American Economic Journal: Applied Economics 2018, 10(1): 236–265
https://doi.org/10.1257/app.20160031

College Party Culture and Sexual Assault†

By Jason M. Lindo, Peter Siminski, and Isaac D. Swensen*

This paper considers the degree to which events that intensify partying increase sexual assault. Estimates are based on panel data from campus and local law enforcement agencies and an identification strategy that exploits plausibly random variation in the timing of Division 1 football games. The estimates indicate that these events increase daily reports of rape with 17–24-year-old victims by 28 percent. The effects are driven largely by 17–24-year-old offenders and by offenders unknown to the victim, but we also find significant effects on incidents involving offenders of other ages and on incidents involving offenders known to the victim. (*JEL* I23, J16, K42, Z13)

There are several mechanisms through which partying may increase the incidence of rape among college students. The most obvious relate to alcohol consumption, which has direct pharmacological effects on aggression and cognitive functioning. Moreover, consistent with Becker's (1968) seminal model of crime, potential perpetrators may believe that the probability of being punished (and the degree of punishment) will be lower if they and/or their victims are inebriated.[1] That said, partying may also increase the incidence of rape by increasing social contact and by altering the context in which social contact takes place. These potential pathways are supported by statistics indicating that over a half of incapacitated rapes and a quarter of forcible rapes take place at parties (Krebs et al. 2009) and statistics indicating that two-thirds of student rape victims are intoxicated or impaired by drugs at the time of the incident (Kilpatrick et al. 2007). Moreover, 77 percent of students agree that reducing drinking would be very effective, or somewhat effective, in preventing sexual assault on their campus (*Washington Post*-Kaiser Family Foundation 2015). Despite these strongly suggestive statistics, evidence on the

* Lindo: Department of Economics, Texas A&M University, College Station, TX 77843, NBER, and IZA (email: jlindo@tamu.edu); Swensen: Department of Agricultural Economics and Economics, Montana State University, Bozeman, MT 59717 (email: isaac.swensen@montana.edu); Siminski: University of Technology Sydney, NSW 2522 Australia and IZA (email: Peter.Siminski@uts.edu.au). The authors thank Mark Anderson, Andrew Barr, Alan Barreca, Alex Brown, Scott Cunningham, Tim Fitzgerald, Melanie Guldi, Mark Hoekstra, Jonathan Meer, Steve Puller, Dan Rees, and Carly Urban for helpful comments, along with seminar and conference participants at Baylor University, Texas Tech University, the Meetings of the Southern Economic Association, the Annual Health Econometrics Workshop, the NBER's Children's Program Meetings, and the American Economic Association Annual Meetings. The authors also thank Brenton Cooper and Sam Bondurant for excellent research assistance.

† Go to https://doi.org/10.1257/app.20160031 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

[1] For in-depth discussions of the mechanisms linking alcohol and violent crime, see Cook and Moore (1993a, b), Markowitz (2005), Carpenter and Dobkin (2011), and Cook and Durrance (2013), among others.

causal link between partying (or drinking) at college and the incidence of sexual assault has eluded researchers to date.[2]

In this paper, we aim to fill this gap in the literature by considering the effects of football games—which intensify partying among college students—on the incidence of rape at schools with Division 1 programs.[3] Specifically, we use panel data from the National Incident Based Reporting System to estimate the increases in reports of rape caused by football games using an identification strategy that exploits plausibly random variation in the timing of game days. Intuitively, we identify the effects by comparing reports of rape to law enforcement agencies serving students on game days to reports on nongame days, while controlling for differences expected across different days of the week and across different times of the year. This approach is similar to that of Rees and Schnepel (2009), who analyze the effects of college football games on assault, vandalism, disorderly conduct, and alcohol-related crimes.[4]

We find significant and robust evidence that football game days increase reports of rape victimization among 17–24-year-old women by 28 percent. Home games increase reports by 41 percent on the day of the game and away games increase reports by 15 percent. These effects are greater for schools playing in the more prominent subdivision of Division 1 and for relatively prominent games. There is no evidence that these effects are offset by reductions in nearby areas, on adjacent days, or during other times of the fall term. Moreover, the effects are driven largely by 17–24-year-old offenders and by offenders unknown to the victim, though we also find significant effects on incidents involving offenders of other ages and on incidents involving offenders known to the victim. Estimates by race indicate that the main results are not driven solely by white victims or black victims, nor by white offenders or black offenders.

Back of the envelope calculations based on our estimates imply that the effects of Division 1A football games explain 5 percent of fall semester (September through December) reports of rape involving 17–24-year-old victims to law enforcement agencies serving students attending these schools. Moreover, they imply that these games cause 724 additional rapes per year across the 128 schools participating in Division 1A. Based on an estimated social cost of $267,000 per rape (McCollister, French, and Fang 2010), this implies an annual social cost of rapes caused by Division 1A games of $193 million. The estimated effects for schools participating in Division 1AA are smaller, suggesting 108 additional rapes per year across 125 schools.

[2] Several quasi-experimental studies have documented effects of alcohol policies on crime using variation driven by the minimum legal drinking age (Carpenter and Dobkin 2015), taxes (Cook and Moore 1993b, Markowitz and Grossman 2000, Markowitz 2005, Durrance et al. 2011, Cook and Durrance 2013), drunk driving laws (Carpenter 2005, 2007), and changes in "wet" laws (Biderman, De Mello, and Schneider 2010; Anderson, Crost, and Rees 2014). Of those studies that include estimated effects on sexual assault or rape, Cook and Durrance (2013) and Markowitz (2005) found no statistically significant evidence that beer taxes affect the probability of rape; Cook and Moore (1993b) found that beer tax increases reduce rape rates; Anderson et al. (forthcoming) found that the number of licensed premises has a positive effect on rape rates; and Carpenter and Dobkin (2015) find no evidence of a discontinuity in arrest rates for rape at the minimum legal drinking age.

[3] See Neal and Fromme (2007), Glassman et al. (2007), Rees and Schnepel (2009), and Glassman et al. (2010) on the heavy alcohol consumption and partying behaviors associated with collegiate football.

[4] Specifically, we build upon their identification strategy in a way that controls more flexibly time-varying, agency-specific confounders.

The reduced-form nature of the analysis implies that we cannot say with certainty that the estimated effects on reports of rape are driven by the increase in partying associated with football games. There are indeed a number of theories on spectator violence, although to our knowledge, none of these theories explicitly discuss sexual violence.[5] We provide support for partying as the likely causal pathway with a parallel analysis of other criminal offenses that serve as proxies for excessive partying, including drunkenness, DUIs, liquor law violations and public order offenses. We also find that the one game *outcome* associated with significant increases in these proxies (upset wins) is also where we see the strongest evidence that the outcome affects reports of rape. In contrast, one of the leading theories on spectator violence, the frustration-aggression hypothesis (Wann 1993), predicts the opposite: that spectator aggression is an attempt to reestablish self-esteem following a loss, which is consistent with Card and Dahl's (2011) results examining the effects of NFL game outcomes on domestic violence. Furthermore, we find that the effects are larger-than-average for schools that have reputations as "party schools." Finally, an analysis of the timing of the impacts reveals significant effects on reports of rape the night before, during, and after home games whereas effects are only apparent after away games. This evidence is consistent with there being an effect of pregame partying, which we would expect to be much more common for home than away games.

While we have focused thus far on how our study contributes to understanding the links between partying and sexual assault, we believe there is equal merit to the insights it provides into the effects of college football. Division 1 football is a multi-billion dollar industry that is intimately tied to the higher education system in the United States. Universities are making substantial and rapidly growing investments in this industry. Many have questioned the wisdom of such investments for a variety of reasons: nearly all Division 1 athletics programs are subsidized by their student bodies or their university's general fund (Lindo, Swensen, and Waddell 2012); there are concerns about long-run effects on players' health, including chronic traumatic encephalopathy (CTE); there is uncertainty about the amateur status of players and what constitutes fair compensation; and, finally, it is not clear how big-time sports programs affect universities and students—positively or negatively—along a number of important dimensions. Most of the research in this final area focuses on student applications, student enrollment, and alumni giving in order to speak to the advertising effects of big-time sports.[6] Only recently have researchers taken steps to quantify the causal effects on students' experiences in college. Specifically, Lindo, Swensen, and Waddell (2012) and Hernández-Julián and Rotthoff (2014) present evidence that the success of a university's sports program impairs academic performance.[7] This paper adds to this literature by considering

[5] See Branscombe and Wann (1992) and Wann (1993) for reviews of these theories, which include physiological arousal, mimicking behavior, or social learning, as well as the frustration-aggression hypothesis, which links self-esteem to game outcomes.

[6] For example, see Anderson (2017) and Pope and Pope (2014) and the references therein.

[7] See also Clotfelter (2011), who examines the number of JSTOR articles viewed (as a measure of work done by students and faculty) around the time of the NCAA basketball tournament. He finds that having a team in the tournament reduces the number of article views.

the effects of big-time sports on a social outcome that is of particular importance to student welfare.

The remainder of the paper is structured as follows. The next section provides a brief discussion about the incidence of sexual assault among college students, and what is known and what is being done to promote student safety. The following two sections discuss the data and the empirical approach that we use, including issues related to the underreporting of sexual assault. We then present the results of our analysis and discuss these results before concluding.

I. Background on Sexual Assault Incidence and Prevention

The oft-cited statistic that one-in-five women has been sexually assaulted while in college originally was based on the Campus Sexual Assault Study, a web-based survey of approximately 5,000 female undergraduates at two large public universities in which 19.8 percent of seniors reported incidents of sexual assault since entering college (Krebs et al. 2009). More recently, the *Washington Post*-Kaiser Family Foundation Survey, a nationally representative phone survey of over 1,000 current and recent undergraduates conducted in 2015, documented similar victimization rates and the AAU Campus Survey on Sexual Assault and Sexual Misconduct, a web-based survey of over 150,000 students administered at 27 universities in 2015, documented somewhat higher victimization rates.[8] In terms of the most serious forms of sexual assault, 13.5 percent of senior undergraduate females and 2.9 percent of senior undergraduate males participating in the AAU survey reported that they had experienced nonconsensual penetration involving physical force or incapacitation since enrolling in college. This survey also documented that victimization rates vary considerably across universities. Although more work is needed to evaluate a broader set of universities and to address low survey response rates, there is widespread agreement that sexual assault victimization is an important social problem affecting college students, and there are a wide array of efforts under way to address it.

The federal government has played a key role in bringing attention to sexual assault victimization and shaping efforts to promote student safety.[9] Its guidance for prevention efforts is based on a review of rigorously evaluated interventions conducted by the Centers for Disease Control and Prevention (CDC).[10] The White

[8] Each of these surveys measured sexual assault by asking respondents behaviorally specific questions instead of explicitly asking whether they have been sexually assaulted and assuming an accurate understanding of what constitutes a sexual assault. The importance of this measurement approach is highlighted by a recent survey at MIT where only 65 percent of females who had been sexually assaulted (based on the legal definition and their responses to behaviorally specific questions) responded affirmatively to a question that explicitly asked whether they had been sexually assaulted (Massachusetts Institute of Technology 2014).

[9] Some of the major milestones include the Campus Sexual Violence Elimination Act, which required primary prevention programs and awareness programs, expanded reporting requirements, and provided guidelines for the support of victims (March 2013); the establishment of the White House Task Force to Protect Students from Sexual Assault (January 2014); the "1 is 2 Many" and "It's On Us" campaigns; the decision to make public the list of schools under investigation for their handling of sexual violence reports; as well as the NotAlone.gov website to provide information on how to respond to and prevent sexual assault.

[10] See "Preventing Sexual Violence on College Campuses: Lessons from Research and Practice." Rigorous evaluation in this context is defined as randomized control trials and quasi-experimental analyses with non-immediate follow-ups.

