, this relationship is noisy, and the slope is not statistically different from zero at the 5% level. This may explain why the previous analysis only provided suggestive rather than clear evidence.

Last, note that the Niche.com Colleges with Best Greek Life rankings list is similar to the Niche.com Top Party Schools in America list which I analyze in Section 6.1, Table 7. Specifically, 16 of the highest 18 ranked Greek Life universities in the sample are also defined as party schools.

4.2 Major Suggestion 2: Separating Effect of Triggering Event

One interesting result is that impacts of moratoriums seem larger following a student death (Figure 8). This begs the question of whether the incident (death) changes behavior for a short time, or whether the moratorium changes behavior. I would like to see whether a death without a moratorium causes a change in alcohol and sexual assault violations. Can you test this hypothesis by considering the 64 days (avg moratorium length) after these deaths as treated? Note – this suggestion is distinct from the test conducted in Appendix C7. I want to know whether a death causes a treatment effect absent a moratorium. Your test in C7 shows a related but separate idea—that moratoriums still cause a treatment effect when compared to colleges that have deaths but no moratoriums. Given that a large portion of the observations for a school with only a death would still be considered “untreated” (days prior to the event or days after the treatment period ended) even if it had a moratorium, the test in C7 may be a weak test, and I would encourage you to use more conservative language in describing the results.

This is a very thoughtful suggestion that certainly strengthens the paper. As requested, I estimate the main specification using the 15 universities that undergo a fraternity-related death but do not undergo a moratorium. I create a binary 64-day treatment variable that starts with the day of the death for each university. The results of this test are shown in Panel C of Appendix Figures E8 and E9. Ultimately, there does not appear to be any evidence of a death-effect. I further describe this analysis in the proceeding paragraphs.

Panel C of Appendix Figures E8 and E9 show that there is no statistically significant effect when considering a 64-day period after a fraternity-related death. In Panel C of Appendix Figure E8, alcohol offenses show small and positive point estimates, while in Panel C of Appendix Figure E9, sexual assaults show negative point estimates when including all days of the week or only weekdays. This provides evidence that the moratorium, rather than the triggering event, is driving the effects reported in the main results. While moratoriums that are triggered by a fraternity-related death drive the results as discussed in Section 6.2, this may be due to students taking the moratorium more seriously.

Given the results and intuitive nature of this test, I believe that this analysis is better suited for the paper than the previous material I presented which utilized these 15 universities as never-treated control units. As a result, I have omitted the old analysis and included the new analysis in Appendix Figures E8 and E9. Moreover, Panel D of Appendix Figures E8 and E9 shows an extension of the above analysis by supplementing the 15 universities with the 14 never-treated groups that were used in a robustness check in Section 5.1. This inclusion of never-treated units is motivated by the relatively small number of clusters (15) in the initial analysis described above and is performed to increase precision. As reported in the figures, the addition of these 29 universities slightly increases precision, but the results remain consistent.

Please note that Section 6.2 has now been updated to reflect the suggestions and subsequent analysis. What follows is the updated passage from Section 6.2 paragraph 2, which discusses this test and the results:

> 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.[7] 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

[7] These universities were found using Hank Nuwer's repository of hazing-related deaths in the US: https://www.hanknuwer.com/hazing-deaths/.

period after the death instead of the $InMoratorium_{u,t}$ treatment variable. Panel C of 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 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.

4.3 Major Suggestion 3: Representativeness

> It would be helpful to place the sample in context relative to the Greek-life ecosystem to comment about generalizability. For instance, what is a typical share IFC across all 4-year US colleges and universities and how does this differ from the sample? Some small private colleges have very large Greek presence in percentage terms (e.g. Depauw University is ~70% Greek), but these schools are largely unrepresented in the sample. Would we expect results to generalize etc.?

I certainly agree that there is not enough context surrounding the sample universities and their relation to other universities in terms of Fraternity/Sorority Life. Unfortunately, a data repository of IFC fraternity populations at the university level is not available; the North American Interfraternity Conference (NIC) has informed me that they do not keep such records. Moreover, the US News Rankings of universities with the highest share of men in fraternities, has two major shortcomings. First, it only reports fraternity membership percentages for 100 universities in the US, many of which are small. Second, and more importantly, the US News Rankings does not specify whether their statistics are based on strictly IFC fraternities. As mentioned in Section 2.1, there are other types of fraternities such as academic, professional, and service fraternities. Although the statistics that I have collected for universities that are in both the list and in the sample coincide (for instance, I find Rollins College has 23% of their undergraduate male population as a member of an IFC fraternity, and the list finds 22%), I would prefer to be cautious about this source of data given its lack of clarity.[8]

As an alternative way to give context surrounding the importance of fraternities at the sample universities relative to other schools, I use the Best Greek Life Colleges in America ranking from Niche.com. These rankings are based on survey responses from Niche.com users on the quality of Fraternity/Sorority Life at their school and 300 universities nation-wide are ranked. Furthermore, this list is used in Section 5.1 of the paper as criteria for adding never-treated universities.

Figure E3 shows that the 37 universities in the sample are generally representative of schools with high-fraternity activity with the schools exhibiting a median ranking (represented by the dashed red line) of 64. In addition, 14 of the 37 universities (38%) are ranked in the top 50, while only 5 of 37 (13%) are not ranked. This figure has been added to the appendix as well as an additional sentence referring to the figure in Section 3.3 which reads, "Although IFC members represent a small number of enrolled students, the universities in the sample are representative of schools with active Fraternity and Sorority Life (see Appendix Figure E3)".

4.4 Minor Suggestion 1: Condensing Sections 3.1 and 3.2

> Sections 3.1 and 3.2 could be condensed with some of the material moved to the online appendix.

Thank you for this suggestion to add conciseness to the paper. As recommended, I have shortened Sections 3.1 and 3.2 by approximately 500 words and moved extraneous details to a newly created Appendix A. This has reduced the length of these sections from four pages to two.

[8] Please note that in the paper I report the fraction of undergraduate enrollment belonging to an IFC community rather than the fraction of undergraduate males which is why the maximum number I report in the summary statistics table is 11%.

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6 Figures

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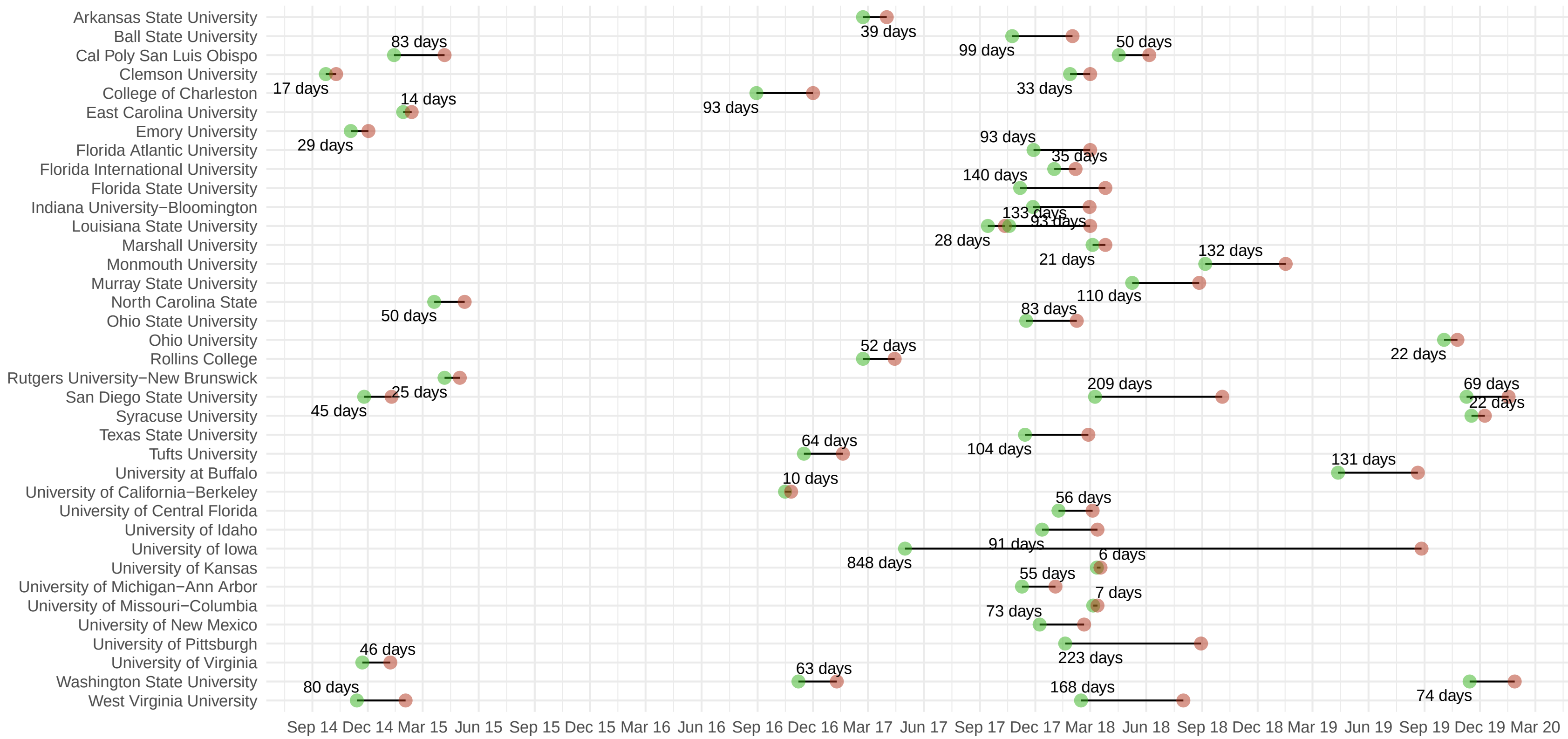

Figure 1: Distribution of Moratoriums Across the Sample Period for all Universities

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.

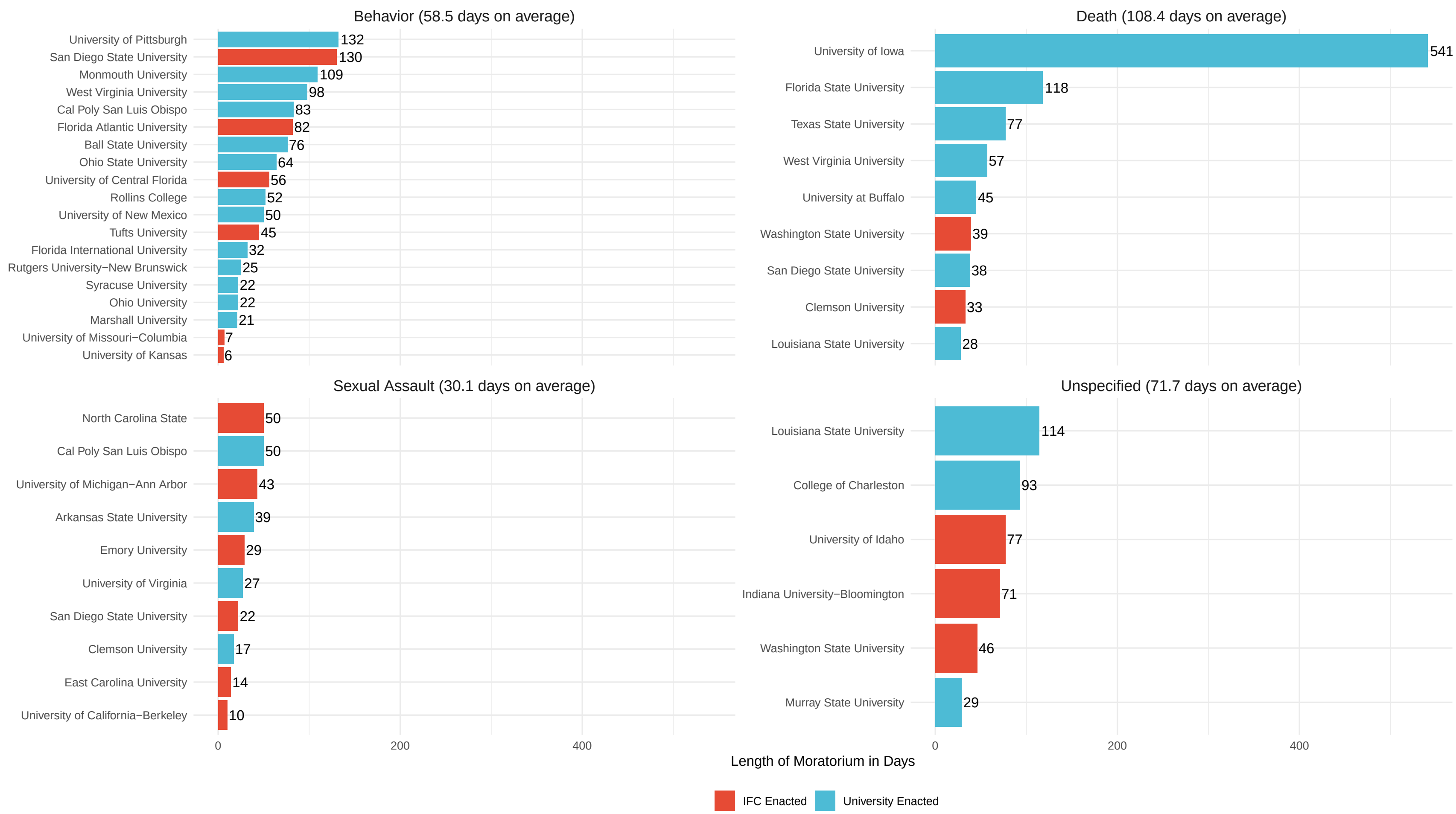

Figure 2: Number of Academic Calendar Days in Each Moratorium by Triggering Event

Note: Lengths of moratoriums represent academic calendar days. Therefore, the lengths of moratoriums differ from Figure 1. Blue shaded regions represent a moratorium that was imposed by the university, while red 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.

