Speed Kills? The Effect of the Hurry-Up-No-Huddle Offense on Injuries in College Football

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Abstract

The National Collegiate Athletic Association (NCAA) Football Rules Committee recently debated a rule that would have slowed the pace of football games. The stated motivation for this proposed rule was player safety, but many speculated that it was an attempt by traditional coaches to negate the advantages gained by rivals who employ an innovative offensive strategy known as the Hurry-Up-No-Huddle (HUNH) offense. To inform this debate, we collect data on player injuries from five years of team press releases. We use both fixed effects and regression discontinuity specifications to estimate the causal effect of pace on injuries and empirically test the hypothesis that faster paced games result in more injuries. We find robust evidence that defensive player injuries increase linearly with the pace of play: every ten additional plays cause a 9 increase in injuries. Results for offensive players are mixed, and we find no evidence of nonlinear effects. We discuss the policy implications of this patterns of results.

JEL Codes: I10, I12, Z20, Z28

Keywords: NCAA Football, Injury, Poisson regression, Regression discontinuity

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1 Introduction

There is a shift in the way college football is being played: teams are running more plays per game.[1] This increase is due in part to a new innovation in college football known as the Hurry-Up-No-Huddle (HUNH) offense. As current University of Auburn Head Coach and HUNH proponent Gus Malzahn (2003) explains, "The HURRY UP NO HUDDLE is just as its title says... we run our offense at [a] fast and furious pace the entire game." Teams that employ this strategy execute offensive plays in a rapid succession in order to gain an advantage by giving their opponent's defense less time to make adjustments and substitutions. This strategy has largely been successful.[2]

Against the backdrop of this innovation, in February of 2014, the NCAA Football Rules Committee announced a proposed a rule change that would require that ten seconds elapse between offensive plays to allow the defense an opportunity to make substitutions (Johnson February 12, 2014a). The committee acknowledged that the new rule was in response to an increasing trend in the number of plays being run per game and the stated reason for the proposed change was to preserve student-athlete health. However, numerous coaches who employ quick-paced offenses charged that the proposed rule was due to an ulterior motivate: it was an attempt by their rivals to negate some of the efficacy of the HUNH offense (Mandel February 13, 2014).[3] Ultimately, the Football Rules Committee decided against implementing the rule, citing a lack of quantitative evidence that it would improve player safety, but admitting that additional study of the topic was warranted (Johnson March 5, 2014b).[4] To date, the NCAA has not conducted such a study.

We seek to inform this debate by testing for a plausibly causal relationship between the pace of play and injuries in college football. To do so, we construct a novel dataset by linking several

[1] Over the five years covered by the data used for our analysis, the average number of plays per game increased by 4.7 percent. The change in the right tail of the distribution of plays per game is even more stark: there was a 6.6 (8.2) percent increase in the 90th (95th) percentile of plays per game over that same time period.

[2] Anecdotally, HUNH teams such as Auburn, Clemson, and Oregon have either won or played for the National Championship in recent years.

[3] Prior to discussion of the proposed rule, Alabama's Nick Saban and Arkansas' Bret Bilema made public comments expressing concern over a link between the HUNH offense and player injuries. Both coaches employ more traditional offenses and are conference rivals of prominent HUNH teams. Additionally, both were present when the new rule was discussed.

[4] Although the Rules Committee did not implement the rule, the issue is far from settled. Over a year later, Bilema renewed his concern about up-tempo offenses after one of his former player retired from the National Football League, citing injury concerns (Hayes March 17, 2015).

publicly available sources to create a five year panel that contains information on all games in the NCAA's highest division. Our dataset contains information on the number of injuries reported after each game; game statistics (including the number of plays run per game); and a wide array of team, opponent, and game controls. We use this data to estimate Poisson regressions of the determinants of reported injuries, including measures of the pace of play. Since the number of plays that a team runs is not determined at random, potential confounding factors prevent causal inference.[5] We identify causal relationships between pace of play and injuries in two ways.

First, we specify a model that uses a combination of fixed-effects and control variables to address endogeneity concerns. Specifically, by including team-by-season fixed effects, we control for unobserved factors specific to each particular group of players and coaches.[6] To address match-up specific factors that vary within each season for a given team, we include the betting market point spread to control for all known differences between teams that are expected to affect the outcome of the game.[7] We also control for game-varying weather related conditions and within-game factors that may not affect perceptions of the outcome, but potentially influence the number of player injuries. The identifying assumption in these specifications is that we adequately control for all relevant unobserved factors that are correlated with the pace of the game, rendering injuries as good as random conditional on our controls.

Second, we inform the validity of this assumption by estimating a regression discontinuity (RD) model specification that requires different conditions for identification. The RD model compares injury outcomes in games that narrowly go to overtime and in those that do not. Overtime games experience a plausibly exogenous increase in the number of plays run. Causal identification in the RD specification occurs so long as unobserved factors do not similarly change discretely when a game goes to overtime. There is a trade-off in using this alternative means of identification: it is a

[5]For instance, HUNH teams may be constructed, coached, or conditioned in ways that make their players less likely to be injured as more plays are executed. Alternatively, a team playing a vastly superior opponent may need to defend more plays and suffer more injuries because of the talent gap.

[6]These include talent level, training methods, coaching philosophies, and injury reporting policies.

[7]See Sauer (1998, Section 5) for a discussion of the theory of informationally efficient gambling markets and a review of the empirical evidence. Exploiting the information contained in the spread to facilitate analysis is common in the literature. Card and Dahl (2011) rely on the informational efficiency of gambling markets to identify the impact that upset losses in professional football have on domestic violence. In terms of collegiate football, Humphreys et al. (2016) use the spread as an unbiased measure of performance expectations to study the determinants of coaching turnover.

local estimate based on a particular subset of the range of plays per game that may not extrapolate globally (Imbens and Lemieux 2008). Thus, we use both specifications to inform our research question.

We find that the number of plays run in a game increases the number of injuries that occur as a result of increased exposures to a risk of injury on each play, but we find no evidence of an additive effect of increased pace. In other words, the effect of running the first play is the same as the effect of running the $100th$ play. This effect is heterogeneous by platoon: every ten additional plays cause a 3 increase in the number of injuries to offensive players and a 9 increase in the number of injuries to defensive players. RD model estimates indicate that we are able to estimate the causal impact of plays on injuries for defensive players, but are inconclusive for offenses. We are unable to determine whether the insignificant offensive effects in our RD models indicate that our offensive platoon fixed effects model estimates are inconsistently estimated or whether local and global effects are simply different. In contrast, results for defensive platoons are robust, both to a variety of different fixed effects and when we disaggregate platoons to the position level. We further examine the effects of plays per game by injury type, finding that there is a statistically significant, 13% increase in the number of defensive players who suffer a concussion for each additional ten plays run. Overall, we determine that there are grounds to limit the number of plays per game in college football in the interest of player safety, but we find no evidence that the HUNH offense leads to worse outcomes for players above and beyond the effects of additional exposure to injury associated with each play. This suggests that any rule change that limits exposure will reduce injuries, not just those that target the HUNH offense.

We make several contributions to the literature. First, to the best of our knowledge, we are the first to develop methodologies that identify the causal effect that coaching strategies have on the frequency of athlete injuries.[8] More than that, we know of no systematic, empirical analysis of the causal effect of any factors on injuries in football players.[9] Developing a methodology to recover the causes of athlete injuries is of interest not only to inform the policy debate this paper directly

[8]Martini et al. (2013) compare head impacts in run-first and pass-first offenses based on data from accelerometers placed in the helmets of high school football players. They do so using tests of differences in (unconditional) means.

[9]The only tangentially related work in the economics field analyzes the impact playing professional football has on on player mortality (Williams 2012, Koning et al. 2014).

addresses, but also as a building block for future researchers working on issues related to player safety. These issues are of particular importance, and are receiving increased scrutiny, due to our growing understanding of the severity of the long-term effects injuries from football, particularly a debilitating form of brain damage known as Chronic Traumatic Encephalopathy (CTE) (Mez et al. 2017, Montenigro et al. 2017). For example, in an effort to better understand the long-run effects of such injuries, several states have begun to collect data on concussions suffered by their high school athletes, and researchers at the Centers for Disease Control and Prevention (CDC) are working to establish a national brain injury database (Vertuno December 12, 2016). Methodologies that address the issues we outline in this work are essential for such observational "big data" to be useful for analysis of player-safety interventions in a context not well suited to randomized, controlled trials.