House Task Force to Protect Students from Sexual Assault (2014) says that this guide “points to steps colleges can take now to prevent sexual assault on their campuses,” but a close reading of the guide reveals just how little is known. The two interventions in its “what works” category only have been shown to be effective among sixth through ninth graders in New York City and in a rural North Carolina county, respectively.[11] Furthermore, bystander intervention—the type of intervention the Task Force emphasizes as being “among the most promising prevention strategies”—falls under the “what might work” category because such strategies have been shown to affect risk factors associated with sexual assault but have not been shown to affect incidence rates. Alcohol-control policies and other efforts to encourage safer partying largely have been in the periphery of recent discussions about sexual assault prevention.[12] That said, whether such policies should feature prominently in these discussions depends on the degree to which the incidence of sexual assault is caused by the party culture associated with college and the degree to which this atmosphere can be influenced. In this study, we aim to provide empirical evidence on this issue by estimating the causal effect of football games, which often serve as a focal point for college parties, on reports of rape at schools with Division 1 programs.

II. Data

Our analysis uses crime data from the National Incident Based Reporting System (NIBRS) collected by the Federal Bureau of Investigation (FBI). NIBRS is a voluntary program that collects information on incidents of crime from law enforcement agencies across the United States. The detail provided in these micro data allows us to identify reports of rape that occur on or around college football game days. We use the FBI’s recently expanded definition of rape, which includes both male and female victims and offenders, non-consenting acts of sodomy, and sexual assault with an object. Except where otherwise noted, our analysis focuses on reports of rape involving college-aged (17–24) victims.[13] We also consider incidents involving victims in different age groups, incidents involving perpetrators in various age

[11] As described in DeGue et al. (2014), *Safe Dates* was a “10-session curriculum focused on consequences of dating violence, gender stereotyping, conflict management skills, and attributions for violence.” It focused on eighth and ninth graders in a rural North Carolina county. *Shifting Boundaries* focused on sixth and seventh graders in New York City and involved “temporary building-based restraining orders, poster campaigns to increase awareness of dating violence, “hotspot” mapping and school staff monitoring over a 6–10 week period.”

[12] For example, such policies are not part of the CDC’s list of “what works, what might work, and what doesn’t work” despite being mentioned in its subsequent discussion as having the potential for reducing sexual violence. In addition, alcohol control policies are not mentioned in the Task Force’s first report, and alcohol use is not mentioned in any of the federal government’s public service announcements. The conclusion from Lippy and DeGue’s (2016) review of the literature on alcohol policy approaches to preventing sexual violence is that such policies may be promising but “additional research is needed to directly examine effects on sexual violence outcomes.”

[13] We choose to include 17-year-olds in the analysis because of the widespread belief that college freshman at the beginning of the academic year are especially vulnerable targets for rape and we want to make sure that our analysis can capture effects on those members of this group that have yet to turn 18. We include students through age 24 out of respect for the fact that only 44 percent of first-time bachelors degree recipients completed their degree within 48 months of their initial postsecondary enrollment; 23 percent completed between 49–60 months and 9 percent completed within 61–72 months (Cataldi et al. 2011). We also note that the 17–24 age range captures over 90 percent of full-time undergraduates attending schools with Division 1 football programs. Source: Authors’ calculation using 2005 Integrated Postsecondary Education Data System.

groups, incidents involving different types of relationships between victim and perpetrator, incidents in which the perpetrator is reported to be under the influence of alcohol, and incidents involving victims and perpetrators of different races.

Participation in NIBRS has increased steadily since it began in 1991 when only three states' agencies participated. As of 2012, agencies representing 30 percent of the US population across 36 states are actively reporting incidents. Our analysis is based on NIBRS data for law enforcement agencies that serve students at universities with Division 1 football programs, including university-based agencies and municipal agencies in the same city.[14] The 138 such agencies in NIBRS, corresponding to the 96 universities listed in the online Appendix (Table A1), are the focus of our analysis. We consider the representativeness of these universities and agencies in detail in the online Appendix (Table A2). Notably, the universities included in our analysis are considerably less likely to be private as compared to the full set of universities with Division 1 football programs (15 percent versus 26 percent). They have slightly larger average enrollment (13,228 versus 12,057) and share of white students (0.74 versus 0.68). In other respects, including share of female students, retention rates, SAT scores, rates of financial aid receipt, and per capita rape reports, the estimation sample does not differ markedly from the full set of Division 1 schools and the agencies which serve them.

We use details of incidents recorded in NIBRS to construct measures of rape at the daily level for each agency. Using data on the times and dates of incidents, we define days as spanning from 6:00AM to 5:59AM so that incidents are better matched to late night activities that spill over into the morning, noting that incidence rates are highest between midnight and 4:00AM.[15] We combine these data with information compiled from sports-reference.com on the football games played by the universities with which each agency is associated.[16] These data include the dates of games played by each team, whether the game is home or away, and the outcome of the game. In order to further consider heterogeneous effects, we also use information from ESPN.com dating back to 2001 to construct an indicator variable for "ESPN-listed television coverage;" we think of this variable as a proxy for game prominence and for television access to view a game because it does not appear to comprehensively cover conference-specific or local television coverage.[17] We also use ten-year (2005–2014) college football team rankings from football-sickness.com and information from a wide variety of websites to consider whether games

[14] We do not use data from the handful of municipal law enforcement agencies in cities with more than one school participating in Division 1 football (e.g., the Los Angeles Police Department).

[15] The time-of-day distribution of reported incidents is shown in Figure A1 in the online Appendix.

[16] We do not include bowl games in our analysis because they are atypical and usually take place when classes are not in session.

[17] Of the 13,773 games included in our sample, ESPN.com lists television coverage for roughly half. Of those games, 35 percent are listed as having aired on ESPN, 25 percent on ESPN Gameplan, 9 percent on ESPN2, and 8 percent on ESPNU. They also list games televised on ABC, CBS Sports, ESPN Classic, Fox, Fox Sports Net, NBC Sports, PAC-12 Network, TBS, and Mountain West Sports. Thus, the only listed conference-specific network is the PAC-12 network, while Big-Ten, ACC (Raycom Sports), Sun Belt Conference, and Western Athletic conference all have their own networks and are not included. There are also other syndicated networks not included in the list (e.g., the American Sports Network recently made an agreement to cover games from Conference USA, the Colonial Athletic Association, the Big South Conference, the Southern Conference, Southland Conference and Patriot League). Some colleges (e.g., BYU) are also known to broadcast their own games, but such local coverage is not included in the ESPN list.

Table 1—Reported Incidents per Day for NIBRS Analysis Sample

Rapes, victims ages 17–24	0.051
Rapes, victims ages 17–20	0.031
Rapes, victims ages 21–24	0.020
Rapes, victims ages 25–28	0.012
Rapes, victims ages > 28	0.060
Rapes, victims ages 17–24, offenders ages 17–24	0.022
Rapes, victims ages 17–24, offenders of other ages	0.021
Rapes, victims ages 17–24, offender known	0.032
Rapes, victims ages 17–24, offender unknown	0.019
Rapes, victims ages 17–24, offender is black	0.022
Rapes, victims ages 17–24, offender is white	0.024
Rapes, victims ages 17–24, victim is black	0.014
Rapes, victims ages 17–24, victim is white	0.035
Disorderly conduct incidents, ages 17–24	0.178
Driving under the influence incidents, ages 17–24	0.227
Drunkenness incidents, ages 17–24	0.154
Liquor-law violations, ages 17–24	0.457

Notes: These statistics are based on daily data (excluding June, July, and August) spanning 1991–2012 for 138 municipal and university-based law-enforcement agencies participating in the National Incident Based Reporting System that have been matched to 96 universities participating in Division 1 football, as described in Section II.

against traditionally strong teams, and/or games against rivals, have comparatively large effects.[18] And we use Princeton Review Top Party School Rankings to consider whether the effects are relatively large at schools that are viewed as having reputations as party schools. Finally, we use the pregame point spread predictions for each Division 1A game from covers.com to consider the degree to which the effects differ for games with different expected outcomes as well as the degree to which games with unexpected outcomes have different effects than games with outcomes that are consistent with expectations.

Ultimately, we produce a dataset at the agency-by-day level with reports of rape and indicators for whether the day is a game day for the school that the agency is associated with, in addition to a host of variables to capture characteristics associated with the games played. In supplementary analyses, we also consider data on alcohol-related offenses that are similarly constructed using the same sources of data. We exclude from our analyses the dates between June 1 and August 31, when students are less likely to be in town. In a similar spirit, our statistical analyses control for holidays taking place at other times of the year.

The data used in our main analysis consist of 425,190 observations. This includes 43,793 Saturdays without football games and 17,062 Saturdays with football games. The data only include 1,128 games played on other days of the week. Table 1 shows daily incident rates based on these data. Notably, victims aged 17–24 comprise approximately one-third of all victims reported to the agencies in our analysis. These agencies indicate one reported rape every 20 days for victims in this age

[18] Football-sickness ten-year rankings are based on an algorithm that uses winning percentage, strength of schedule, winning the national championship, and participation/victory in the most prominent bowl games. Our inexact process for identifying rivals involved searches on Wikipedia, university websites, and websites dedicated to covering university athletics. We list the rivals identified for each school in the online Appendix (Table A1).

range. The perpetrators involved in these incidents are split fairly evenly across the age groups 17–20, 21–24, 25–28, and other. Consistent with what is born out in many datasets involving different types of victims, a majority of these college-aged victims (60 percent) knew the perpetrator. Approximately 20 percent of incidents involving college-aged victims specify that the perpetrator was under the influence of alcohol.[19]

In light of the statistics cited in the introduction regarding the prevalence of rape, we note that the incidence rates implied by NIBRS data are low. This is consistent with Kilpatrick et al. (2007), which finds that only 12 percent of college students experiencing a rape report it to law enforcement. Students state many reasons for not reporting, including not wanting others to know, fear of retaliation, perceived lack of evidence, uncertainty about how to report, uncertainty about whether the incident constituted a crime, and uncertainty about the perpetrator's intent. In Section III, we discuss in detail this measurement error and its implications for our analysis.

III. Empirical Approach

We estimate the effects of football games played by schools with Division 1 programs using within law enforcement agency variation over time. Our models' identifying assumption is that the proportional changes in reports of rape observed across days of the week during weeks without football games is a good counterfactual for changes that would be expected on game days in the absence of games, adjusting for expected differences across years, months, weeks, etc. Given the discrete nature of reports, and because we often have cells with zero reports, our estimates are based on Poisson models.[20] In particular, our baseline approach to estimating the effect of college football game days on the number of daily rape reports corresponds to the following equation:

$$E[R_{act} \mid Gameday_{ct}, \theta_a, X_t] = \exp(\beta Gameday_{ct} + \theta_a + \gamma \mathbf{X}_t), \tag{1}$$

where R_{act} is the number of rapes reported to law enforcement agency a, which serves students at college c, taking place on day t; $Gameday_{ct}$ is an indicator equal to one if college c has a game on day t; θ_a are agency fixed effects; and X_t is a set of time-varying controls that are common to agencies—these include day-of-week fixed effects, indicators for holidays, and year fixed effects.[21] We calculate sandwiched standard error estimates allowing errors to be correlated over time within an agency and across agencies corresponding to the same college—i.e., clustered at the

[19] Given that survey data indicates offender alcohol use in a majority of incidents, it is likely underreported in these data that are instead based on law enforcement agency reports.

[20] Like linear models, the Poisson model is not subject to inconsistency caused by the incidental parameters problem associated with fixed effects. While the possibility of overdispersion is the main theoretical argument that might favor alternative models, overdispersion is corrected by calculating sandwiched standard errors (Cameron and Trivedi 2005). Moreover, the conditional fixed effects negative binomial model has been demonstrated to not be a true fixed effects model (Allison and Waterman 2002).

[21] Holiday controls include dummy variables for Labor Day, Columbus Day, Halloween, Veterans Day, Thanksgiving, Christmas, New Year's Day, New Year's Eve, and Valentines Day.

college level, c. While not shown in equation (1), we also include a single day lag and lead from game days to account for any short-run spillover effects.

Including law enforcement agency fixed effects controls for time-invariant characteristics of each police agency, and other characteristics of the local area, both of which may be related to rape victimization and the scheduling of football game days. Their inclusion ensures that the estimated effects are driven by within law enforcement agency variation over time rather than variation across agencies. This has the potential to be particularly important because NIBRS does not provide a balanced sample of agencies and because schools vary in the number and timing with which they schedule games.

