COLLEGE PARTY CULTURE AND SEXUAL ASSAULT

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NBER Working Paper No. 21828
December 2015
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.

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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 or otherwise) 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.

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. We note that the estimated

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[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 drinking on crime, including violent crime. To our knowledge, however, none of these have explicitly studied effects on sexual assault. Quasi-experimental studies of alcohol consumption have used variation driven by the minimum legal drinking age (Carpenter and Dobkin, 2014), taxes (Cook and Moore, 1993; Markowitz, 2000, 2005; Durrance et al., 2011; Cook and Durrance, 2013), drunk driving laws (Carpenter 2005, 2007), and changes in "wet" laws (Biderman 2010; Anderson, Crost, and Rees, 2014).

[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.

effects of away games can be emphasized for conservative estimates that largely avoid threats to validity posed by spatial displacement (i.e. population inflows) and by any enhanced policing efforts. 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. Back-of-the-envelope calculations based on our estimates suggest that football games cause 253–770 additional rapes per year across the 128 schools participating in Division 1A, depending on the degree to which one attributes the larger effect of home games to heightened partying or to changes in the number of potential victims and/or perpetrators in town or policing efforts. 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 between $68M and $205M. The estimated effects for schools participating in Division 1AA are smaller, suggesting 6–115 additional rapes per year across 125 schools.

While 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, we provide support for this interpretation 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.[4] We also present the results from supporting analyses that: examine the effects on victims of different ages; consider heterogeneity across schools in different divisions within and outside of Division 1; consider the degree to which the effects are greater for relatively prominent games and/or related game outcomes; verify that similar spikes are not observed on within-season Saturdays without games (as Saturdays are by far the most common day for games); and verify that the estimated effects are not sensitive to the inclusion of any particular months of the year.

We also present supporting evidence along the lines of Card and Dahl (2011), which shows that unexpected negative emotional cues, in the form of upset losses in professional football games, increase the incidence of domestic violence. We find that upset wins increase reports of rape while upset losses do not, which highlights that the mechanisms leading to sexual assault are quite different from those leading to domestic violence. Consistent with our emphasis on partying as the most likely causal pathway, upset wins increase arrests for drunkenness, DUIs, liquor law violations and public order offenses while upset losses appear to have no such effects.

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[4]This work builds on Rees and Schnepel (2009), which is similar in spirit to our paper in its investigation of the effects of college football games on crime. It also finds significant effects on alcohol-related crimes using data from 26 police agencies associated with schools with Division 1A football programs.

Beyond offering insight into the causal link between collegiate football, partying, and sexual assault, this paper also contributes to the literature on the the role of big-time sports programs at universities. Despite substantial and rapidly increasing investments in such programs—nearly all of which are subsidized by their student bodies and/or the university's general fund—there is a paucity of rigorous quantitative research into their effects on universities and students.[5] Most of the research in this 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 estimate the causal effects on students' experiences in college: using an approach that exploits variation in the football team's winning percentage across the years a student is at a university, Lindo, Swensen, and Waddell (2012) and Hernández-Julián and Rotthoff (2014) find 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.

We view the results of our analyses as having several implications for policy. First, by providing convincing evidence that spikes in the degree of partying at a university escalate the incidence of rape, our results suggest that efforts to avoid such spikes could serve to reduce the incidence of rape. Second, by documenting the types of rapes that are affected and the degree to which they are affected, our results can be used in information campaigns aimed at preventing rape. Third, by estimating an important non-monetary cost associated with big-time sports, our results can contribute to more comprehensive cost-benefit analyses as students, administrators, and other interested parties consider their investments in such programs.

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.

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[5]For information on subsidies in 2005 and 2010 for schools subject to open records laws, see "Athletics Subsidies at Division I Schools" from *USA Today*: http://usatoday30.usatoday.com/sports/college/2011-06-23-2011-athletic-department-subsidy-table_n.htm.

[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

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[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

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[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. Despite this, in response to sexual misconduct allegations including a sexual assault and drink spiking, Brown University recently (January 2015) banned alcohol at fraternity parties (and all other events in campus residential areas). Dartmouth University cited the inter-relatedness between high-risk drinking and sexual assault when it recently (January 2015) banned hard liquor on campus. 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."

college-aged (17–24) victims. We also consider incidents involving victims in different age groups, incidents involving perpetrators in various age groups, incidents involving different types of relationships between victim and perpetrator, and incidents in which the perpetrator is reported to be under the influence of alcohol.

