Results

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

Main Results

Table **??** reports that fraternity moratoriums result in significantly fewer alcohol offenses on university campuses and provides suggestive evidence of decreases in sexual assaults. In Column 1, the baseline specification from Equation **??** is shown. This specification includes day of the week, holiday, semester, football gameday, and academic-year fixed effects. In Columns 2 and 3, increasingly flexible fixed effects are added, with Column 2 being the preferred specification as noted in Section **??**. Panel A shows that alcohol offenses decrease during moratorium days compared to non-moratorium days. On average, moratorium days experience between 26% and 28% fewer alcohol offenses compared to an average academic-calendar day, as indicated by the point estimates in the first three columns. These estimates are statistically significant in all three specifications, emphasizing the impact of moratoriums on reducing alcohol offenses on campus. Although the point estimates on alcohol offenses are robust, the estimates on sexual assaults do not reach statistical significance in each specification. Additionally, the magnitude varies considerably, with sexual assaults showing a 14-20 percent reduction from the mean across the different estimations.

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

Importantly, these findings persist across various robustness and sensitivity tests. First, given the non-negative count nature of the incident data and the sensitivity of OLS estimation to outliers, Appendix Table **??** reaffirms the results reported in Table **??** using Poisson estimation instead of OLS. Specifically, Poisson estimation shows a statistically significant 27% and 32% average reduction in alcohol offenses and sexual assaults on the weekends, respectively. Second, to ensure that the results are not driven by a single university, Appendix Figures **??** and **??** show the leave-one-out coefficient estimates for each offense. In particular, 37 unique regressions are estimated for each offense, omitting one university in each iteration—all which demonstrate similar findings to the main results. Third, due to the large variation in university size, the models in Table **??** are weighted by total enrollment in Appendix Table **??**. The weighted estimations exhibit similar results to the unweighted models with alcohol offenses and sexual assaults decreasing by 29% and 32% on the weekends respectively, while the standard errors remain similar in magnitude. Finally, recall from Section **??** that negative weights occur in the difference-in-difference estimator when treated units are used as control groups. Given that the sample includes only treated universities, 14 additional universities

that never underwent a moratorium in the period of analysis are included to potentially mitigate the negative weighting issue. This results in 51 universities for a total of approximately 75,000 academic calendar days. Each of the additional universities are chosen from the Colleges with the Best Greek Life list on Niche.com.[1] The additional universities are selected if they are regarded as a Top 50 Greek Life school.[2] Fourteen of these universities are already included in the sample due to experiencing a fraternity moratorium, further justifying the remaining 36 Top 50 Greek Life universities as a good counterfactual. However, only 14 of these universities are included in the sample while the remaining 22 are excluded since they are unable to provide Daily Crime Logs. Appendix Figures **??** and **??** show the effect of moratoriums when including these never-treated universities (see *Main Sample + Never Treated* rows). Overall, the results remain similar, with weekend decreases in alcohol offenses and sexual assaults of approximately 18 percent and 26 percent respectively.

Are There Spillovers to Nearby Areas?

One potential caveat to the main results in Table **??** is that the reported decreases in alcohol offenses and sexual assaults may be being displaced to potentially riskier areas. For instance, while campus-wide alcohol is decreasing, it may be that fraternity members and other students are substituting their behaviors on-campus to off-campus areas that are less regulated. If this is true, the net effect of a moratorium may be worse than never implementing a moratorium. Unfortunately, there does not exist a perfect data source to explore such mechanism directly; the National Incidence-Based Reporting System (NIBRS) only reliably covers 24 percent of the sample universities' neighboring police departments and includes only alcohol arrests rather than all incidents.[3] Furthermore, the Campus Safety and Security (CSS) data, while containing all incidences of crime reported on university campuses, is aggregated to the yearly level.

Despite these challenges, two sets of analyses are performed using these data. First, to identify whether crime incidence is displaced into nearby areas, I use the NIBRS data to compare the reported incidence of crimes at nearby police departments to the crimes reported at university-specific police departments using the Daily Crime Logs. Nearby police departments are defined as police departments that serve the surrounding area, but are not affiliated directly with a university.[4] This results in a comparison of nine university police departments from the Daily Crime Logs and their corresponding nearby police departments from the NIBRS. To harmonize the NIBRS data with the Daily Crime Logs, I define each offense from the NIBRS as per-25000 enrolled students at the corresponding university and limit the panel to only academic-calendar days. Both alcohol offenses and sexual assaults are restricted to incidences involving college-aged individuals (i.e., 17-22), although the results are consistent when broadening the definition to include all ages. Moreover, I define sexual assaults in the NIBRS data to include fondling, rape, and sexual assault with an object to align with the definition using the Daily Crime Logs.