We include day-of-week fixed effects in our baseline model to address the fact that most games are held on Saturdays (94 percent of those we consider), which themselves are associated with increases in partying activities. As such, our estimates should be thought of as identifying the effects of activities associated with game days, above and beyond what is expected based on the day of the week of the game, usually Saturday. It is important to note that we can separately identify the effects of Saturdays from the effects of game days because most Saturdays during the academic year do not involve football games. That said, separate identification is possible even when restricting the analysis to weeks within the football season because teams typically schedule "bye weeks" without games, and because some games are played on other days of the week. While our preferred approach uses all of the data during the academic year to achieve greater precision, we show that estimates based on this alternative approach support our main results.

Finally, our baseline model includes indicators for holidays and year fixed effects. The inclusion of the former is potentially important because holidays often are associated with systematic changes in the incidence of rape. If we did not account for these systematic changes, our estimates might be directly biased through the association between holidays and the days on which games are played, or indirectly biased through the day-of-week fixed effects because certain holidays fall on particular days of the week. The year fixed effects account for any aggregate annual variation in the number of reported incidents that potentially could be related to trends in game scheduling over time.[22]

Taken together, the control variables included in our baseline model account for potential bias driven by inherent differences across agency jurisdictions as well as spikes in sexual assault related to the day of the week, specific holidays, and the calendar year. We expand on the baseline model by progressively adding agency-by-month fixed effects, agency-by-week fixed effects, agency-by-year-by-month fixed effects, and agency-by-year-by-week fixed effects. In so doing, we control, in a flexible manner, for systematic changes in the degree of partying over the course of the year for each university. For the richest specification, which includes agency-by-year-by-week fixed effects, the estimated effects of game days are identified based on a comparison of reports to an agency on the game day to reports on other days of the same week, controlling for changes that are expected across days of the week.

[22] Notably, the number of games played by each university has grown since the 1990s.

Given the empirical strategy described thus far, we believe that there are two main challenges to interpreting β as the causal effect of game days on the incidence of sexual assault. The first challenge is that there could be spillover effects onto other areas or onto other times. Naturally, if these spillover effects are positive, then our estimates would understate the true effect. On the other hand, if the positive effects we find are driven by sharp population flows into town and there are offsetting effects in the towns from which these individuals are drawn, our estimates would overstate the true effects overall.[23] Moreover, our estimates would overstate the true effects if they are driven by changes in the timing of partying to days with football games from other possible times of the year. We address these possibilities in our empirical analyses that follow by evaluating nearby areas, the days adjacent to game days, and Saturdays during the fall without games. We find no evidence that the effects we identify in our main results are offset by reductions in nearby areas, on adjacent days, or on other Saturdays during the fall. Where they are statistically significant, these estimates always indicate positive spillover effects.

The second main interpretational challenge stems from the fact that we only observe *reports* of rape, and reports severely understate true incident rates, as discussed in the introduction. While this sort of issue is typical for studies analyzing illicit behavior, it does imply that our estimates are appropriately interpreted as reflecting the effects of game days on reports of rape. That said, we note that the Poisson model captures proportional changes in reports of rape associated with game days, not level changes. If the activities surrounding game days do not systematically change the probability that an incident is reported to a law enforcement agency as a rape, our estimates will correctly capture proportional changes in the actual incidence of rape.[24] While we cannot know for certain either way, we note that a majority of student rape victims who do not report incidents to police cite concerns about lacking proof, uncertainty about whether the incident constituted a crime, and uncertainty about the perpetrator's intent (Kilpatrick 2007). If the alcohol and other activities surrounding game days exacerbate these issues, our estimated effects on reports would understate the effects on rape.[25] As an indirect test of whether reporting propensities are affected, we analyze the probability that an incident results in arrest in our empirical analysis. This test is motivated by the notion that differential reporting of rapes could be reflected in differential arrest rates if the types of incidents reported on game days systematically differ from those reported on other days. This is obviously an imperfect test because there are other mechanisms through which arrest rates may be affected by game days. Nonetheless, these results indicate that incidents reported on game days are no more likely or less likely to result in an arrest than reports on other days.

[23] See Billings and Depkin (2011) for an analysis of the spatial displacement of crime associated with professional football and professional basketball games.

[24] For example, consider a constant rate at which incidents are reported equal to 10 percent and a baseline incident rate of 500 rapes, which implies a baseline *reported* incident rate of 50. In this scenario, a 30 percent increase in the number of incidents (150) would be reflected in a 30 percent increase in reported incidents (15).

[25] Other stated reasons for not reporting rapes to law enforcement include concerns about privacy, fear of retaliation, uncertainty about how to report, and a belief that "the incident was not serious enough."

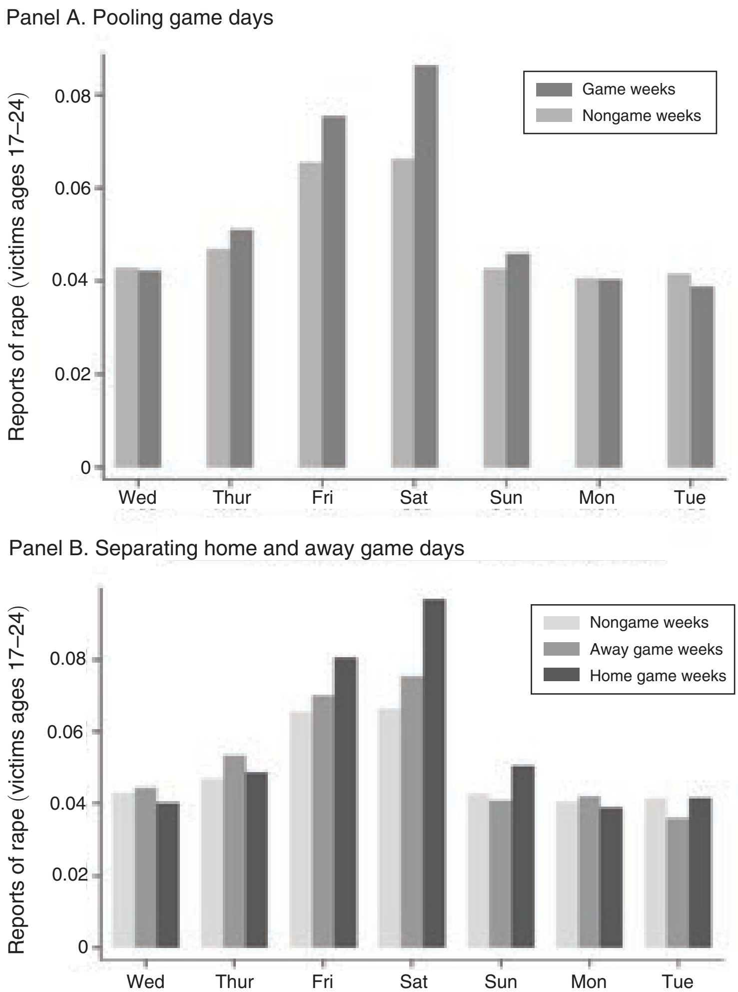

FIGURE 1. DAILY REPORTS OF RAPE PER AGENCY ON SATURDAY GAME WEEKS AND NONGAME WEEKS

Notes: This figure shows mean daily reports of rape, involving 17–24-year-old victims, to agencies that cover college campuses with a Division 1 football team. Results are shown for weeks in which the local team played a Saturday game (either home or away) and for weeks in which the local team did not play a game. For this figure, a week spans from Wednesday to Tuesday.

IV. Results

A. *Main Results*

Our analysis focuses on rapes involving college-aged (17–24-year-old) victims, but we subsequently consider effects on other age groups. Figure 1 serves as an intuitive preview to the main results. Drawing on the same data as the main analysis, it shows reports of rape per police agency across different days of the week. These daily rates are shown for weeks in which the local team played a Saturday game (either home or away) as well as for weeks in which the local team did not play a game. Weeks are defined here as spanning from Wednesday to Tuesday, so they are centered around Saturdays. The upper panel shows that reports are considerably higher on Saturday game days compared to Saturdays in weeks where there is no game. Reports are also somewhat higher for Fridays before a game day relative to other Fridays. In contrast, reports do not differ greatly between game weeks and nongame weeks on other days of the week (Wednesdays, Thursdays, Sundays, Mondays, or Tuesdays). The lower panel of Figure V considers home game weeks and away game weeks separately. The rates of reports on Saturdays are clearly highest on home game days, followed by away game days, compared with the lower rates on Saturdays in nongame weeks. The rates are also slightly higher on the days before and after Saturday home games, but not for away game weeks. For Mondays, Tuesdays, Wednesdays, and Thursdays, the rates do not differ markedly between the three series. This within-week variation in reports, and specifically how this pattern differs between game weeks and nongame weeks, is what drives our main results.

These main results are shown in Table 2. Column 1 of panel A shows estimates corresponding to equation (1), with the additional inclusion of a lag and a lead on *Gameday*. This baseline model controls for law enforcement agency fixed effects, day of week fixed effects, and holidays in order to address the potential concerns described earlier. Columns 2–5 show results from models with progressively more flexible fixed effects to account for systematic changes in the incidence of rape across the year that are agency-specific.[26] While we focus our discussion and subsequent analyses on the model corresponding to column 5, which includes agency-by-year-by-week fixed effects, the estimates vary little across these specifications. The results suggest that football games increase reports of college-aged rape victimization by 28 percent on game days in the law enforcement agency jurisdiction areas that include the teams that played.[27] In each specification, the estimates are highly significant. There is also evidence of a significant but smaller lead effect—that is, reports of rape also are elevated on the day before a game, by approximately 11 percent.

As we discussed above, games may have heterogeneous effects on the incidence of rape for many reasons, but we have especially strong reasons to expect that home games and away games will have different effects. Perhaps most importantly, home games allow many students to attend, they can involve a great deal of tailgating, and they are generally more salient. Additionally, changes in the incidence rate could be driven by the inflow, or less than usual outflow, of potential victims and/or perpetrators to the area for the game. Acknowledging that we cannot separate out these mechanisms (though we will analyze spatial spillovers below), we show the estimated effects of home and away games in panel B of Table 2, based on the same models as panel A but replacing the *Gameday* indicator, its lead, and its lag, with indicators corresponding to home games and away games. These estimates indicate that rape victimization is elevated by 41 percent on home game days and 15 percent on away game days. There is also evidence of significant effects on the day before (19 percent) and the day after (13 percent) home games, but not away games.

[26] We use the same sample in each specification, but note that the reported number of schools and agencies changes across columns. Whenever the model perfectly predicts zeros for particular observations, those observations do not contribute to identification, and the Stata module we are running drops these from the sample prior to estimation. This varies in particular according to the set of fixed effects that are included.

[27] Percent effects are calculated as $(e^{\beta} - 1) \times 100\%$.