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.[13] The 137 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 time and date of incident, 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. 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.[14] 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 reliably measure local television coverage.[15] We also use ten-year (2005–2014) college football team rankings from football-sickness.com and information from a wide variety of websites to consider whether games against traditionally strong teams, and/or games against rivals, have comparatively large effects.[16] Finally, we use the pre-game point spread predictions for each

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[13]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).

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

[15]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.

[16]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 Table A1.

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.

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.

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. 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.[17]

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.[18] These data demonstrate that the NIBRS-matched agencies and the non-NIBRS-matched agencies are similar in composition: roughly 40 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 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

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[17]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.

[18]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.

Poisson models.[19] 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:

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

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.[20] 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 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

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[19]Like linear models, the Poisson model is not subject to the incidental parameters problem associated with fixed effects because they can be eliminated from the model to be estimated. We relax the assumption of equality between the conditional mean and variance by calculating sandwiched standard errors.

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

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.[21]

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 to interpreting β as the causal effect of game days on the incidence of sexual assault. The first challenge is that estimates in part could be driven by temporary fluctuations in the population covered by the agency—while the inclusion of agency-by-year-by-week fixed effects will address the ebb and flow of an agency's covered population across weeks of the year, it will not address sharp changes associated with game days.[22] Because such changes are of particular concern for home games, we separately estimate the effects of home and away games by replacing the $Gameday_{ct}$ variable (and its lag and lead) with indicators for home games and away games. Then one can think of the estimated effect of home games as an upper bound for the true behavioral effect of a game day, because individuals coming to town may drive up the incident count. Similarly, the estimated effects of away games may be thought of as a lower bound because players and fans travel

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

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

to away games, potentially reducing incident counts. The extent to which an effect is present for both home and away games would indicate that these effects are likely to be driven by behavioral changes rather than to an influx of people to the local area. That said, we note that we would expect home games to have larger effects than away games—even in the absence of changes in the at-risk population—because they allow students to attend the game, to attend tailgate parties, and are generally more salient.

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. As such, 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. Assuming that 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.[23]

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. The 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. While we focus our discussion and subsequent analyses on the model corresponding to Column (5), which includes agency-by-year-by-week fixed effects, the estimates vary little across these specifications. The results suggest that football games increase reports of college-aged rape victimization by 28 percent on game days in the law enforcement agency jurisdiction areas that include whose teams

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[23]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).

played.[24] 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 have discussed, 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, 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 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.

5.2   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. 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. 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 4 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

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

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 validity of this study, which hinges on the notion that football games meaningfully affect the social context for college students.

Table 5 again focuses on reported offenses with college-aged (17–24) victims and now considers heterogeneity across various offender characteristics. Columns (1) and (2) show the estimated effects on reports of rape involving college-aged offenders and non-college-aged offenders, respectively.[25] 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.[26]

Columns (3) and (4) of Table 5 show a summary of results by victim-offender relationship. Column (3) shows the estimated effects on reports of rape in which the offender was known to the victim, which account for 63 percent of reports. In the majority (69 percent) of cases with known offenders, the offender was an "acquaintance" or "friend" of the victim. Column (4) shows results for cases where the offender is not known to the victim, or whose identity was not recorded. These estimates suggest that the effects are considerably larger for reports of rape in which 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 5 separately show results for incidents in which the offender was identified as having used alcohol or not.[27] These results provide suggestive evidence that football

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[25]15 percent of offenses are excluded due to missing offender age.

[26]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.

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

games increase alcohol-related rapes more so than other 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, as mentioned in the introduction.

5.3 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 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 6 shows the results separately for universities in subdivisions 1A and 1AA 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.[28]

Table 6 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

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[28]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.

point estimates indicate that home games played by these universities increase rape reports by 41 percent while away games increase rape reports by 18 percent. There are smaller effects for games played by schools with Division 1AA teams: home games increase reports of rape by 31 percent while away games have no impact on the reported incidence of rape. These results are consistent with the notion that football games are less prominent at universities with Division 1AA teams than at universities with Division 1A teams and that Division 1AA games are less likely to be televised. However, we note that the estimates focusing on universities with Division 1AA teams have relatively large standard errors.

Again, while we acknowledge relatively large standard errors, the estimated effects of games played by Division 2 and Division 3 teams are never statistically significant, whether they are pooled or considered separately. This provides further support for the validity of our research design.

