NBER WORKING PAPER SERIES

COLLEGE PARTY CULTURE AND SEXUAL ASSAULT

Jason M. Lindo
Peter M. Siminski
Isaac D. Swensen

Working Paper 21828
http://www.nber.org/papers/w21828

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2015

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, Carly Urban for helpful comments and along with seminar and conference participants at Baylor University, Texas Tech University, the Meetings of the Southern Economic Association, the Annual Health Econometrics Workshop, and the NBER's Children's Program Meetings. The authors also thank Brenton Cooper and Sam Bondurant for excellent research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

College Party Culture and Sexual Assault
Jason M. Lindo, Peter M. Siminski, and Isaac D. Swensen
NBER Working Paper No. 21828
December 2015, October 2016
JEL No. I23,K42

ABSTRACT

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.

Jason M. Lindo
Department of Economics
Texas A&M University
4228 TAMU
College Station, TX 77843
and NBER
jlindo@econmail.tamu.edu

Peter M. Siminski
School of Accounting
Economics and Finance University of
Wollongong
NSW 2522 AUSTRALIA
siminski@uow.edu.au

Isaac D. Swensen
Department of Agricultural Economics,
and Economics
Montana State University
P.O. Box 17290
Bozeman, MT 59717
isaac.swensen@montana.edu

1 Introduction

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 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 to campus and local police departments 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 non-game 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

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

[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. (2015) 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.

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 *et al.* 2010), this implies an annual social cost of rapes caused by Division 1A games of $193M. The estimated effects for schools participating in Division 1AA are smaller, suggesting 108 additional rapes per year across 125 schools.

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 re-establish self-esteem following a

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

[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.

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 pre-game 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 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.

[6]See the Knight Commission on Intercollegiate Athletics for further discussion. For more recent work, see Anderson (2012) and Pope and Pope (2014).

[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.

2 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 House Task Force to Protect Students from Sexual Assault 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

[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% 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." Rigorously evaluation in this context is defined as randomized control trials and quasi-experimental analyses with non-immediate follow-ups.

works" category only have been shown to be effective among 6th–9th 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.

3 Data

3.1 Details of Data Construction

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

[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 8th and 9th graders in a rural North Carolina county. *Shifting Boundaries* focused on 6th and 7th 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 (2014) review of the literature on alcohol-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

perpetrators in various age 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 U.S. 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 Appendix Table A1, are the focus of our analysis. After further description of the data used in the analysis, we discuss the representativeness of these universities and agencies.

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

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 (US Department of Education 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.

[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.

[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, 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.,

(2005–2014) college football team rankings from football-sickness.com and information from a wide variety of websites to consider whether games 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 pre-game 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 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 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 borne out in many data sets 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 (2007), which finds that only 12 percent of college students experiencing a rape report it to law enforcement.

BYU) are also known to broadcast their own games, but such local coverage is not included in the ESPN list.

[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 Appendix Table A1.

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

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 4, we discuss in detail this measurement error and its implications for our analysis.

3.2 Schools included in the analysis in comparison

Panel A of Table 2 shows how the 96 universities that are matched to agencies participating in NIBRS compared to all 245 universities with Division 1 football programs between 1991 and 2012, based on data from the Integrated Postsecondary Education Data System (IPEDS) from 1991–2012 (the years of our analysis of sexual assault). Most notably, the NIBRS-matched universities tend to be larger (average enrollment of 13,228 versus 12,057), are less likely to be private (15 percent versus 26 percent), and have a higher share of white students (74 percent versus 68 percent). That said, NIBRS-matched and non-NIBRS-matched universities have similar shares of female students, similar retention rates, and students with similar SAT scores and rates of financial aid receipt.[20]

Panel B of Table 2 shows how the law-enforcement agencies whose data are used in our analysis compare to the broader set of agencies associated with universities participating in Division 1 football that can be identified in Uniform Crime Reports (UCR) data, which covers agencies serving approximately 95 percent of the U.S. population.[21] These data demonstrate that the NIBRS-matched agencies and the non-NIBRS-matched agencies are similar in composition: roughly 60 percent of agencies are municipal agencies as opposed to university agencies. NIBRS-matched agencies serve a somewhat smaller number of people than non-NIBRS matched agencies, but have the same number of annual per-capita rape reports.

Finally, Panel C of Table 2 shows how the football programs at the universities represented in our analysis compare to all Division 1 football programs, based on data from sports-reference.com. Overall, these statistics indicate that football is likely to be somewhat more prominent at the schools used in our analysis, because they have higher winning percentages (53 percent versus 51 percent), higher rates of bowl game participation (23 percent versus 20 percent), and are disproportionally in the higher tier of Division 1 (56 percent versus 51 percent). We consider the importance of such differences in our analysis, described below, by separately estimating the effects for schools in each

[20]Note that SAT math and reading scores are only available beginning in 2003; SAT writing scores are only available beginning in 2006; the full-time retention rate is only available beginning in 2003; and financial aid receipt is only available beginning in 2000.

[21]Note that Uniform Crime Reports (UCR) data are not suitable for our analysis because they are aggregated and lack details on the timing of incidents.

of the two tiers of Division 1.

4 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.[22] 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:

$$E[R_{act}|Gameday_{ct}, \theta_a, X_t] = exp(\beta Gameday_{ct} + \theta_a + \boldsymbol{\gamma X_t}) \quad (1)$$

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.[23] 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 college level, c. While not shown in Eq. (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

[22] 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).

[23] Holiday controls include dummy variables for Labor Day, Columbus Day, Halloween, Veterans Day, Thanksgiving, Christmas, New Years, New Years Eve, and Valentines Day.

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.[24]

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.

Given the empirical strategy described thus far, we believe that there are two main challenges

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

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 the 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.[25] 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.[26] 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.[27] 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.

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

[26] 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).

[27] 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."

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.

5 Results

5.1 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 non-game weeks on other days of the week (Wednesdays, Thursdays, Sundays, Mondays or Tuesdays). The lower panel of Figure 1 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 non-game 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 non-game weeks, is what drives our main results.

These main results are shown in Table 3. 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.[28] While we focus our discussion and subsequent analyses

[28]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.

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.[29] 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 3, 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.

In appendix Figures A2 and A3, we show residual plots which correspond with each of the ten versions of the analysis shown in Table 3. These figures are similar to the raw plots Figure 1, conveying that within-week variation drives the results. Specifically, the residuals are calculated as the actual count minus the number predicted by the Poisson model with the relevant controls (excluding day of week fixed effects). In both figures, the pattern is similar across panels. This reflects the fact that the results in Table 3 do not vary greatly across columns.

In the Appendix, Figures A2 and A3 show residual plots which correspond to each of the ten versions of the analysis shown in Table 3. These figures are the same as Figure 1 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 3: Panel A plots residuals from a Poisson model with agency fixed effects, year fixed effects, and holiday controls (the control variables used

This varies in particular according to the set of fixed effects that are included.

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

in Column 1 of Table 3 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 3 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 3 do not vary much across columns. The pattern is also consistent with Table 3 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 3 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.[30] 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 pre-game partying for home games, but not away games.[31]

5.2 Testing Threats to Validity

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

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 the football season as during the other months of the year included in

[30] 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.

[31] 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.

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 report the results of doing so in Table 4. Specifically, after reproducing our main results in Column (1), we show the results of an analysis that focuses only on the Fall (September through December) in Column (2) and the results of an analysis that focuses only on the regular season of football in Column (3). For the latter, the regular football season is defined for each school and year based on the team's first game (minus six days) and last regular season game (plus six days). In this analysis, the effects of game days can be identified separately from day-of-week effects because of "bye weeks" in which games are not played and because games are sometimes played on days other than Saturdays. As expected, these estimates are less precise than those based on the broader set of months of the academic year. However, they are similar in magnitude and thus provide support for our identification strategy.