Table 2—Estimated Effects of Game Days on Reports of Rape

	(1)	(2)	(3)	(4)	(5)
Panel A. Pooling the effects of home and away games					
Day before game	0.154	0.118	0.102	0.112	0.107
	(0.049)	(0.052)	(0.051)	(0.055)	(0.051)
Game day	0.283	0.250	0.235	0.245	0.247
	(0.045)	(0.047)	(0.048)	(0.048)	(0.052)
Day after game	0.080	0.049	0.039	0.039	0.036
	(0.044)	(0.046)	(0.046)	(0.048)	(0.047)
Schools	96	96	96	96	96
Agencies	138	138	138	138	138
Observations	422,308	370,583	273,919	176,281	77,191
Day-of-week fixed effects	Yes	Yes	Yes	Yes	Yes
Holiday controls	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	–	–
Agency fixed effects	Yes	–	–	–	–
Agency by month of year fixed effects	No	Yes	–	–	–
Agency by week of year fixed effects	No	No	Yes	–	–
Agency-by-year-by-month fixed effects	No	No	No	Yes	–
Agency-by-year-by-week fixed effects	No	No	No	No	Yes
Panel B. Separately considering effects of home and away games					
Day before home game	0.209	0.171	0.151	0.174	0.178
	(0.065)	(0.066)	(0.065)	(0.069)	(0.067)
Home game day	0.367	0.333	0.317	0.339	0.343
	(0.054)	(0.054)	(0.055)	(0.056)	(0.069)
Day after home game	0.169	0.135	0.129	0.137	0.125
	(0.051)	(0.053)	(0.054)	(0.057)	(0.057)
Day before away game	0.091	0.057	0.047	0.043	0.029
	(0.050)	(0.056)	(0.057)	(0.056)	(0.058)
Away game day	0.181	0.150	0.138	0.135	0.136
	(0.048)	(0.053)	(0.054)	(0.052)	(0.054)
Day after away game	−0.027	−0.054	−0.067	−0.076	−0.070
	(0.063)	(0.064)	(0.063)	(0.064)	(0.070)
Schools	96	96	96	96	96
Agencies	138	138	138	138	138
Observations	422,308	370,583	273,919	176,281	77,191
Day-of-week fixed effects	Yes	Yes	Yes	Yes	Yes
Holiday controls	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	–	–
Agency fixed effects	Yes	–	–	–	–
Agency by month of year fixed effects	No	Yes	–	–	–
Agency by week of year fixed effects	No	No	Yes	–	–
Agency-by-year-by-month fixed effects	No	No	No	Yes	–
Agency-by-year-by-week fixed effects	No	No	No	No	Yes

Notes: Estimates are based on Poisson models using daily data (excluding June, July, and August) spanning 1991–2012 for law enforcement agencies participating in the National Incident Based Reporting System that have been matched to universities participating in Division 1 football. The outcome variable is the reported number of 17–24-year-old rape victims for a given agency on a given day. Days are redefined to span from 6:00 AM to 5:59 AM to accommodate the fact that parties often extend past midnight. Standard error estimates are clustered at the university level.

In the online Appendix, Figures A2 and A3 show residual plots which correspond to each of the ten versions of the analysis shown in Table 2. These figures are the same as Figure V in that they depict how reports of rape vary across the different days of the week for weeks with Saturday games and weeks without games, but these figures show residualized reports of rape based on the different sets of control variables used across the columns of Table 2: panel A plots residuals from a Poisson model with agency fixed effects, year fixed effects, and holiday controls (the control variables used in column 1 of Table 2 minus the day-of-week fixed effects); panel B plots residuals from a Poisson model agency with agency-by-month-of-year fixed effects, year fixed effects, and holiday controls (the control variables used in column 2 of Table 2 minus the day-of-week fixed effects); etc. Across the five panels of each figure, the pattern is extremely similar, which is consistent with the fact that the results in Table 2 do not vary much across columns. The pattern is also consistent with Table 2 in indicating that reports of rape are especially high on Saturdays with game days, relative to other days of the same week and relative to Saturdays without football games.

In order to better understand when the additional rapes are reported to have occurred, which may be useful for understanding mechanisms, Figure 2 presents a more detailed examination of the effects we documented previously. Specifically, this figure is based on an analysis of *hourly* (instead of daily) reports to agencies from 2001 forward, which corresponds to the availability of game times from ESPN.com. Estimates are based on an augmented version of the richest specification considered in Table 2 that distinguishes between hours before, during, and after a game on the day of the game, and distinguishes between day and night the day before and the day after a game.[28] The model is also modified to control for day-of-week-by-hour-of-day fixed effects instead of day-of-week fixed effects. The results of this analysis reveal significant effects on reports of rape during home games, as well as after home games on the same day and during the night before. For away games we only see significant effects after the game on the same day. This evidence is consistent with there being an effect of pregame partying for home games, but not away games.[29]

B. *Testing Threats to Validity*

In this section, we consider whether there are spillover or displacement effects across space and across time, the sensitivity of our main results to the dates used in the analysis, and whether there is any evidence that the estimates are driven by changes in reporting.

The estimated effects of game days on reports of rape to campus and municipal agencies serving students require careful interpretation and scrutiny given

[28] As before, we define days as spanning from 6:00AM to 5:59AM. For this analysis we consider 6:00AM to 9:59PM as day and 10:00PM to 5:59AM as night. Because we have data on game start times and not game end times, we assume that games last three hours.

[29] In a series of related analyses, we have also investigated whether the time of day a game is played, or the day of the week on which it is played alters the magnitude of the effect. We do not report the results of these analyses due to imprecision.

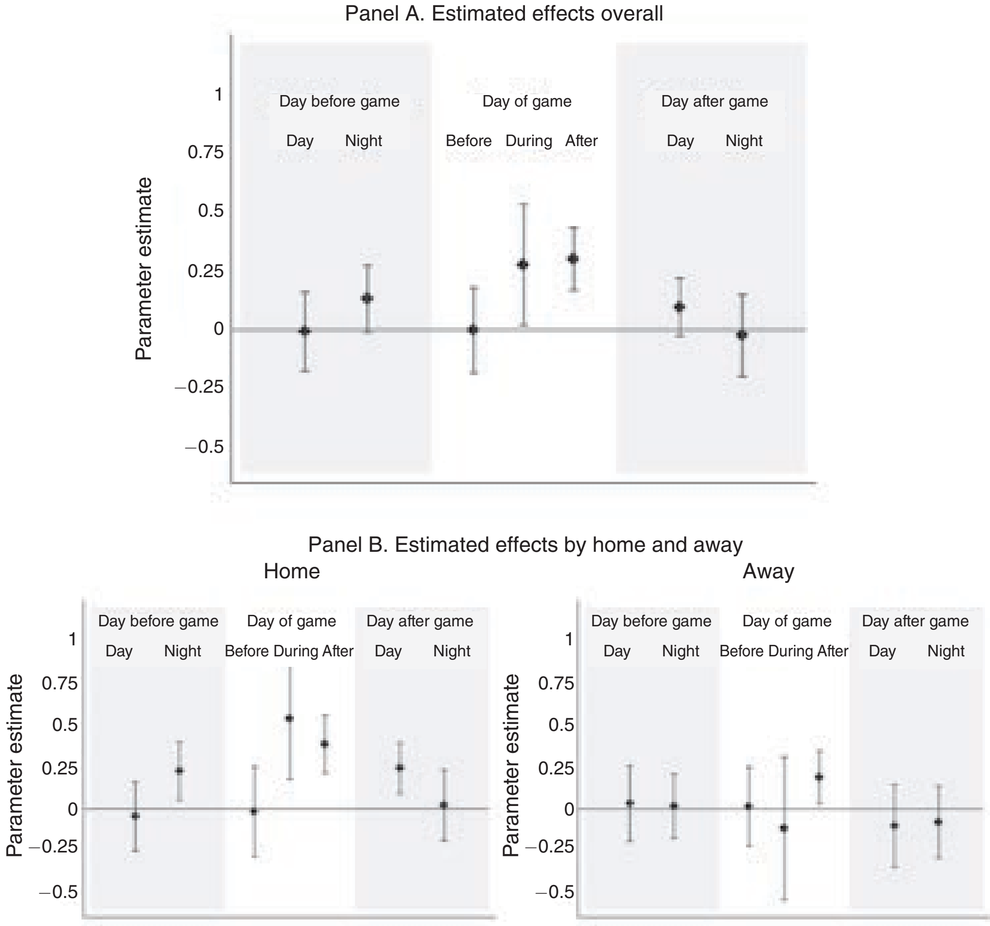

Figure 2. Estimated Effects by Time of Day

Notes: This figure shows estimates and 95 percent confidence intervals from Poisson models using hourly data (excluding June, July, and August) spanning 2001–2012 for law enforcement agencies participating in the National Incident Based Reporting System that have been matched to universities participating in Division 1 football and to game times (available beginning in 2001). The outcome variable is the reported number of 17–24-year-old rape victims for a given agency on a given day in a given hour. In addition to the variables highlighted in the graphs, the models include holiday fixed effects, agency-by-year-by-week fixed effects, and day-of-week-by-hour-of-day fixed effects. The estimates in panel A are based on a model that doesn't distinguish between home and away games, whereas the estimates in panel B are based on a model that allows the effects to differ for home and away games. Days are redefined to span from 6:00 AM to 5:59 AM to accommodate the fact that parties often extend past midnight. Games are assumed to last three hours. Standard error estimates are clustered at the university level.

the possibility of spatial displacement and spillover effects. It is possible that the increases in reports of rape to these agencies are offset by reductions in other areas due to population flows around game days, especially for home games. Alternatively, viewership-associated partying may also increase reports of rape in nearby areas, which would cause our main results to understate the effects overall. To examine these possibilities, we estimate the effects on reports of rape to agencies that are in close proximity to Division 1 schools, excluding the campus and municipal agencies evaluated in our main results. For this analysis, we consider

Table 3—Estimated Effects on Reports of Rape for Nearby Municipalities

Schools:	D1A	D1A	All D1	All D1
Radius:	25 mi	50 mi	25 mi	50 mi
	(1)	(2)	(3)	(4)
Day before home game	−0.000	0.093	0.056	0.134
	(0.101)	(0.072)	(0.086)	(0.061)
Home game day	−0.090	0.005	0.019	0.043
	(0.123)	(0.078)	(0.086)	(0.066)
Day after home game	0.015	−0.064	0.030	−0.054
	(0.124)	(0.095)	(0.100)	(0.077)
Day before away game	0.076	0.015	0.031	0.024
	(0.090)	(0.076)	(0.082)	(0.064)
Away game day	0.006	−0.037	0.040	0.003
	(0.119)	(0.082)	(0.089)	(0.063)
Day after away game	0.189	0.157	0.043	0.069
	(0.115)	(0.086)	(0.097)	(0.071)
Schools	58	76	101	129
Agencies	488	951	767	1,469
Observations	38,968	77,216	70,821	126,810

Notes: All analyses exclude campus agencies and municipal agencies in the same city as a D1 school. Column 1 focuses on agencies within 25 miles of a single D1A program. Column 2 additionally considers agencies within 50 miles of a single D1A program. Column 3 considers agencies within 25 miles of a single D1A program and otherwise excluded agencies within 25 miles of a single D1B school. Column 4 includes agencies within 25 miles of a single D1A school, otherwise excluded agencies within 50 miles of a single D1A program, otherwise excluded agencies within 25 miles of a single D1B program, and otherwise excluded agencies within 50 miles of a single D1B program. These estimates consider reports of 17–24-year-old rape victims using the same Poisson model as column 5 of Table 2 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls in addition to one-day leads and lags from home game days and away game days). See Table 2 for additional details. Standard error estimates are clustered on universities based on the universities to which each agency has been assigned.

agencies in close proximity to the full set of schools participating in Division 1 football, as opposed to those near the schools that we are able to consider in our main analyses, which allows us to expand the number of agencies included in the analysis by nearly 50 percent.[30]

The other decisions we make for this analysis are motivated by a desire to detect effects if they exist. We begin by focusing on agencies in cities that are within 25 miles of a single D1A school. This allows us to define the treatment variables in a straightforward manner and to reduce the possibility that the estimates are confounded or attenuated by games played by other nearby schools. The results of this analysis are shown in column 1 of Table 3. In column 2, we investigate a larger set of agencies by additionally incorporating agencies if they are within 50 miles of a single D1A

[30] As we would expect, estimates based on the more restricted set of agencies surrounding the schools involved in our main analysis are less precise. However, they do provide some stronger evidence of effects on reports of rape than the estimates based on the full set of schools. That said, the statistically significant estimates are concentrated on the indicator for the day after an away game where an effect seems implausible because there is no apparent effect on the day of an away game for these agencies and our prior analyses showed no effect the day after an away game for campus and municipal agencies serving these schools. As such, we are inclined to view those significant estimates as a statistical aberration.

school.[31] Column 3 takes an alternative approach to expanding on the set of agencies considered in column 1 by additionally incorporating agencies within 25 miles of a single D1B program.[32] Lastly, column 4 expands on the set of agencies considered in column 1 by expanding both the radius and by including agencies close to Division 1B schools. Specifically, column 4 considers agencies within 25 miles of a single D1A school, otherwise excluded agencies within 50 miles of a single D1A program, otherwise excluded agencies within 25 miles of a single D1B program, and otherwise unassigned agencies within 50 miles of a single D1B program. Across these four columns there is no consistent evidence of effects on agencies in cities that are in close proximity to Division 1 schools. The estimates tend to be close to zero and are not statistically significant any more often than one would expect from random chance when testing multiple hypotheses. Moreover, those estimates that are statistically significant are not consistent across specifications. We interpret the results of these analyses as evidence that there are no displacement or spillover effects, or that any such effects are likely to be offsetting one another or to be small. As such, spatial displacement is not a candidate to explain our main results. We also note that these results are consistent with the idea that the effects are concentrated on students attending the schools, though future research using alternative sources of data will be necessary to further investigate this possibility.