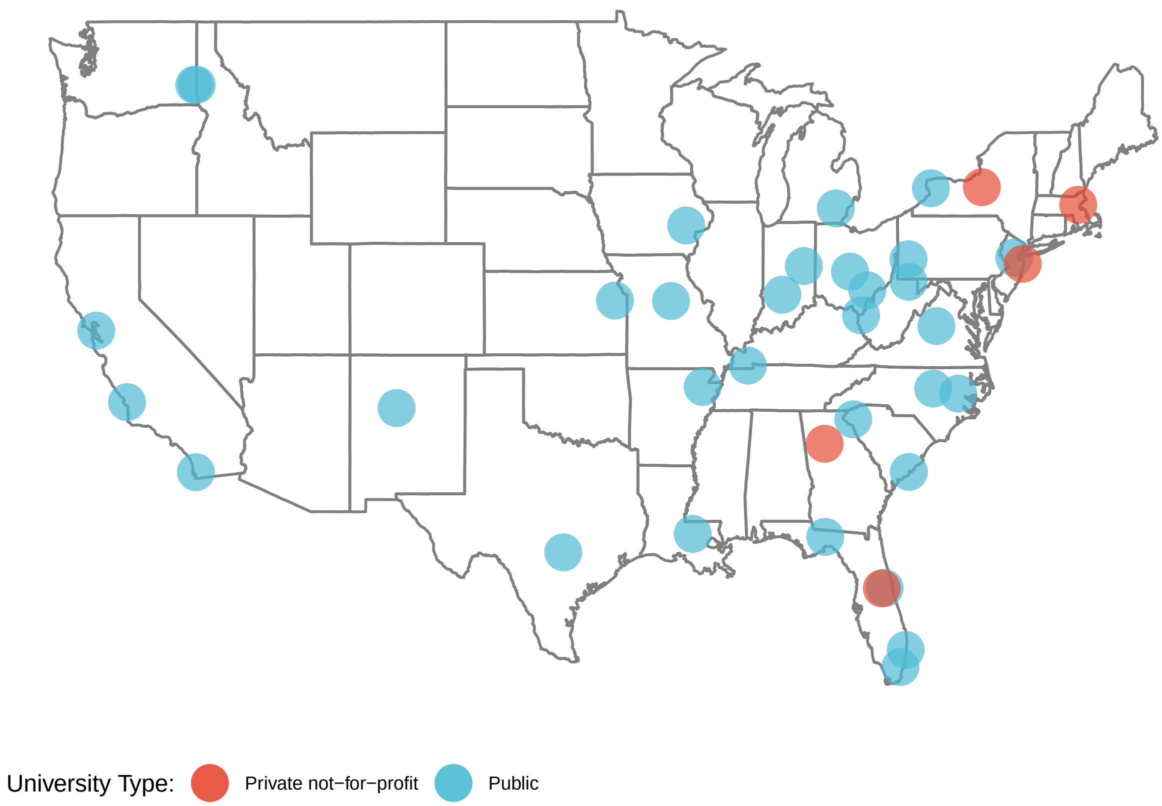

Figure 3: Locations of the Universities Included in the Sample
Note: There are a total of 37 universities in the sample, five of which are private universities. Data on both geographic location and private/public entity are obtained from the Integrated Postsecondary Education Data System (IPEDS).

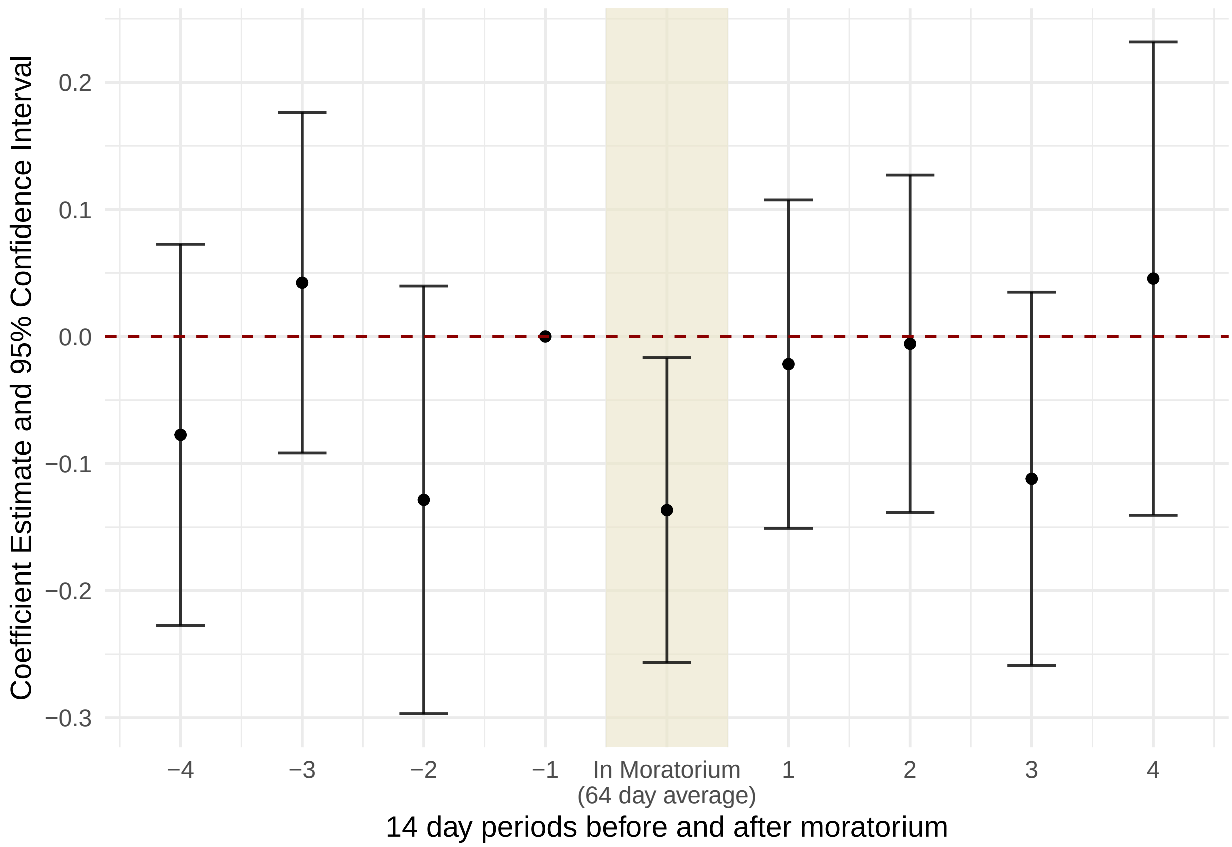

Figure 4: Event Study for Alcohol Offenses

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.

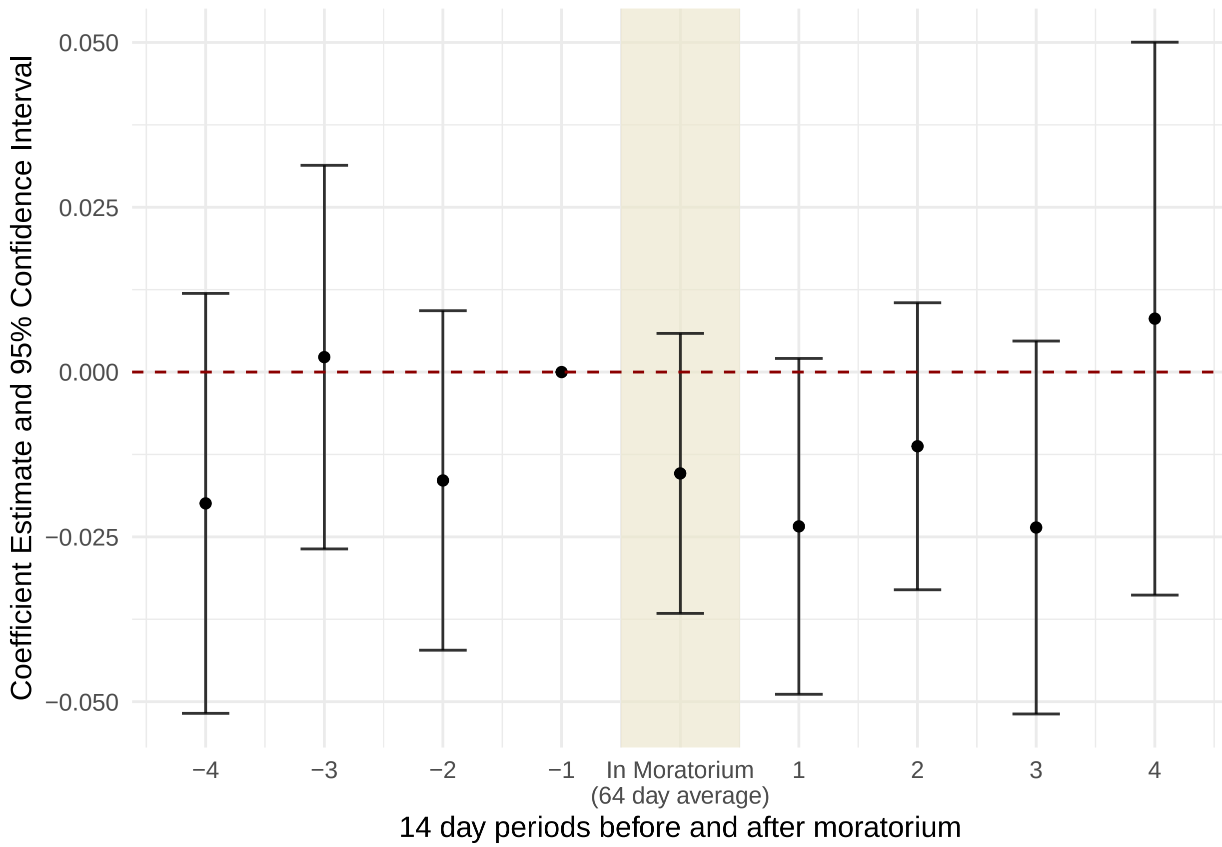

Figure 5: Event Study for Sexual Assault Offenses

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.

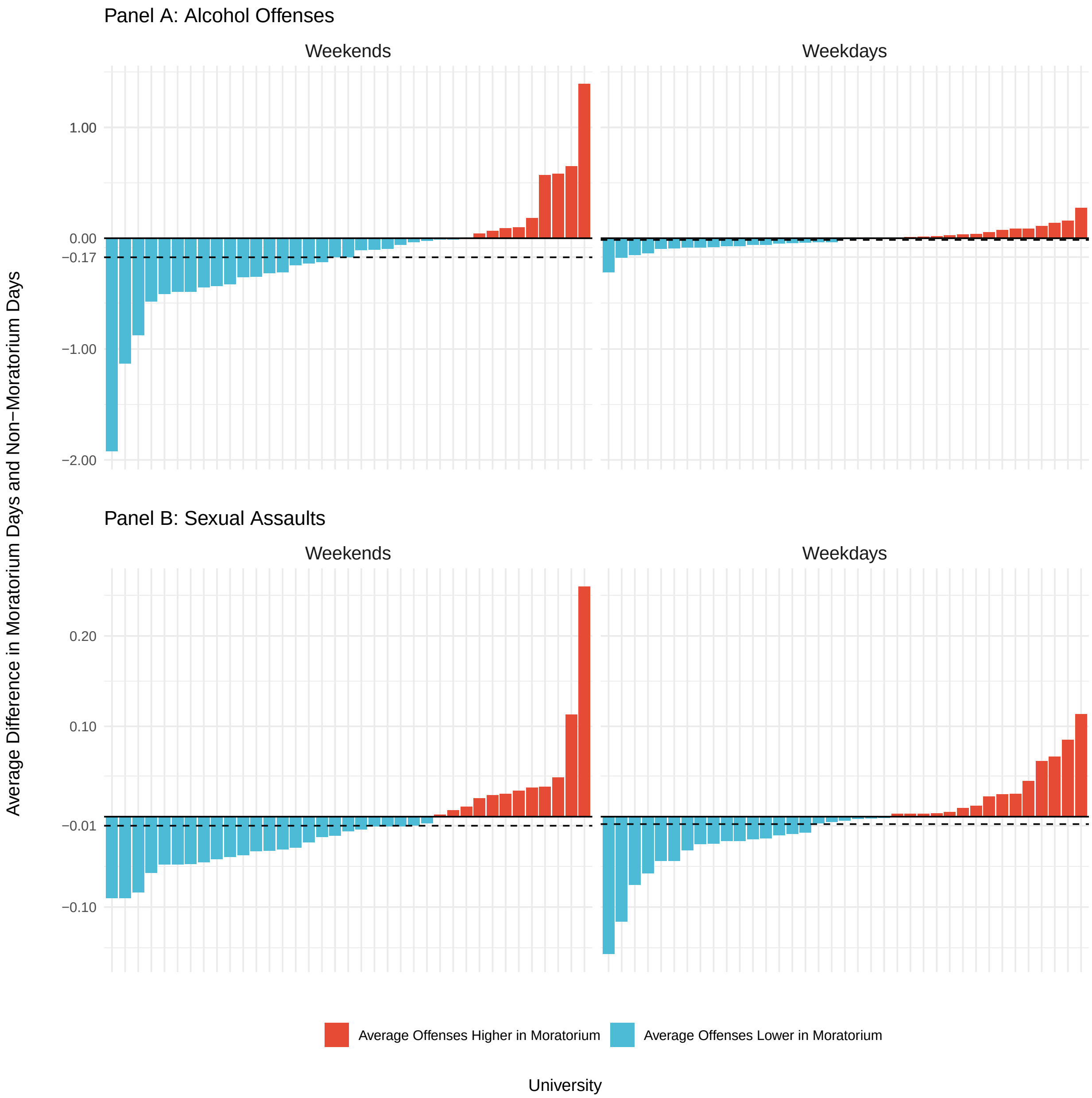

Figure 6: Difference in Average Offenses on Moratorium Days and Non-Moratorium Days
Note: The y-axis represents the average difference in offenses per-25000 enrolled students on moratorium days and non-moratorium days for each university. Negative y-axis values indicate that average offenses were lower 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.

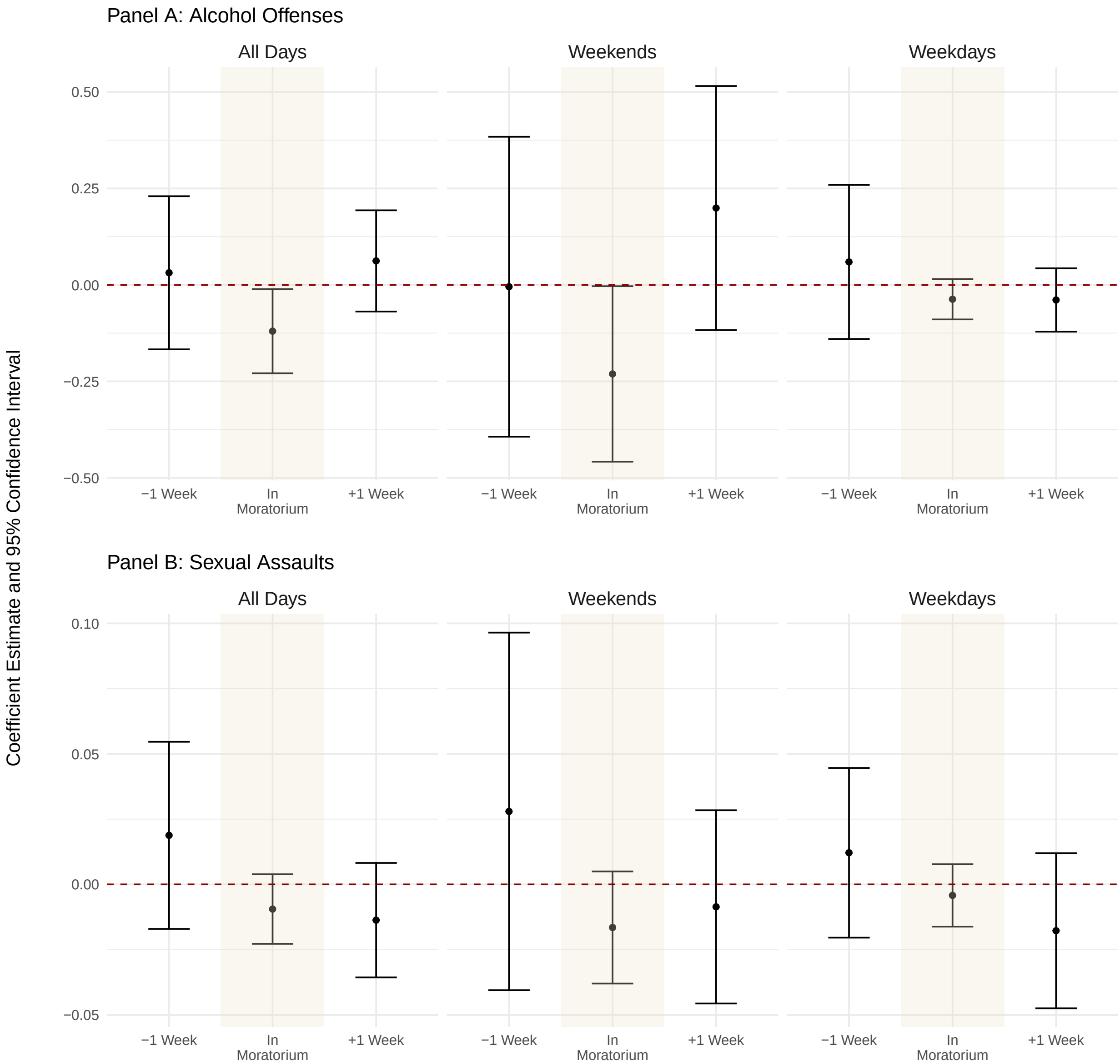

Figure 7: Coefficient Estimates Including a Week Before and Week After Indicator

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.