We further explore the sensitivity of our main results to the dates considered in Columns (4) through (7) of Table 4. Specifically, these columns report the results of analyses excluding specific months during the football season (September, October, November, December). These results can be thought of as a robustness check—because we might be less confident in our estimates if they turned out to be driven by games played during a single month of the year—but they also allow us to indirectly consider heterogeneous effects. Across these four columns, the estimates routinely indicate that home games increase reports of rape by 35–46 percent on the day of the game, 15–25 percent the day before the game, and 11–18 percent the day after the game. Away games increase reports of rape by 12–17 percent on the day of the game. These estimates are not systematically larger or smaller when earlier or later months of the football season are omitted from the analysis, suggesting that the effects do not vary across different times during the fall semester.

The estimated effects of game days on reports of rape to campus and municipal agencies serving students require careful interpretation and scrutiny given 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 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.[32]

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 5. In Column 2 we investigate a larger set of agencies by additionally incorporating agencies if they are within 50 miles of a single D1A school.[33] 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.[34] 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 explanation 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

[32] 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.

[33] 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.

[34] 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.

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 6, 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 non-game-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.

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. Nonetheless, we examine arrest rates using the incident-level data used to construct the aggregated we have examined thus far. Specifically, we estimate a linear probability model, regressing the probability an incident leads to arrest on variables indicating whether the incident took place on the day of a home game, the day of an away game, or the day before or after such a game, controlling for day-of-week-by-hour-of-day fixed-effects, holiday fixed effects and agency-by-year-by-week fixed effects.[35] The estimates, shown in Table 7, are close to zero and are never statistically significant, which supports the notion that

[35]For incidents involving a single victim and multiple perpetrators (20 percent), we classify an incident as leading to an arrest if any perpetrator is arrested. For incidents involving multiple victims (6.5 percent), we cannot verify that the person arrested for rape was arrested for the rape of the 17-24 year old victim but nonetheless classify an incident as leading to an arrest in such circumstances.

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.

5.3 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.[36] 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.

Table 8 shows the estimated effects by victims age in four-year groups for ages 13–28 and a residual "other ages" 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 13-16 year olds and for the residual age category are close to zero. 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 non-students will be necessary to further investigate this possibility..

Table 9 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

[36] 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.

on reports of rape involving college-aged offenders and non-college-aged offenders, respectively.[37] 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 non-college-aged offenders, at least for home games.[38]

Columns (3) and (4) of Table 9 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 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.

Columns (5) and (6) of Table 9 separately show results for incidents in which the offender was identified as having used alcohol or not.[39] These results 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 under-reported 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.

Table 10 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.

[37] 15 percent of offenses are excluded due to missing offender age.

[38] 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.

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

5.4 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 includes 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.

The other tables in this paper present the results of analyses that pool together all universities with Division 1 football programs, but Table 11 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.[40]

Table 11 presents 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

[40]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.

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.

Table 12 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: Column (1) shows estimates separately considering the effects of games against rival opponents and games against non-rival opponents; Column (2) shows estimates separately 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 3; and Column (3) shows estimates 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 3, 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 Column (1) and Column (2) 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 Column (3) 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.[41]

[41] 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.

5.5 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.[42] The results of our analysis of the effects of football games on reports of rape at "party schools" and "non-party schools" are shown in Table 13. The estimates suggest that the effects (for 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 5:59 am 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

[42] 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 5 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 Appendix Table A1.

after the game."[43] 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 Table 14, 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.[44] 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.

5.6 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).[45]

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

[43] 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.

[44] 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.

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

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 pre-game point spread calculated by Las Vegas 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 3 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.[46]

As a starting point, we begin by simply documenting the effects of games with different predicted outcomes, omitting the interactions with game outcomes. Table 15, 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 five percent level). We do note, however, that the lead terms on the game outcomes are sometimes significant at the ten-percent level which suggests that the results of this analysis should be viewed with some caution since game outcomes should not influence pre-game behavior.

6 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

[46] The regression equation is as follows:

$$E[R_{act}] = exp(\beta_3 GamedayExpectLoss_{ct} \times Win_{ct} + \beta_2 GamedayExpectClose_{ct} \times Win_{ct} + \beta_1 GamedayExpectWin_{ct} \times Lost_{ct} \\ \beta_6 GamedayExpectLoss_{ct} + \beta_5 GamedayExpectClose_{ct} + \beta_4 GamedayExpectWin_{ct} + \boldsymbol{\gamma X_{act}}) \quad (2)$$

where $\boldsymbol{\gamma X_{act}}$ 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.

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 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 pre-game 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.[47] Based on an estimated societal cost of $267,000 per offense (McCollister *et al.* 2010), these numbers imply a social cost of rapes induced by Division 1A football games of $193M each year.[48] 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 $29M.[49]

We view the results of our analyses as having several implications for policy. Most directly,

[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 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.

[48]Note that we have adjusted the cost estimate reported in McCollister *et al.* (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.

[49]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), 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.

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 2, policies relating to partying and drinking have not featured prominently in discussions about how 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 inter-relatedness 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, P. D., and R. P. Waterman (2002): "Fixed Effects Negative Binomial Regression Models," Sociological Methodology, 32(1), 247–265.

Anderson, D. M., B. Crost, and D. I. Rees (2014): "Wet Laws, Drinking Establishments, and Violent Crime," Discussion paper, Working Paper.

Anderson, M. L. (2012): "The Benefits of College Athletic Success: An Application of the Propensity Score Design with Instrumental Variables," Discussion paper, National Bureau of Economic Research.

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

Biderman, C., J. M. De Mello, and A. Schneider (2010): "Dry Laws and Homicides: Evidence from the São Paulo Metropolitan Area," The Economic Journal, 120(543), 157–182.

Billings, S. B., and C. A. Depken II (2012): "Sport Events and Criminal Activity: A Spatial Analysis," in Violence and Aggression in Sporting Contests, ed. by R. T. Jewell, vol. 4 of Sports Economics, Management and Policy, pp. 175–187. Springer New York.

Branscombe, N. R., and D. L. Wann (1992): "Role of Identification with a Group, Arousal, Categorization Processes, and Self- Esteem in Sports Spectator Aggression," Human Relations, 45(10), 1013–1033.

Cameron, A. C., and P. K. Trivedi (2005): Microeconometrics: Methods and Applications. Cambridge University Press, New York.

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

Carpenter, C. (2005): "Heavy Alcohol Use and the Commission of Nuisance Crime: Evidence from Underage Drunk Driving Laws," American Economic Review, pp. 267–272.

——— (2007): "Heavy Alcohol Use and Crime: Evidence from Underage Drunk-Driving Laws," Journal of Law and Economics, 50(3), 539–557.

Carpenter, C., and C. Dobkin (2011): "Alcohol Regulation and Crime," in Controlling Crime: Strategies and Tradeoffs, ed. by P. Cook, J. Ludwig, and J. McCrary, pp. 291–329. University of Chicago Press.

——— (2015): "The Minimum Legal Drinking Age and Crime," Review of Economics and Statistics, 97(2), 521–524.

Clotfelter, C. T. (2011): Big-Time Sports in American Universities. Cambridge University Press.

Cook, P. J., and C. P. Durrance (2013): "The virtuous tax: Lifesaving and crime-prevention effects of the 1991 federal alcohol-tax increase," Journal of Health Economics, 32(1), 261 – 267.