Table 7 presents the results of our analysis of whether the effects are larger for relatively prominent games played by Division 1 teams. We replace the home and away day-of-game 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 the incidence of rape. Relative to "normal games," the differences 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

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is not.[29]

5.4 Estimated effects on proxies for excessive partying

Because we motivated our analysis as an opportunity to address the effects of elevated levels of partying and alcohol consumption on the incidence of rape, we now document the link between football games and measures of excessive partying. In particular, we focus on four categories of 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 after the game."[30] 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 8, 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. 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). They 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.

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[29]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.

[30]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.

5.5 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).[31]

Here we 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, but game outcomes are as good as random, conditional on the predicted outcome. As such, the model estimates 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 estimates 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 estimates the effects of games with no clear favorite, allowing the effects of wins and losses to vary. Again following Card and Dahl (2011), we measure upsets using the pre-game point spread calculated by Las Vegas bookmakers to equilibrate betting markets. An upset loss occurs 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.

Column (1) of Table 9 shows the results of this analysis for reports of rapes. The results of this analysis for crimes associated with excessive partying are shown in Column (2). Although the estimated effects of game outcomes have large standard errors, they provide suggestive evidence (at the 10-percent level of statistical significance) that upset wins increase reports of rape. The point estimate implies that upset wins increase reports of rape by 38 percent more than games in which the team lost according to expectations. The corresponding estimate for upset losses is small and statistically insignificant. The lower rows additionally show that the effects are smaller for expected losses than for other expected outcomes; this can be explained in part by the fact that expected losses are more likely to be away games.

We note that these results should be viewed with caution, because the model also generates a

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

marginally significant lead effect of upset losses. Because game outcomes should not influence pre-game behavior, this serves to weaken our confidence in the validity of the estimated effect of upset wins on reports of rape. However, it is noteworthy that the estimated effects on crimes related to excessive partying closely resemble those for reports of rape. Just like the results for rape, upset wins are found to increase the incidence of excessive-partying-related crimes, while upset losses have no apparent effect. Also similar to the rape results, the effect of expected losses is smaller than the effect of expected wins.[32] This further supports the suggestion that the effect of football games on rape may be particularly pronounced after upset wins. It also supports the argument that partying and alcohol consumption are the likely mechanisms by which football games increase the incidence of rape.

5.6 Additional tests of validity

Thus far, we have shown several sets of results that support the idea that football games meaningfully alter the social context of students at universities with Division 1 teams and further support the validity of our research design for estimating their effects on reports of rape. Here we add to that evidence by way of a falsification exercise and a robustness check.

Except for the first four columns of Table 3, all of the estimated effects we have shown in the preceding tables exploit within-agency-week variation while controlling for day-of-week fixed effects. Because these estimates are based on data from September through May to approximate the academic year, 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 the analysis. To test this assumption, we conduct a falsification test based on an augmented version of our empirical model: it includes an indicator for non-game-day Saturdays within the football season (along with a one-day lead and lag) and removes actual game days from the sample (along with a one-day lead and lag). This is possible because there are some weeks without any games (bye weeks) and some weeks in which games are not scheduled on Saturday. The results of this analysis, shown in Column (1) of Table 10, imply 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. 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

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[32]Rees and Schnepel (2009) also found that upset wins increase a variety of alcohol/disorder offenses. In contrast to our results, they found that upset losses similarly increase offense rates. Their analysis differs in several respects to ours, most notably their restriction to home games for the analysis of upsets.

only for Saturdays of weeks in which there is no game at all.

As an additional check on our identification strategy, and in a similar spirit to Table 10, we consider the robustness of our main results to the exclusion of various months of the year. After reproducing our main results, which exclude June through August, Column (2) of Table 11 shows estimates that additionally exclude January through May from the analysis. 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. Columns (3) through (6) explore the sensitivity of the estimates to the exclusion of each month of the year during the college football season. 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 Columns (3) through (6), 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.

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 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. There is also suggestive evidence that upset wins have larger effects on the incidence of both rape and alcohol-related crimes.