Second, we believe that ours is the first study of its kind, in part, due to data constraints. Our analysis is based on a novel panel dataset that we construct to overcome those constraints. We develop this dataset by linking the scant available systematic information on injuries to readily and amply available game, team, and gambling market data. No similar dataset exists. Previous work in this area is largely based on NCAA Injury Surveillance System (ISS) data.[10] This data contains a great deal of information on the circumstances surrounding an athlete's injury, the type of injury, and the duration of recovery, but it also has two notable shortcomings. First, it is based on a voluntary survey of a small number of programs in any given year. Thus, it represents a potentially severely selected sample. Second, due to Health Insurance Portability and Accountability Act (HIPAA) restrictions, ISS data neither contains player identities nor any other identifiable player information (e.g., player team or date of injury) (Kerr et al. 2014). Thus, researchers are unable to use this official, NCAA data source to perform studies that can adequately control for a myriad of relevant factors that confound causal estimation.[11] The dataset that we construct addresses both of these issues. It contains information about all football games in the NCAA's top division over a five year period. While we are unable to link player-level injuries to their time on the field directly,

[10] See Dick et al. (2007a) and Kerr et al. (2014) for a complete description of the ISS data.

[11] The literature based on the ISS data is largely descriptive (Dick et al. (2007b)) or focuses on the correlation of risk factors with specific injuries (see, for instance: Dragoo et al. (2012a), Dragoo et al. (2013) (ACL injuries); Dragoo et al. (2012b) (AC joint injuries), or Cross et al. (2013) (hamstring injuries).

we are able to control for a much richer set of important covariates than would be possible with the ISS data.

Third, we explicitly test whether the HUNH offense leads to increased injuries in NCAA football. We do so to address an important player-safety policy issue first raised by coaches in NCAA football over three seasons ago. Discussion of the proposed policy was tabled because the information that we provide in this paper did not exist, and there has been no subsequent discussion of the policy. Our hope is that our work will inform future debates of this and similar policies aimed at improving athlete health.

Finally, our estimates speak to more than just our HUNH research question. More generally, they accurately and concretely inform athletes, coaches, policymakers, and parents of the consequences of their decisions. Specifically, our estimates report the propensity for injury associated with each play in NCAA football. In doing so, we estimate the causal relationship between participation in college football games and concussions. The magnitude and significance of this estimate for defensive players is a cause for concern given the burgeoning evidence of a link between concussive (and/or repeated sub-concussive) injuries and CTE (Stein et al. 2015, McKee et al. 2016). Knowledge of these injury risks will allow athletes to better understand the health costs of their participation in the sport and make fully informed decisions regarding participation.

In addition to the implications of our findings for the health of NCAA athletes, there are broad policy implications of our work to the health of the NCAA itself. NCAA football is an over $4 billion per year industry, but the industry faces an uncertain future.[12] The debate over the the effects of the proposed "pace of play" rule on athlete health is a microcosm of a broader issue confronting the NCAA, the National Football League (NFL), and their players. CTE and other injury concerns are a serious threat to the long-term viability of the sport. In order to for college football to remain sustainable, the NCAA must develop ways to protect the health of the athletes that are the essential input in the production of their product. Understanding the causes of injuries is the first step in developing such protections for the over 90,000 students who participate in college football each

[12]The dollar per year estimate is based on the authors' calculations of total football revenue using 2016 United States Department of Education (DOE) Equity in Athletics Disclosure Act (EADA) available from http://ope.ed.gov/athletics/.

year and to the benefit of the tens of millions of fans who follow the sport.[13]

The next section details a simple model that forms the basis of our empirical tests. Section 3 provides details of the dataset we construct to conduct our analysis, and the following section outlines our empirical specification. Section 5 presents our results and the final section offers concluding thoughts.

2 Conceptual Framework

We develop a simple model to formalize the relationship between pace of play and injuries. As this relationship is not an economic one, our modest goal in this section is to develop a framework that will allow us to formalize the hypotheses stated by stakeholders in the controversy and that can easily be generalized to other contexts. Let y denote a measure of injuries and let p denote a measure of the pace of the game (number of plays, time between plays, etc.). Additionally, define a vector of additional, observable characteristics that influence injuries, X, and an error term, u. The latter accounts for unobservable factors that affect the occurrence of injuries. We model the occurrence of injuries as

$$y = f(p, X, u) \tag{1}$$

where $f(\cdot)$ is a function that maps pace and the other factors into the injury measure.

Pace of play can affect the likelihood of injury along both the extensive and intensive margins. The extensive effect is due to each additional play resulting in an additional opportunity for an injury to occur. Formally, we test for evidence of this effect by testing the following hypothesis against the alternative: $H_0: \frac{\delta y}{\delta p} = 0$; $H_A: \frac{\delta y}{\delta p} > 0$. We refer to this as the "Saban hypothesis" based on Coach Saban's colorful explanation for why he is a proponent of the rule change.[14]

[13] The estimate of college football athletes is based on the authors' calculations of total participants across all NCAA divisions reported in the 2016 EADA data. Participants include student athletes on a team's roster, students receiving athletically related aid, and those who practice with the team. The estimate of the number of college football fans comes from Silver (September 19, 2011).

[14] In Axson (March 4, 2014), Saban is quoted as saying, "The fastball guys (up-tempo coaches) say there's no data out there, and I guess you have to use some logic. What's the logic? If you smoke one cigarette, do you have the same chances of getting cancer if you smoke 20? I guess there's no study that specifically says that. But logically, we would say, 'Yeah, there probably is.'"

Much in the same way that flipping a coin more times will result in more realizations of "heads," the extensive effect should be evident even if injuries are idiosyncratic events. The more troubling concern for NCAA policymakers is that HUNH offenses may affect injuries along the intensive margin. This would occur if players are more susceptible to injuries when they are fatigued, either for physical reasons or because fatigue leads to poor decision-making or technique that causes injury. In terms of our framework, we can test for an intensive effect by testing whether injuries increase at an increasing rate in the number of plays: $H_0 : \frac{\delta^2 y}{\delta p^2} = 0$; $H_A : \frac{\delta^2 y}{\delta p^2} > 0$. We refer to this as the "Malzahn hypothesis" as Coach Malzahn (2003) explains that the ultimate goal of the HUNH is to "mentally and physically wear down your opponent."[15]

3 Data

In order to test these hypotheses, we build a novel panel dataset of all football games over a five year period encompassing the 2008 through 2012 seasons.[16] Our panel links publicly available information on player injuries by team and date to football game statistics. We augment this information with data on the point spread and local weather conditions to provide additional controls.

3.1 Injury Data

Our primary data source is detailed injury information obtained from the now defunct College Injury Report website (www.collegeinjuryreport.com).[17] The website collected and organized weekly press releases made by NCAA football school athletic departments related to player news, including injuries. This data is available for all NCAA Division I Football Bowl Subdivision (FBS

[15]Note that it may also be the case that the HUNH reduces injuries because teams that run more plays are better conditioned or because some players take plays off (Clegg (November 12, 2015) provides anecdotal evidence that the latter occurs). If so, our alternative hypothesis would instead require a "less than" sign. Our empirical specification is agnostic to whether the $f(\cdot)$ function is convex or concave in p.

[16]We limit our analysis to this time period based on the availability of our injury data. See Section 3.1 for more detail.

[17]After we downloaded the data, the website posted a message that says, "There will be no College Injury Report this year as we work to make the site better." An archive of the website can be accessed via the Internet Archive's Wayback Machine at https://web.archive.org/web/20151022055142/http://www.collegeinjuryreport.com/injury-report-archive.php.

- formerly known as Division I-A) football teams covering the 2008-2012 seasons. Our empirical analyses are limited to the seasons for which these data are available. From this source, we obtain information on the date of the press release, the player's team, name, position, type of injury, and body part injured.