Cook, P. J., and M. J. Moore (1993a): "Economic perspectives on reducing alcohol-related violence," Alcohol and Interpersonal Violence: Fostering Multidisciplinary Perspectives, 24, 193–211.

Cook, P. J., and M. J. Moore (1993b): "Violence reduction through restrictions on alcohol availability," Alcohol Health and Research World, 17, 151 – 156.

DeGue, S. (2014): "Evidence-Based Strategies for the Primary Prevention of Sexual Violence Perpetration," Preventing Sexual Violence on College Campuses: Lessons from Research and Practice.

DeGue, S., L. A. Valle, M. K. Holt, G. M. Massetti, J. L. Matjasko, and A. T. Tharp (2014): "A Systematic Review of Primary Prevention Strategies for Sexual Violence Perpetration," Aggression and Violent Behavior, 19(4), 346–362.

Durrance, C. P., S. Golden, K. Perreira, and P. Cook (2011): "Taxing sin and saving lives: Can alcohol taxation reduce female homicides?," Social Science & Medicine, 73(1), 169 – 176.

Glassman, T., C. E. Werch, E. Jobli, and H. Bian (2007): "Alcohol-Related Fan Behavior on College Football Game Day," Journal of American College Health, 56(3), 255–260.

Glassman, T. J., V. J. Dodd, J.-J. Sheu, B. A. Rienzo, and A. C. Wagenaar (2010): "Extreme Ritualistic Alcohol Consumption among College Students on Game Day," Journal of American College Health, 58(5), 413–423.

Hernández-Julián, R., and K. W. Rotthoff (2014): "The Impact of College Football on Academic Achievement," Economics of Education Review, 43, 141–147.

Kilpatrick, D. G., H. S. Resnick, K. J. Ruggiero, L. M. Conoscenti, and J. McCauley (2007): Drug-Facilitated, Incapacitated, and Forcible Rape: A National Study. Medical University of South Carolina, National Crime Victims Research & Treatment Center Charleston, SC.

Krebs, C. P., C. H. Lindquist, T. D. Warner, B. S. Fisher, and S. 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), 639–649.

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

Lippy, C., and S. DeGue (2014): "Exploring alcohol policy approaches to prevent sexual violence perpetration," Trauma, Violence, & Abuse.

Lisak, D., L. Gardinier, S. C. Nicksa, and A. M. Cote (2010): "False Allegations of Sexual Assualt: An Analysis of Ten Years of Reported Cases," Violence Against Women, 16(12), 1318–1334.

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

Markowitz, S., and M. Grossman (2000): "The Effects of Beer Taxes on Physical Child Abuse," Journal of Health Economics, 19(2), 271–282.

McCollister, K. E., M. T. French, and H. Fang (2010): "The Cost of Crime to Society: New Crime-Specific Estimates for Policy and Program Evaluation," Drug and Alcohol Dependence, 108(1), 98–109.

Neal, D. J., and K. 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.

Pope, D. G., and J. C. Pope (2014): "Understanding College Application Decisions why College Sports Success Matters," Journal of Sports Economics, 15(2), 107–131.

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

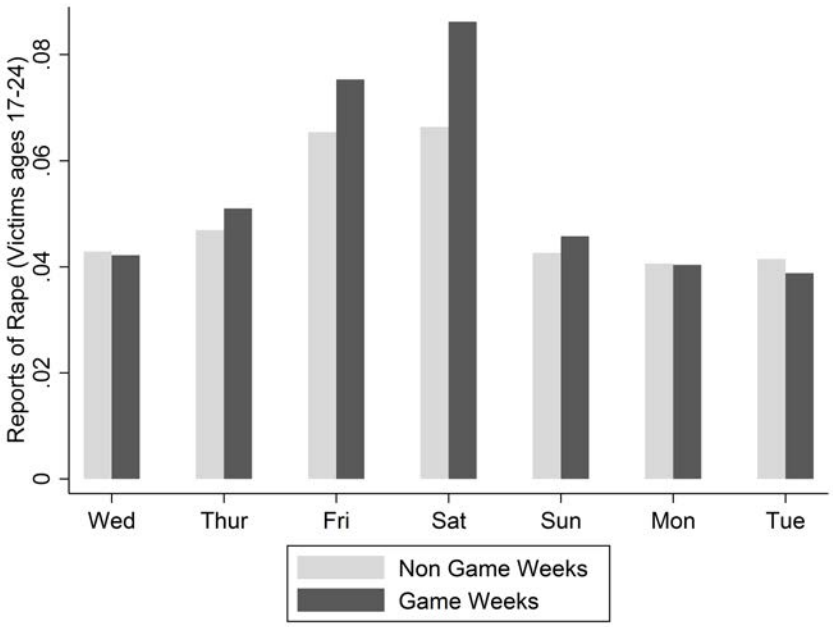
The White House Task Force to Protect Students from Sexual Assault (2014): Not alone: The first report of the White House task force to protect students from sexual assault.

U.S. Department of Education, National Center for Education Statistics (2011): 2008-09 Baccalaureate and Beyond Longitudinal Study (B&B:08/09): A First Look at Recent College Graduates.

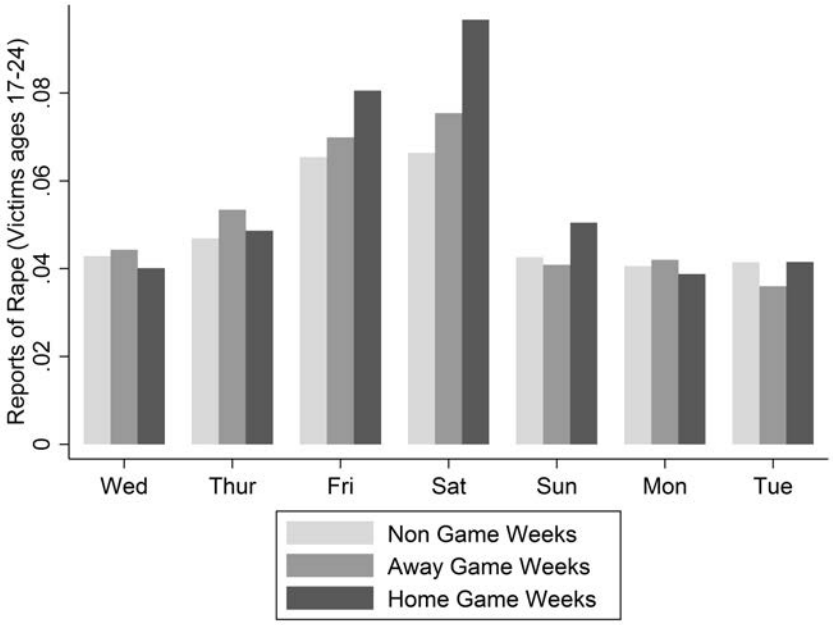
Wann, D. L. (1993): "Aggression among Highly Identified Spectators as a Function of their Need to Maintain Positive Social Identity," Journal of Sport and Social Issues, 17, 134–143.