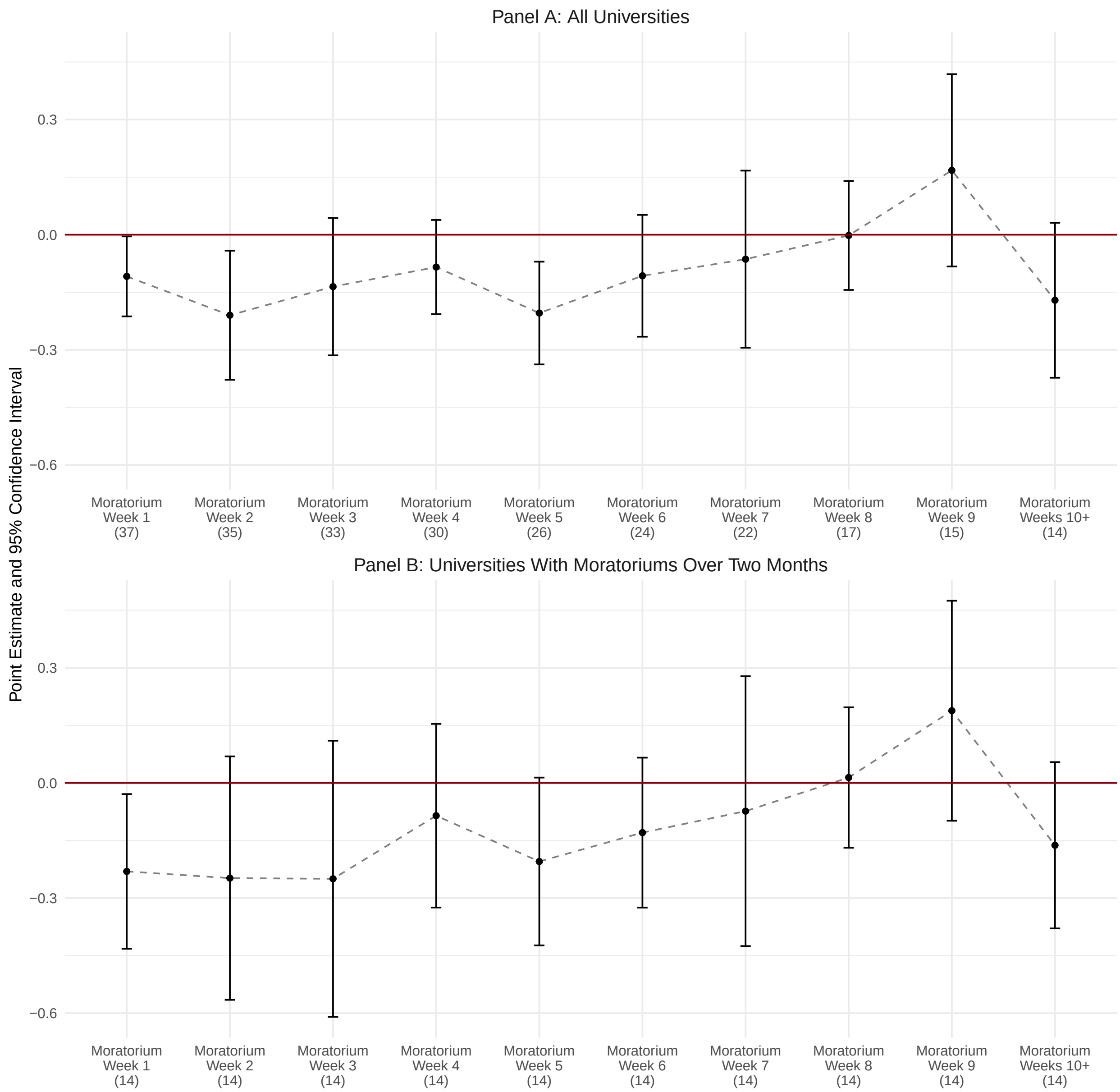

Figure 8: The Dynamics of a Moratorium (Alcohol Offenses)

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.

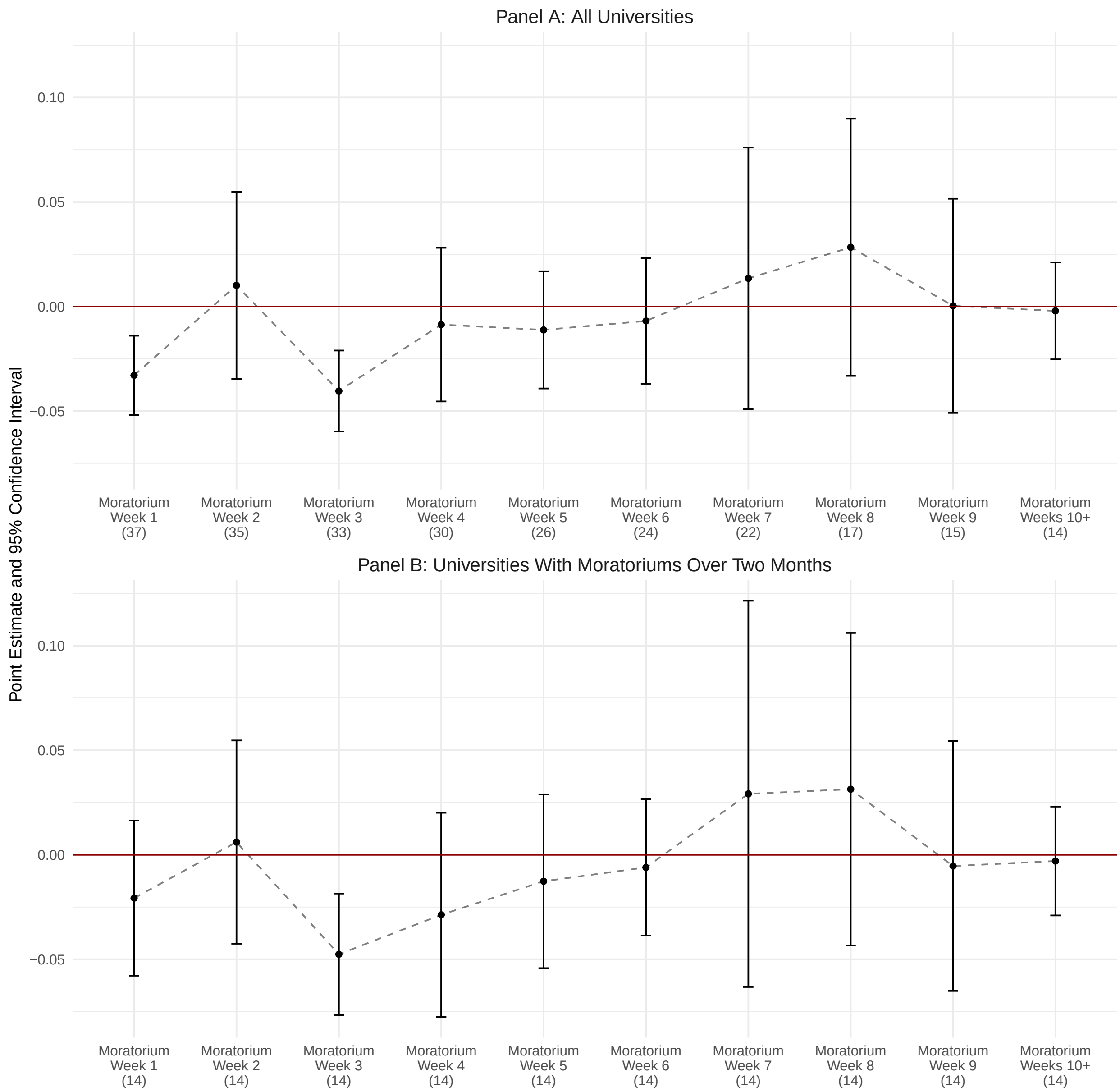

Figure 9: The Dynamics of a Moratorium (Sexual Assaults)

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.

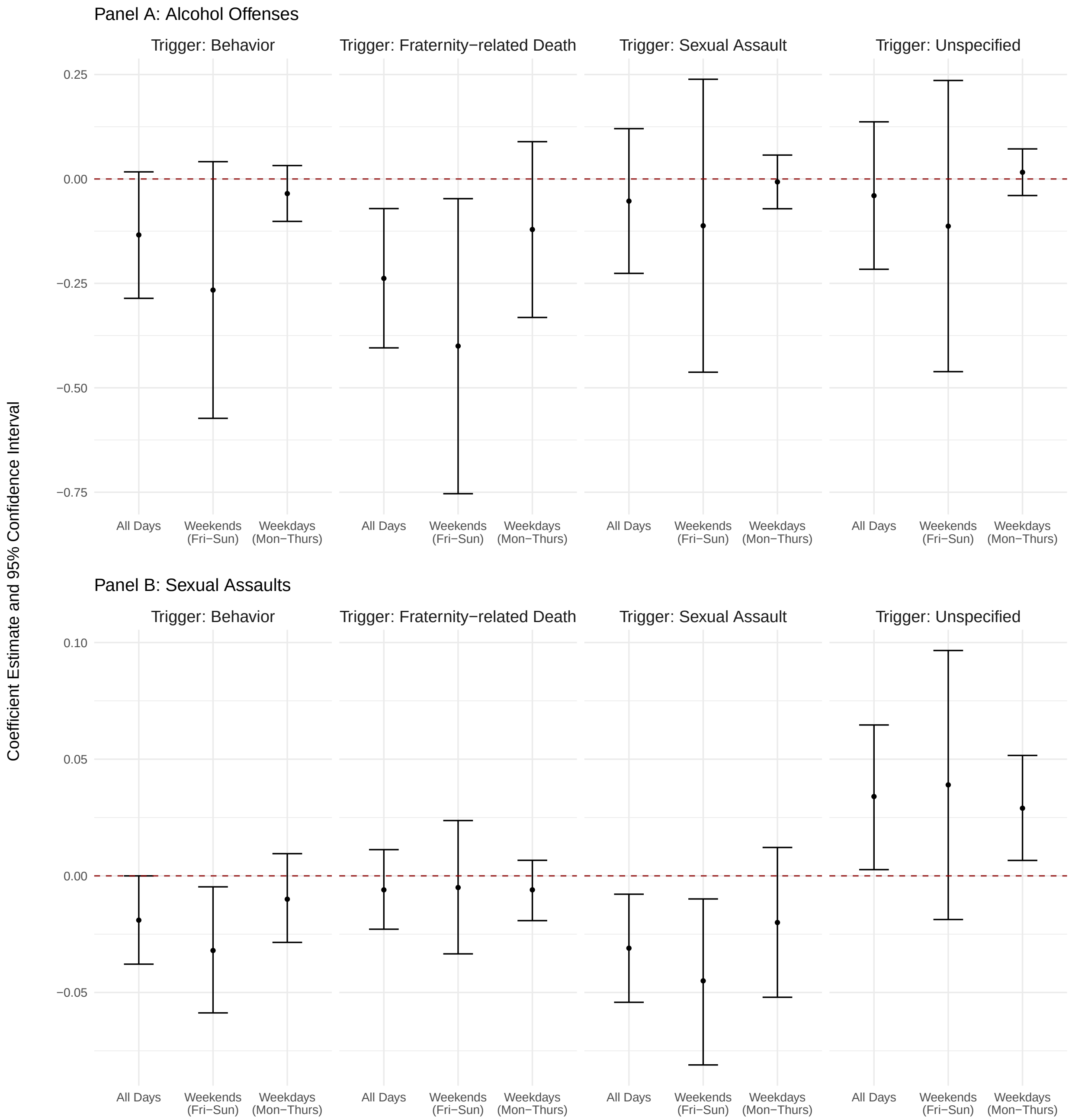

Figure 10: Heterogeneous Effects of Moratoriums by Triggering Event

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.

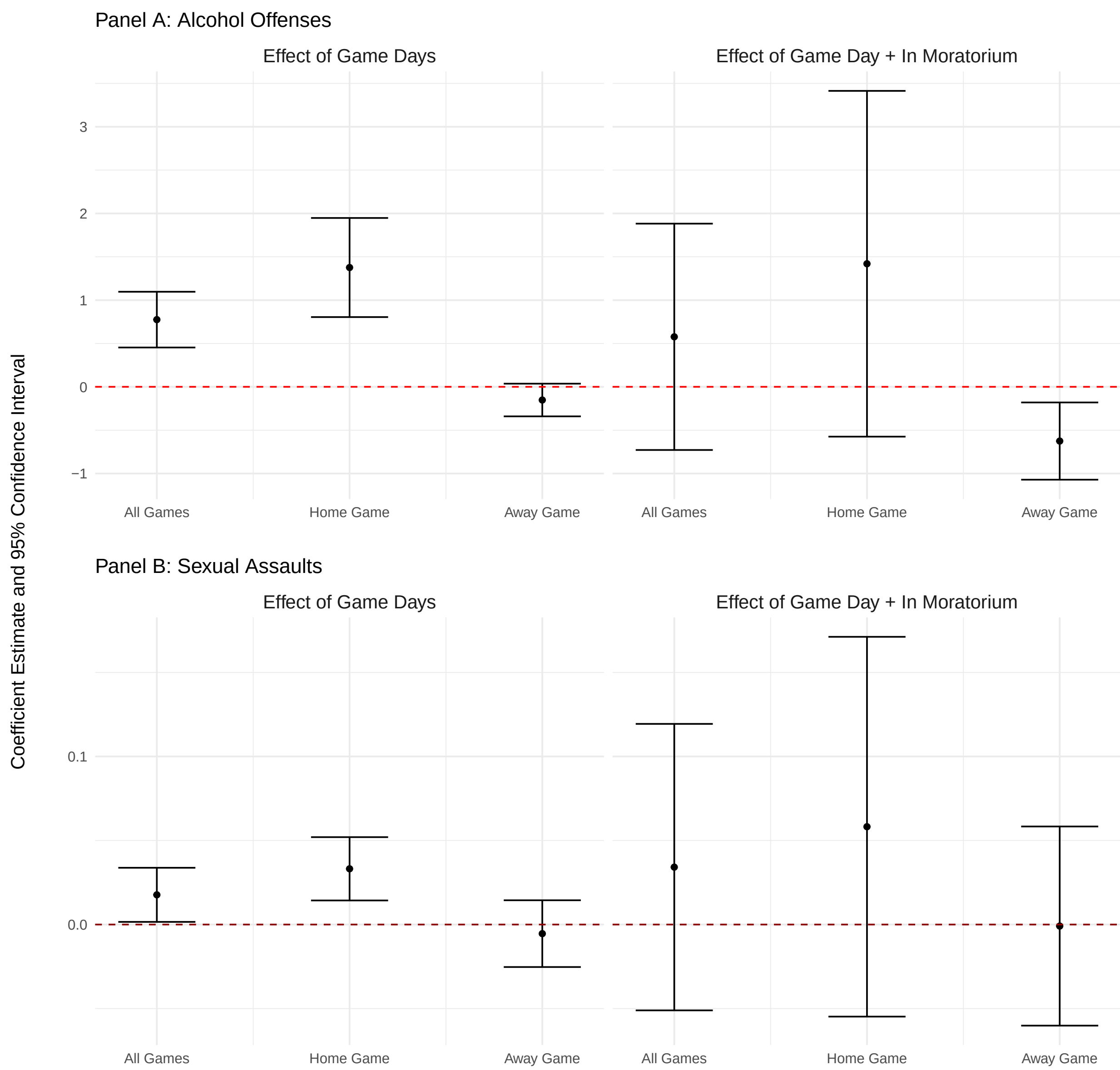

Figure 11: The Effect of Football Game-days and Football Game-days + Moratoriums
Note: Game days include all football games occurring in the sample period. 34 of the 37 universities have football teams and corresponding game days. The y-axis represents coefficient estimates. Errorbars represent 95% confidence 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.