As there are a vast number of injury types listed in the press releases, we aggregate all injury types together and focus on total injuries in our main analyses. Press releases unrelated to injuries (such as suspensions, transfers, illness/disease, etc.) are dropped from the dataset. For a subsequent analysis, we also aggregate injury types into the following five general categories: concussion/head, musculoskeletal upper body, musculoskeletal lower body, miscellaneous, mixed, and undisclosed.[18] Similarly, since the press releases record over 50 player positions, we aggregate to the platoon level: offense, defense, and special teams. If a player played with special teams in addition to an offensive or defensive platoon, then that player was assigned to the non-special teams platoon. If a player only participated on special teams, his injuries are not counted in our analyses because the special teams platoon is not directly affected by the HUNH offense.[19] Analogous to the injury categories, we categorize the full universe of positions into more general groups. For the offense, these position groups are: Quarterbacks (QB), Running Backs (RB), Offensive Linemen (OL), Wide Receivers (WR); and for the defense, they are: Defensive Linemen (DL), Linebackers (LB), Defensive Backs (DB).

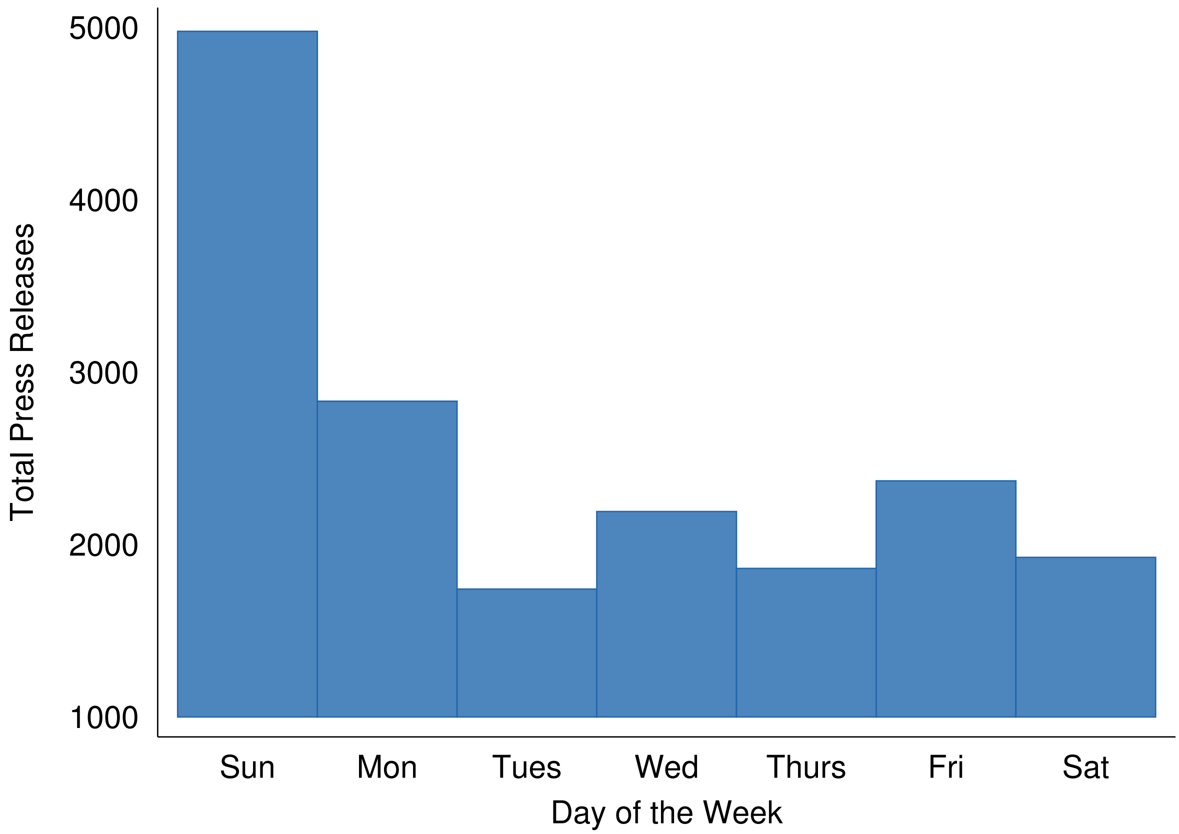
There are two key limitations to this data source. First, the only temporal information in this data is the date of the press release (and not the exact date or time of the injury), so we map injuries to games by including injuries reported the day of a given game until the day before the next game as occurring as a result of the given game. Injuries are frequently recorded on days on which the player's team did not play a game. While some of the listed injuries were no doubt sustained during practice, this is likely an artifact of the delay in fully diagnosing and releasing information on injuries sustained in a game. Most college football games are played on Saturdays. Consistent with our information lag hypothesis, Figure 1 shows that the majority of injury announcements are

[18]Appendix Section A.1 contains a table that lists all injuries by category.

[19]Thus, a limitation of our data is that it will over-estimate injuries for offense or defense in the case of a player who plays on offense or defense and is injured during a special teams play.

released on Sundays and Mondays. [20]

Figure 1: Press Releases by Day of the Week



Second, we observe multiple press release entries related to the same injury for some players. For instance, a school may issue a press release shortly after the injury occurs that says that the player sustained a "lower leg injury" and issue an update the following week that the injury was diagnosed to be a "torn anterior cruciate ligament (ACL)." Without reading each press release and coding each of over $25,000$ observations by hand, we are able to differentiate between new injuries and updates about an existing injury. To address this issue, we create three measures of

[20]To the extent that this pattern is not due to a delay in reporting, our injury measure is constructed under the assumption that all injuries that occur after a game are indirectly the result of the game. This is equivalent to the assumption that the effects of additional plays during a game are cumulative and have the potential to affect the propensity for injury throughout the following week. Under this assumption, our injury counts are likely to over-report injuries sustained during the game and bias our results towards rejecting our null hypotheses. This is the most conservative assumption with regards to athlete safety, but is problematic with regards to quantifying injury rates per play.

injury counts based on different definitions of what constitutes a new injury: "First," "Week Gap," and "All."

1. First - count only the first injury reported for each player each season and ignore all subsequent injuries. This will provide an underestimate of the true number of injuries as some players sustain multiple, distinct injuries in a season.

2. Week Gap - count an injury only if the player did not have an injury report in the same category the previous week, but otherwise allow for multiple injuries. In other words, there must be a week between press releases reporting an injury in a given category to add to the count. This mitigates the measurement error from counting only a player's first injury, but it is not a perfect measure. A player may be out for multiple weeks due to an injury when a press release updating his recovery is issued. In such a case we would count the second press release as a new injury, so this definition may overstate the actual count of injuries (but not to the extent that counting all listed injuries as new injuries would).

3. All - count all reported injures as new injuries. This will overstate the true number of injuries.

Our first measure is a lower bound on the true injury count, and both the second and third measures are upper bounds. The "Week Gap" definition of a new injury is our preferred specification. Panel A of Table 1 provides empirical support for this decision. It contains summary statistics for the injury data. The table displays mean injury counts by platoon in Columns (1) and (3) and the associated standard deviations in Columns (2) and (4). The "Total Injuries" subpanel shows the average number of injuries per game for each of our three injury definitions.[21] Mean injury counts are similar for the "First" and "Week Gap" definitions, but the "All" definition averages are over twice as large. Since the true number of injuries should fall in the narrow range between the first two measures, either is a reasonable proxy for the true number of injuries.[22]

[21] The "Injuries by Type (Week Gap)" subpanel disaggregates injuries by category based on our preferred "Week Gap" injury definition. Table 1 provides moments disaggregated by player position.

[22] Empirically, estimates based on the "First" and "Week Gap" injury definitions are very similar, but those based on the "All" injury definition are much lower. We attribute this to attenuation bias due to measurement error in the third definition. See Appendix Section A.4 for more details.