We now consider the possibility of temporal displacement. Our main results suggested there are no short-term temporal displacement effects, as home games *increase* reports of rape the day before and the day after the game, while away games only have significant effects the day of the game. However, it is possible that the increases in reports of rapes occurring on these days are offset by reductions on other days of the year. While there is no way to completely rule out this possibility, we can investigate whether there are offsetting effects on other Saturdays during the football season by estimating an augmented version of our empirical model that considers whether reports systematically deviate from their expected levels on Saturdays within the football season without games. We do so by estimating our richest model (with agency-by-year-by-week fixed effects, day of week fixed effects, and holiday controls) and including in the model an indicator for "within-season Saturday without a football game" along with its one day lead and lag. We omit game days, along with their one day lead and lag, from this analysis. The results, shown in column 1 of Table 4, indicate that Saturdays without football games during the football season and Saturdays during the rest of the academic year do not differ with respect to proportional changes in reports of rape. This suggests that the effects of football games on reports of rape that we identify in our main results are not offset by opposite-signed effects on Saturdays during the season without games. As shown in column 2, this result is robust to only considering nongame day Saturdays within the season during bye weeks, which we do by setting the indicator equal to one only for Saturdays of weeks in which there is no game at all.

[31] Note that this approach implies that schools within 25 miles of one school and within 50 miles of another will be included in the analysis and the agency will be assigned the closer school.

[32] Note that this approach implies that agencies within 25 miles of a single D1A school and a single D1B school will be included in the analysis, and that the agency will be assigned to the D1A school.

Table 4—Are There Offsetting Effects on In-Season Saturdays without Games?

	All in-season Saturdays (1)	Only bye weeks (2)
Day before	−0.043	−0.027
	(0.113)	(0.117)
Within-season Saturday without football game	−0.027	−0.023
	(0.075)	(0.076)
Day after	−0.017	0.009
	(0.116)	(0.123)
Schools	94	94
Agencies	136	136
Observations	55,107	55,107

Notes: These estimates consider the same outcome (reports of 17–24-year-old rape victims to an agency on a given day) using the same Poisson model as column 5 of Table 2 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls). See Table 2 for additional details. Column 1 examines "effects" for all Saturdays without games that fall between the first and last regular season games played by a school (plus or minus six days). Column 2 only estimates the "effects" of such Saturdays when the team did not play on Thursday, Friday, or Sunday, which would typically be considered a "bye week." Actual game days (and the day before and after) are removed from the data for this exercise.

As we discussed above, our main results are based on models that exploit within agency-week variation while controlling for day-of-week fixed effects, and which use data spanning from September through May to approximate the academic year. As such, their validity relies on the assumption that day-of-week effects are the same during weeks with football games as during the other weeks of the year included in the analysis. Although it may sacrifice precision, we can relax this assumption by instead focusing on a more narrow window of dates around the football season. We present the results of doing so in Table A3 in the online Appendix. These results show that the estimates are quite similar if we restrict the sample to dates in the fall (September through December) or to dates within a six-day window around each school's regular season of football. Online Appendix Table A3 also shows results of analyses excluding specific months *during* the football season, which suggests that the effects do not vary across different times during the fall semester.

As a final test relating to the interpretation of our main results, we examine whether the probability of arrest differs for reports of rapes occurring on game days and reports of rapes occurring on other days. We do so in an attempt to indirectly test whether the types of incidents reported on game days are systematically different from those reported on other days, which might be taken as evidence that football games alter the probability that incidents are reported to the police independent of the effects on incidents taking place. At the same time, we note that other factors could contribute to systematic differences in arrest rates. The estimates, shown in Table A4 in the online Appendix, are close to zero and are never statistically significant, which supports the notion that the incidents reported on game days (and the day before and the day after) are no more likely or less likely to result in an arrest than reports on other days. At the same time, we note that only 12 percent of the reported incidents in the analysis result in arrest and thus the estimates are not precise enough to rule out fairly large effects.

C. *Who Are the Victims and Perpetrators?*

Now we consider in greater detail the types of rape offenses that are induced by college football games. We consider heterogeneity of the estimated effects by victim characteristics, offender characteristics, the relationships between victims and offenders, and the role of alcohol.[33] We hypothesize that college football games increase rapes primarily because of their role in campus social life, specifically the college party culture. Thus, we expect the effects to be larger for offenses with college-aged victims and offenders. And we expect alcohol to be an important factor. In the results that follow, we show estimates based on the richest empirical model described above, which includes agency-by-year-by-week fixed effects, applied to daily agency-level data. Only the dependent variable differs across specifications, in each case considering a different subset of rape offenses. As in the main results, the estimated models include a one-day lag and lead although we do not show their estimated coefficients for brevity.

Panel A of Table 5 shows the estimated effects by victims age in four-year groups for ages 17–28 and an "over 28" category. These results support our first hypothesis. For both home and away games, the estimated effect is largest for the 17–20 and 21–24-year-old victim groups. We also note that the magnitude appears similar across these two groups despite the fact that only the latter group can legally consume alcohol. We also find some evidence of an effect of home games on reports of 25–28-year-old victims (significant at the 10 percent level). The estimates for over 28-year-olds are close to zero.[34] In addition to shedding further light on the characteristics of the individuals who are at elevated risk of rape victimization on game days, these results support the idea that the effects are concentrated on students, though future research using alternative sources of data that distinguish between students and nonstudents will be necessary to further investigate this possibility.

Panel B of Table 5 again focuses on reported offenses with college-aged (17–24) victims and now considers heterogeneity across various offender characteristics. Columns 1 and 2 show the estimated effects on reports of rape involving college-aged offenders and noncollege-aged offenders, respectively.[35] These results highlight that the effects are particularly large for reports of rape involving college-aged offenders. The point estimates indicate that home games increase the incidence of rape involving college-aged offenders and college-aged victims by 58 percent, while away games increase the incidence by 15 percent. That said, the estimates shown in column 2 indicate that there are also (smaller) effects on reports of rape involving noncollege-aged offenders, at least for home games.[36]

[33] We do not show results by gender. However, we note that the main results are virtually identical if male victims are excluded from the analysis, which is to be expected because they only make up 4 percent of those reporting rapes in our sample. These results are available upon request.

[34] Estimates for 13–16-year-olds (not shown) are also close to zero and statistically insignificant.

[35] Fifteen percent of offenses are excluded due to missing offender age.

[36] In results not shown but available upon request, we have separately considered the effects on reports of incidents involving narrower age groups of offenders. The results of this analysis indicated that home and away games have similar effects on incidents involving 17–20-year-old and 21–24-year-old offenders, but that home games have a greater effect on reports of offenses involving 21–24-year-old offenders. We found no systematic evidence suggesting that any particular age group is driving the estimated effects on incidents involving non-college-aged offenders.

TABLE 5—EFFECTS ON REPORTS INVOLVING DIFFERENT SUBSETS OF VICTIMS AND OFFENDERS

	Ages 17–20 (1)	Ages 21–24 (2)	Ages 25–28 (3)	Ages > 28 (4)
Panel A. Effects for victims in different age groups				
Home game day	0.320	0.378	0.212	0.036
	(0.079)	(0.106)	(0.124)	(0.063)
Away game day	0.139	0.127	−0.007	−0.014
	(0.070)	(0.104)	(0.146)	(0.063)
Schools	95	89	74	90
Agencies	137	117	87	111
Observations	54,619	38,004	24,000	71,379
	Offender's age		Relationship to victim	
	17–24	Other	Known	Unknown
Panel B. Effects by offender types (victims ages 17–24)				
Home game day	0.461	0.204	0.251	0.476
	(0.108)	(0.092)	(0.080)	(0.102)
Away game day	0.139	0.053	0.057	0.253
	(0.080)	(0.084)	(0.077)	(0.104)
Schools	94	88	95	90
Agencies	135	119	135	126
Observations	43,169	40,392	58,009	33,761
	Black victim	White victim	Black offender	White offender
Panel C. Effects by victim and offender race				
Home game day	0.182	0.426	0.272	0.408
	(0.117)	(0.086)	(0.083)	(0.087)
Away game day	0.168	0.137	0.210	0.069
	(0.092)	(0.065)	(0.077)	(0.084)
Schools	84	92	88	90
Agencies	110	133	124	129
Observations	23,924	61,446	35,594	46,850

Notes: Panel A estimates consider reports of rape victims in different age groups. Panel B estimates consider reports of 17–24-year-old rape victims involving various offender characteristics. Panel C estimates also focus on reports of 17–24-year-old rape, while separately considering reports involving victims and offenders of different races. The estimates are based on the same Poisson model as column 5 of Table 2 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls in addition to one-day leads and lags from the game day). See Table 2 for additional details.

Columns 3 and 4 of panel B (Table 5) show a summary of results by victim-offender relationship. Column 3 shows the estimated effects on reports of rape in which the offender was known to the victim, which account for 63 percent of reports. In the majority (69 percent) of cases with known offenders, the offender was an "acquaintance" or "friend" of the victim. Column 4 shows results for cases where the offender is not known to the victim, or whose identity was not recorded. These estimates suggest that the effects are considerably larger for reports of rape in which the offender is unknown. In particular, they indicate that home games increase reports of rape involving unknown offenders by 61 percent; away games increase such reports by 29 percent. In contrast, these estimates indicate that home games increase reports of rape involving *known* offenders by 28 percent and away games increase such reports by 5 percent, although the latter is not statistically significant.

We also estimated models in which the offender was identified as having used alcohol or not.[37] These results (not shown, but available on request) provide suggestive evidence that football games have an especially large effect on reports of alcohol-related rapes. That said, these results should be interpreted with caution, because the activities involved with game days could affect the probability that alcohol use is *recorded* on a report, and on whether alcohol is involved independent of its effect on rape. We also note that offender alcohol use is likely underreported in these data, because they indicate offender alcohol use in less than 20 percent of incidents, whereas survey data indicates offender alcohol use in a majority of incidents.

Panel C of Table 5 shows results by race of victim and perpetrator. The main takeaway from these results is that the effects are not driven solely by white victims or black victims, nor by white offenders or black offenders. We tend to find significant effects for each category, and the estimates are not routinely greater for one group or the other.

D. *Are the Effects Larger for Prominent Teams and for Prominent Games?*

Our focus on universities with Division 1 football teams is motivated by the idea that football games played by these universities are more prominent, generate more interest, and have larger effects on partying than games played by schools with lower division teams. Consistent with this reasoning, we expect the reduced-form relationship between game days and reports of rape offenses to vary with team prominence. And in a similar spirit, we expect larger effects for particularly important games. Here we explore these ideas with proxies for team and game prominence, focusing again on estimates from the model that include one-day leads and lags of game days, agency-year-week fixed effects, day-of-week fixed effects, and holiday controls. We again restrict our attention to reports involving 17–24-year-old victims.

All other results shown in this paper are from models that pool together all universities with Division 1 football programs, but panel A of Table 6 shows the results separately for universities in subdivisions 1A (presently called the FBS) and 1AA (presently called the FCS) and universities with Division 2 and Division 3 football programs. Division 1A is the highest level of college football, followed by Division 1AA, Division 2, and Division 3. Universities playing in higher (sub) divisions tend to attract more highly touted players, have more players drafted to the National Football League, offer more scholarships to players, have larger budgets and stadiums, are more likely to have games televised, etc. Data collected by *USA Today* and the Knight Commission on Intercollegiate Athletics indicate that median spending by football programs in Division 1A was \$14 million in 2013 versus \$3 million for football programs in Division 1AA. This amounts to approximately \$118,000 per player for Division 1A programs and only \$31,000 per player for Division 1AA programs.[38]

[37] Similar information corresponding to the victim is not included in the data.

