Referee Remarks

Referee #1:

\*\*Suggestion for Improvement\*\*

1. 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.

Things to do:

* Specify whether the CSS data for liquor law violations is arrests or disciplinary actions.
* Wants regressions on both liquor law violations and disciplinary actions for “additional insights” only.

Referee #2:

This is a straight-down-the-line policy evaluation. It's contribution is on the substantive issue. It doesn't claim any methodological or theoretical contributions. My main concerns are whether:

a) The topic is big enough to warrant publication in a leading international journal.

b) The results/conclusions are strong enough to warrant publication in a leading international journal

a) Is an editorial decision, so I'll leave this alone.

Regarding b), 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 enrolment (or perhaps undergraduate enrolment 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.

• 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

These are my only real comments and suggestions. Whether the paper is published in JHR or elsewhere, it is solid work for which the author should be congratulated.

To-do:

* This referee is particularly concerned about whether the results are strong enough to warrant publication in a good journal.
  + Bring up good point that the CI are very large, and therefore, we cannot actually say whether or not these effects are large, since we can only rule out very small effects.
* Referee believes that the question seems a little obvious. Imposing restrictions on events with alcohol will lead to less alcohol. I think this is a stupid comment and I motivated this in the beginning….but maybe need to really push the fact that kids can still just drink whenever they want..as long as they don’t get caught.
* Wants a couple of new wants to improve precision:
  + Weight by total enrollment or undergraduate enrollment (can do both).
  + To interact the treatment variable with the share of students who are in IFC fraternities. This is difficult to do and would muddy the results quite a bit. Lots of missing data. Could interpolate? Doesn’t sound good to me though.

Referee #3:

Overview

The author asks whether the policy of placing a moratorium on alcohol at fraternity social events at U.S. universities causes a reduction in campus-wide reports of alcohol offenses and sexual assaults. To attempt to answer these questions the author assembles a novel dataset of daily crime reports from 37 universities across a six-year period for the primary analysis. Each of the universities experience at least one moratorium on alcohol at fraternity social events during the sample period. Additional schools are included in supplemental analysis; one set of analysis focuses on top party schools and a second set focuses on schools that experience a fraternity-related death but do no undergo a moratorium. The author categorizing the approximately 500,000 incidents in the daily crime reports as alcohol offenses, sexual assaults, or other incidents. The full set of data are uniquely well-suited to addressing the questions at hand. The primary analysis and robust checks provide convincing evidence that moratoriums do reduce reports of alcohol offenses on campus and suggestive evidence that sexual assaults are also prevented.

Comments

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.

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 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.

The author clearly situates the analysis in the existing literature that highlights the benefits of membership in social fraternities and the costs of social fraternities associated with promoting risky behaviors. The author provides useful context to familiarize a general reader who is less familiar with social fraternities.

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.

To do:

* Touch upon the death factor misconstruing the actual effect of the moratorium earlier.
* Clarify what is going on in Appendix Figure C7 in regards to the death/moratorium. Referee #4 had a good suggestion on how to go about this.
  + Create a greater spotlight on this issue, even if it weakens the results.
* To better Table 7, the mean of the dependent variable would be helpful for the reader.
* Wanted some sort of progression over time of the moratorium…similar to what I did before per-Kevin’s request which Heather called “reading tea-leaves”.
  + This is probably the most difficult thing to get at. How can we see how the moratorium progresses over time?
  + They want to know some way to inform the optimal length and whether we would expect any benefit from a permanent ban on alcohol at these events.

Major Points

**1) Leverage variation in share of the population directly impacted by moratorium:**

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.

**2) Can you more clearly separate the impact of the precipitating event from the effect of**

**the moratorium itself?** 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.

**3) Representativeness of sample in context of Greek universe:** 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.?

**Minor Comments**

1. 1)  The paper is clearly written, which is much appreciated!
2. 2)  Sections 3.1 and 3.2 could be condensed with some of the material moved to the online

appendix.

To do:

* Interacting the treatment with the share of IFC fraternities.
* Wants to know whether a death causes an effect absent treatment. Using a 64 average pseudo treatment with the never-treated-but-death group.
* Representativeness of sample in context of Fraternity/Sorority life universe – what is a typical share and what is my share?
  + This is extremely tough to get at..might be some statistics online or maybe the IFC has some? The IFC would never give me their data…or so I think.
* Condense 3.1 and 3.2 by moving some of the material to the appendix.