Table 1: Sample Moments of Injury and Control Variables

	(1)	(2)	(3)	(4)
	Offense		Defense	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Panel A: Injury Data				
Total Injuries				
Total Injuries (First)	0.524	0.909	0.433	0.830
Total Injuries (Week Gap)	0.574	0.955	0.463	0.866
Total Injuries (All)	1.220	1.488	0.979	1.344
Injuries by Type (Week Gap)				
Concussion/Head Injuries	0.048	0.226	0.038	0.203
Upper Body Injuries	0.129	0.386	0.108	0.368
Lower Body Injuries	0.338	0.679	0.260	0.595
Miscellaneous Injuries	0.005	0.071	0.004	0.060
Mixed Injuries	0.004	0.065	0.002	0.046
Undisclosed Injuries	0.050	0.250	0.051	0.272
Panel B: Game Statistics and Additional Controls Data				
Pace				
Plays per Game	91.510	13.090	91.510	13.090
Game				
Consensus Line	-1.030	14.790	-1.030	14.790
Dummy = 1 if Consensus Line Missing	0.081	0.274	0.081	0.274
Dummy = 1 if Home Team	0.500	0.500	0.500	0.500
Dummy = 1 if Neutral Site	0.068	0.251	0.068	0.251
Dummy = 1 if Bowl Game	0.043	0.202	0.043	0.202
Dummy = 1 if Turf Field	0.549	0.498	0.549	0.498
Cumulative Plays per Season	602.300	356.000	595.600	350.500
Days Since Last Game	8.017	6.569	8.017	6.569
Dummy = 1 if First Game of Season	0.120	0.325	0.120	0.325
Weather				
Daily Max. Temp. (F)	59.190	29.420	59.190	29.420
Dummy = 1 if Max. Temp. Missing	0.163	0.369	0.163	0.369
Daily Min. Temp. (F)	40.890	22.320	40.890	22.320
Dummy = 1 if Min. Temp. Missing	0.163	0.370	0.163	0.370
Daily Precipitation (in)	0.077	0.326	0.077	0.326
Dummy = 1 if Precipitation Missing	0.056	0.229	0.056	0.229
Within Game				
Q1 Score Differential (abs val)	7.083	5.830	7.083	5.830
Q2 Score Differential (abs val)	12.400	9.995	12.400	9.995
Q3 Score Differential (abs val)	16.220	12.920	16.220	12.920
Q4 Score Differential (abs val)	18.220	14.620	18.220	14.620
Dummy = 1 if Overtime	0.042	0.200	0.042	0.200
Dummy = 1 if Game Called Early	0.001	0.031	0.001	0.031
Duration of Game (Minutes)	194.200	18.290	194.200	18.290
Rush Attempts per Play	0.409	0.098	0.409	0.098
Pass Attempts per Play	0.342	0.102	0.342	0.102
Observations	8140		8140	

Notes: Due to the adversarial nature of football games, most moments of the "Game Statistics" and "Additional Controls" variables are equivalent for the offensive and defensive platoons.

3.2 Game Statistics Data

Our primary source for explanatory variables consists of game statistics for FBS games each season.[23] SportSource Analytics, LLC collects and standardizes these statistics as part of their Coaches by the Numbers (CBTN) data product.[24] We obtained this data through the College Football Statistics website (cfbstats.com). Observations are recorded at several different levels of aggregation, and we make use of both game- and play-level information. We observe game date, participating teams, location, outcome measures, and basic information about each play (e.g., down and distance, run or pass, yards gained). From this data, we are able to calculate the number of plays per game for both the offense and defense, but we do not observe the actual players on the field for each play. For this reason, we perform our main analyses at the season-game-team-platoon level.[25]

Figure 2 plots the empirical density of total plays per game by team (offense and defense) over our period of analysis.[26] The figure illustrates that there are an average of about 180 plays per game in college football over our period of analysis, so each platoon participates in about 90 plays per game. The standard deviation is over 17 plays, and games with as few as 150 total plays per game or as many 225 plays are not uncommon.[27]

In addition to the number of plays, the game statistics data contain or allow us to calculate numerous control variables. We categorize these variables as game-level, weather, and within game-level controls. Panel B of Table 1 contains sample moments of these measures. At the game level, we are able to control for the location of the game (home, away, or neutral) and a measure of cumulative plays per season.[28] The former allows us to control for potentially different injury effects based on whether the team has a home field advantage or is playing in a bowl game. The latter allows the effect of plays run to accumulate over the course of the season. At the

[23]These statistics are defined according to *The Official National Collegiate Athletic Association Football Statisticians Manual* (2012).

[24]The vendor is the official data provider for the College Football Playoff Selection Committee.

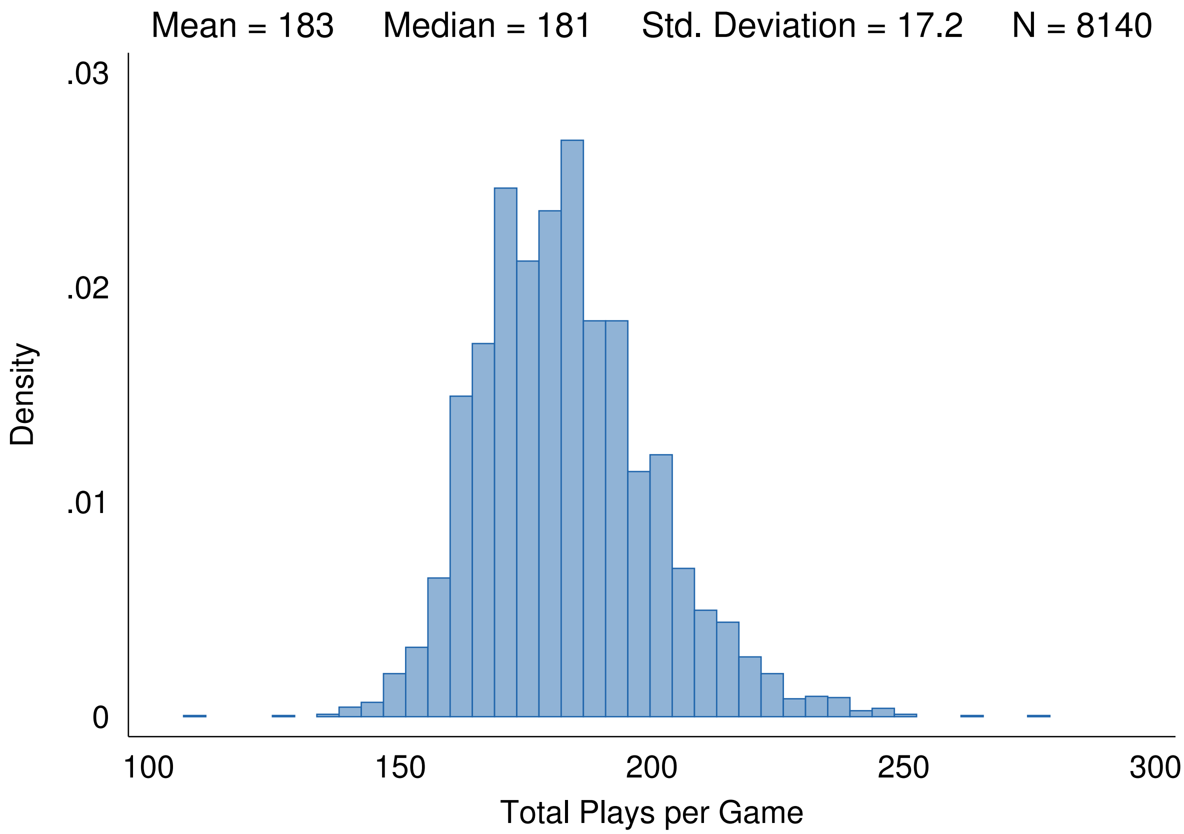
[25]We also estimate models at the season-game-team-position level as a robustness check. See Appendix Section A.6 for additional details.

[26]Panel B of Table 1 contains sample moments of the number of plays per game at the platoon level.

[27]Appendix Section A.2 presents analogous histograms by year to show how the distribution of plays per game changes over time.

[28]We discuss the consensus line and weather variables in the next section.

Figure 2: Histogram of Plays per Game for NCAA Football Teams, 2008-12



within-game level, we calculate measures of the point differential after each quarter. The score difference measures control for the intensity of the game. More injuries may occur if a team is losing and taking risks while attempting a comeback or if players become complacent when their team is winning. We also control for whether the game went into overtime, was called early due to weather, and the total duration of the game in minutes. We include the overtime measure to control for potential fatigue when teams play games longer than what they used to and the game called measure in case knowledge of an approaching lightening storm affects the way athletes play. Finally, we include the duration measure to control for the fact that televised games that have additional timeouts to allow broadcasters to air commercials, and these additional stoppages give players additional time to recuperate.