Figure 1
Daily Reports of Rape Per Agency on Saturday Game Weeks and Non-Game Weeks

Panel A
Pooling Game Days



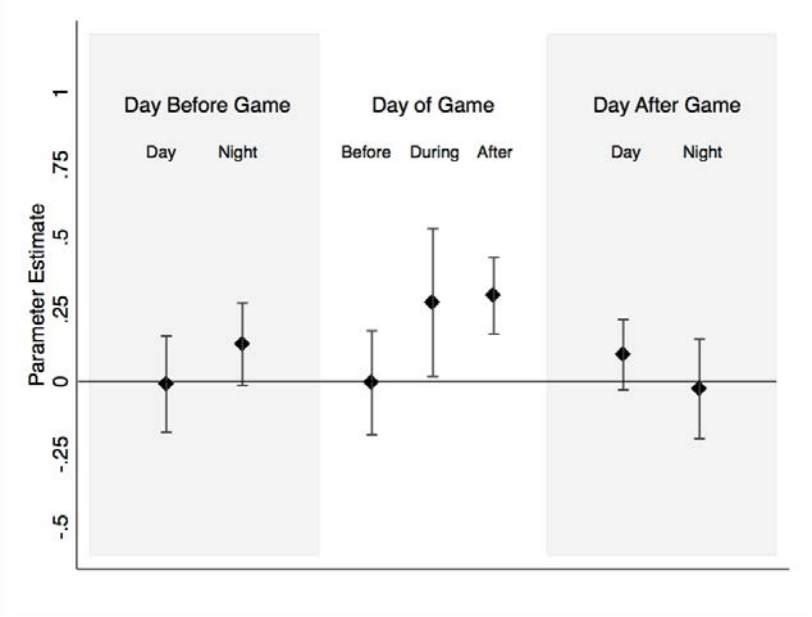
Panel B
Separating Home and Away Game Days



Notes: This figure shows mean daily reports of rape—involving 17-24 year old victims—to agencies which 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.

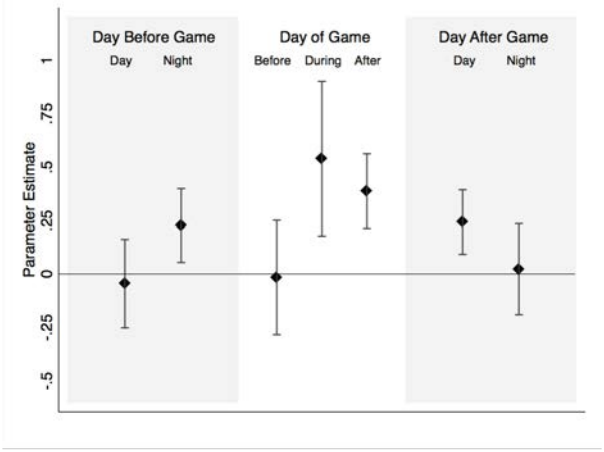
Figure 2
Estimated Effects By Time of Day

Panel A
Estimated Effects Overall

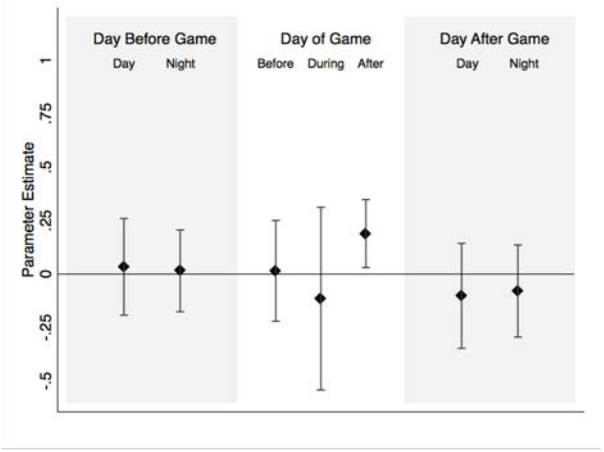


Panel B
Estimated Effects By Home and Away

Home



Away



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 6:00am 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.

Table 1
Reported Incidents Per Day for NIBRS Analysis Sample

Rapes	0.157
Rapes, victims ages 17-24	0.051
Rapes, victims ages 13-16	0.033
Rapes, victims ages 17-20	0.031
Rapes, victims ages 21-24	0.020
Rapes, victims ages 25-28	0.012
Rapes, victims of other ages	0.060
Rapes, victims ages 17-24, offenders ages 17-20	0.010
Rapes, victims ages 17-24, offenders ages 21-24	0.012
Rapes, victims ages 17-24, offenders ages 25-28	0.007
Rapes, victims ages 17-24, offenders of other ages	0.014
Rapes, victims ages 17-24, offender known	0.032
Rapes, victims ages 17-24, offender friend or aquantaince	0.022
Rapes, victims ages 17-24, offender is a partner	0.004
Rapes, victims ages 17-24, offender unknown	0.019
Rapes, victims ages 17-24, offender using alcohol	0.010
Rapes, victims ages 17-24, offender not using alchohol	0.041
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
Drunkeness 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 3.

Table 2
Universities Included in Analysis Compared to All With Division 1 Football

	NIBRS-Matched Schools	All Schools
Panel A: School Characteristics, IPEDS		
Schools	96	245
Enrollment	13,228	12,057
SAT writing 25th percentile score	490	507
SAT writing 75th percentile score	597	610
SAT critical Reading 25th percentile score	492	505
SAT critical Reading 75th percentile score	604	613
SAT math 25th percentile score	507	520
SAT math 75th percentile score	619	628
Retention rate (%)	79	81
Fraction private schools	0.15	0.26
Fraction receiving federal-grant aid	0.27	0.27
Fraction receiving student-loan aid	0.48	0.47
Fraction receiving any financial aid	0.78	0.79
Fraction male	0.48	0.48
Fraction female	0.52	0.52
Fraction White	0.74	0.68
Fraction Black	0.12	0.15
Fraction Asian	0.03	0.05
Fraction Hispanic	0.04	0.05
Panel B: Law Enforcement Agency Statistics, UCR		
Fraction Municipal Agencies	0.57	0.58
Number of Residents	78,743	81,448
Annual reports of rape per 10,000	1.10	1.10
Panel C: Football Performance Statistics, Sports Reference		
Season winning percentage	0.53	0.51
Fraction home games	0.49	0.49
Fraction of seasons with bowl game participation	0.23	0.20
Fractions of schools in NCAA Subdivision 1A	0.56	0.51

Notes: Statistics in panels A, B, and C are are based on 1991–2012 data from the Integrated Postsecondary Education Data System (IPEDS), the Uniform Crime Reports, and Sports Reference, respectively.

Table 3
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
N	422,308	370,583	273,919	176,281	77,191
Day-of-Week FE	yes	yes	yes	yes	yes
Holiday Controls	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	-	-
Agency FE	yes	-	-	-	-
Agency by Month of Year FE	no	yes	-	-	-
Agency by Week of Year FE	no	no	yes	-	-
Agency by Year by Month FE	no	no	no	yes	-
Agency by Year by Week FE	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
N	422,308	370,583	273,919	176,281	77,191
Day-of-Week FE	yes	yes	yes	yes	yes
Holiday Controls	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	-	-
Agency FE	yes	-	-	-	-
Agency by Month of Year FE	no	yes	-	-	-
Agency by Week of Year FE	no	no	yes	-	-
Agency by Year by Month FE	no	no	no	yes	-
Agency by Year by Week FE	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 6:00am to 5:59 am to accommodate the fact that parties often extend past midnight. Standard-error estimates are clustered at the university level.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 4
Robustness to Dates Included in the Analysis