[38] These numbers include the cost of scholarships. *USA Today* and the Knight Commission on Intercollegiate Athletics provide more detailed statistics, including numbers for individual universities, at http://spendingdatabase.knightcommission.org/reports.

Table 6—Effects by Team and Game Prominence

	All D1 (1)	D1-A (2)	D1-AA (3)	D2 + D3 (4)	D2 (5)	D3 (6)
Panel A. Effects by division of football program						
Home game day	0.343	0.355	0.268	0.170	0.100	0.228
	(0.069)	(0.076)	(0.153)	(0.122)	(0.163)	(0.161)
Away game day	0.136	0.164	−0.019	−0.073	−0.057	−0.087
	(0.054)	(0.057)	(0.135)	(0.103)	(0.183)	(0.112)
Schools	96	55	41	118	52	66
Agencies	138	89	49	124	56	68
Observations	77,191	57,996	19,195	27,663	12,722	14,941
	Rival	Non-rival	Ranked opponent	Unranked opponent	Listed TV coverage	No listed TV coverage
Panel B. Effects by game prominence (Division 1)						
Home game day	0.602	0.293	0.443	0.258	0.348	0.388
	(0.114)	(0.075)	(0.111)	(0.092)	(0.091)	(0.107)
Away game day	0.156	0.132	0.163	0.103	0.217	0.112
	(0.132)	(0.065)	(0.080)	(0.085)	(0.087)	(0.086)
Schools	96	96	96	96	85	85
Agencies	138	138	138	138	124	124
Observations	77,191	77,191	77,191	77,191	62,315	62,315

Notes: Panel A estimates consider effects across the two subdivisions of Division 1 and in divisions outside of Division 1. Panel B estimates consider heterogeneous effects of different types of games played by schools participating in Division 1. The estimates in panel B columns 1 and 2 are estimated using a single model that includes game day interactions with rivals and non-rivals. The same is true in columns 3 and 4 for the interacted estimates of ranked and unranked teams, and in columns 5 and 6 when estimating interacted effects on games with and without ESPN-listed TV coverage. The rivals used for each school are listed in Table A1 in the online Appendix. Ranked teams are defined as those in the top 50 of the ten-year ranking described in the text. We note that ESPN-listed television coverage data does not include local coverage. The estimates are based on the same Poisson model as column 5 of Table 2 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls in addition to one-day leads and lags from the game day). See Table 2 for additional details.

These results (panel A of Table 6) provide strong statistical evidence of elevated reports of rape to local law enforcement agencies associated with both home and away games played by universities in Division 1A. The point estimates indicate that home games played by these universities increase rape reports by 41 percent, while away games increase rape reports by 18 percent. There are smaller effects for games played by schools with Division 1AA teams: home games increase reports of rape by 31 percent, while away games have no impact on the reported incidence of rape. These results are consistent with the notion that football games are less prominent at universities with Division 1AA teams than at universities with Division 1A teams and that Division 1AA games are less likely to be televised. However, we note that the estimates focusing on universities with Division 1AA teams have relatively large standard errors.

Again, while we acknowledge relatively large standard errors, the estimated effects of games played by Division 2 and Division 3 teams are never statistically significant, whether they are pooled together or considered separately.

Panel B of Table 6 presents the results of our analysis of whether the effects are larger for relatively prominent games played by Division 1 teams. For this analysis, we replace the home and away day-of-game indicator variables with their respective interactions with measures of game prominence: columns 1 and 2 show results from a model that estimates the effects of games against rival opponents and games against non-rival opponents; columns 3 and 4 show results from a model considering the effects of games against traditionally strong teams, as indicated by their being in the top 50 of the ten-year ranking described in Section II, and the effects of games against other teams; columns 5 and 6 show results from a model separately considering the effects of games for which ESPN.com lists television coverage or not. This latter analysis only uses data after 2001 to correspond with the availability of the ESPN.com data. As discussed in Section II, the "ESPN-listed television coverage" indicator should be thought of as a proxy for game prominence and for television access to view a game—it does not appear to reliably measure local television coverage.

The results shown in columns 1–4 support the notion that prominent games—as measured by team rivalries and games against ranked opponents—have especially large effects on reports of rape. Moreover, the differences relative to "normal games" are particularly pronounced for home games. However, the point estimates shown in columns 5 and 6 do not suggest any meaningful difference in the effects of home games with and without ESPN-listed television coverage. This could be taken as evidence that prominent games do not have larger effects than normal games, but we note that these estimates are relatively imprecise. Furthermore, the estimated effect of away games with ESPN-listed television coverage is larger than the estimated effect of away games without ESPN-listed television coverage, and the former is statistically significant, while the latter is not.[39]

E. *Estimated Effects on "Party Schools" and on Proxies for Excessive Partying*

Because we motivated our study as an opportunity to address the effects of elevated levels of partying and alcohol consumption on the incidence of rape, we now consider whether the effects are systematically different for schools considered "party schools," and then document the link between football games and measures of excessive partying.

In order to classify schools as "party schools" we use the Princeton Review Top Party School Rankings, which are based on student surveys. The Princeton Review reports that schools on this list are "those at which surveyed students' answers indicated a combination of low personal daily study hours (outside of class), high usages of alcohol and drugs on campus, and high popularity on campus for frats/sororities." For our analysis we consider a school a party school if has appeared in the Top Party School Rankings at least once.[40] The results of our analysis of the effects of football games on reports of rape at "party schools" and "nonparty schools" are shown in panel A of Table 7. The estimates suggest that the effects (for

[39] Unfortunately, there are only 38 games in our sample listed as having been televised on one of the "Big Four" stations (ABC, CBS, NBC, and Fox), making a richer analysis of televised game prominence infeasible.

[40] We used the 20-school list published in each year from 2001 to 2012, as well as partial data from earlier years (the top ten party schools from 1995–1997 and 1999, and the top five from 1998). These data were obtained from various websites and news articles on the internet. Sixteen of the schools in our data are coded as party schools, all but one of which are D1A schools. These schools are highlighted in Table A1 in the online Appendix.

Table 7—Effects on "Party Schools" and Proxies for Excessive Partying

	All schools (1)	Party schools (2)	Nonparty schools (3)	D1A party (4)	D1A nonparty (5)
Panel A. Effects on reports of rape at "party schools"					
Home game day	0.343	0.534	0.266	0.535	0.263
	(0.069)	(0.137)	(0.077)	(0.137)	(0.089)
Away game day	0.136	0.292	0.084	0.295	0.110
	(0.054)	(0.142)	(0.054)	(0.142)	(0.054)
Schools	96	16	80	15	40
Agencies	138	30	108	29	60
Observations	77,191	17,346	59,845	17,311	40,685
	All	Disorderly conduct	DUI	Drunkenness	Liquor law violations
Panel B. Effects on crimes related to excessive partying					
Home game and day after	0.587	0.434	0.188	0.628	0.708
	(0.080)	(0.085)	(0.041)	(0.137)	(0.083)
Away game and day after	0.124	0.153	0.095	0.109	0.104
	(0.029)	(0.047)	(0.026)	(0.045)	(0.038)
Schools	96	94	92	68	95
Agencies	141	136	133	97	137
Observations	291,806	144,995	182,494	112,984	199,402

Notes: Panel A estimates consider reports of 17–24-year-old rape victims by "party school" classification, which is based on the 20-school list in the Princeton Top Party School Rankings published each year from 2001 to 2012, as well as partial data from earlier years (the top 10 party schools from 1995–1997 and 1999, and the top 5 from 1998). Those schools defined as a "party school" based on this measure are highlighted in the full list of schools in Table A1 in the online Appendix. Panel B estimates consider 17–24-year-olds arrested for crimes related to excessive partying. Because these arrest data do not include the time of the incident, we cannot redefine days to span 6:00 AM to 5:59 AM for this analysis as we have throughout the analysis of rape incidence. We instead estimate the effect on the day of the game and the day after to accommodate the fact that parties often extend past midnight. The estimates are based on the same Poisson model as column 5 of Table 2 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls in addition to one-day leads and lags from the game day). See Table 2 for additional details.

both home and away games) are indeed larger for party schools, whether or not we restrict the sample to D1A or all D1 schools. Specifically, the point estimates indicate that home games increase reports of rape by 70 percent on the day of the game for party schools, versus 30 percent at other schools, while away games increase reports of rape by 34 percent on the day of the game for party schools, versus an estimated effect of 12 percent at other Division 1A schools.

In order to document the link between football games and excessive partying, we focus on four categories of crime offenses (committed by 17–24-year-old offenders) recorded in the National Incident Based Reporting System: disorderly conduct; driving under the influence; drunkenness; and liquor offenses. These crimes are categorized in NIBRS as "Group B offenses," as opposed to rapes, which are "Group A" offenses, so this analysis necessarily considers arrests rather than all reported incidents. Another difference between Group A and Group B crimes in NIBRS is that the time of the incident is not included for the Group B crimes. Because we cannot account for the fact that parties often extend past midnight by redefining the day to span from 6:00AM to 5:59AM as we did with our analysis of rapes, our analysis of

Group B offenses estimates the combined effects on the day of the game and the subsequent day. We replace the two separate indicators for the day of the game and the day after the game with one indicator for "either the day of the game or the day after the game."[41] Otherwise, our empirical model is the same as the one that has been the focus of our preceding analyses.

The results of this analysis, shown in panel B of Table 7, provide clear evidence of large positive effects of game days on arrests for offenses related to excessive partying. They indicate that home games increase arrests for all four categories by approximately 80 percent over two days, disorderly conduct by 54 percent, DUI by 20 percent, drunkenness by 87 percent, and liquor law violations by 102 percent.[42] Consistent with the estimated effects on reports of rape, we find that away games have smaller statistically significant effects. These results are also consistent with Rees and Schnepel (2009), who also find that college football games increase the incidence of alcohol-related crimes, with especially large effects for home games, in their analysis of 26 law enforcement agencies associated with universities participating in Division 1A football.

F. *Heterogeneity by Predicted and Actual Game Outcomes*

In this subsection, we consider whether game outcomes affect reports of rape. This analysis is motivated primarily by prior findings that emotional cues—as measured by unexpected losses of National Football League (NFL) teams—precipitate family violence among residents in the team's local market area. In particular, Card and Dahl (2011) find that an unexpected or "upset" loss experienced by the local NFL team leads to a 10 percent increase in domestic violence. This analysis also is motivated by survey results in which 20–30 percent of students reported drinking more when their college football team wins (Lindo, Swensen, and Waddell 2012).[43]

Here we will ultimately follow Card and Dahl's (2011) main specification, in which additional effects of "upset" results are identified separately from the effects of games with certain predicted outcomes. The idea is that games with different predicted outcomes are likely to be systematically different from one another and game outcomes are as good as random conditional on predicted outcomes. As such, we will estimate a model that evaluates the effect of games that a team is predicted to lose, and the marginal effect of such games that end as unexpected wins. Likewise, the model will evaluate the effect of games in which the team is expected to win, and the marginal effect of those ending as unexpected losses. Finally, the model will estimate the effects of games with no clear favorite and allows the effects of these games to be different for wins and losses. Again following Card and Dahl (2011), we measure upsets using the pregame point spread calculated by Las Vegas

[41] We assume that the recorded date of arrest is the same as the date of incident for "on-view" arrests (no previous warrant or incident) and for "cited or summoned" arrests (i.e., not taken into custody). We exclude arrests where the individual was taken into custody based on a previous warrant or incident because the date of arrest may not be indicative of the date of incident.

[42] We note that a potential threat to the validity of these estimates is that arrests may be endogenous to policing efforts, which may be affected by game days.