3.3 Additional Controls Data

We augment our primary datasets with information from two additional sources. First, we obtain the consensus line, or point spread, for each game from The Gold Sheet website. The point spread is a pre-game measure of which team is expected to win and by how many points: a spread of -14 (+14) for a given team indicates that that team is expected to win (lose) by two touchdowns. The consensus line is used to clear gambling markets, and can be thought of as the price of placing a bet on a given team, denominated in points. In this way, it aggregates all the information in the market about the discrepancy in the quality of the competing teams, and it has been shown to be an unbiased and powerful predictor of the outcome of the game (Card and Dahl 2011). We use this measure as a control for the discrepancy in talent, conditioning, and preparation levels between teams. The "Game" subpanel in Panel B of Table 1 contains the sample moments of this variable at the platoon level.

Second, the National Oceanic and Atmospheric Administration (NOAA) provides historic daily weather data at the weather station level as part of their Global Historical Climatological Network (GHCN) dataset (Menne et al. 2012a).[29] We consistently observe measures of the minimum and maximum daily temperature and total precipitation in the GHCN data. We link this information to the game statistic data using the date of the game and the location of the stadium so we can control for local weather conditions on the day of the game.[30] Weather related factors may not be captured by the point spread if they affect both teams equally, but they may have an effect on the propensity for injuries (for instance, if players are more likely to pull muscles in wet conditions or become more fatigued on hot days). Panel B of Table 1 contains the sample moments of our weather variables at the platoon level.

4 Empirical Model

In this section, we develop an econometric framework to test the hypotheses derived from the theoretical model outlined in Equation 1. The appropriate form of $f(\cdot)$ depends on how the injury

[29] See Menne et al. (2012b) for a detailed description of the database.

[30] More precisely, we geo-match stadiums to the local NOAA GHCN weather station.

measure (y) is specified, which, in turn, is a function of the available data. In our case, we observe the number of injuries reported after each game as non-negative integers, so we use panel count data methods (Hausman et al. 1984).[31] Since we observe injury counts for all teams, squads, and games over several seasons, we are able to exploit variation in the multiple longitudinal dimensions of our data, and our main specification is a fixed effects Poisson regression of the effect of pace of play on number of injuries, conditional on controls and fixed effects. We formally outline this model in the next subsection. Throughout, we index teams with j, platoons (offense or defense) with k, season with s, and game with g.[32]

4.1 Baseline Models

Since we are primarily interested in the within game effects of pace of play on player injuries, we define y_{jksg} as the count of injuries that occur to the players on platoon k of team j during and following game g of season s. We assume that y_{jksg} is distributed Poisson and the conditional mean number of injuries is defined using the commonly used exponential form.[33] We estimate separate models by platoon and specify the conditional mean as

$$\mu_{jksg} = \exp\left(\alpha_{js}^{k} + h\left(p_{jksg};\beta^{k}\right) + X_{jksg}\gamma^{k}\right) \; for\ each\ k \in K, \tag{2}$$

where α_{js}^{k} is a team-by-season fixed effect; $h\left(p_{jksg};\beta^{k}\right)$ is a general function of the number of plays (p_{jksg}) for which the given team/platoon was on the field during the given game, as well as parameters (β^{k}); X_{jksg} is a set of controls; and $K = \{offense,\ defense\}$.[34]

[31] See Cameron and Trivedi (2013) for a detailed treatment of these methods.

[32] We also denote positions with ℓ and injury type with m in some specifications.

[33] The fixed effects Poisson model is commonly used in panel data contexts because it is consistent under much weaker distributional assumptions than the fixed effects negative binomial model (Cameron and Trivedi 2013) and there can still be under or over dispersion in the latter model (Wooldridge 2010). We use cluster-robust standard errors to address both overdispersion and serial correlation. Section A.4 indicates that both Poisson and negative binomial model specifications yield similar results.

[34] Formally, the Poisson density is

$$f\left(y_{jksg} \mid p_{jksg}, X_{jksg}, \alpha_{js}^{k}\right) = \frac{\mu_{jksg}\exp\left(-\mu_{jksg}\right)}{y_{jksg}!}.$$

The function $h(\cdot)$ is defined based on the specification. To test the Saban Hypothesis, we specify the function as linear such that $h\left(p_{jksg};\beta^k\right) = p_{jksg}\beta^k$. To test the Malzahn Hypothesis, we specify the function in multiple ways to allow the relationship between plays and injuries to differ based on the number of plays per game. We estimate models that allow for differential intercepts, differential slopes, and both differential slopes and intercepts by quintile.

4.2 Threats to Identification

Consistent estimation of the parameters in the fixed effects Poisson model hinges on the proper specification of the conditional mean and the exogeneity of the explanatory variables conditional on the included fixed effects (Cameron and Trivedi 2005, Wooldridge 2010). The exogeneity of the regressors and the identification of causal β parameters may fail for one of three reasons.[35]

First, our main concern is that teams that run the HUNH are better conditioned, recruit different types of athletes, or may in some other way be predisposed to avoiding injury may gravitate towards the HUNH offense. If so, failure to address these issues would bias our estimates downward. Additionally, injury diagnosis and treatment abilities as well as reporting standards may not uniform across teams or over seasons. To address these concerns, we include team-by-season fixed effects to account for these and other team-season specific unobservables. This requires the assumption that although these abilities and standards may change over, they do so only between seasons. We view this as a reasonable assumption, as schools rarely change coaches, staff, or philosophies during the season.

Second, teams that have a size, speed, or skill advantage or are in some dimension more talented, better coached, or better conditioned than their opponent may run more (or fewer) plays and suffer fewer injuries because of their superiority. Although we can control for team-by-season differences with fixed effects, those measures do not account for match-up specific omitted factors. Card and Dahl (2011) identify a model that requires that the outcomes of NFL games are deter-

[35] The ideal experiment would randomly determine the number of plays a team executes each game or assign teams to an offensive philosophy, then compare the injury rates accordingly. Such experiments are not feasible, so we focus on the likely reasons the number of plays run each game are not determined at random. We then address those issues with our empirical strategy in order to appropriately inform policy discussions.

mined at random by making the assumption that, conditional on the spread, they are. Analogous to Card and Dahl (2011), we assume that betting markets contain all information about relevant differences in overall quality between teams. Thus, the outcome of the game against the spread is random because teams are (conditionally) "evenly matched."

Additionally, game-specific weather conditions may affect both the pace of play and the number of injuries during a game. If poor weather conditions lead to fewer plays being run, but more injuries, our estimates will be biased down. We cannot rely on the spread to control for these differences, as game-level weather conditions may affect expectations of victory for both teams in the same way. To address this issue, we also include controls for the weather conditions on the day of the game.

Finally, within game factors may influence both the number of injuries and the plays called. For instance, teams who are losing may run risky plays at a quick pace when they are frantically attempting a come-back. If so, we would falsely attribute an increase in injuries to the number of plays, rather than the types of plays called, biasing our estimates up. We are unable to observe sufficient detail about the types of plays run during the game, so we control for within game factors by including the score differential after each quarter.[36]

In order to identify the causal effect of plays on injuries, the maintained assumption in our baseline model is that the number of plays run are "as good as random," conditional on our control variables and included fixed effects. We argue that the fixed effects and controls included in our baseline models address all possible endogeneity concerns and our estimates of the β parameters can plausibly be viewed as causal. However, we acknowledge that if any of our previously listed strategies fail to fully address those issues, our identifying assumption fails and our estimates are not consistent.

[36] An additional concern is that if a key player is injured during the game, the team may alter play calling. We do not consider this a major concern because data limitations prevent us from conducting our analysis as the player level, so any effect from such a situation is likely to be small. Additionally, we also estimate regression discontinuity models that are less susceptible to this concern.