7 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
Total Number of Universities	*37*				

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 univerisities, 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)
Panel A: Proportion of Alcohol Offenses Reported with Lag				
In Moratorium	0.002	0.000	0.000	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Observations	48026	48026	48026	48026
Mean of Dependent Variable	0.003	0.002	0.001	0.001
Panel B: Proportion of Sexual Assaults Reported with Lag				
In Moratorium	-0.001	-0.003	-0.001	0.000
	(0.004)	(0.004)	(0.004)	(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)
Panel A: Alcohol Offenses					
In Moratorium	-0.125**	-0.123**	-0.131***	-0.238**	-0.038
	(0.047)	(0.051)	(0.046)	(0.106)	(0.026)
Observations	55115	55115	55115	23643	31472
Mean of Dependent Variable	0.464	0.464	0.464	0.828	0.190
Wild Bootstrap P-Value	0.004	0.010	0.006	0.012	0.179
Panel B: Sexual Assaults					
In Moratorium	-0.009**	-0.010	-0.007	-0.017*	-0.004
	(0.004)	(0.006)	(0.006)	(0.010)	(0.006)
Observations	55115	55115	55115	23643	31472
Mean of Dependent Variable	0.049	0.049	0.049	0.058	0.042
Wild Bootstrap P-Value	0.014	0.149	0.246	0.094	0.518
FE: Holiday	X	X	X	X	X
FE: Game Day	X	X	X	X	X
FE: Semester (Spring/Fall)	X	X		X	X
FE: University	X				
FE: Academic Year	X				
FE: University by Academic Year		X		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	Weekends	Weekdays	All Days	Weekends	Weekdays
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Alcohol Offenses						
In Moratorium	-0.156	-0.201	-0.126	-0.320*	-0.714**	-0.029
	(0.130)	(0.206)	(0.114)	(0.141)	(0.290)	(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
Panel B: Sexual Assaults						
In Moratorium	-0.025	-0.011	-0.035	-0.003	-0.013	0.004
	(0.016)	(0.017)	(0.021)	(0.017)	(0.029)	(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)
Panel A: Full Sample		
Estimates from Figures 4 and 5		
In Moratorium	-0.137**	-0.015
	(0.059)	(0.010)
Observations	55115	55115
F-test P-value of Lags	0.158	0.102
Panel B: Quantiles by Moratorium Length		
Moratorium Length: 1st Quantile		
In Moratorium	0.062	-0.015
	(0.036)	(0.021)
Observations	22503	22503
F-test P-value of Lags	0.459	0.070
Moratorium Length: 2nd Quantile		
In Moratorium	-0.238**	-0.021
	(0.097)	(0.012)
Observations	19241	19241
F-test P-value of Lags	0.552	0.408
Moratorium Length: 3rd Quantile		
In Moratorium	-0.128	-0.007
	(0.087)	(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)
Panel A: Alcohol Offenses			
In Moratorium	-0.123**	-0.223**	-0.053
	(0.051)	(0.101)	(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
Panel B: Sexual Assaults			
In Moratorium	-0.010	-0.008	-0.011
	(0.006)	(0.007)	(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)
Panel A: University-Enacted Moratoriums			
Alcohol Offense			
In Moratorium	-0.132*	-0.252*	-0.041
	(0.065)	(0.136)	(0.035)
Observations	55115	23643	31472
Sexual Assault			
In Moratorium	-0.010	-0.019	-0.003
	(0.008)	(0.013)	(0.007)
Observations	55115	23643	31472
Panel B: IFC-Enacted Moratoriums			
Alcohol Offense			
In Moratorium	-0.101	-0.197	-0.030
	(0.082)	(0.166)	(0.026)
Observations	55115	23643	31472
Sexual Assault			
In Moratorium	-0.010	-0.014	-0.007
	(0.010)	(0.010)	(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 (i.e., Column 2 in the main results table with day of week, holiday, semester, football game-day, and universitiy 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.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix

A Details on Data Collection

In this appendix, I cover minute details of the data collection. First, a list of universities that underwent moratoriums but are not included in the sample are explained. Second, the small portion of missing data in the Daily Crime Logs is discussed.

A.1 Sample Selection Details

Recall from Section 3.1 that the main sample includes 37 universities which experienced moratoriums between 2014 and 2019. However, these do not represent the universe of fraternity moratoriums that occur in this time period. In particular, there are six schools that are known to have experienced a moratorium in this time frame, but are excluded due to data issues or their definition of a moratorium. First, Miami University is excluded since the end-date of their moratorium could not be verified. Second, Pennsylvania State University is excluded because they did not digitally release their Daily Crime Logs. Third, the University of Texas at Arlington is excluded because the crime logs are scanned images that cannot be read reliably by any computer software. Fourth, Cal State Northridge is excluded because it is unclear whether the moratorium includes a ban on alcohol. Fifth, the University of North Florida is excluded because of a discrepancy between public records information and newspaper articles—newspaper articles claim there is a moratorium beginning 12/4/17, but the public records department claims this is untrue. Last, the University of Vermont is excluded due to issues with the reliably of the data—crimes often are reported to have occurred in large intervals of days (or months) for nearly 40% of the data provided which is not suitable for the daily-level analysis in this paper. There may exist other universities that experienced a moratorium without news coverage—these are also excluded from the sample.

A.2 Daily Crime Log Details

As outlined in Section 3.1, the Daily Crime Logs are mandated under the Clery Act to include a set of characteristics for each crime and to be maintained for seven years. Despite these mandates, there are exceptions to each of these. First, while the date occurred is mandated to be included in the Daily Crime Logs, only 32 of the 37 universities' crime logs contain the date occurred. However, these five schools contain the date reported, and therefore, I use the date reported in lieu of the date occurred when the date occurred is missing. Second, the seven-year record mandate is not interpreted uniformly across universities. In particular, if Daily Crime Logs from 2014 are requested in year 2021, the police departments of Rollins College and North Carolina State University consider seven-years to be inclusive of their current year, and hence, only retain records from 2015-2021 or have only partially completed records in 2014 respectively.

B Robustness Under TWFE

In this appendix, I estimate a model that contains no negative weights to acknowledge the potential issues with the difference-in-differences estimator as discussed in Section 4.3. These weights are calculated using the TwoWayFEWeights package (Chaisemartin, D'Haultfoeuille, and Deeb 2020). The estimated model is the following TWFE specification:

$$Y_{ut} = \beta Moratorium_{ut} + \gamma_u + \alpha_t + \epsilon_{ut}$$

where Y_{ut} is the outcome for university u at time t measured by per-25000 enrolled students per academic-calendar day, $Moratorium_{ut}$ is an indicator equal to one if university u is in a moratorium at time t, γ_u are university fixed effects, α_t are day by month by year fixed effects, and ϵ_{ut} is the error term. Hence, this model compares academic-calendar days within a moratorium to the same calendar days without a moratorium while controlling for systematic differences between universities. As mentioned above, there are no negative weights in this specification and therefore sign reversal is impossible. With this advantage, I re-estimate the results in Columns 2, 3, and 5 in Table 4.

Table B1 shows that the results of the TWFE specification with no negative weights are mostly consistent with the results in Table 4. In Panel A, alcohol offenses exhibit a 19% decrease from the mean during a moratorium, with a 25% decrease on the weekends. Although sexual assaults do not exhibit statistically significant decreases on the weekends, this is potentially due to the loss of identifying variation from the data-intensive controls. However, it is important to note that the coefficient sign remains the same on all of the estimates. Hence, under the identifying assumptions of the model, moratoriums decrease alcohol offenses.

Table B1: Effect of Moratoriums on Alcohol Offenses and Sexual Assault by Weekend/Weekdays (No Negative Weights-OLS)

	Days of the Week		
	All Days (1)	Weekends (2)	Weekdays (3)
Panel A: Alcohol Offenses			
In Moratorium	-0.091*	-0.211**	-0.004
	(0.045)	(0.097)	(0.017)
Observations	55115	23643	31472
Mean of Dependent Variable	0.464	0.828	0.190
Panel B: Sexual Assaults			
In Moratorium	-0.006	-0.008	-0.004
	(0.005)	(0.007)	(0.007)
Observations	55115	23643	31472
Mean of Dependent Variable	0.049	0.058	0.042
FE: University	X	X	X
FE: Day by Month by Year	X	X	X

Note:
Standard errors are clustered by university and each offense is defined as per-25000 enrolled students. Column 1 represents the preferred specification from the main results table, Column 2. Weekends consist of Fridays, Saturdays, and Sundays. Weekdays consist of Monday through Thursday. The specification used in this table has no negative weights and thus, sign reversal is impossible.
* p < 0.1, ** p < 0.05, *** p < 0.01

C Spillover Analysis Using CSS Data

In this appendix, I use the Campus Safety and Security (CSS) data to indirectly analyze whether alcohol offenses and sexual assaults are being displaced to riskier areas during a moratorium. I compare the yearly aggregation of the Daily Crime Logs to the CSS data using a model that is less suited for a causal analysis due to the yearly aggregation of the CSS data. Therefore, the estimates in this appendix should be taken as speculative only.

C.1 CSS Data and Empirical Strategy

The CSS data is maintained by the US Department of Education. This data is mandated by the federal government to be updated each calendar year with the yearly totals of liquor law disciplinary actions and arrests, and sexual assault violations that are reported *to any entity* at a university. Hence, this data will not match one-to-one with the Daily Crime Logs as the Daily Crime Logs contain only incidents *reported to or by the university police.* For instance, if a residence hall administrator issues a liquor violation to an underage student, but handles the issue internally without involving the police, then this would be included in the CSS data as a liquor law disciplinary action, but not the Daily Crime Logs. However, the advantage of the CSS data is that it contains counts of offenses that occur on-campus, not-on-campus, and on public property.[9] Most importantly, I am able to delineate whether incidents occur in student residence halls.

Since the CSS data is aggregated by calendar-year, the CSS data is not a preferred data source for causal analysis. In spite of this shortcoming, I estimate the following difference-in-differences specification:

$$Y_{u,t} = \beta Moratorium_{u,t} + \gamma_u + \lambda_t + \epsilon_{u,t} \tag{C0}$$

where $Y_{u,t}$ is the offense of interest defined as per-25000 enrolled students per-calendar-year, $Moratorium_{u,t}$ is the *number* of calendar-days with a moratorium within a year, γ_u are university fixed effects, λ_t are calendar-year fixed effects, and $\epsilon_{u,t}$ is the error term. Standard errors are clustered at the university level to account for serial correlation within universities.

[9]Not-on-campus is defined by the Department of Education as "(1) Any building or property owned or controlled by a student organization that is officially recognized by the institution; or (2) Any building or property owned or controlled by an institution that is used in direct support of, or in relation to, the institution's educational purposes, is frequently used by students, and is not within the same reasonably contiguous geographic area of the institution." Furthermore, public property is defined as "All public property, including thoroughfares, streets, sidewalks, and parking facilities, that is within the campus, or immediately adjacent to and accessible from the campus."

C.2 Results

Table C1 shows the comparison of estimating Equation C0 with the Daily Crime Logs aggregated to the calendar-year level with the CSS data.[10] The Daily Crime Logs show relatively consistent results with those found in Table 4; yearly averages of alcohol offenses per-25,000 enrolled students decrease by approximately 0.134 per additional calendar day with a moratorium and sexual assaults decrease by approximately 0.013.

Although the results using aggregated Daily Crime Logs are consistent with the findings in Table 4, the CSS data shows that residence halls experience a 0.270 *increase* in yearly liquor law disciplinary violations per-25,000 enrolled students and a 0.033 *decrease* in reports of sexual assault for each additional calendar-year-day with a moratorium (Column 3). Each of these estimates are significant at the 5% level. However, there is little evidence of an effect on liquor law arrests as shown in Columns 4 and 5—consistent with the literature that campus police do not typically arrest students for alcohol violations (Bernat et al. 2014). As discussed in Section 5.2, this supports the notion that if moratoriums displace alcohol-fueled behavior, they displace it to *less* risky areas whereby behavior can more easily be intervened before it becomes dangerous.

[10] This aggregation includes all calendar-year days rather than only academic-calendar days that were used in the main analysis.

Table C1: Effect of Moratoriums on Alcohol Offenses and Sexual Assaults: Comparison of Daily Crime Logs and Campus Safety and Security (OLS)

		Campus Safety and Security			
	Daily Crime Logs	Displinary Actions/Reported Crime		Arrests	
	All Reports	All Reports	Residence Halls	All Reports	Residence Halls
	(1)	(2)	(3)	(4)	(5)
Panel A: Alcohol Offenses					
In Moratorium	-0.134*	0.297**	0.270**	-0.022	-0.025
	(0.077)	(0.118)	(0.125)	(0.056)	(0.040)
Mean of Dependent Variable	131.861	362.978	343.616	55.961	24.280
Observations	220	222	222	222	222
FE: Year	X	X	X	X	X
FE: University	X	X	X	X	X
Panel B: Sexual Assaults					
In Moratorium	-0.013	-0.046	-0.033**		
	(0.011)	(0.039)	(0.014)		
Mean of Dependent Variable	14.099	28.732	14.444		
Observations	220	222	222		
FE: Year	X	X	X		
FE: University	X	X	X		

Note:
Standard errors are clustered by university and each offense is defined as offense per-25000 enrolled students per-calendar year. Recall that Daily Crime Logs are the primary source of data used in prior analysis. In this model, the In Moratorium treatment variable is defined as the number of calendar-days that experienced a moratorium in a calendar-year. All Reports columns include the entire Daily Crime Logs/Campus Safety and Security Data (CSS), while Residence Halls is a subset of the CSS. All Reports in the CSS data contains both off-campus and on-campus reports. CSS data does not necessarily need to be reported to the university police and hence, may not show up in the Daily Crime Logs. Columns 2 and 3 refer to disciplinary actions for liquor law violations and reported crime for sexual assaults. Columns 4 and 5 refer to arrests for liquor law violations. Fixed effects include university and year fixed effects.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D Is the Share of Students in a Fraternity Important for Effectiveness?