[43] The survey focuses on non-first-year undergraduate students at the University of Oregon.

bookmakers to equilibrate betting markets. Because such spreads are usually missing for games contested by schools with Division 1B football programs, we restrict this analysis to schools with Division 1A programs. An upset loss is defined as occurring when the team predicted to win (by more than three points) loses; an upset win is defined similarly. The model otherwise follows our preferred specification, including agency-by-year-by-week fixed effects and single-day lags and leads for each game-outcome indicator.[44]

As a starting point, we begin by simply documenting the effects of games with different predicted outcomes, omitting the interactions with game outcomes. Table 8, column 1 shows the estimated effects on reports of rape and column 2 shows the estimated effects on arrests for crimes associated with excess partying. There is no clear evidence from these results that the effects of games are systematically different when a team is expected to win versus when it is expected to lose versus when it is expected to have a close match. Columns 3 and 4 show how the estimated effects of these types of games vary depending on whether the team wins or loses. Although the estimated effects of game outcomes have large standard errors and are usually not statistically significant, we note that the one game outcome associated with significant increases in arrests for alcohol-related crimes is also where we see the strongest evidence that the outcome affects reports of rape. Specifically, our estimates indicate that upset wins increase arrests for alcohol related crimes on the day of the game and the day after (jointly significant at the one percent level), while also increasing reports of rape on the day of the game and the day after (jointly significant at the 5 percent level). We do note, however, that the lead terms on the game outcomes are sometimes significant at the 10 percent level, which suggests that the results of this analysis should be viewed with some caution since game outcomes should not influence pregame behavior.

V. Discussion and Conclusion

Our results indicate that Division 1 college football games significantly increase reports of rape involving college-aged victims. The estimates are largest for rapes in which offenders are also college-aged and are unknown to the victim. The effects are also comparatively large for schools with prominent teams (those playing in Division 1A) and for prominent games (rivalry games and games against ranked teams). For away games, the effects are only statistically significant where we can verify that the game was televised. We find similar effects on crimes associated with excessive partying: disorderly conduct, DUI, drunkenness, and liquor law

[44] The regression equation is as follows:

$$
\begin{aligned}
(2) \qquad E[R_{act}] = \exp\big(&\beta_3 GamedayExpectLoss_{ct} \times Win_{ct} + \beta_2 GamedayExpectClose_{ct} \times Win_{ct} \\
&+ \beta_1 GamedayExpectWin_{ct} \times Lost_{ct} + \beta_6 GamedayExpectLoss_{ct} \\
&+ \beta_5 GamedayExpectClose_{ct} + \beta_4 GamedayExpectWin_{ct} + \gamma \mathbf{X}_{act}\big),
\end{aligned}
$$

where $\gamma \mathbf{X}_{act}$ includes agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls. Whilst not shown, one day leads and lags on each of the game-day indicators are also included.

Table 8—Estimated Effects by Favorite/Underdog Status and by Game Outcomes

	Rape (1)	Alcohol-related crimes (2)	Rape (3)	Alcohol-related crimes (4)
Day before game, expected to lose	0.164	0.188	0.150	0.164
	(0.068)	(0.033)	(0.075)	(0.041)
Game day, expected to lose	0.202	0.436	0.130	0.374
	(0.090)	(0.074)	(0.103)	(0.068)
Day after game, expected to lose	0.018	0.229	−0.054	0.199
	(0.073)	(0.033)	(0.077)	(0.032)
Day before game, expected to be close	0.081	0.254	0.073	0.248
	(0.107)	(0.075)	(0.190)	(0.068)
Game day, expected to be close	0.321	0.604	0.281	0.629
	(0.120)	(0.085)	(0.148)	(0.101)
Day after game, expected to be close	0.013	0.250	0.078	0.284
	(0.076)	(0.050)	(0.113)	(0.059)
Day before game, expected to win	0.127	0.174	0.168	0.157
	(0.087)	(0.072)	(0.097)	(0.081)
Game day, expected to win	0.302	0.611	0.313	0.621
	(0.079)	(0.120)	(0.090)	(0.126)
Day after game, expected to win	0.034	0.227	0.054	0.227
	(0.077)	(0.058)	(0.078)	(0.057)
Day before game, expected to lose and won (upset win)			0.051	0.104
			(0.190)	(0.054)
Game day, expected to lose and won (upset win)			0.321	0.270
			(0.182)	(0.067)
Day after game, expected to lose and won (upset win)			0.320	0.132
			(0.177)	(0.049)
Day before game, expected to be close and won			0.016	0.012
			(0.200)	(0.068)
Game day, expected to be close and won			0.071	−0.044
			(0.169)	(0.060)
Day after game, expected to be close and won			−0.121	−0.062
			(0.193)	(0.044)
Day before game, expected to win and lost (upset loss)			−0.271	0.098
			(0.160)	(0.076)
Game day, expected to win and lost (upset loss)			−0.062	−0.061
			(0.172)	(0.092)
Day after game, expected to win and lost (upset loss)			−0.120	0.000
			(0.221)	(0.079)
Schools	52	52	52	52
Agencies	85	88	85	88
Observations	56,810	201,013	56,810	201,013

Notes: Estimated effects on reports of rape of 17–24-year-old rape victims and on 17–24-year-olds arrested for alcohol related crimes are based on the data described in prior tables and based on a similar Poisson model as column 5 of Table 2 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls), which can be seen for additional details. Games expected to be close are those in which the betting-market spread is no greater than three points. This analysis focuses only on schools participating in Division 1A.

violations. Other pieces of evidence that support the notion that partying is likely to be the key mechanism underlying our main results include: our finding that the one game *outcome* associated with significant increases in arrests for alcohol-related crimes (upset wins) is also where we see the strongest evidence that the outcome affects reports of rape; especially large effects at schools with reputations as "party schools;" and a pattern of estimated effects on reports of rape that is consistent with there being effects of pregame partying associated with home games.

A back of the envelope calculation based on our estimates implies that the effects of Division 1A football games explain 5 percent of fall semester (September through December) reports of rape involving 17–24-year-old victims to law enforcement agencies serving students attending these schools. Moreover, based on an estimated 12 percent of student victims reporting to the police (Kilpatrick 2007) and 6 percent of police reports involving false allegations (Lisak et al. 2010), our estimates indicate that the activities surrounding Division 1A football games cause 724 additional rapes of college-aged victims per year across 128 universities.[45] Based on an estimated societal cost of $267,000 per offense (McCollister, French, and Fang 2010), these numbers imply a social cost of rapes induced by Division 1A football games of $193 million each year.[46] Back-of-the-envelope estimates for the effects of Division 1AA football games are much smaller—they suggest an additional 108 rapes per year across 125 schools, which implies a social cost of $29 million.[47]

We view the results of our analyses as having several implications for policy. Most directly, they contribute to a more complete understanding of the non-pecuniary costs associated with college football, which is relevant to ongoing debates about the merits of universities continuing to invest in Division 1 sports programs. While there are likely to be benefits to having a university's football team participate in the most competitive division and otherwise playing relatively prominent games, our results indicate that such games have especially large costs in terms of sexual violence victimization. It will be important for future research to consider the degree to which it is possible to reduce or eliminate these costs by making football games less prominent, through game day-specific policies (such as those relating to the tailgating and alcohol sales inside stadiums), or through broader university policies and initiatives (such as those relating to alcohol and sexual assault).

More indirectly, our study contributes to policy discussions by providing evidence that spikes in the degree of partying at a university increase the incidence of rape, which suggests that efforts to avoid such spikes (or to avoid such large spikes) could serve to reduce the incidence of rape. As we discussed in Section I, policies relating to partying and drinking have not featured prominently in discussions about how

[45] These calculations are based on the estimated effects the day before, the day of, and the day after a home (away) game equal to 23.4 percent (3.5 percent), 42.0 percent (17.8 percent), and 12.2 percent (−9.3 percent), respectively; baseline daily incident reports equal to 0.085, 0.084, and 0.051 for the day before, the day of, and the day after a game (the average number of reports to campus or municipal agencies on Fridays, Saturdays, and Sundays during weeks without a Saturday game); and 751 home games played in 2014; 799 away games played (including neutral-site games) in 2014.

[46] Note that we have adjusted the cost estimate reported in McCollister, French, and Fang (2010) for inflation to put the amount into 2015 dollars. These estimates of the average cost of rape are not specific to college-age victims, for whom the costs may be higher or lower. We are not aware of any comprehensive social cost calculations that are specific to college-aged victims.

[47] These calculations are based on the estimated effects the day before, the day of, and the day after a home (away) game equal to −1.0 percent (−0.1 percent), 30.7 percent (−1.9 percent), and 17.1 percent (5.1 percent), respectively; baseline daily incident reports equal to 0.040, 0.045, and 0.027 for the day before, the day of, and the day after a game (the average number of reports to campus or municipal agencies on Fridays, Saturdays, and Sundays during weeks without a Saturday game); 811 home games played in 2014; and 731 away games played (including neutral-site games) in 2014.

to reduce the incidence of rape, perhaps out of concern that ascribing an important role to such policies might serve to minimize the degree to which perpetrators are viewed as being responsible for their crimes. Nonetheless, some universities have recently implemented these types of policies in an effort to address problems of sexual violence. Specifically, in January of 2015, Brown University banned alcohol at fraternity parties (and all other events in campus residential areas) in response to sexual misconduct allegations including sexual assault and drink spiking, while Dartmouth College cited the interrelatedness between high-risk drinking and sexual assault when it banned hard liquor on campus. It will be critical for future work to assess the degree to which these sorts of policies have their desired impacts on the incidence of rape.

REFERENCES

Allison, Paul D., and Richard P. Waterman. 2002. "Fixed Effects Negative Binomial Regression Models." *Sociological Methodology* 32: 247–65.

Anderson, Michael L. 2017. "The Benefits of College Athletic Success: An Application of the Propensity Score Design with Instrumental Variables." *Review of Economics and Statistics* 99 (1): 119–34.

Anderson, D. Mark, Benjamin Crost, and Daniel I. Rees. Forthcoming. "Wet Laws, Drinking Establishments, and Violent Crime." *Economic Journal*.

Anderson, Nick, and Scott Clement. 2015. "College Sexual Assault: 1 in 5 Women were Violated." *Washington Post*, June 12.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76 (2): 169–217.

Biderman, Ciro, João M. P. De Mello, and Alexandre Schneider. 2010. "Dry Laws and Homicides: Evidence from the São Paulo Metropolitan Area." *Economic Journal* 120 (543): 157–82.

Billings, Stephen B., and Craig A. Depken, II. 2011. "Sport Events and Criminal Activity: A Spatial Analysis." In *Violence and Aggression in Sporting Contests: Economics, History, and Policy*. Sports Economics, Managements and Policy, edited by R. Todd Jewell, 175–87. New York: Springer.

Branscombe, Nyla R., and Daniel L. Wann. 1992. "Role of Identification with a Group, Arousal, Categorization Processes, and Self-Esteem in Sports Spectator Aggression." *Human Relations* 45 (10): 1013–33.

Cameron, A. Colin, and Pravin K. Trivedi. 2005. *Microeconometrics: Methods and Applications*. Cambridge: Cambridge University Press.

Card, David, and Gordon B. Dahl. 2011. "Family Violence and Football: The Effect of Unexpected Emotional Cues on Violent Behavior." *Quarterly Journal of Economics* 126 (1): 103–43.

Carpenter, Christopher S. 2005. "Heavy Alcohol Use and the Commission of Nuisance Crime: Evidence from Underage Drunk Driving Laws." *American Economic Review* 95 (2): 267–72.

Carpenter, Christopher. 2007. "Heavy Alcohol Use and Crime: Evidence from Underage Drunk-Driving Laws." *Journal of Law and Economics* 50 (3): 539–57.

Carpenter, Christopher, and Carlos Dobkin. 2011. "Alcohol Regulation and Crime." In *Controlling Crime: Strategies and Tradeoffs*, edited by Philip J. Cook, Jens Ludwig, and Justin McCrary, 291–329. Chicago: University of Chicago Press.

Carpenter, Christopher, and Carlos Dobkin. 2015. "The Minimum Legal Drinking Age and Crime." *Review of Economics and Statistics* 97 (2): 521–24.

Cataldi, Emily Forrest, Caitlin Green, Robin Henke, Terry Lew, Jennie Woo, Bryan Shepherd, and Peter Siegel. 2011. *2008–09 Baccalaureate and Beyond Longitudinal Study* (*B&B:08/09*)*: First Look*. US Department of Education and National Center for Education Statistics. Washington, DC, July.