4.3 Regression Discontinuity Model

Although we cannot formally test the identifying assumption in our baseline model, we can perform a robustness check to inform its validity. To do so, we modify our preferred specification to accommodate a regression discontinuity (RD) design (Thistlethwaite and Campbell 1960).[37] By restricting our sample to the subset of games that that finish four quarters with a minimal score differential (e.g., three or fewer points), we exploit the discontinuous, plausibly exogenous increase in plays that occurs when a game goes to overtime.[38]

Formally, we specify the conditional mean in these models as

$$\mu_{jksg} = \exp\left(\alpha_j^k + \delta^k o_{jksg} + \tilde{h}\left(p_{jksg}, o_{jksg}; \beta^k\right) + X_{jksg}\gamma^k\right) \; for\ each\ k \in K, \tag{3}$$

where α_j^k is a team fixed effect; o_{jksg} is an indicator equal to one if the game went into overtime; $\tilde{h}\left(p_{jksg}, o_{jksg}; \beta^k\right)$ is a function of the number of plays the team/platoon participated in during the given game that is allowed to differ based on whether or not game g extended into overtime; and the remaining variables and parameters are defined as in Equation (2).[39] The coefficients of interest in this specification are the δ^ks.

Intuitively, one can think of this specification as comparing the number of injuries in a game where one team hits a game-tying field goal as time expires to the injuries in the game if the last-second field goal sails wide. Thus, the resulting increase in the number of plays run in the overtime game is due to the outcome of a kick that is orthogonal to the number of plays run (as opposed to strategy or some other factor that might be correlated with the number of plays), and the effect of

[37]See Hahn et al. (2001) for a formal discussion of identifying conditions in RD, and Imbens and Lemieux (2008) and Lee and Lemieux (2010) for overviews of the RD literature.

[38]Using common RD terminology, our context involves a continuous treatment variable (plays per game). There is a discontinuity in this treatment variable at a cutoff of zero in the running or forcing variable (the fourth quarter score differential).

[39]Note that we include team fixed effects in this specification, as opposed to the team-by-season effects used in previous specifications. Including fixed effects requires adequate within variation for identification. As we restrict our sample to games that have a close finish, including team-by-season fixed effects requires that a team plays multiple close games in a season to be included in the sample. This intersection of restrictions can be quite limiting, so to ensure an adequate sample size, we relax these restrictions to by including team fixed effects. This requires only that a given team plays multiple close games during any season in our panel. To the extent that unobserved team-by-year factors vary continuously as games go to overtime, this is a benign modeling assumption.

the number of plays on injuries has a causal interpretation. While we cannot conduct such an ideal experiment, we can perform an analogous experiment by comparing the average number of plays in “treated” games that go to overtime to those in “control” games that almost went to overtime. Identification requires that a) there is a discrete jump in the number of plays run between these two types of contests, but that b) other season, game and within-game factors vary continuously as the game goes to overtime. By restricting the sample to games with close scores at the end of regulation play, we can hold those factors constant and isolate the causal effect of plays on injuries.

To illustrate that the first of the identification conditions holds, the subfigures in Panel (a) of Figure 3 show a comparison of the average number of plays run in overtime games relative to games with close-finishes. The first subfigure in the panel displays the fourth quarter score differential along the horizontal axis and the average number of plays per game along the vertical axis. The points represent the mean number of plays per game conditional on the given score differential, and the vertical bars display the 95% confidence interval associated with each estimate. The left-most point in the figure indicates that there are an average of 100.4 plays run in the overtime games in our sample. Compared to that average, the remaining points indicate that there were between six and ten fewer plays run in games that conclude regulation play with a minimal point differential.[40] The confidence interval bars for these averages all overlap, indicating that they are not statistically different from one another, but none overlap with the interval for the overtime estimate. Thus, the figure shows that there is a significant increase in the mean number of plays in overtime games.

The second subfigure in Panel (a) of Figure 3 illustrates that going to overtime causes an increase in not just the mean number of plays, but at all points in the distribution of plays per game. The figure plots the kernel density of plays per game conditional on the fourth quarter score differential. The thicker, blue density corresponds to games that went to overtime. Relative to games that ended with a score differential of between one and seven points (depicted by the thinner lines of varying colors), the entire distribution of plays for overtime games is shifted to the right. As one would expect, there are more plays run in overtime games.

While we cannot directly test whether the second condition (about the continuous nature of unobservable factors) holds, we can inform its validity by comparing observable factors between

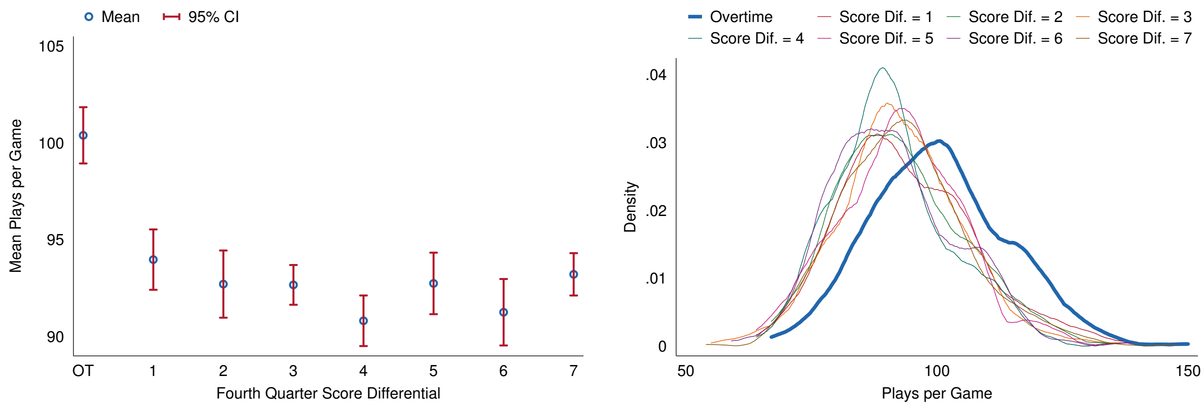
[40]For instance, games that finished regulation with a difference in scores of one point averaged 94.0 plays per game.

regulation and overtime games. We are primarily concerned that games that go to overtime may be played differently than those that do not. For instance, coaches may call different plays when trying to break a tie than they would when they are leading or trailing. To inform the validity of our RD design, Panels (b) and (c) of Figure 3 present subfigures analogous to the previous scatter plots of means and kernel densities for two important within game measures: rush attempts per play and pass attempts per play.[41] Unlike the previous figures, there is no evidence of systematic differences in these measures between overtime and regulation games. We present the results of formal tests analogous to these visual inspections of the continuity of key control variables in Appendix Section A.3. Across multiple specifications, those additional tests show little evidence of discontinuities and support the conclusions drawn from our graphical analysis that our RD design is valid.

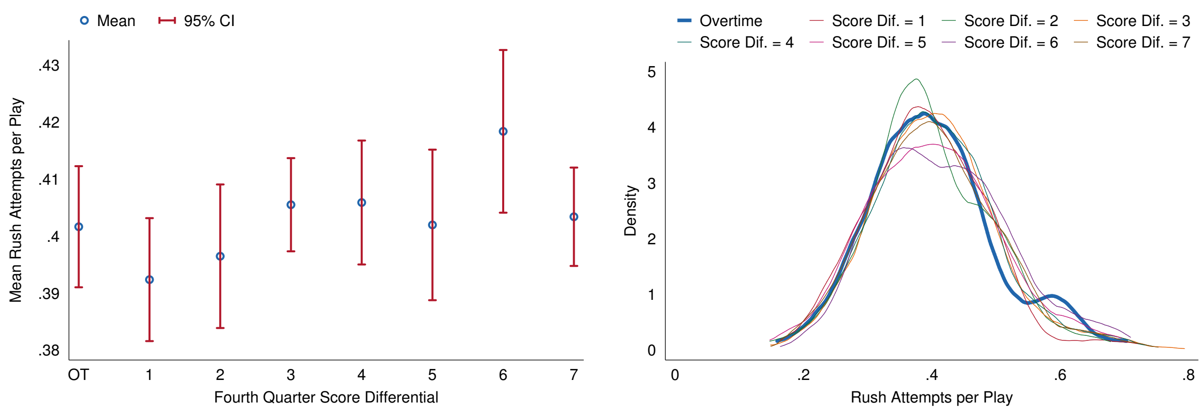
In a broader sense, Lee (2008) shows that RD designs identify causal effects even if the treatment is not completely determined at random. In our case, even though teams have some influence over whether a game goes to overtime, so long as their control over the score at the end of regulation is imperfect, the RD design is valid. This is likely to be true by the adversarial nature of the game. In order to test whether this is the case, Imbens and Lemieux (2008) and Lee and Lemieux (2010) advocate plotting the density of the running variable and examining whether there is a discontinuity at the cutoff that would suggest that sorting around the threshold. Figure 4 displays the empirical density of the fourth quarter score differential. While the nature of scoring in football results in mass points at score differences of three, seven, and ten points, there is no evidence of such a discontinuity at the threshold that sends games to overtime. This is confirmation of the fundamental identifying assumption required for causal identification in the RD design: that teams have imprecise control over the final score.