	Main Sample (1)	Fall Sep–Dec (2)	Football Reg Season (3)	Omitting Sept (4)	Omitting Oct (5)	Omitting Nov (6)	Omitting Dec (7)
Day before home game	0.178***	0.140*	0.129	0.192*	0.152**	0.221***	0.194***
	(0.067)	(0.077)	(0.082)	(0.100)	(0.066)	(0.073)	(0.067)
Home game day	0.343***	0.379***	0.352***	0.361***	0.300***	0.377***	0.344***
	(0.069)	(0.084)	(0.069)	(0.091)	(0.076)	(0.068)	(0.066)
Day after Home Game	0.125**	0.133	0.150*	0.113*	0.103	0.165**	0.126**
	(0.057)	(0.095)	(0.085)	(0.069)	(0.066)	(0.077)	(0.054)
Day before away game	0.029	-0.008	-0.012	0.003	0.027	0.054	0.049
	(0.058)	(0.067)	(0.078)	(0.069)	(0.071)	(0.069)	(0.060)
Away Game Day	0.136**	0.173***	0.162**	0.161***	0.138**	0.109	0.135***
	(0.054)	(0.067)	(0.063)	(0.058)	(0.067)	(0.067)	(0.052)
Day after Away Game	-0.070	-0.061	-0.033	-0.070	-0.074	-0.093	-0.064
	(0.070)	(0.100)	(0.081)	(0.086)	(0.097)	(0.079)	(0.074)
Schools	96	94	93	96	95	96	94
Agencies	138	136	135	138	134	137	136
N	77,191	35,984	31,994	67,429	67,010	67,861	68,765
Day-of-Week FE	yes	yes	yes	yes	yes	yes	yes
Holiday Controls	yes	yes	yes	yes	yes	yes	yes
Agency by Year by Week FE	yes	yes	yes	yes	yes	yes	yes

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 3 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls). See Table 3 for additional details. The main sample includes all months of the year excluding June, July, and August. The "Football Regular Season" is defined for each school and year based on the team's first game (minus six days) and last regular season game (plus six days).
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 5
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
N	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 3 (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 3 for additional details. Standard-error estimates are clustered on universities based on the universities to which each agency has been assigned.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 6
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
N	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 3 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls). See Table 3 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.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 7
Estimated Effects on the Probability that a Report Leads to Arrest

Day before home game	-0.007
	(0.023)
Home game day	-0.030
	(0.033)
Day after home game	-0.036
	(0.031)
Day before away game	0.028
	(0.036)
Away game day	0.030
	(0.025)
Day after away game	-0.011
	(0.024)
Schools	95
Agencies	137
N	21,298

Notes: These estimates consider whether reports of rape of 17–24 year old rape victims lead to arrest. Estimates are based on a linear probability model using incident-level 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 model additionally controls for agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls. Standard-error estimates are clustered at the university level. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 8
Estimated Effects for Victims of Different Ages

	(1)	(2)	(3)	(4)	(5)
Victime Age	13-16	17-20	21-24	25-28	Other
Home Game Day	-0.023	0.320***	0.378***	0.212*	0.036
	(0.080)	(0.079)	(0.106)	(0.124)	(0.063)
Away Game Day	-0.026	0.139**	0.127	-0.007	-0.014
	(0.095)	(0.070)	(0.104)	(0.146)	(0.063)
Schools	79	95	89	74	90
Agencies	90	137	117	87	111
N	48,423	54,619	38,004	24,000	71,379

Notes: These estimates consider reports of rape victims in different age groups using the same Poisson model as Column (5) of Table 3 (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 3 for additional details.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 9
Which Offender Types Are Responsible for the Additional Reported Rapes?

	Offender's Age		Relationship to Victim		Alcohol Consumption	
	17–24	Other	Known	Unknown	Under the influence	Reportedly Not Under Influence
	(1)	(2)	(3)	(4)	(5)	(6)
Home game day	0.461***	0.204**	0.251***	0.476***	0.410***	0.311***
	(0.108)	(0.092)	(0.080)	(0.102)	(0.118)	(0.084)
Away Game Day	0.139*	0.053	0.057	0.253**	0.208	0.117*
	(0.080)	(0.084)	(0.077)	(0.104)	(0.140)	(0.064)
Schools	94	88	95	90	79	95
Agencies	135	119	135	126	110	136
N	43,169	40,392	58,009	33,761	19,840	64,561

Notes: These estimates consider reports of 17–24 year old rape victims involving various offender characteristics using the same Poisson model as Column (5) of Table 3 (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 3 for additional details.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 10
Estimated Effects on Reports of Rape, By Victims' and Offenders' Race

	(1) Black Victim	(2) White Victim	(3) Black Offender	(4) White Offender
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
N	23,924	61,446	35,594	46,850

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 3 (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 3 for additional details.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 11
Estimated Effects By Division of Football Program

Division	(1) All DI	(2) DI-A	(3) DI-AA	(4) DII + DIII	(5) DII	(6) DIII
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
N	77,191	57,996	19,195	27,663	12,722	14,941

Notes: These estimates consider reports of 17–24 year old rape victims using the same Poisson model as Column (5) of Table 3 (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 3 for additional details. Here we consider the effects across the two subdivisions of Division 1 and in divisions outside of Division 1.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 12
Estimated Effects By Game Prominence

	(1)	(2)	(3)
Home against rival	0.602***		
	(0.114)		
Home against non-rival	0.293***		
	(0.075)		
Away against rival	0.156		
	(0.132)		
Away against non-rival	0.132**		
	(0.065)		
Home against ranked team		0.443***	
		(0.111)	
Home against unranked team		0.258***	
		(0.092)	
Away against ranked team		0.163**	
		(0.080)	
Away against unranked team		0.103	
		(0.085)	
Home with ESPN-listed TV coverage			0.348***
			(0.091)
Home without ESPN-listed TV coverage			0.388***
			(0.107)
Away with ESPN-listed TV coverage			0.217**
			(0.087)
Away without ESPN-listed TV coverage			0.112
			(0.086)
Schools	96	96	85
Agencies	138	138	124
N	77,191	77,191	62,315

Notes: These estimates consider reports of 17–24 year old rape victims using the same Poisson model as Column (5) of Table 3 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls in addition to one-day leads and lags interacted with game day variables). The rivals used for each school are listed in Table A1. 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.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 13
Effects By Status on "Princeton Review's Party School List"

	(1) All Schools	(2) Party Schools	(3) Non-Party Schools	(4) D1A Party	(5) D1A Non-Party
Day before home game	0.178***	0.364***	0.086	0.367***	0.111
	(0.067)	(0.063)	(0.082)	(0.062)	(0.090)
Home game day	0.343***	0.534***	0.266***	0.535***	0.263***
	(0.069)	(0.137)	(0.077)	(0.137)	(0.089)
Day after Home Game	0.125**	0.131	0.119*	0.135	0.103*
	(0.057)	(0.086)	(0.069)	(0.086)	(0.058)
Day before away game	0.029	0.030	0.029	0.032	0.035
	(0.058)	(0.071)	(0.074)	(0.071)	(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)
Day after Away Game	-0.070	-0.032	-0.083	-0.030	-0.125
	(0.070)	(0.151)	(0.078)	(0.151)	(0.092)
Schools	96	16	80	15	40
Agencies	138	30	108	29	60
N	77,191	17,346	59,845	17,311	40,685

Notes: Party schools were identified based on 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 5 from 1998). Those schools defined as a "party school" based on this measure are highlighted in the full list of schools in the Appendix. These estimates consider reports of 17–24 year old rape victims using the same Poisson model as Column (5) of Table 3 (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 3 for additional details.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 14
Estimated Effects on Crimes Related to Excessive Partying

	(1) All	(2) Disorderly Conduct	(3) DUI	(4) Drunkenness	(5) Liquor-Law Violations
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
N	291,806	144,995	182,494	112,984	199,402

Notes: These estimates consider 17–24 year olds arrested for crimes using the same Poisson model as Column (5) of Table 3 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls in addition to one-day leads and lags interacted with game day variables). Because these data do not include the time of the incident, we cannot redefine days to span 6:00am 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 analysis of liquor-law offenses, the results of which are shown in Column (4), focus on 17–20 year old offenders. See Table 3 for additional details.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 15
Estimated Effects By Favorite/Underdog Status and By Game Outcomes