In this appendix, I analyze whether universities with a higher fraction of undergraduates belonging to IFC fraternities exhibit larger effects during a moratorium. Each university in the sample has a different share of its student population belonging to IFC fraternities. Recall from Table 2 that the fraction of undergraduate students with IFC membership can range from 1% to as high as 11%. Presumably, a moratorium has a greater effect on student behavior when the restrictions apply to a greater share of students.

To conduct this analysis, I supplement the preferred specification with an interaction of $InMoratorium_{u,t}$ and $FractionIFC_u$, where $FractionIFC_u$ is the earliest recorded count of IFC fraternity members over 2014-2019 at university u, divided by the undergraduate enrollment, and centered at its mean. I use the earliest count of IFC members for two reasons; first, to avoid the potential issue of declines in IFC membership after a moratorium due to permanent suspensions of specific IFC chapters, and second, many universities do not maintain records of IFC numbers for every year in the sample period. However, in the universities that do supply complete records, I do not find substantial semester-to-semester changes in IFC populations.[11] Therefore, an early one-year measure of the IFC population is a good approximation for the other corresponding years. In effect, the interaction of $InMoratorium_{u,t}$ and $FractionIFC_u$ creates a measure of moratorium intensity—universities with a higher fraction of IFC members receive a more intense treatment than universities with lower shares.

Table D1 provides suggestive evidence that moratoriums with a higher fraction of student enrollment belonging to an IFC fraternity result in larger decreases in alcohol offenses and sexual assaults during a moratorium period. In Panel A, the point estimates for the interaction term show patterns consistent with the main findings in the paper—the effects are negative with the strongest effects are observed on the weekends when partying is more frequent. Similarly, in Column 1 of Panel B, the interaction term coefficient shows suggestive evidence that moratoriums in universities with a higher share of IFC members exhibit larger decreases of sexual assaults. However, none of the interaction coefficients presented in either panel are significant, indicating only a suggestive relationship between the share of IFC members and the impact of moratoriums.

The results of Table D1 may appear surprisingly inconclusive given the expectation that universities with a higher share of fraternity members exhibit larger effects. One possible reason for these inconclusive results is that the share of fraternity members is a noisy in-

[11] West Virginia University is an exception to this. Their official IFC count decreased by over 60 percent in years following the moratorium.

dicator for a fraternity-related activity—schools with a small share of fraternity life may have chapters that are particularly active, or vice-versa. To demonstrate this, I plot each university's undergraduate IFC fraction against its Niche.com Colleges with the Best Greek Life ranking. The ranking, based on survey responses from Niche.com users, ranges from 1-300, and 32 out of the 37 universities in the sample are ranked in the top 300. For the remaining five schools, I assign a ranking between 301-305. Figure D1 shows the inverse relationship between these two measures: as the Greek Life ranking increases, the fraction of undergraduates in an IFC fraternity generally decreases. This likely contributes to the negative point estimates in the previous analysis. However, this relationship is noisy, and the slope is not statistically different from zero at the 5% level. This may explain why the previous analysis only provided suggestive rather than clear evidence.

Table D1: The Effect of Moratoriums Interacted with the Centered IFC Share (OLS)

	All Days	Weekends	Weekdays
	(1)	(2)	(3)
Panel A: Alcohol Offenses			
In Moratorium	-0.124**	-0.239**	-0.038
	(0.051)	(0.107)	(0.026)
In Moratorium x Fraction IFC	-0.231	-0.729	-0.209
	(1.402)	(2.629)	(0.733)
Mean of Dependent Variable	0.464	0.828	0.190
Observations	55115	23643	31472
Panel B: Sexual Assaults			
In Moratorium	-0.010	-0.017	-0.004
	(0.007)	(0.010)	(0.006)
In Moratorium x Fraction IFC	-0.068	0.164	-0.242
	(0.235)	(0.304)	(0.234)
Mean of Dependent Variable	0.049	0.058	0.042
Observations	55115	23643	31472
FE: Day of Week	X	X	X
FE: Holiday	X	X	X
FE: Game Day	X	X	X
FE: Semester (Spring/Fall)	X	X	X
FE: University by Academic Year	X	X	X

Note:

Fraction IFC is the average share of undergraduates that are in an IFC fraternity, centered at the mean. Note that not every university keeps record of their IFC numbers over time, and therefore, the most recent number of IFC members is used in this calculation when sample-period data is missing. However, based on the few universities that provided year-to-year data on their IFC populations, the total number does not substantially change over time. Standard errors shown in parenthesis are clustered by university (37 clusters) and each offense is defined as per-25000 enrolled students. The interaction of In Moratorium and Fraction IFC gives a measure of moratorium intensity based on the fraction of IFC members. The regression specification is the preferred specification which includes day of week, holiday, football game-day, semester, and university-by-acacdemic-year fixed effects.

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

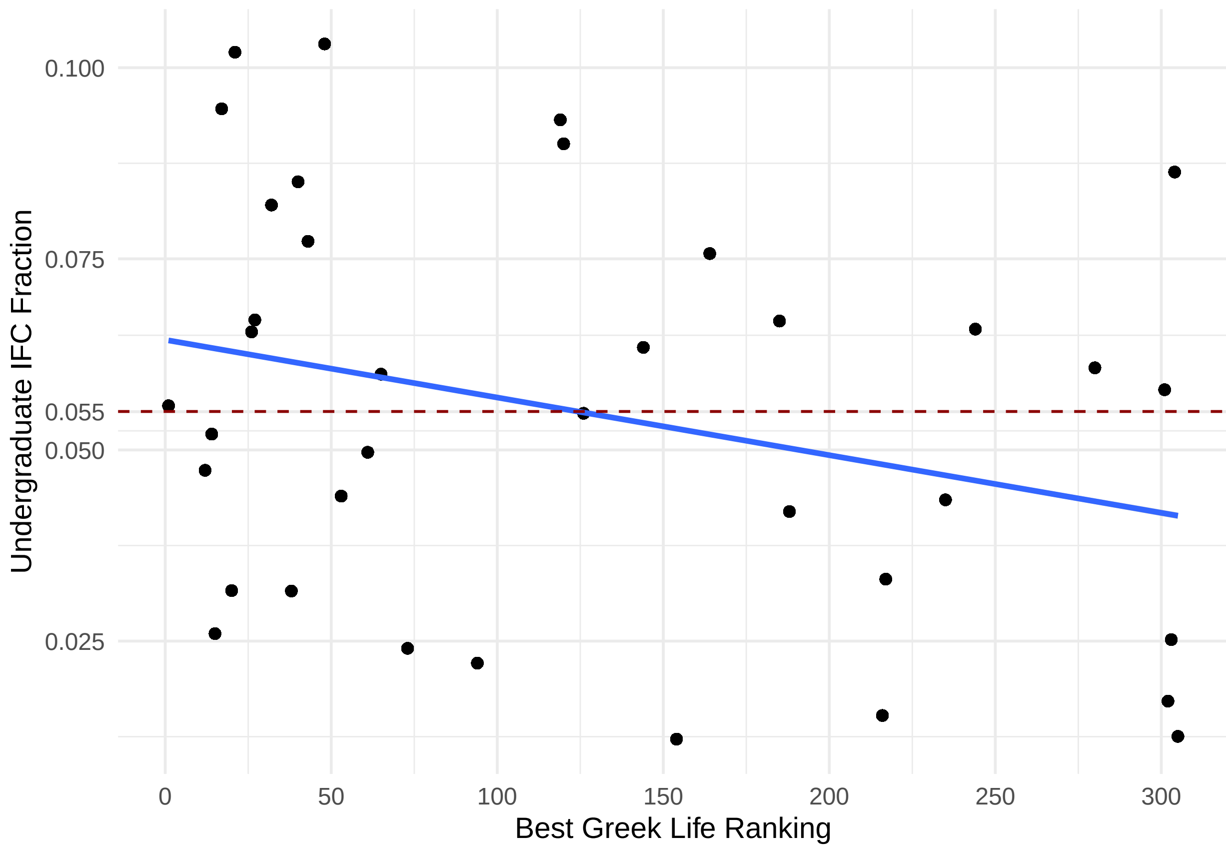

Figure D1: Scatterplot of Best Greek Life Ranking and IFC Fraction

Note: The x-axis represents the ranking from Niche.com's Colleges with the Best Greek Life list. There are 300 rankings within this list. Of the four universities that are not ranked, a ranking between 301 and 305 is assigned. The y-axis represents the share of undergraduate students that are a member of an IFC fraternity. The dashed red line denotes the average share of undergraduate students that are in an IFC fraternity, while the blue line represents the regression estimation of the share of undergraduate students on the Colleges with the Best Greek Life ranking. Note that, at the five percent level, the slope of the regression line is not statistically different from zero.

E Appendix Figures and Tables

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Indiana University, Bloomington
Police Department
Student Right To Know CAD Daily Log

From Jan 20, 2014 to Jan 20, 2014.

Date Reported: 01/20/14 - MON at 12:22 **Location :** EIGENMANN HALL **Event #:** 14-01-20-001434
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 17:03 **Location :** ALL OTHER ROADWAYS/INTERS **Event #:** 14-01-20-001446
Date and Time Occurred From - Occurred To 01/20/14 - MON at 17:02 - 01/20/14 - MON at 17:03
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:** 140154
Disposition: CLOSED BY ARREST

Date Reported: 01/20/14 - MON at 19:30 **Location :** EIGENMANN HALL **Event #:** 14-01-20-001464
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 20:22 **Location :** EIGENMANN HALL **Event #:** 14-01-20-001466
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 20:45 **Location :** FOSTER HARPER HALL **Event #:** 14-01-20-001468
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 21:38 **Location :** ALL OTHER NON-UNIVERSITY **Event #:** 14-01-20-001476
Date and Time Occurred From - Occurred To
Incident : ALL OTHER OFFENSES - HARASSMENT/INTIMIDATION **Report #:**
Disposition: NO CASE REPORT

Date Reported: 01/20/14 - MON at 21:53 **Location :** ROSE AVE RESIDENCE HALL **Event #:** 14-01-20-001479
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 22:30 **Location :** COLLINS COMMON AREA **Event #:** 14-01-20-001486
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 23:02 **Location :** FOREST QUAD **Event #:** 14-01-20-001487
Date and Time Occurred From - Occurred To 01/20/14 - MON at 22:45 - 01/20/14 - MON at 23:02
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:** 140157
Disposition: CLOSED NO ARREST.

Date Reported: 01/20/14 - MON at 23:07 **Location :** FOSTER JENKINSON HALL **Event #:** 14-01-20-001491
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 23:35 **Location :** ALL OTHER OPEN AREAS **Event #:** 14-01-20-001494
Date and Time Occurred From - Occurred To 01/20/14 - MON at 23:35 - 01/20/14 - MON at 23:41
Incident : ASSAULT - OTHER ASSAULTS - SIMPLE, NOT AGGRAVATED **Report #:** 140159
Disposition: CLOSED BY ARREST.

11 Incidents Listed.

Print Date and Time 1/21/2014 12:23:52PM at Page No. 1

Figure E1: An Example of a Daily Crime Log

Note: The main analysis uses data from 37 universities' Daily Crime Logs—each unique in their own respect. All Daily Crime Logs are collected from each university and harmonized using the pattern matching technique outlined in Section 3.

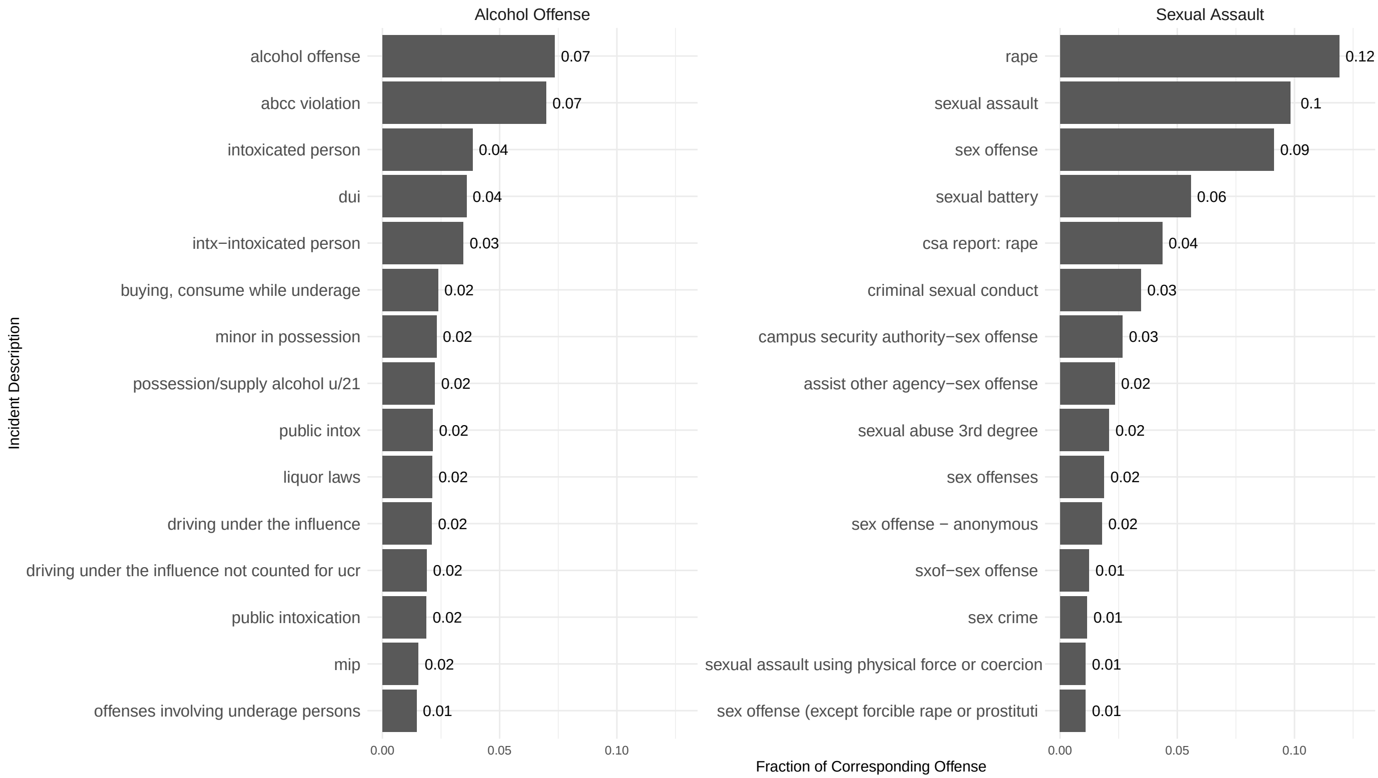

Figure E2: Top 15 Most Frequent Offense Matches

Note: The top 15 most frequent offense matches represent the 15 most frequent incidents after the pattern matching exercise. The x-axis represents the fraction of the total number of offenses in each category.