[41] We standardize by the number of plays to separate the effect of the the given measure from the previously established increase in plays.

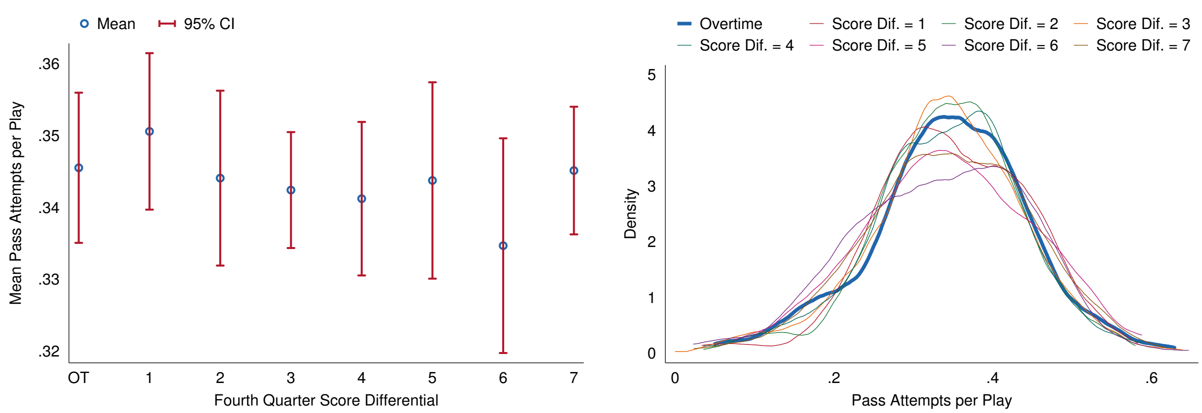
Figure 3: Means and Kernel Densities of Plays per Game, Rush/Pass Attempts per Play by Fourth Quarter Score Differential



(a) Plays per Game

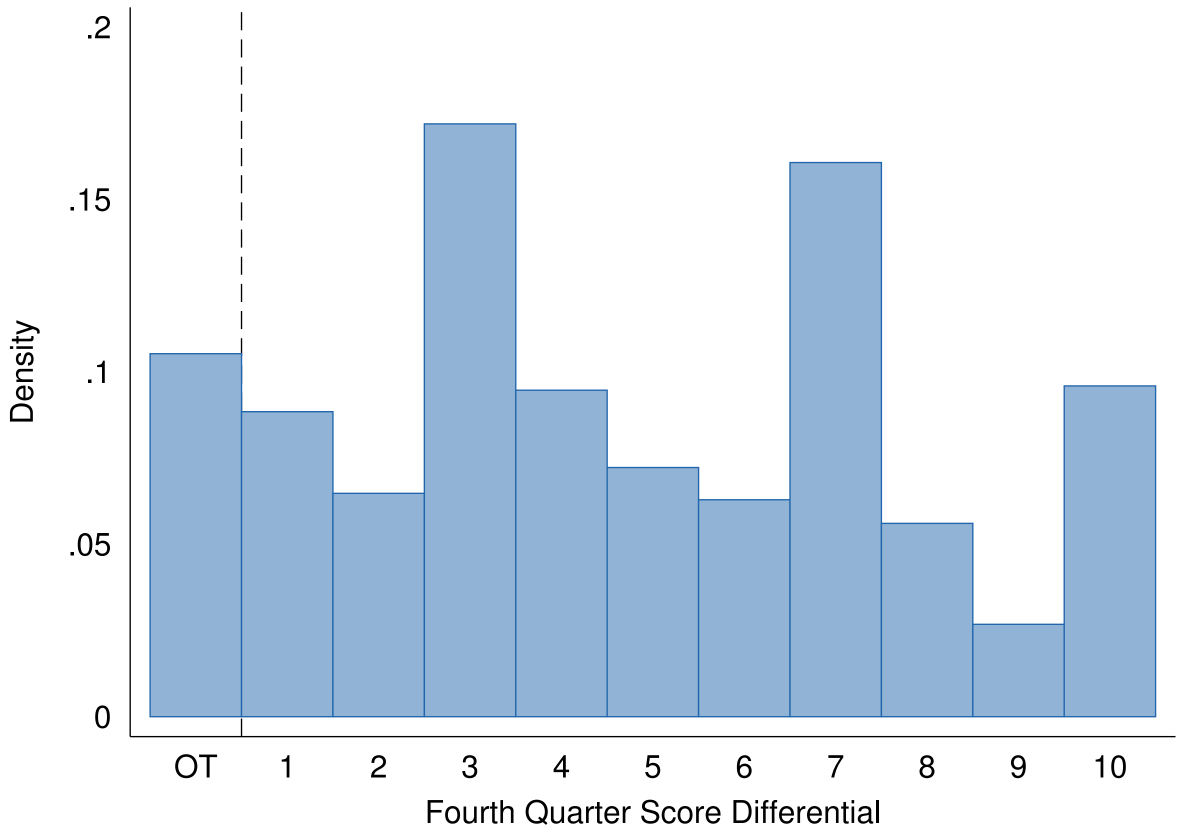


(b) Rush Attempts per Play



(c) Pass Attempts per Play

Figure 4: Histogram of Fourth Quarter Score Differentials



5 Results

All results in this section are based on fixed effects Poisson models using the "Week Gap" injury definition. We present the results of exploratory analyses in Appendix Section A.4 that illustrate that estimated effects are robust to model specification and justify our choice of the "Week Gap" injury definition as our preferred outcome measure. We proceed by presenting the results models used to test both the Saban and Malzahn hypotheses. Next, we present the results of our RD models as a robustness check. Finally, we report position- and injury-specific results.

5.1 Main Results

5.1.1 Testing the Saban Hypothesis

Table 2 contains estimates from our preferred fixed effects Poisson model specification that contain progressively more within-season and within-game controls.[42] Columns (1) and (5) present baseline estimates for offenses and defenses, respectively.[43] Coefficients are semi-elasticities, so a one unit increase in plays-per-game can be interpreted as being associated with a $\left(100 \times \hat{\beta}\right)$ % change in the number of injuries. Thus, the estimate of 0.005 in Column (1) indicates that each additional 10 plays is associated with a 5% increase in injuries for both platoons. As reported in Table 1, results are relative to a baseline of 0.58 (0.47) injuries per offensive (defensive) platoon per game using the "Week Gap" injury definition. We report cluster-robust standard errors that cluster on team-seasons to control for both overdispersion and correlation over the course of a season for a given team.

Table 2: Count Models of the Effect of Pace on Injuries by Platoon and Included Control Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Offense				Defense			
Variable	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.
Plays per Game	0.005***	0.004***	0.004**	0.003*	0.005***	0.007***	0.008***	0.009***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Consensus Line Controls		x	x	x		x	x	x
Game & Weather Controls			x	x			x	x
Within-Game Controls				x				x
Team-Season Effects	x	x	x	x	x	x	x	x
Number of Team-Seasons	600	600	600	600	587	587	587	587
Observations	7615	7615	7615	7615	7447	7447	7447	7447

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
The fixed effects Poisson model drops team-seasons that have fewer than two periods of data (FCS schools that only appear in the data when they play an FBS team) or do not report any injuries over the course of the season. This leads to differences in observation and team-by-season counts between the offensive and defensive fixed effects Poisson models.