	(1) Rape	(2) Alcohol-related crimes	(3) Rape	(4) Alcohol-related crimes
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
N	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 3 (including agency-by-year-by-week fixed effects, day-of-week fixed effects, and holiday controls), which can be seen for additional details.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A1
List of Division 1 Schools, Corresponding Law-Enforcement Agencies, and Rivals

School	Agencies (Years in NIBRS)	Rival(s)
Air Force Academy	Colorado Springs Police Dept (1997-2012)	Colorado State, Hawaii
Alabama State Univ	Alabama State Univ Police (1991-1992)	Tuskegee
Arkansas State Univ	Arkansas State Univ Police Dept. (2003-2012) Jonesboro Police Dept (2003-2012)	Memphis
Auburn Univ	Auburn Police Dept (1991-1992)	Alabama, Georgia
Austin Peay State Univ	Clarksville Police Dept (1997-2012) Austin Peay State Univ Police (1997-2012)	Murray State
Boise State Univ	Boise Police Dept (1992-2012)	Fresno State, Idaho, Nevada
Bowling Green State Univ	Bowling Green Police Division (2006-2012) Bowling Green State Univ Police (2011-2012)	Kent State, Toledo
Brigham Young Univ	Provo Police Dept (1992-2012) Brigham Young Univ (1995-2012)	Utah, Utah State
Brown Univ	Providence Police Dept (2006-2012)	Rhode Island, Harvard
Bryant Univ	Smithfield Police Dept (2004-2012)	
Central Connecticut State Univ	C Connecticut State Univ Police (1998-2012)	Sacred Heart (CT)
Central Michigan Univ	C Michigan Univ Police Dept (1995-2012) Mount Pleasant Police Dept (1995-2012)	Eastern Michigan, Western Michigan
Charleston Southern Univ	North Charleston Police Dept (1991-2012)	Coastal Carolina
Clemson Univ*	Clemson Police Dept (1991-2012) Clemson Univ Police (1991-2012)	South Carolina, North Carolina State
Coastal Carolina Univ	Coastal Carolina Univ Police Dept (2003-2012) Conway Police Dept (2003-2012)	Charleston Southern (SC), Liberty
College of William & Mary	William & Mary College Campus Police (1998-2012) Williamsburg Police Dept (1997-2012)	Richmond, James Madison
Colorado State Univ	Fort Collins Police Dept (2006-2012) Colorado State Univ Police Dept (2006-2012)	Air Force, Wyoming, Colorado
Dartmouth College	Hanover Police Dept (2003-2012)	New Hampshire
Drake Univ	Des Moines Police Dept (1991-2012)	
East Carolina Univ	Greenville Police Dept (1991-2012)	N. Carolina St, Marshall, S. Mississippi, N. Carolina
Eastern Illinois Univ	Charleston Police Dept (1993-1994)	Illinois State
Eastern Kentucky Univ	Richmond Police Dept (2001-2012)	Western Kentucky
Eastern Michigan Univ	E Michigan Univ Police Dept (1995-2012)	Western Michigan, Central Michigan
Eastern Washington Univ	Cheney Police Dept (2009-2012)	Montana
Hampton Univ	Hampton Police Dept (2000-2012)	Norfolk State, Howard
Idaho State Univ	Pocatello Police Dept (1992-2012)	Weber State, Montana
Illinois State Univ	Normal Police Dept (1993-1994)	Eastern Illinois
Iowa State Univ	Iowa State Univ Police (1992-2012) Ames Police Dept (1992-2012)	Iowa, Kansas State, Missouri
Jacksonville State Univ	Jacksonville St Univ Police Dept (1991-1992) Jacksonville Police Dept (1991-1992)	Troy, Samford
James Madison Univ	Harrisonburg Police Dept (1997-2012)	Delaware, William & Mary
Kansas State Univ	Riley County Police Dept (2000-2012) Kansas St Univ Police Dept (2001, 2003-2012)	Kansas, Iowa State, Nebraska
Liberty Univ	Lynchburg Police Dept (2000-2012)	Coastal Carolina
Marshall Univ	Huntington Police Dept (2000-2012) Marshall Univ Police Dept (1999-2012)	Ohio, East Carolina, West Virginia
Michigan State Univ*	East Lansing Police Dept (2000-2012) Michigan St Univ Police Dept (2000-2012)	Indiana, Penn State , Michigan, Notre Dame
Middle Tennessee State Univ	Murfreesboro Police Dept (1998-2012) Middle Tennessee St Univ Police (1997-2012)	Troy, Western Kentucky
Montana State Univ	Bozeman Police Dept (2005-2012)	Montana
Morehead State Univ	Morehead Police Dept (2001-2005, 2008-2012)	Eastern Kentucky
Murray State Univ	Murray Police Dept (2000-2004, 2008-2012)	Western Kentucky
Norfolk State Univ	Norfolk State Univ Police (1997-2012)	Hampton (Va)
North Dakota State Univ	North Dakota State Univ Police (1997-2012) Fargo Police Dept (1991-2012)	South Dakota State, North Dakota
Northern Illinois Univ	Northern Illinois Univ Police (1993-1994)	Toledo, Ball State
Ohio State Univ*	Columbus Police Dept (2003-2012) Ohio State Univ Police Dept (2003-2012)	Illinois, Michigan, Penn State
Ohio Univ	Athens Police Dept (2003-2012)	Marsharll, Miami (OH)
Old Dominion Univ	Old Dominion Univ Police Dept. (2009-2012) Norfolk Police Dept (2009-2012)	James Madison
Oregon State Univ	Corvallis Police Dept (2003-2012)	Oregon, Washington, Washington State
Presbyterian College	Clinton Dept of Public Safety (1991-2012) Presbyterian Col (1997-00, 2002-04, 2007-08, 2012)	Newberry
Sacred Heart Univ	Fairfield Police Dept (2001-2012)	Central Connecticut State
South Carolina State Univ	S. Carolina State Univ Police Dept (1991-2012)	North Carolina A&T
South Dakota State Univ	Brookings Police Dept (2001-2006, 2008-2012)	North Dakota State, South Dakota
Southern Illinois Univ Carbondale	Carbondale Police Dept (1993-1994)	Northern Iowa, Southeast Missouri State
Tennessee State Univ	Tennessee State Univ (1997-2012)	
Texas Christian Univ	Fort Worth Police Dept (2005-2012)	Baylor, Southern Methodist, Texas Tech
Texas State Univ	San Marcos Police Dept (2002-2007)	Nicholls State, Sam Houston State
The Citadel	Citadel Public Safety (1991-2001, 2003-2012) Charleston Police Dept (1991-2012)	Virginia Military Institute, Furman
Univ Northern Iowa	Cedar Falls Police Dept (1992-2012) Univ of Northern Iowa Police (1992-2012)	Iowa, Iowa State, N. Dakota State, S. Illinois
Univ Toledo	Univ of Toledo Police Dept (2012)	Bowling Green State
Univ of Akron	Akron Police Dept (1998-2012)	Kent State
Univ of Alabama*	Tuscaloosa Police Dept (1991-1992) Univ of Alabama Pd Univ (1991-1992)	Auburn, Louisiana State, Tennessee
Univ of Alabama-Birmingham	Birmingham Police Dept (1991-1992)	Southern Mississippi, Troy
Univ of Arkansas	Univ of AK - Fayetteville Police (2007-2012)	Louisiana State, Texas, Mississippi, Texas A&M