Clotfelter, Charles T. 2011. *Big-Time Sports in American Universities*. Cambridge: Cambridge University Press.

Cook, Philip J., and Christine Piette Durrance. 2013. "The virtuous tax: Lifesaving and crime-prevention effects of the 1991 federal alcohol-tax increase." *Journal of Health Economics* 32 (1): 261–67.

Cook, Philip J., and Michael J. Moore. 1993a. "Economic Perspectives on Reducing Alcohol-Related Violence." In *Alcohol and Interpersonal Violence: Fostering Multidisciplinary Perspectives*, Vol. 24, edited by Susan E. Martin, 193–211. Rockville, MD: U.S. Department of Health and Human Services.

Cook, Philip J., and Michael J. Moore. 1993b. "Violence reduction through restrictions on alcohol availability." *Alcohol Health and Research World* 17 (2): 151–56.

DeGue, Sarah. 2014. "Evidence-Based Strategies for the Primary Prevention of Sexual Violence Perpetration." In *Preventing Sexual Violence on College Campuses: Lessons from Research and Practice*. National Center for Injury Prevention and Control and Centers for Disease Control and Prevention. Atlanta, April.

DeGue, Sarah, Linda Anne Valle, Melissa K. Holt, Greta M. Massetti, Jennifer L. Matjasko, and Andra Teten Tharp. 2014. "A systematic review of primary prevention strategies for sexual violence perpetration." *Aggression and Violent Behavior* 19 (4): 346–62.

Durrance, Christina Piette, Shelley Golden, Krista Perreira, and Philip Cook. 2011. "Taxing sin and saving lives: Can alcohol taxation reduce female homicides?" *Social Science and Medicine* 73 (1): 169–76.

Glassman, Tavis J., Virginia J. Dodd, Jiunn-Jye Sheu, Barbara A. Rienzo, and Alex C. Wagenaar. 2010. "Extreme Ritualistic Alcohol Consumption among College Students on Game Day." *Journal of American College Health* 58 (5): 413–23.

Glassman, Tavis, Chudley E. Werch, Edessa Jobli, and Hui Bian. 2007. "Alcohol-Related Fan Behavior on College Football Game Day." *Journal of American College Health* 56 (3): 255–60.

Hernández-Julián, Rey, and Kurt W. Rotthoff. 2014. "The impact of college football on academic achievement." *Economics of Education Review* 43: 141–47.

Kilpatrick, Dean G., Heidi S. Resnick, Kenneth J. Ruggiero, Lauren M. Conoscenti, and Jenna McCauley. 2007. *Drug-facilitated, Incapacitated, and Forcible Rape: A National Study*. Medical University of South Carolina National Crime Victims Research and Treatment Center, Charleston, SC, February.

Krebs, Christopher P., Christine H. Lindquist, Tara D. Warner, Bonnie S. Fisher, and Sandra L. Martin. 2009. "College Women's Experiences with Physically Forced, Alcohol- or Other Drug-Enabled, and Drug-Facilitated Sexual Assault Before and Since Entering College." *Journal of American College Health* 57 (6): 639–49.

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

Lindo, Jason M., Isaac D. Swensen, and Glen R. Waddell. 2012. "Are Big-Time Sports a Threat to Student Achievement?" *American Economic Journal: Applied Economics* 4 (4): 254–74.

Lippy, Caroline, and Sarah DeGue. 2016. "Exploring Alcohol Policy Approaches to Prevent Sexual Violence Perpetration." *Trauma, Violence, and Abuse* 17 (1): 26–42.

Lisak, David, Lori Gardinier, Sarah C. Nicksa, and Ashley M. Cote. 2010. "False Allegations of Sexual Assault: An Analysis of Ten Years of Reported Cases." *Violence Against Women* 16 (12): 1318–34.

Markowitz, Sara. 2005. "Alcohol, Drugs and Violent Crime." *International Review of Law and Economics* 25 (1): 20–44.

Markowitz, Sara, and Michael Grossman. 2000. "The effects of beer taxes on physical child abuse." *Journal of Health Economics* 19 (2): 271–82.

Massachusetts Institute of Technology. 2014. "Survey Results: 2014 Community Attitudes on Sexual Assault." web.mit.edu/surveys/health/MIT-CASA-Survey-Summary.pdf.

McCollister, Kathryn E., Michael T. French, and Hai Fang. 2010. "The cost of crime to society: New crime-specific estimates for policy and program evaluation." *Drug and Alcohol Dependence* 108 (1–2): 98–109.

Neal, Dan J., and Kim Fromme. 2007. "Event-level covariation of alcohol intoxication and behavioral risks during the first year of college." *Journal of Consulting and Clinical Psychology* 75 (2): 294–306.

Pope, Devin G., and Jaren C. Pope. 2014. "Understanding College Application Decisions: Why College Sports Success Matters." *Journal of Sports Economics* 15 (2): 107–31.

Rees, Daniel I., and Kevin T. Schnepel. 2009. "College Football Games and Crime." *Journal of Sports Economics* 10 (1): 68–87.

Wann, Daniel L. 1993. "Aggression Among Highly Identified Spectators as a Function of Their Need to Maintain Positive Social Identity." *Journal of Sport and Social Issues* 17 (2): 134–43.

White House Task Force to Protect Students from Sexual Assault. 2014. *Not Alone: The First Report of the White House Task Force to Protect Students from Sexual Assault*. Office of the Vice President and the White House Council on Women and Girls. Washington, DC, April.

This article has been cited by:

1. Eric R. Pedersen, Graham DiGuiseppi, David J. Klein, Jordan P. Davis, Coreen Farris, Elizabeth J. D'Amico, Joseph W. LaBrie, Beth Ann Griffin. 2023. Sexual violence victimization among American college students studying abroad. *Journal of American College Health* **71**:6, 1947-1956. [Crossref]
2. Ryan Bagwell, Kristina Block, Wanda E. Leal, Alex R. Piquero. 2023. The association between professional sporting events and police calls for service in San Antonio, Texas. *Journal of Crime and Justice* **3**, 1-14. [Crossref]
3. Zachary S. Fone. 2023. You Booze, You Lose? Spillovers to Crime from Alcohol Sales at College Football Games. *Journal of Sports Economics* **24**:2, 193-240. [Crossref]
4. Avanti Adhia, Alice M. Ellyson, Emily Kroshus. 2023. Prevalence and Formal Reporting of Sexual Violence among Undergraduate Student-Athletes: A Multi-State Study. *Journal of Interpersonal Violence* **38**:1-2, 418-442. [Crossref]
5. Manu Raghav, Timothy M. Diette. 2022. Greek myth or fact? The role of Greek houses in alcohol and drug violations on American campuses. *Applied Economics* **54**:55, 6406-6417. [Crossref]
6. Austin F. Eggers, Peter A. Groothuis. 2022. Party on dude, but not if you're a top academic achieving student: how being named a top party school changes the academic profile of a university. *Applied Economics* **54**:51, 5932-5942. [Crossref]
7. Eric Pedersen, Jordan P. Davis, Claude Setodji, Emily R. Dworkin, Isabel Leamon, Justin F. Hummer, Rushil Zutshi, John D. Clapp. 2022. Increased Risk for Sexual Violence Victimization on Drinking Days Involving Pregaming. *Journal of Aggression, Maltreatment & Trauma* **31**:9, 1224-1240. [Crossref]
8. Brandyn F. Churchill, Andrew Dickinson, Taylor Mackay, Joseph J. Sabia. 2022. The Effect of E-Verify Laws on Crime. *ILR Review* **75**:5, 1294-1320. [Crossref]
9. Pamela J. Trangenstein, Patrick J.D. Tiongson, Yi Lu, Sarah K. Lipson, Ziming Xuan, Timothy S. Naimi, David H. Jernigan. 2022. Gender and sexual identity and harms from others' drinking among U.S. college students: Results from a multi-campus survey. *Journal of American College Health* **3**, 1-5. [Crossref]
10. Ana P. Gantman, Elizabeth Levy Paluck. 2022. A Behavioral-Science Framework for Understanding College Campus Sexual Assault. *Perspectives on Psychological Science* **17**:4, 979-994. [Crossref]
11. Saloni Khurana, Kanika Mahajan. 2022. Public Safety for Women: Is Regulation of Social Drinking Spaces Effective?. *The Journal of Development Studies* **58**:1, 164-182. [Crossref]
12. Daria Denti, Simona Iammarino. 2022. Coming Out of the Woods. Do local support services influence the propensity to report sexual violence?. *Journal of Economic Behavior & Organization* **193**, 334-352. [Crossref]
13. Sarah A. Rogers, Baker A. Rogers. 2021. Expanding Our View: Demographic, Behavioral, and Contextual Factors in College Sexual Victimization. *Journal of Interpersonal Violence* **36**:23-24, NP13094-NP13120. [Crossref]
14. Luke Petach, Dustin Rumbaugh. 2021. Are You Ready for Some Football? Estimating the Effect of American Football Season on Labor Supply in the United States. *Journal of Sports Economics* **22**:8, 893-920. [Crossref]

15. Raphael E. Cuomo, Vidya L. Purushothaman, Jiawei Li, Cortni Bardier, Matthew Nali, Neal Shah, Nick Obradovich, Joshua Yang, Tim K. Mackey. 2021. Characterizing Self-Reported Tobacco, Vaping, and Marijuana-Related Tweets Geolocated for California College Campuses. *Frontiers in Public Health* **9**. . [Crossref]
16. M. Amelia Gibbons, Martín A. Rossi. 2021. WHEN YOU CAN'T TUBE... IMPACT OF A MAJOR YOUTUBE OUTAGE ON RAPES. *Economic Inquiry* **59**:2, 762-775. [Crossref]
17. Gaurav Khanna, Carlos Medina, Anant Nyshadham, Christian Posso, Jorge Tamayo. 2021. Job Loss, Credit, and Crime in Colombia. *American Economic Review: Insights* **3**:1, 97-114. [Abstract] [View PDF article] [PDF with links]
18. Kyosuke Kikuta, Mamoru Uesugi. 2021. The Behavioral Origins of Conflict: Professional Football in Europe and Conflicts in Africa. *SSRN Electronic Journal* **80**. . [Crossref]
19. Brett Watson, Mouhcine Guettabi, Matthew Reimer. 2020. Universal Cash and Crime. *The Review of Economics and Statistics* **102**:4, 678-689. [Crossref]
20. Anna Bindler, Nadine Ketel, Randi Hjalmarsson. Costs of Victimization 1-31. [Crossref]
21. Anna Bindler, Nadine Ketel, Randi Hjalmarsson. Costs of Victimization 1-31. [Crossref]
22. Daniel Mangrum, Paul Niekamp. 2020. College Student Contribution to Local COVID-19 Spread: Evidence from University Spring Break Timing. *SSRN Electronic Journal* . [Crossref]
23. Austin F. Eggers, Peter A. Groothuis, Parker Redding, Kurt W. Rotthoff, Michael Solimini. 2020. Universities Behaving Badly: The Impact of Athletic Malfeasance on Student Quality and Enrollment. *Journal of Sports Economics* **21**:1, 87-100. [Crossref]
24. Dustin R. White, Benjamin W. Cowan, Jadrian J. Wooten. 2019. MARCH MADNESS: NCAA TOURNAMENT PARTICIPATION AND COLLEGE ALCOHOL USE. *Contemporary Economic Policy* **37**:3, 449-461. [Crossref]
25. Heather M. Bush, Samuel C. Bell, Ann L. Coker. 2019. Measurement of Bystander Actions in Violence Intervention Evaluation: Opportunities and Challenges. *Current Epidemiology Reports* **6**:2, 208-214. [Crossref]
26. Jeff Victoroff. Why Outcomes Vary 285-382. [Crossref]
27. Ignacio Sarmiento-Barbieri, Qi Ge, Rodrigo Schneider. 2018. Emotional Cues and Crime: Spatial and Temporal Evidence from Brazilian Soccer Games. *SSRN Electronic Journal* . [Crossref]