Table E1: Description of the Triggering Events that lead to a Moratorium

University	Description of Triggering Event	Triggering Event Date	Moratorium Start Date	Classification
Arkansas State University-Main Campus	Arrest of a man suspected of raping a 19-year old woman at a party in a fraternity house.	2017-02-10	2017-02-21	Sexual Assault
Ball State University	Concerns regarding the behavior and actions of members of IFC fraternities.		2017-10-24	Behavior
Cal Poly San Luis Obispo	A report of a sexual assault that allegedly took place at a social event hosted by a Greek group.		2015-01-13	Sexual Assault
Cal Poly San Luis Obispo	Racially insensitive photos surfacing on social media featuring fraternity members in both blackface and gang-related images.	2018-04-08	2018-04-17	Behavior
Clemson University	Alleged sexual assault.	2018-01-27	2018-01-29	Sexual Assault
College of Charleston	Decision was made after consulting with student leaders within the community.		2016-08-30	Unspecified
East Carolina University	An alleged sexual assault on Jan. 25 that provoked an ongoing investigation with the Greenville Police Department.	2015-01-25	2015-01-28	Sexual Assault
Emory University	Report of a sexual assault in a fraternity house.	2014-11-02	2014-11-03	Sexual Assault
Florida Atlantic University	Tailgating issues involving alcohol.		2017-11-28	Behavior
Florida International University	Growing concerns about the state of fraternity and sorority life at FIU as well as around the nation.		2018-01-01	Unspecified
Florida State University	Death of Andrew Coffey.	2017-11-03	2017-11-06	Death
Indiana University-Bloomington	A university spokesperson said the decision came in light of the ongoing national conversation about Greek life and its place on college campuses, as well as challenges on IU's Bloomington campus. The decision is not attributable to one particular incident.		2017-11-27	Unspecified
Louisiana State University	Death of Maxwell Gruver.	2017-09-14	2017-09-14	Death
Louisiana State University	Unclear.		2017-10-19	Unspecified
Marshall University	High-risk behavior in the fraternity community.		2018-03-05	Behavior
Monmouth University	Troubles within the fraternity system.		2018-09-06	Behavior
Murray State University	The letter implementing the suspension indicates that "national trends, and our own review...".		2018-08-27	Unspecified
North Carolina State University at Raleigh	Surfaced newstory of a pledge book that featured racially insensitive remarks and rape jokes.	2018-03-20	2018-03-20	Sexual Assault
Ohio State University-Main Campus	Proactive step based on the significantly high number of investigations this semester, not on the nature of any specific case or cases.		2017-11-16	Behavior
Ohio University-Main Campus	Allegations within the past week of hazing at seven of the fraternities.		2019-10-03	Behavior
Rollins College	The temporary suspension was issued after reviewing a 'series of student conduct concerns.'		2017-02-21	Behavior
Rutgers University-New Brunswick	Several incidents with alcohol .		2015-04-06	Behavior
San Diego State University	Sexual assault allegations.		2014-11-25	Sexual Assault
San Diego State University	Ongoing concerns related to alcohol.		2018-03-09	Behavior
San Diego State University	Death of Dylan Hernandez.	2019-11-07	2019-11-09	Death

Syracuse University	A string of racist and anti-Semitic incidents.		2019-11-17	Behavior
Texas State University	Death of Matthew Ellis.	2017-11-13	2017-11-14	Death
Tufts University	Accusations of hazing and discrimination.		2016-11-16	Behavior
University at Buffalo	Death of Sebastian Serafin-Bazaan.		2019-04-12	Death
University of California-Berkeley	Reports of sexual assault at off-campus fraternity functions.		2016-10-16	Sexual Assault
University of Central Florida	Decision was made in light of drinking-related controversies.		2018-01-08	Behavior
University of Idaho	A response to the growing national crisis surrounding personal violence like hazing and sexual assault.		2017-12-12	Unspecified
University of Iowa	Death of Kamil Jackowski.	2017-04-30	2017-05-01	Death
University of Kansas	Poor behavior among some Greek groups at the University of Kansas.		2018-03-12	Behavior
University of Michigan-Ann Arbor	Claims of sexual misconduct cases involving fraternity brothers, six incidents of reported hazing, more than 30 hospital transports for students during the weekend of the football game against Michigan State.		2017-11-09	Sexual Assault
University of Missouri-Columbia	Hazing allegations.		2018-03-06	Behavior
University of New Mexico-Main Campus	With three UNM fraternities already in “emergency suspension” following allegations of hazing or alcohol policy violations, administrators have ordered a two-month halt to most social events within the university’s larger Greek system.		2017-12-08	Behavior
University of Pittsburgh	A serious alcohol incident involving members and non-members of one of the fraternities.	2018-01-18	2018-01-19	Behavior
University of Virginia-Main Campus	Rolling Stone article describing the fraternity culture at the school.	2014-11-19	2014-11-22	Sexual Assault
Washington State University	Due to the current negative reputation of the community.		2016-11-07	Unspecified
Washington State University	Death of Samuel Martinez.	2019-11-12	2019-11-14	Death
West Virginia University	Death of Nolan Burch	2014-11-12	2014-11-13	Death
West Virginia University	The result of a Theta Chi brother published a Snapchat video on social media using a racial slur directed at a bartender in a downtown Morgantown club.		2018-02-14	Behavior

Note:

Description of the triggering event is summarized based on newsarticles or conversations with Fraternity and Sorority Life staff. The date of the triggering event is shown if provided. The classification of each event is based off of the description and aligns with Figure 2.

Table E2: Moratorium Dates of Each University in the Sample

University	Start 1	End 1	Start 2	End 2	Start 3	End 3
Arkansas State University-Main Campus	2017-02-21	2017-04-01				
Ball State University	2017-10-24	2018-01-31				
California Polytechnic State University-San Luis Obispo	2015-01-13	2015-04-06	2018-04-17	2018-06-06		
Clemson University	2014-09-23	2014-10-10	2018-01-27	2018-03-01		
College of Charleston	2016-08-30	2016-12-01				
East Carolina University	2015-01-28	2015-02-11				
Emory University	2014-11-03	2014-12-02				
Florida Atlantic University	2017-11-28	2018-03-01				
Florida International University	2018-01-01	2018-02-05				
Florida State University	2017-11-06	2018-03-26				
Indiana University-Bloomington	2017-11-27	2018-02-28				
Louisiana State University and Agricultural & Mechanical College	2017-09-14	2017-10-12	2017-10-19	2018-03-01		
Marshall University	2018-03-05	2018-03-26				
Monmouth University	2018-09-06	2019-01-16				
Murray State University	2018-05-09	2018-08-27				
North Carolina State University at Raleigh	2015-03-20	2015-05-09				
Ohio State University-Main Campus	2017-11-16	2018-02-07				
Ohio University-Main Campus	2019-10-03	2019-10-25				
Rollins College	2017-02-21	2017-04-14				
Rutgers University-New Brunswick	2015-04-06	2015-05-01				
San Diego State University	2014-11-25	2015-01-09	2018-03-09	2018-10-04	2019-11-09	2020-01-17
Syracuse University	2019-11-17	2019-12-09				
Texas State University	2017-11-14	2018-02-26				
Tufts University	2016-11-16	2017-01-19				
University at Buffalo	2019-04-12	2019-08-21				
University of California-Berkeley	2016-10-16	2016-10-26				
University of Central Florida	2018-01-08	2018-03-05				
University of Idaho	2017-12-12	2018-03-13				
University of Iowa	2017-05-01	2019-08-27				
University of Kansas	2018-03-12	2018-03-18				
University of Michigan-Ann Arbor	2017-11-09	2018-01-03				
University of Missouri-Columbia	2018-03-06	2018-03-13				
University of New Mexico-Main Campus	2017-12-08	2018-02-19				
University of Pittsburgh-Pittsburgh Campus	2018-01-19	2018-08-30				
University of Virginia-Main Campus	2014-11-22	2015-01-07				
Washington State University	2016-11-07	2017-01-09	2019-11-14	2020-01-27		
West Virginia University	2014-11-13	2015-02-01	2018-02-14	2018-08-01		

Note:
Universities can have multiple moratoriums in the sample period. Each moratorium date was verified by either a Fraternity and Sorority Life advisor, a news article, or a public records request. However, the first San Diego State University moratorium end date could not be directly verified by either a fraternity or sorority advisor, news article, or public record request. Based on the following news article link, I am confident that the moratorium ended before the start of the 2015 semester. Link: https://newscenter.sdsu.edu/sdsu_newscenter/news_story.aspx?sid=75357

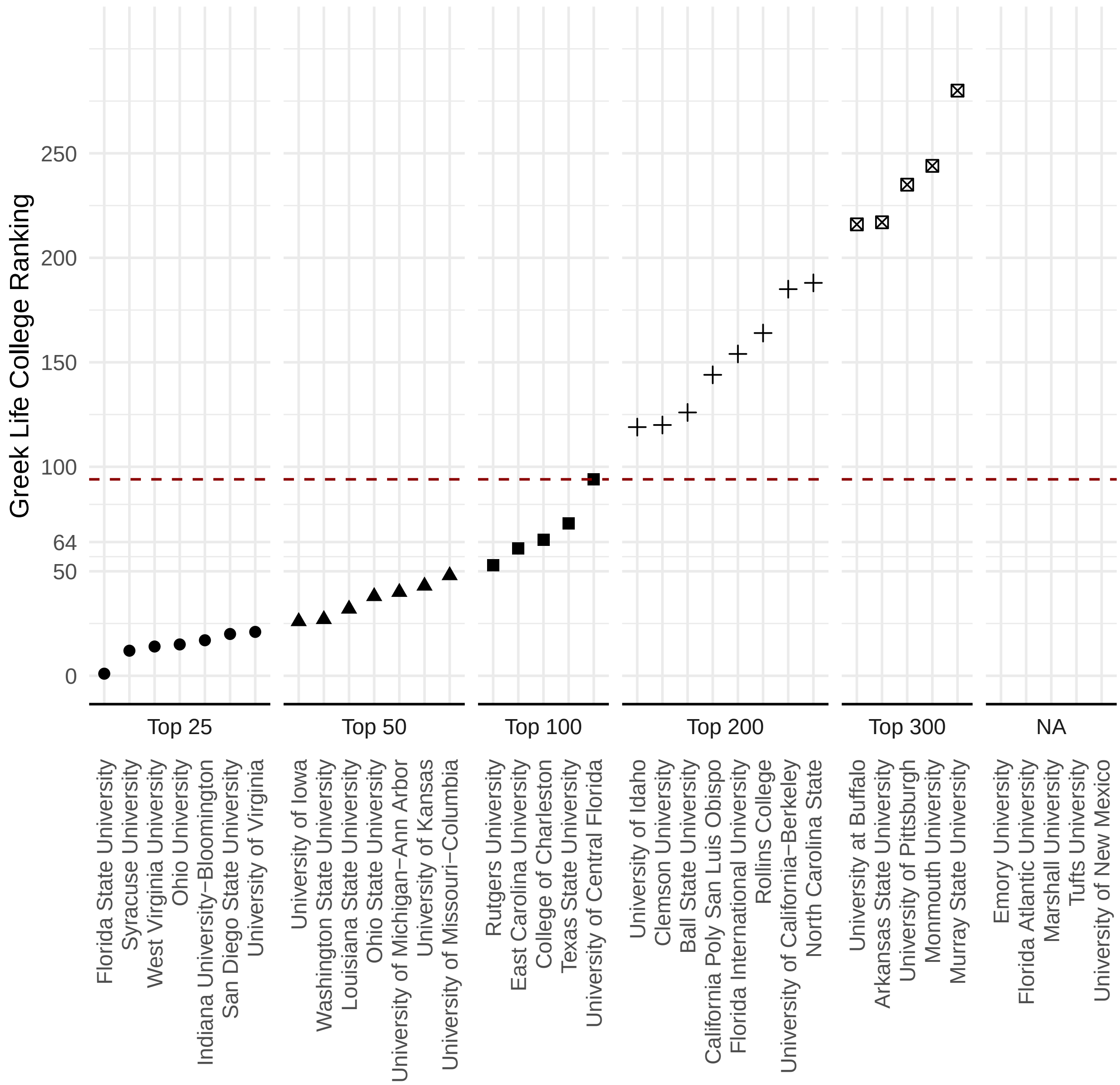

Figure E3: Greek-life Rankings of Universities Included in the Sample

Note: Greek-life rankings are based on Niche.com's 2023 list of Colleges with the Best Greek Life. Rankings are based on survey responses from Niche.com users on the quality of Greek Life at their school. The dashed red line represents the median ranking of the 37 universities in the sample. Three-hundred universities are ranked in the list. Of the universities in the sample, 14 of the 37 universities (38%) are ranked in the top 50, while only 5 of 37 (13%) are not ranked.

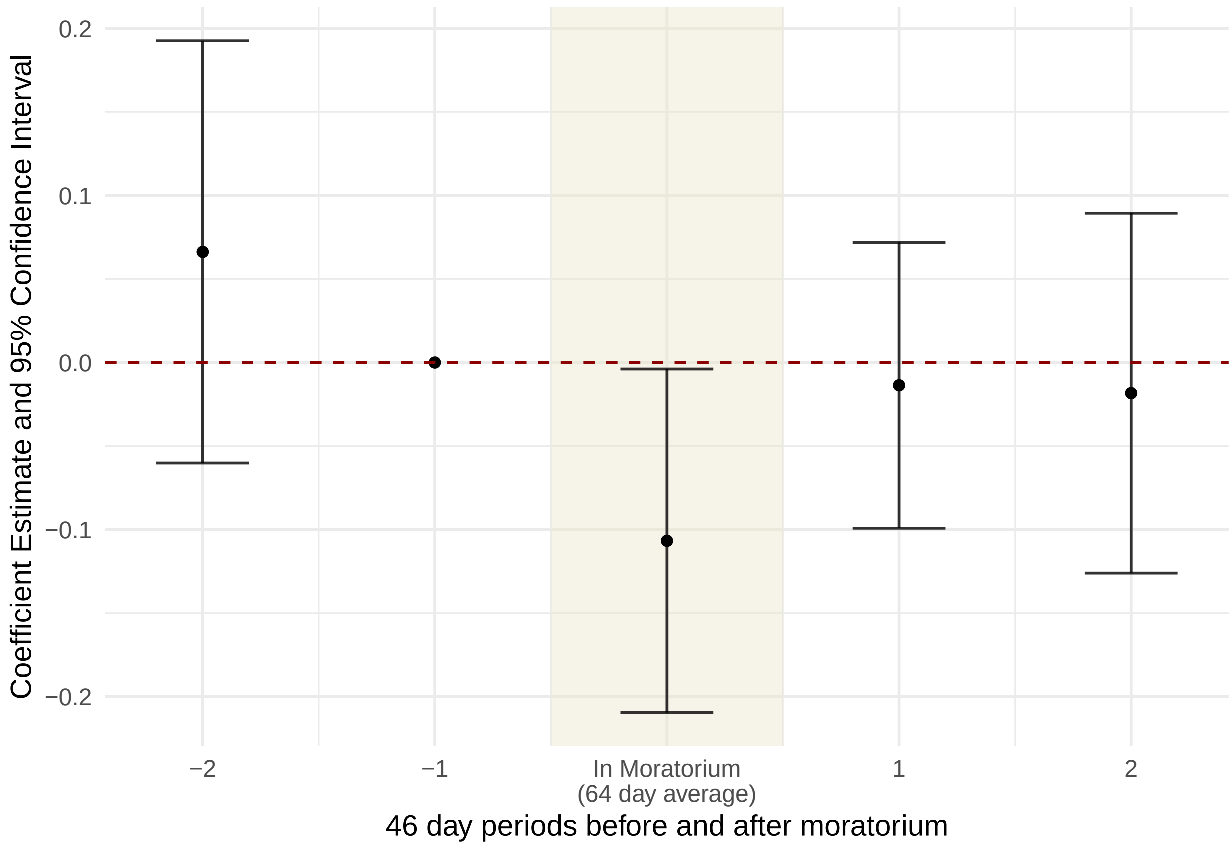

Figure E4: Event Study for Alcohol Offenses

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 46 day periods. Number of days within a period was chosen to give approximately two median-length (46 days) moratorium on each side of the shaded area. All periods are normalized by the 46-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.