A back-of-the-envelope calculation based on our estimates indicates that Division 1A football games cause as many as 746 additional rapes of college-aged victims per year across 128 univer- sities.[33] That said, if one is inclined to believe that spatial displacement is the sole reason why

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[33]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;

away games have smaller effects than home games, the estimate is instead 243. The true effect is likely to lie somewhere between these numbers, because home games do induce population inflows but they also generate greater interest. 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 to be between $68M and $206M each year.[34] Back-of-the-envelope estimates for the effects of Division 1AA football games are much smaller—they suggest an additional 6–115 rapes per year across 125 schools, which implies a social cost of $1.6–31M.[35]

We view these results as contributing to a more complete understanding of the non-pecuniary costs associated with college football. Moreover, by documenting how specific contexts modify the effects of football games on the incidence of rape, and what types of rapes are affected most, our results can inform awareness and prevention efforts. That being said, we recognize that college football games are but one component of a college culture that contributes to excessive partying. Therefore, these results can be viewed as highlighting the potentially large effects on rape that can result from various policies and activities that alter the social context.

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); 751 home games played in 2014; 799 away games played (including neutral-site games) in 2014; and 12 percent of student victims reporting to the police (Kilpatrick, 2007).

[34]Note that we have adjusted the cost estimate reported in McCollister *et al.* (2010) for inflation to put the amount into 2015 dollars.

[35]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; 731 away games played (including neutral-site games) in 2014; and 12 percent of student victims reporting to the police (Kilpatrick, 2007).

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THE WHITE HOUSE TASK FORCE TO PROTECT STUDENTS FROM SEXUAL ASSAULT (2014): <u>Not alone: The first report of the White House task force to protect students from sexual assault.</u>

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
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	0.43	0.42
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	422308	370583	273919	176281	77191
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	422308	370583	273919	176281	77191
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

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	48423	54619	38004	24000	71379

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 5

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	43169	40392	58009	33761	19840	64561

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 6

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	77191	57996	19195	27663	12722	14941

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 7
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	77191	77191	62315

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 8

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	291806	144995	182494	112984	199402

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 9

Estimates by Game Outcome

	(1) Rapes	(2) Alcohol-related crimes
Upset loss	-0.062 (0.172)	-0.037 (0.079)
Expected to be close and lost	-0.071 (0.169)	0.056 (0.050)
Upset win	0.321* (0.182)	0.210*** (0.054)
Game day, expected to win	0.313*** (0.090)	0.460*** (0.098)
Game day, expected to be close	0.352** (0.146)	0.431*** (0.064)
Game day, expected to lose	0.130 (0.103)	0.301*** (0.050)
Schools	52	52
Agencies	85	88
N	56810	201013

Notes: Column (1) considers reports of 17–24 year old rape victims. Column (2) considers 17–24 year olds arrested for alcohol-related crimes–namely Disorderly Conduct, DUI, Drunkenness and Liquor-Law Violations. The results in both columns are from specifications which draw on 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). See the notes to Table 3 and Table 8 for additional details.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 10

Falsification Tests Considering "Effects" of In-Season Saturdays Without Games

	All In-Season Saturdays (1)	Only Bye Weeks (2)
Day before	0.026	0.029
	(0.074)	(0.076)
Saturday without game	-0.019	-0.022
	(0.079)	(0.081)
Day after	0.019	0.032
	(0.114)	(0.118)
Schools	96	96
Agencies	138	138
N	60912	60912

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. Actual game days are removed from the data for this exercise. Column (1) considers the degree to which there are spikes on Saturdays during the season on which there is no game while Column (2) considers the degree to which there are spikes on Saturdays during the season for *weeks* in which there is no game (i.e., bye weeks).

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table 11

Robustness Test Omitting Additional Months of the Year

Additional Months Omitted:	None (1)	Jan-May (2)	Sept (3)	Oct (4)	Nov (5)	Dec (6)
Day before home game	0.178***	0.140*	0.192*	0.152**	0.221***	0.194***
	(0.067)	(0.077)	(0.100)	(0.066)	(0.073)	(0.067)
Home game day	0.343***	0.379***	0.361***	0.300***	0.377***	0.344***
	(0.069)	(0.084)	(0.091)	(0.076)	(0.068)	(0.066)
Day after home game	0.125**	0.133	0.113*	0.103	0.165**	0.126**
	(0.057)	(0.095)	(0.069)	(0.066)	(0.077)	(0.054)
Day before away game	0.029	-0.008	0.003	0.027	0.054	0.049
	(0.058)	(0.067)	(0.069)	(0.071)	(0.069)	(0.060)
Away game day	0.136**	0.173***	0.161***	0.138**	0.109	0.135***
	(0.054)	(0.067)	(0.058)	(0.067)	(0.067)	(0.052)
Day after away game	-0.070	-0.061	-0.070	-0.074	-0.093	-0.064
	(0.070)	(0.100)	(0.086)	(0.097)	(0.079)	(0.074)
Schools	96	94	96	95	96	94
Agencies	138	136	138	134	137	136
N	77191	35984	67429	67010	67861	68765