In both Panels A and B of Table **??**, alcohol offenses and sexual assaults have an insignificant and negative point estimate at nearby police departments, thereby showing little evidence of substantial spillovers. Reassuringly, the university-specific police departments continue to show large and significant effects of the moratorium for alcohol offenses despite being a small subset of the main sample. These results give weight to the interpretation that moratoriums are decreasing the number of alcohol offenses on university campuses and students are not moving their risky behaviors to off-campus areas that are less regulated by the university.

As the second set of analysis, I analyze the CSS data to examine if students substitute partying at fraternity houses to different on-campus locations during moratoriums. The CSS data contains all disciplinary actions

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

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

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

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

and arrests corresponding to liquor law violations in addition to reports of sexual assaults that occur in a calendar-year. The main advantage of using the CSS data is that it delineates between crimes that occur within a residence hall or a different on-campus location. Moreover, the CSS data includes liquor violations that may not have been reported to the university police (thus not in the Daily Crime Logs) if they were handled internally by university staff. For instance, if a liquor violation occurs in a residence hall, this citation will be absent from the Daily Crime Logs if it is handled only by the residence hall staff.[5] Therefore, on average, the Daily Crime Logs contain approximately 30% and 50% of the yearly alcohol offenses and sexual assaults reported in the CSS data respectively. However, recall that the biggest disadvantage to this data is the aggregation of all incidents to the calendar-year level. Since moratoriums can last for as few as six days and can continue through multiple calendar-years, this analysis should be taken only as speculative, not causal. See Appendix **??** for a more detailed discussion of the CSS data and the corresponding model.

Using the CSS data, there is evidence that moratoriums move drinking from fraternity houses to residence halls. Residence halls show a 0.270 *increase* in yearly alcohol offenses for each additional moratorium day in a calendar-year. Interestingly, this is accompanied by a 0.033 *decrease* in yearly residence hall sexual assaults. Although these results appear counterintuitive given the literature documents that alcohol offenses and sexual assaults tend to coincide (Lindo, Siminski, and Swensen 2018), these results point to the possibility that moratoriums cause a substitution effect of partying behavior; students substitute drinking at fraternity houses to residence halls. Residence halls, unlike fraternity houses, are far more regulated, contain university staff, and potentially have more sober bystanders to intervene if behavior appears to be escalating dangerously. Taken together, these results support the notion that *if* moratoriums displace dangerous alcohol-fueled behavior, they displace it to *less* risky areas.

Do Moratoriums Have Long-run Effects?

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

Table **??** reports the results of the first set of analysis which fails to show significant evidence of long-run effects. Panel A includes results from the event study estimation shown in Figures **??** and **??**. In addition, p-values from joint F-tests on the four lagged coefficients are reported. The p-values for both alcohol offenses and sexual assaults are above the 10% level of significance, therefore showing little evidence that the effect of the moratorium persists in the four 14-day periods (56 total days) following a moratorium.

While the sample does not collectively exhibit long-run effects, there is potential that longer moratoriums may induce more behavior change than relatively shorter ones. To study this possible heterogeneity, I supplement the analysis above by splitting the sample into quantiles based on the length of a moratorium. Each quantile represents universities with a moratorium less than 32 academic-calendar days (quantile 1), between 33 and 59 academic-calendar days (quantile 2), and more than 60 academic-calendar days (quantile 3).[6] Panel B of Table **??** shows the p-values corresponding to the F-tests on the four lagged coefficients for both alcohol offenses and sexual assaults. Similar to Panel A, there is no statistical significance across each test. Interestingly, there does appear to be evidence that moratoriums with lengths between 33 and 59 days (quantile 2) have the largest instantaneous effect, therefore showing that the length of a moratorium may be crucial to the overall effectiveness.

Last, Figure **??** reports the estimates from the second analysis which extends the specification in Column 2 of Table **??** with an indicator variable for the one-week after and one-week before a moratorium. When considering the entire sample, each offense exhibits decreases that persist only during the moratorium period

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

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

and instantaneously return to previous levels in the week directly following a moratorium. This pattern persists when restricting the sample to weekends where the effects of the moratorium are most prominent.

Is a Moratorium Effective Across its Entire Implementation?

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 **??** and **??**.[7] This amounts to 10 unique coefficients, each identifying the effect of the moratorium in the corresponding week. However, since moratorium lengths differ by university, each point estimate is identified by a different number of schools as shown in parenthesis on the x-axis. For example, the coefficient identifying the effect of a moratorium in Week 3 is identified by 33 universities that have a moratorium that reach the three-week length. Note that if a university has, for instance, a 22-day moratorium, this moratorium will contribute only one day to the identification of the Moratorium Week 4 coefficient.

Panel A of Figures **??** and **??**, exhibit evidence that moratoriums are most effective in the first five weeks. In Panel A of Figure **??**, 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 **??**, 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 **??** and **??**. 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.

Lindo, Jason M., Peter Siminski, and Isaac D. Swensen. 2018. "College Party Culture and Sexual Assault." *American Economic Journal: Applied Economics* 10 (1): 236–65. https://doi.org/10.1257/app.20160031.

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