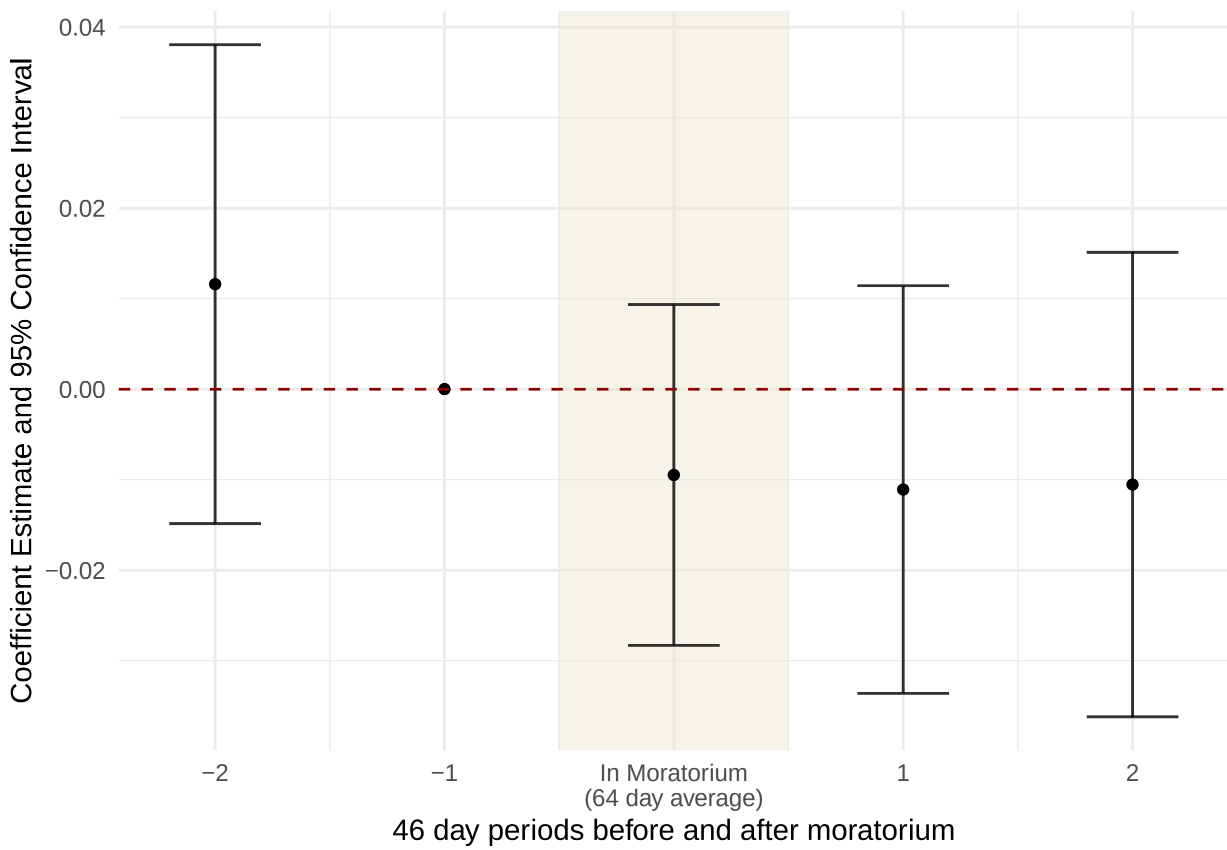

Figure E5: Event Study for Sexual Assault Offenses

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 46 day periods. Number of days within a period was chosen to give approximately two median-length (46 days) moratorium on each side of the shaded area. All periods are normalized by the 46-day period before the moratorium. Sexual assault offenses are defined as sexual assault 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.

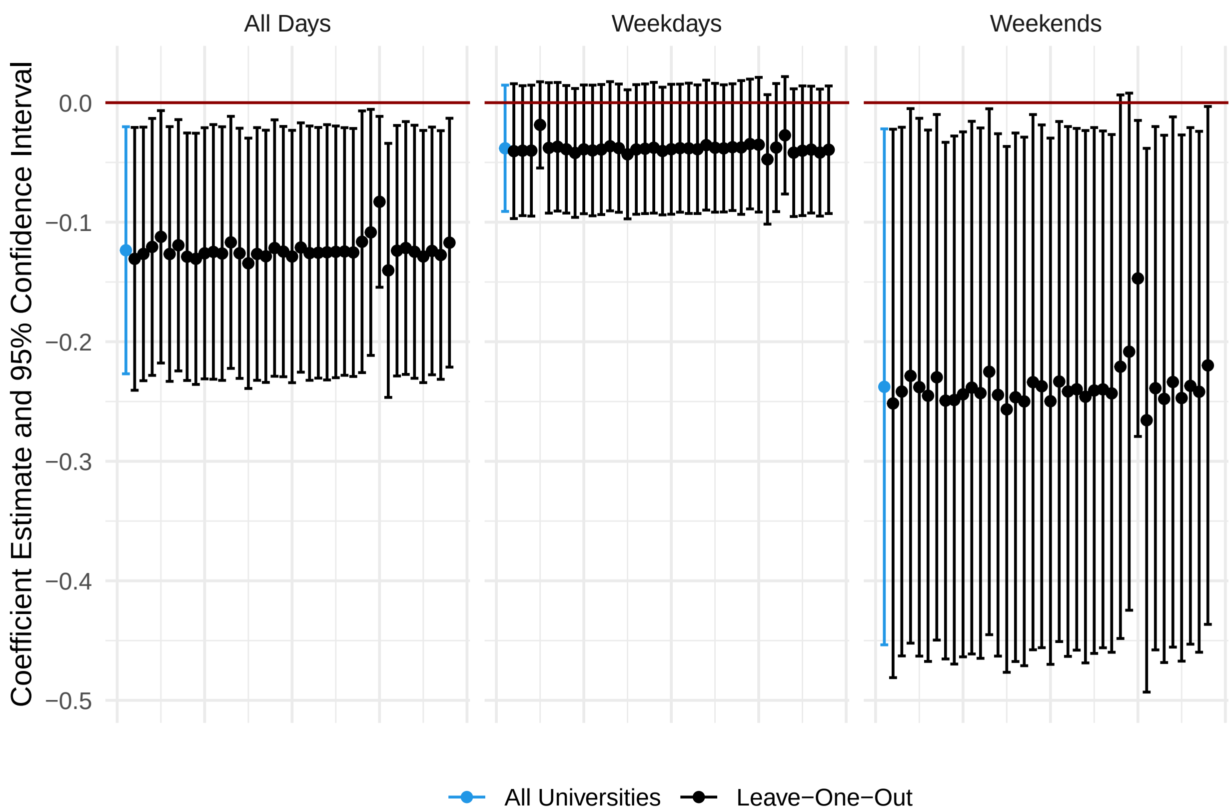

Figure E6: Leave-one-out OLS Regressions of Alcohol Offenses
Note: Each blue point represents the preferred specification (2) from Table 4. Each black point represents specification (2) from Table 4 with one university omitted from the sample. Offenses are per-25000 enrolled students. Errorbars represent 95% confidence intervals. Weekends includes only Friday, Saturday, Sunday, while weekdays includes Monday through Thursday.

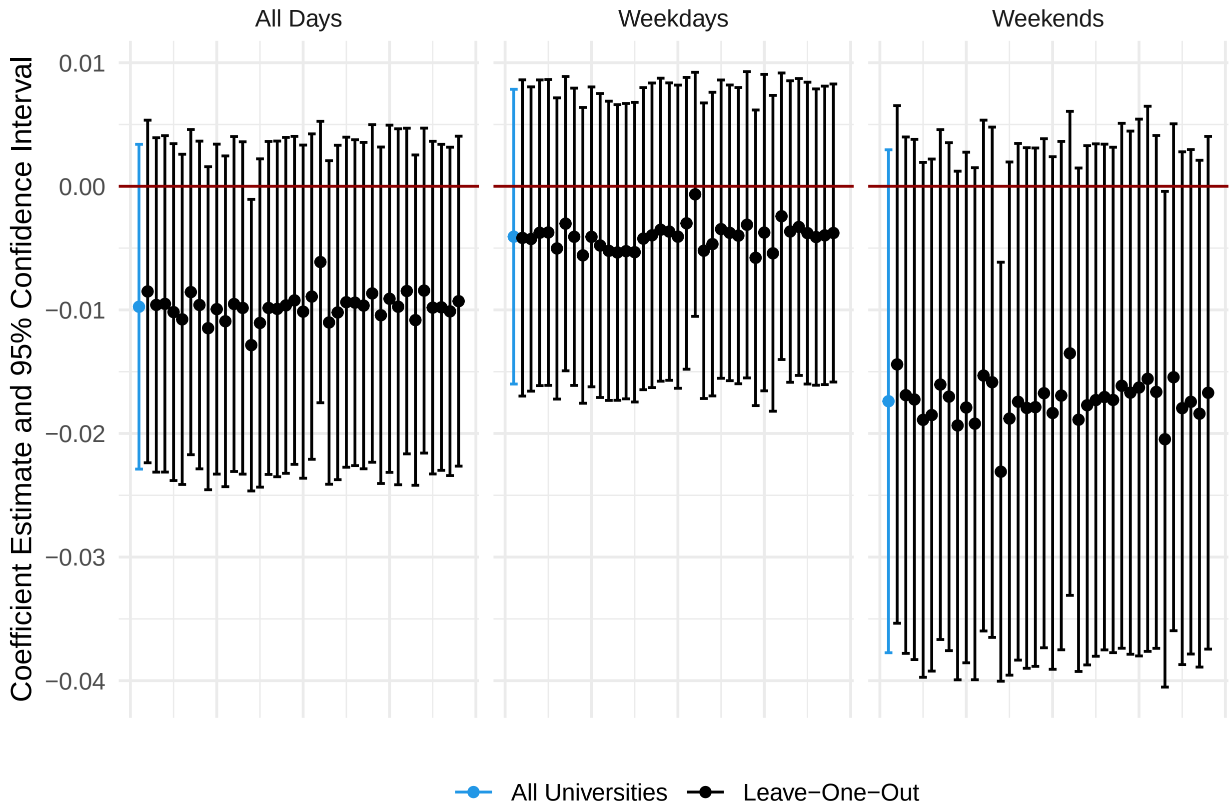

Figure E7: Leave-one-out OLS Regressions of Sexual Assaults

Note: Each blue point represents the preferred specification (2) from Table 4. Each black point represents specification (2) from Table 4 with one university omitted from the sample. Offenses are per-25000 enrolled students. Errorbars represent 95% confidence intervals. Weekends includes only Friday, Saturday, Sunday, while weekdays includes Monday through Thursday.

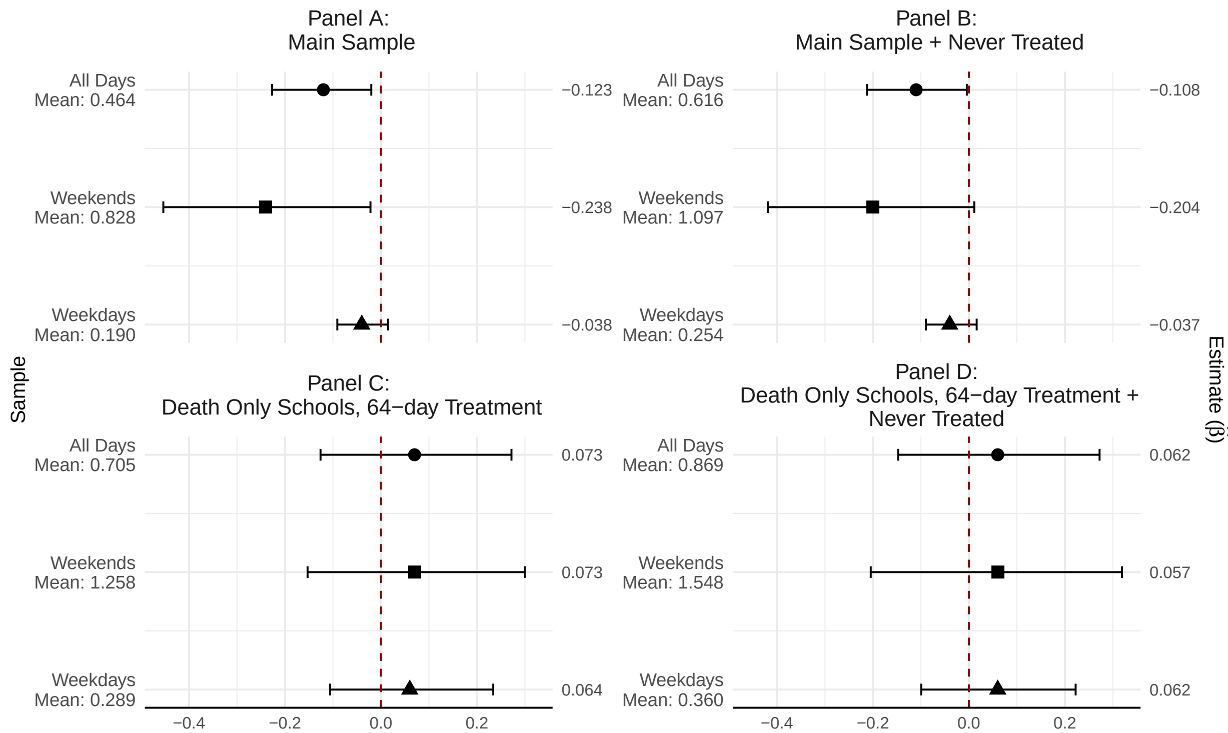

Figure E8: Robustness Across Samples (Alcohol Offenses)

Note: This graph depicts the coefficient estimates and 95% confidence intervals for different subsets of the sample. The y-axis on the left is the sample selection used, while the y-axis on the right is the point estimate. All estimates use the preferred specification from Table 4 Column 2, and all outcomes are in terms of per-25000 enrolled students. Standard errors are clustered at the university level. Panel A uses the main sample as shown in the main results, while Panel B uses the main sample in addition to 14 never-treated schools (see Section **??** for more details). Panel C analyzes 15 universities which undergo a fraternity death, but do not undergo a moratorium. A 64-day binary treatment period is given to each of these universities, beginning on the date of the death. Panel D extends the analysis in Panel C by adding in the 15 never-treated universities as controls, analogous to Panel B in reference to Panel A. See Section **??** for more details.