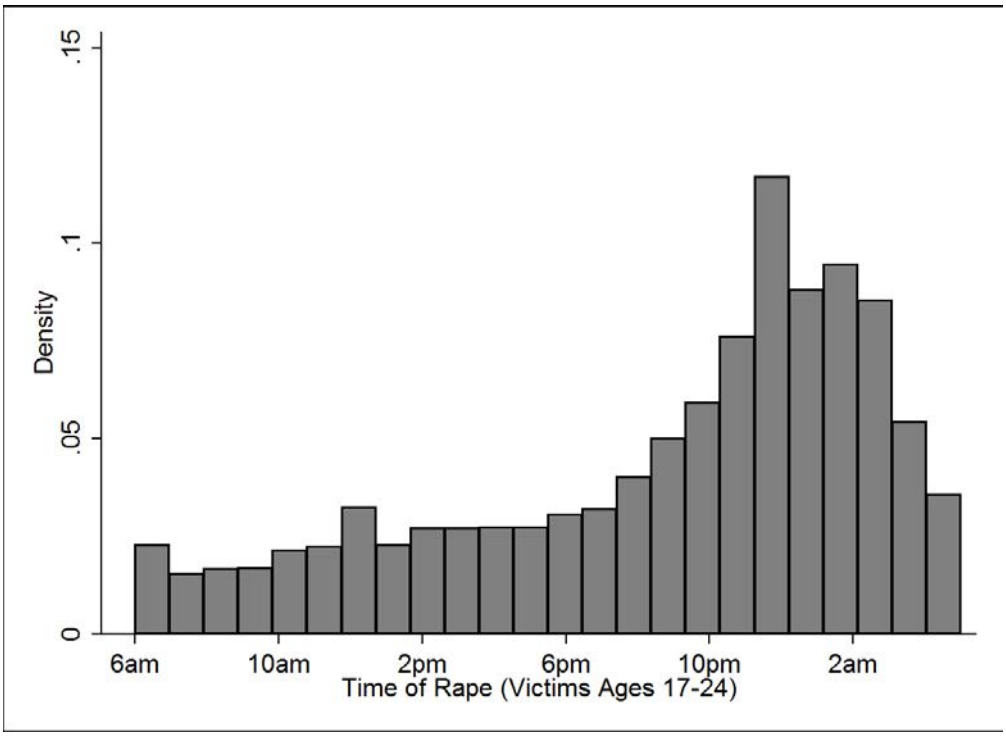
Continued on next page

Table A1 – *Continued from previous page*

School	Agencies (Years in NIBRS)	Rival(s)
	Fayetteville Police Dept (2003-2012)	
Univ of Arkansas as Pine Bluff	Pine Bluff Police Dept (2009-2012)	
Univ of Cincinnati	Cincinnati Police Dept (1998-2012)	Miami (OH), Louisville, Pittsburgh
Univ of Colorado Boulder*	Univ of Colorado - Boulder Police Dept (1997-2012) Boulder Police Dept (2010-2012)	Nebraska, Colorado State, Utah
Univ of Connecticut	Univ of Connecticut Police (1998-2012)	Massachusetts, Rutgers
Univ of Dayton	Dayton Police Dept (1998-2012)	
Univ of Idaho	Moscow Police Dept (1992-2012)	Boise State, Washington State
Univ of Illinois Urbana-Champaign*	Champaign Police Dept (1993-1994) Univ of Illinois Police Dept (1993-1994)	Missouri, Northwestern, Purdue , Ohio State
Univ of Iowa*	Univ of Iowa Police (1996, 1998-2012) Iowa City Police Dept (1993-2012)	Minnesota, Iowa State, Nebraska, Wisconsin
Univ of Kansas*	Univ of KS Police Dept (2001-2009, 2011-2012) Lawrence Police Dept (2001-2012)	Nebraska, Missouri, Kansas State
Univ of Kentucky	Univ of KY Police Dept (2002-2005, 2008-2012) Lexington Division of Police (2011-2012)	Louisville, Mississipi State, Tennessee, Vanderbilt
Univ of Louisville	Univ of Louisville Police Dept (2009-2012)	Cincinnati, Kentucky, Memphis
Univ of Massachusetts*	Amherst Police Dept (2002-2012) Univ of Mass - Amherst Police (1995-2012)	Boston College, Connecticut
Univ of Memphis	Memphis Police Dept (2000-2012) Univ of Memphis Police (1998-2012)	Southern Mississippi
Univ of Michigan*	Ann Arbor Police Dept (2003-2012) Univ of MI Flint Dept of Public Safety (1995-2012)	Michigan State, Notre Dame, Ohio State, Minnesota
Univ of New Hampshire*	Durham Police Dept (2008-2012)	Dartmouth, Maine, Massachusettes
Univ of North Texas	Denton Police Dept (2002-2012)	Southern Methodist
Univ of Northern Colorado	Greeley Police Dept (2005-2012)	
Univ of Richmond	Williamsburg Sheriff'S Dept (1998-2012)	William & Mary
Univ of South Carolina*	Univ of S. Carolina Law Enf & Saf (1991-2012) Columbia Police Dept (1991-2012)	Clemson, Georgia
Univ of South Dakota	Vermillion Police Dept (2001-2012)	South Dakota State
Univ of Tennessee*	Knoxville Police Dept (2000-2012) Univ of Tennessee at Knoxville Police (1997-2012)	Kentucky, Vanderbilt, Alabama, Florida
Univ of Texas at Austin*	Austin Police Dept (1997-2003)	Oklahoma, Texas A&M, Texas Tech, Arkansas
Univ of Texas at El Paso	El Paso Police Dept (2004-2007)	New Mexico State
Univ of Utah	Salt Lake City Police Dept (1999-2012) Univ of Utah Police Dept (1993-2003)	Brigham Young, Utah State
Univ of Virginia	Charlottesville Police Dept. (1997-2012) Univ of Virginia Police Dept (1997-2012)	North Carolina, Virginia Tech
Univ of Washington	Seattle Police Dept (2012)	Washington State, Oregon
Utah State Univ	Logan Police Dept (1993-2012) Utah State Univ Police Dept (2005-2012)	Wyoming, Utah, Brigham Young
Vanderbilt Univ	Nashville Metro Police Dept (1999-2012) Vanderbilt Univ (1997-2012)	Georgia, Kentucky, Mississippi, Tennessee
Virginia Tech*	Virginia Polytechnic Institute Police (2004-2012) Blacksburg Police Dept (1995-2012)	West Virginia, Virginia, Miami
Washington St Univ	Washington St Univ Police Dept (2011-2012) Pullman Police Dept (2006-2012)	Washington, Idaho
Weber State Univ	Ogden Police Dept (2001-2012)	
West Virginia Univ*	West Virginia Univ Police Dept. (2001-2012) Morgantown Police Dept (1999-2012)	Pittsburgh, Maryland, Syracuse, Virginia Tech
Western Illinois Univ	Macomb Police Dept (1993-1994)	
Western Kentucky Univ	Western Kentucky Univ Police (2009-2012) Bowling Green Police Dept (2008-2012)	Murray State, E Kentucky, Middle Tennessee St
Western Michigan Univ	Kalamazoo Dept of Public Safety (2000-2012) Western MI Univ Police Dept (1995-2012)	Eastern Michigan, Central Michigan
Youngstown State Univ	Youngstown Police Dept (2004-2012)	Akron

*School is listed at least once in the top 20 of the Princeton Party School Rankings from 2001-2012.