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.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

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Table A1

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

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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)	Memphis
	Jonesboro Police Dept (2003-2012)	
Auburn Univ	Auburn Police Dept (1991-1992)	Alabama, Georgia
Austin Peay State Univ	Clarksville Police Dept (1997-2012)	Murray State
	Austin Peay State Univ Police (1997-2012)	
Boise State Univ	Boise Police Dept (1992-2012)	Fresno State, Idaho, Nevada
Bowling Green State Univ	Bowling Green Police Division (2006-2012)	Kent State, Toledo
	Bowling Green State Univ Police (2011-2012)	
Brigham Young Univ	Provo Police Dept (1992-2012)	Utah, Utah State
	Brigham Young Univ (1995-2012)	
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)	Eastern Michigan, Western Michigan
	Mount Pleasant Police Dept (1995-2012)	
Charleston Southern Univ	North Charleston Police Dept (1991-2012)	Coastal Carolina
Clemson Univ	Clemson Police Dept (1991-2012)	South Carolina, North Carolina State
	Clemson Univ Police (1991-2012)	
Coastal Carolina Univ	Coastal Carolina Univ Police Dept (2003-2012)	Charleston Southern (SC), Liberty
	Conway Police Dept (2003-2012)	
College of William & Mary	William & Mary College Campus Police (1998-2012)	Richmond, James Madison
	Williamsburg Police Dept (1997-2012)	
Colorado State Univ	Fort Collins Police Dept (2006-2012)	Air Force, Wyoming, Colorado
	Colorado State Univ Police Dept (2006-2012)	
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)	Iowa, Kansas State, Missouri
	Ames Police Dept (1992-2012)	
Jacksonville State Univ	Jacksonville St Univ Police Dept (1991-1992)	Troy, Samford
	Jacksonville Police Dept (1991-1992)	
James Madison Univ	Harrisonburg Police Dept (1997-2012)	Delaware, William & Mary
Kansas State Univ	Riley County Police Dept (2000-2012)	Kansas, Iowa State, Nebraska
	Kansas St Univ Police Dept (2001, 2003-2012)	
Liberty Univ	Lynchburg Police Dept (2000-2012)	Coastal Carolina
Marshall Univ	Huntington Police Dept (2000-2012)	Ohio, East Carolina, West Virginia
	Marshall Univ Police Dept (1999-2012)	
Michigan State Univ	East Lansing Police Dept (2000-2012)	Indiana, Penn State , Michigan, Notre Dame
	Michigan St Univ Police Dept (2000-2012)	
Middle Tennessee State Univ	Murfreesboro Police Dept (1998-2012)	Troy, Western Kentucky
	Middle Tennessee St Univ Police (1997-2012)	
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)	South Dakota State, North Dakota
	Fargo Police Dept (1991-2012)	
Northern Illinois Univ	Northern Illinois Univ Police (1993-1994)	Toledo, Ball State
Ohio State Univ	Columbus Police Dept (2003-2012)	Illinois, Michigan, Penn State
	Ohio State Univ Police Dept (2003-2012)	
Ohio Univ	Athens Police Dept (2003-2012)	Marsharll, Miami (OH)
Old Dominion Univ	Old Dominion Univ Police Dept. (2009-2012)	James Madison
	Norfolk Police Dept (2009-2012)	
Oregon State Univ	Corvallis Police Dept (2003-2012)	Oregon, Washington, Washington State
Presbyterian College	Clinton Dept of Public Safety (1991-2012)	Newberry
	Presbyterian Col (1997-00, 2002-04, 2007-08, 2012)	
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)	Virginia Military Institute, Furman
	Charleston Police Dept (1991-2012)	
Univ Northern Iowa	Cedar Falls Police Dept (1992-2012)	Iowa, Iowa State, N. Dakota State, S. Illinois
	Univ of Northern Iowa Police (1992-2012)	
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 (1991-2012)	Auburn, Louisiana State, Tennessee
	Univ of Alabama Pd Univ (1991-1992)	
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

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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