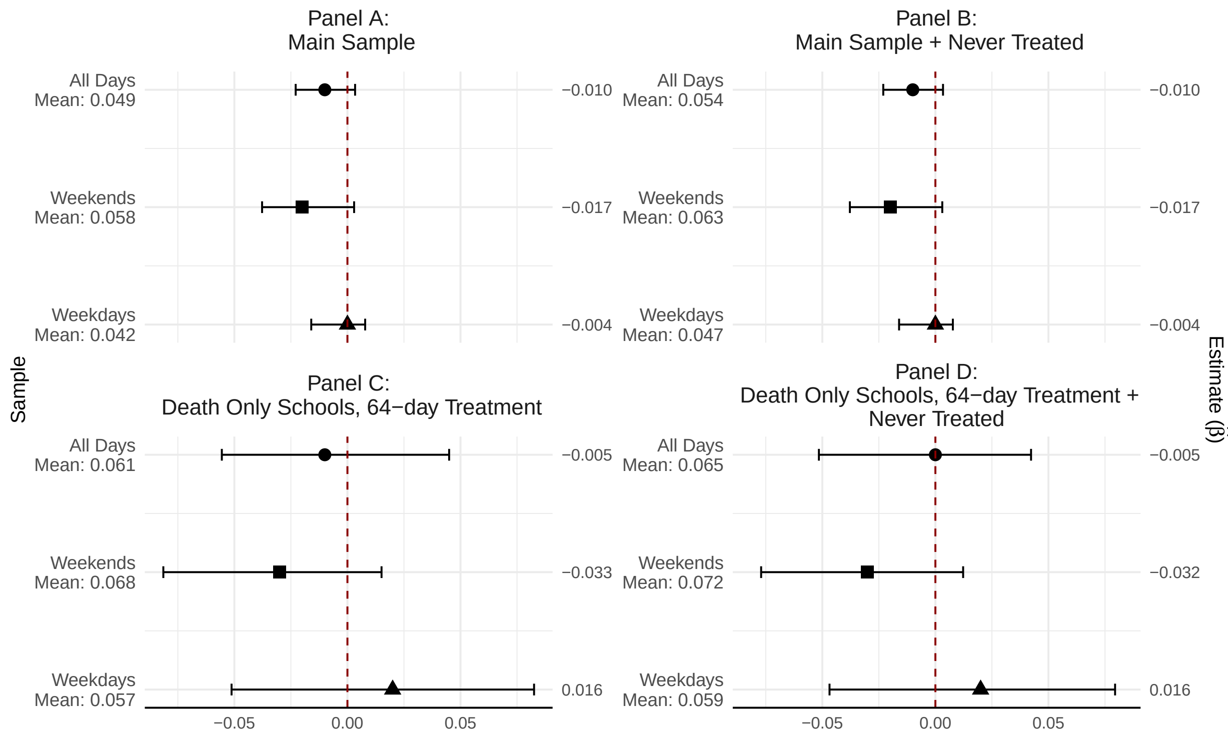

Figure E9: Robustness Across Samples (Sexual Assaults)

Note: This graph depicts the coefficient estimates and 95% confidence intervals for different subsets of the sample. The y-axis on the left is the sample selection used, while the y-axis on the right is the point estimate. All estimates use the preferred specification from Table 4 Column 2, and all outcomes are in terms of per-25000 enrolled students. Standard errors are clustered at the university level. Panel A uses the main sample as shown in the main results, while Panel B uses the main sample in addition to 14 never-treated schools (see Section **??** for more details). Panel C analyzes 15 universities which undergo a fraternity death, but do not undergo a moratorium. A 64-day binary treatment period is given to each of these universities, beginning on the date of the death. Panel D extends the analysis in Panel C by adding in the 15 never-treated universities as controls, analogous to Panel B in reference to Panel A. See Section **??** for more details.

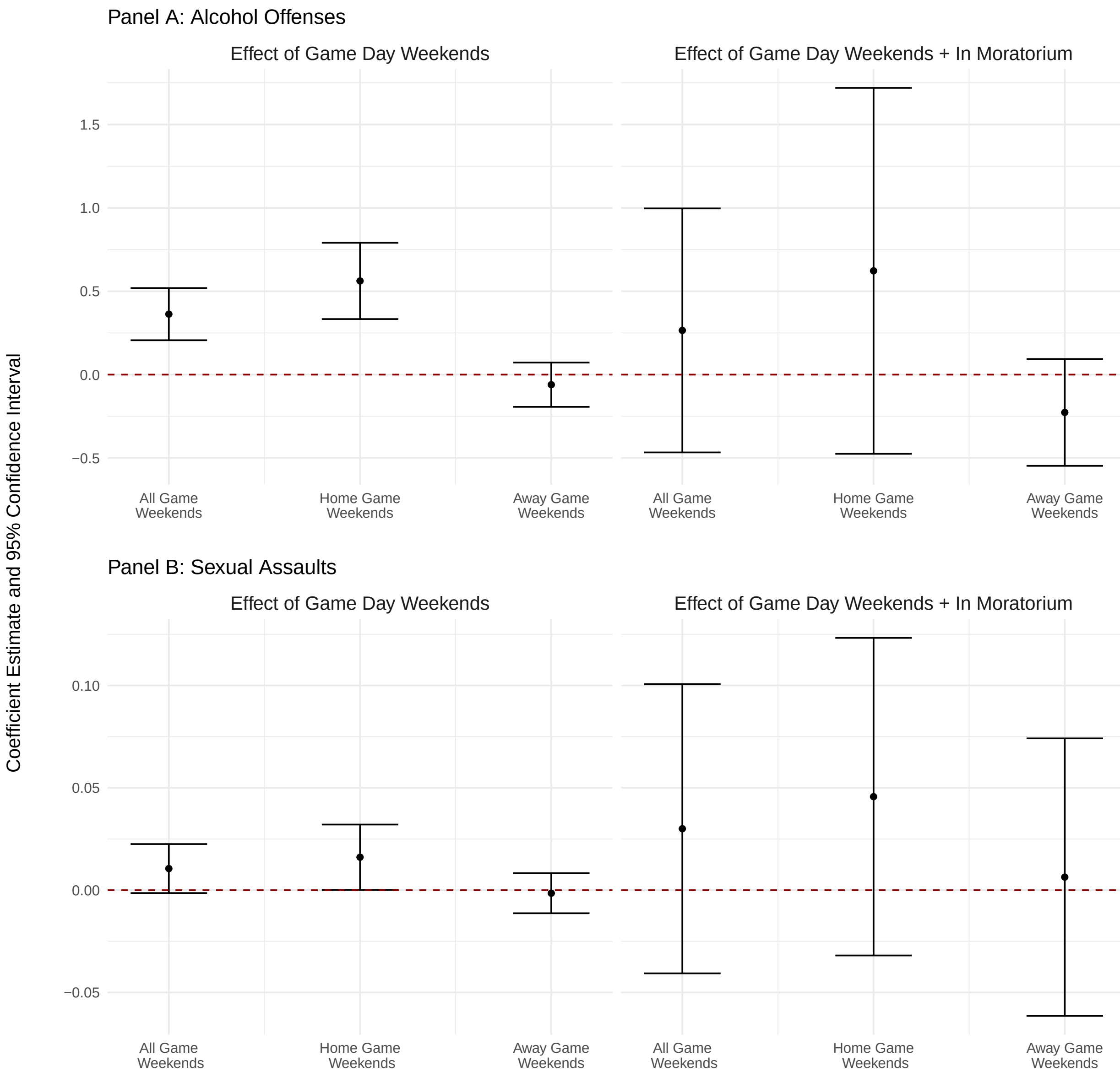

Figure E10: The Effect of Football Game-day Weekends and Football Game-day Weekends + Moratoriums

Note: Game weekends 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-day weekends on the outcome per-25000 enrolled students, and the second being the effect of a football game-day weekend that occurs within a moratorium. A game-day weekend is defined as a weekend in which a football game occurs. For example, if a game occurs on a Friday, then Saturday and Sunday will be included in the game weekend. Note that weekends are defined as Friday/Saturday/Sunday. "All Game Weekends" includes both home and away games. The effects of game-day weekends + moratorium is identified by 245 football game days that coincide with moratoriums. Controls include holiday, spring semester, day of the week, and university by academic year. Standard errors are clustered by university.

Table E3: Comparison of All Relevant Data Sources

	Data Source			
	Daily Crime Logs	CSS	NIBRS	UCR
Source and Requirement:				
Source of Data:	University police departments	US Department of Education	FBI	FBI
Reporting Mandate:	By-law	By-law	Voluntary	Voluntary
Aggregation and Consistency:				
Level of Aggregation:	Incident-level	Yearly	Incident-level	Monthly
Fraction Reporting Consistently:	1.00	1.00	0.24	0.78
Offenses Reported and Location:				
Alcohol Violations:	All incidences reported to or by the university police.	All incidences reported to or by any university entity.	Arrests only	None
Sexual Assaults:	All incidences reported	All incidences reported	All incidences reported	Hierarchy rule
Residence Hall Information:	No	Yes	No	No
Analysis in Paper:	Main analysis	Substitution of partying	Spillovers of partying	Not used

Note:
Appreviations of the data sources are as follows: Campus Safety Security (CSS), National Incidence-based Reporting System (NIBRS), Uniform Crime Report (UCR). The Daily Crime Logs are used for the main analysis due to the advantages it has over the other sources. The fraction reporting consistently refers row corresponds to the fraction of the sample university police departments. For the NIBRS however, the fraction reported consistently refers to the number of university-specific and corresponding nearby police departments that report consistently. The hierarchy rule is a classification rule by the UCR where only the most serious crime in an incident is reported. While over 50 percent of UCR data is recorded to be reported consistently, the true percentage is difficult to know since NAs and 0s are treated as equivalent in the data.

Table E4: Effect of Moratoriums on Alcohol Offenses and Sexual Assaults (Poisson)

				Specification (2)	
				Weekends	Weekdays
	(1)	(2)	(3)	(4)	(5)
Panel A: Alcohol Offenses					
In Moratorium	-0.216**	-0.305***	-0.328***	-0.328***	-0.247
	(0.093)	(0.087)	(0.104)	(0.092)	(0.161)
Observations	55115	54151	52541	22578	29823
Mean of Dependent Variable	0.524	0.524	0.524	0.939	0.211
Panel B: Sexual Assaults					
In Moratorium	-0.164**	-0.199*	-0.187	-0.388**	-0.016
	(0.076)	(0.110)	(0.117)	(0.147)	(0.141)
Observations	55115	52905	50077	21775	28003
Mean of Dependent Variable	0.051	0.051	0.051	0.062	0.043
FE: Day of Week	X	X	X	X	X
FE: Holiday	X	X	X	X	X
FE: Game Day	X	X	X	X	X
FE: Semester (Spring/Fall)	X	X	X	X	X
FE: University	X				
FE: Academic Year	X				
FE: University by Academic Year		X		X	X
FE: University by Academic Year by Semester			X		

Note:
Standard errors are clustered by university and each offense is defined as a count. Observation values may vary between specifications due to no variation with particular fixed effects. Specification (2) is the preferred specification due to the flexibility of the fixed effects and the conservativeness of the estimates in the main results. A weekend is defined as Friday-Sunday while a weekday is defined as Monday-Thursday.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table E5: Effect of Moratoriums on Alcohol Offenses and Sexual Assaults (WLS)

				Specification (2)	
				Weekends	Weekdays
	(1)	(2)	(3)	(4)	(5)
Panel A: Alcohol Offenses					
In Moratorium	-0.128***	-0.129**	-0.131**	-0.243**	-0.042
	(0.046)	(0.050)	(0.049)	(0.103)	(0.030)
Observations	55115	55115	55115	23643	31472
Mean of Dependent Variable	0.464	0.464	0.464	0.828	0.190
Wild Bootstrap P-Value	0.005	0.006	0.010	0.006	0.170
Panel B: Sexual Assaults					
In Moratorium	-0.007*	-0.008*	-0.008	-0.019**	0.000
	(0.004)	(0.005)	(0.005)	(0.008)	(0.005)
Observations	55115	55115	55115	23643	31472
Mean of Dependent Variable	0.049	0.049	0.049	0.058	0.042
Wild Bootstrap P-Value	0.062	0.095	0.121	0.030	0.989
FE: Holiday	X	X	X	X	X
FE: Game Day	X	X	X	X	X
FE: Semester (Spring/Fall)	X	X		X	X
FE: University	X				
FE: Academic Year	X				
FE: University by Academic Year		X		X	X
FE: University by Academic Year by Semester			X		

Note:
Estimates are obtained using WLS. All regressions are weighted by total enrollment. 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.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

F Referee Figures

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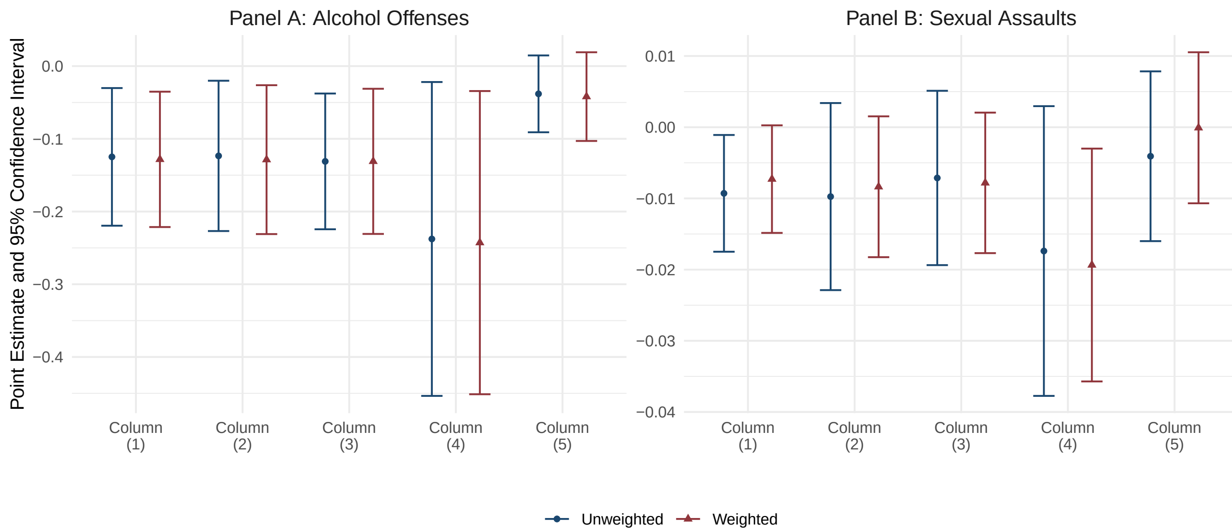

Figure F1: The Effect of Moratoriums: OLS vs. WLS

Note: The x-axis represents the columns from Tables 4 and E5. The y-axis represents the point estimates and 95% confidence intervals. Unweighted regressions represent estimates from Table 4 while weighted regressions represent estimates from Table E5. Weighted regressions are weighted by total enrollment. Controls include holiday, spring semester, day of the week, football game-days, and university-by-academic-year. Standard errors are clustered by university.