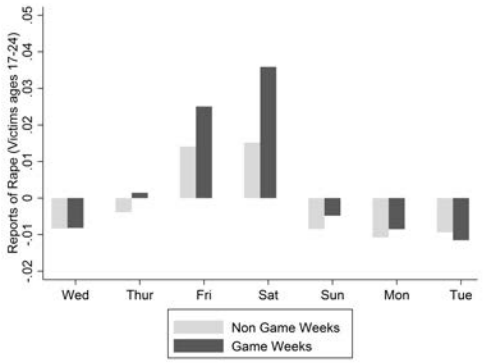
Figure A1
Distribution of Reports of Rape by Time of Incident



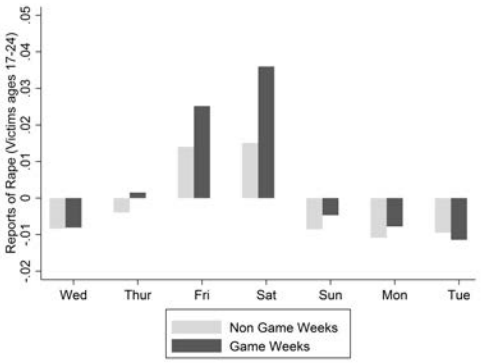
Notes: This figure shows time-of-day distribution of reports of rapes with 17-24 year old victims reported to agencies which cover college campuses with a Division 1 football team by time of incident.

Figure A2
Residualized Daily Reports of Rape Per Agency on Saturday Game Weeks
and Non-Game Weeks

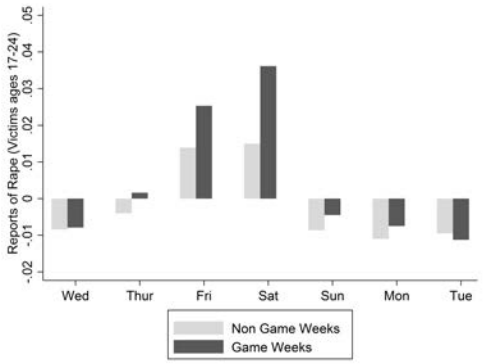
Panel A
Adjusted for agency fixed effects,
year fixed effects, and holiday fixed effects



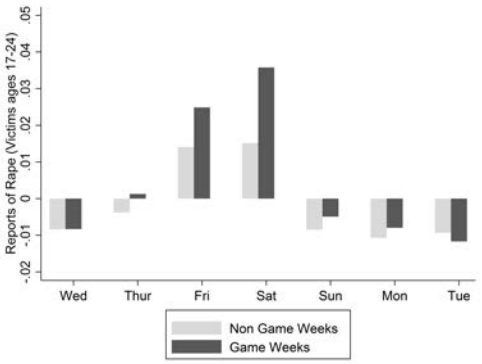
Panel B
Adjusted for agency-by-month-of-year fixed effects,
year fixed fixed effects, and holiday fixed effects



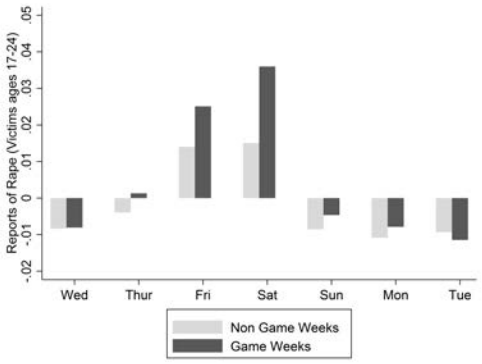
Panel C
Adjusted for agency-by-week-of-year fixed effects,
year fixed effects, and holiday fixed effects



Panel D
Adjusted for agency-by-year-by-month fixed effects
and holiday fixed effects



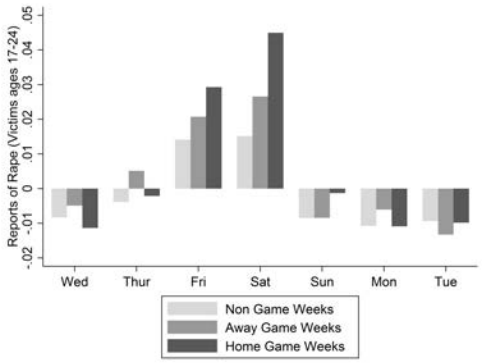
Panel E
Adjusted for agency-by-year-by-week fixed effects
and holiday fixed effects



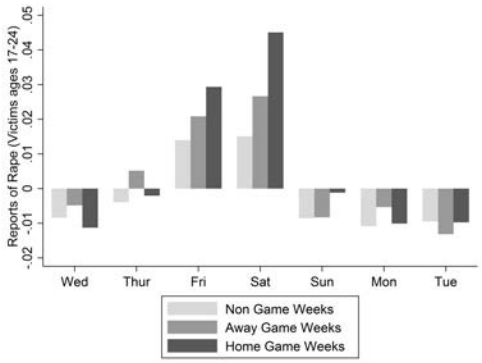
Notes: See Figure 1. Additionally note that the residualized number of reports are calculated as the actual number minus the number predicted by a Poisson model with the relevant controls variables listed in each panel heading.

Figure A3
Residualized Daily Reports of Rape Per Agency on Saturday Game Weeks
and Non-Game Weeks, Separating Home and Away Game Weeks

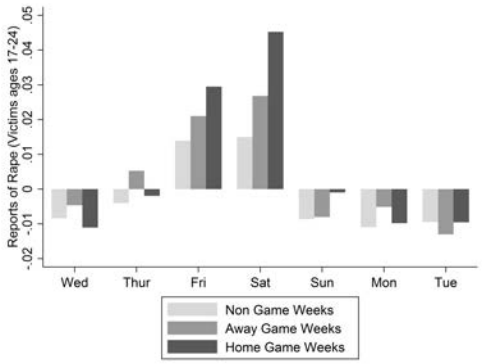
Panel A
Adjusted for agency fixed effects,
year fixed effects, and holiday fixed effects



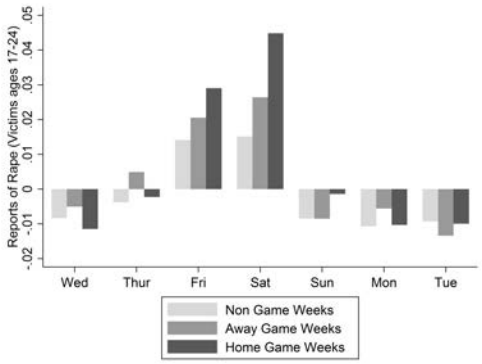
Panel B
Adjusted for agency-by-month-of-year fixed effects,
year fixed fixed effects, and holiday fixed effects



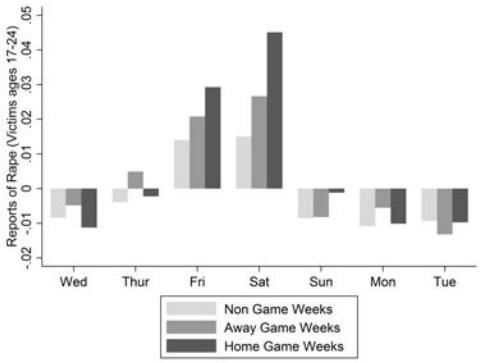
Panel C
Adjusted for agency-by-week-of-year fixed effects,
year fixed effects, and holiday fixed effects



Panel D
Adjusted for agency-by-year-by-month fixed effects
and holiday fixed effects



Panel E
Adjusted for agency-by-year-by-week fixed effects
and holiday fixed effects



Notes: See Figure 1. Additionally note that the residualized number of reports are calculated as the actual number minus the number predicted by a Poisson model with the relevant controls variables listed in each panel heading.