Columns (2) and (6) add our key game level control: the consensus line (point spread). The line, in addition to providing a measure of the discrepancy in talent between teams, aggregates

[42] For the full set of estimates, see Appendix Section A.5.
[43] The estimates are the same as those from Column (7) of Appendix Table 8.

all information about the differences between teams if betting markets are efficient (Sauer 1998). Adding this control influences estimates of our coefficient of interest by mitigating the effects of confounding factors.[44] The plays per game estimates decrease from 0.005 to 0.004 for offenses and increase to 0.007 for defenses. Both estimates are significant at the 1 level.

Next, Columns (3) and (7) add additional game-day level controls for both game-level and weather-related factors.[45] Finally, Columns (4) and (8) also include within-game controls. When we include all controls, estimates of our coefficients of interest decrease to 0.003 for offenses and increase to 0.009 for defenses. Both estimates are statistically significant: the offensive platoon estimate at the 10 level and the defensive platoon analog at the 1% level.

Although the magnitudes of our main effects change based on the included controls, the estimates of the effect of the number of plays-per-game are positive and significant across all specifications for both platoons. To the extent that our controls are such that the remaining, unexplained determinants of injuries are purely random, our estimates can be interpreted as causal.[46] Our preferred estimates with the full set of controls in Columns (4) and (8) suggest that every 10 additional plays run in the course of a game cause a 3% increase in the number of offensive injuries and a 9% increase in the number of injuries to defensive players. Our results provide robust support for the Saban hypothesis. To the coach's point, this is an intuitive result. Running more plays leads to more injuries because football is a contact sport and there is a risk of injury on every play. From a policy perspective, however, this information may not be important because it is consistent with widely held priors. We now turn to examining a more policy relevant question by testing the Malzahn hypothesis.

[44] We reran the analysis adding a quadratic spread term and with splines for positive and negative spreads. The coefficients of interest did not change qualitatively.

[45] These controls are discussed in detail in Section 3.2.

[46] Although we cannot test this argument statistically, we provide an alternative test in Section 5.2. Our RD model estimates provide strong support for our defensive estimates, but are inconclusive for our offensive estimates. Additionally, we analyze the sensitivity of our results to the type of fixed effects included in the model in Appendix Section A.7. Findings from that analysis are consistent with those from the RD analysis.

5.1.2 Testing the Malzahn Hypothesis

The specifications in the previous section restrict the effects of pace on injuries to be homogeneous across the entire distribution of plays per game. In order to better inform NCAA policymakers of the extent to which the HUNH offense is harmful to player health (beyond the general effects of participation in football games), we estimate models that repeat our preferred fixed effects Poisson specification with all available controls from Table 2, but with different functional forms for $h(\cdot)$ (see Equation 2) that relax the assumption of a constant effect.

Table 3 contains the effects of three different specifications that test whether the effects of plays vary along the intensive margin. Columns (1) and (4) contain estimates of models that allow the average effect of plays on injuries to differ by quintile. Estimated coefficients are only identified relative to an omitted category, and we use the middle 20 of the distribution as our baseline. For offenses, the estimates provide no evidence of differential effects by quintile of plays run. In contrast, the defensive estimates show that significantly fewer defensive injuries occur in games where the number of plays run are in the bottom two quintiles relative to the middle quintile. However, we find no evidence that there is a statistical difference in the average number of injuries between games where plays per game are in the top 60 of the distribution. Next, we allow the effect of plays per game on injuries to vary by quintile by breaking the plays-per-game effect into five spline terms. Column (2) shows that on the offensive side, none of these effects are individually significantly different from zero. On the defensive side, plays in the top four quintiles have positive, significant estimates, but none of the five slope coefficients are statistically different from one another. Finally, Columns (3) and (6) contain estimates from a model that combines the measures from the previous two columns and allows for both differential intercepts and differential slopes. Estimates show no significant effects for defensive platoons. On the offensive side, the average effect of running a number of plays in the bottom fifth of the distribution is positive and significant, but only at the 10% level. Given the overall pattern of results, we suspect this result is spurious. [47]

[47] We also estimate the model with both quantiles and deciles instead of quintiles. Results are qualitatively similar.

Table 3: Count Models of the Effect of Pace on Injuries by Platoon and Functional Form

	(1)	(2)	(3)	(4)	(5)	(6)
		Offense			Defense	
Variable	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.
Differential Intercept Terms						
First Quintile Indicator	-0.088		-1.771	-0.263***		1.735
	(0.058)		(2.178)	(0.071)		(2.585)
Second Quintile Indicator	0.004		0.669	-0.121*		3.002
	(0.057)		(2.762)	(0.069)		(3.320)
Third Quintile Indicator	0.000		0.000	0.000		0.000
Fourth Quintile Indicator	0.042		-1.250	0.033		0.589
	(0.054)		(2.592)	(0.068)		(3.139)
Fifth Quintile Indicator	0.001		-0.642	0.026		1.973
	(0.061)		(2.204)	(0.069)		(2.592)
Differential Slope Terms						
First Quintile Spline		0.006	0.014*		0.009	0.009
		(0.005)	(0.007)		(0.006)	(0.008)
Second Quintile Spline		0.006	-0.015		0.010*	-0.005
		(0.004)	(0.022)		(0.005)	(0.025)
Third Quintile Spline		0.006	-0.007		0.010**	0.029
		(0.004)	(0.023)		(0.005)	(0.028)
Fourth Quintile Spline		0.006	0.007		0.010**	0.022
		(0.004)	(0.016)		(0.004)	(0.020)
Fifth Quintile Spline		0.005	0.000		0.009**	0.007
		(0.003)	(0.006)		(0.004)	(0.007)
Controls	x	x	x	x	x	x
Team-Season Effects	x	x	x	x	x	x
Number of Team-Seasons	600	600	600	587	587	587
Observations	7615	7615	7615	7447	7447	7447

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
All models include consensus line, game, weather, and within-game controls, as well as team-season fixed effects. The first quintile indicator is a dummy equal to 1 if the number of plays per game falls in the first quintile of the distribution. The first quintile spline is the product of that indicator variable and the number of plays per game to allow injury effects to vary over the range of plays per game. The indicator for the third quintile of plays per game is omitted for identification, hence there are no standard errors associated with those coefficients. The fixed effects Poisson model drops team-seasons that have fewer than two periods of data (FCS schools that only appear in the data when they play an FBS team) or do not report any injuries over the course of the season. This leads to differences in observation and team-by-season counts between the offensive and defensive fixed effects Poisson models.

As a whole, while we find some evidence that there are differential intensity effects by the number of plays run, the pattern of results is not robust. Additionally, the strongest results suggest that running very few plays reduces injuries relative to the baseline, but not that running a high number of plays, as is common for HUNH offenses, increases injuries above and beyond the extensive effect. Based on the results in Table 2, we reject Coach Malzahn's hypothesis that the HUNH offense physically wears opponents down (at least to the point of injury). Although we attribute this effect to Coach Malzahn, a HUNH coach, as a rhetorical device, the coaches of traditional offenses justified their proposed policy change using this argument. This justification is not sound: we fail to find strong evidence that the propensity for injury is different at different points in the distribution of plays run. In other words, we do not find convincing evidence that the HUNH offense directly causes injuries (beyond the indirect effect that there is a chance of injury on every play).

5.2 Regression Discontinuity Model Results

Table 4 reports the results of estimating our RD models. We estimate the model separately by platoon for different bandwidths of score differentials at the end of the fourth quarter that vary from (less than or equal to) one to six points. Columns (1) through (6) contain estimates for offensive platoons and Columns (7) through (12) for defensive platoons. All models contain controls for the effects of plays per game that are allowed to vary depending on whether the game went to overtime. Panel A reports estimates from our preferred specification: GEE NB models without any additional controls. Although inclusion of additional covariates are unnecessary if our identification strategy is valid, Imbens and Lemieux (2008) indicate that such controls can mitigate small sample biases that result from the inclusion of games where the score differential is not close to zero (as the bandwidth increases). Additionally, covariates can increase precision if they are correlated with injuries. Therefore, we also estimate models with varying degrees of additional controls. Estimates in Panel B are from GEE NB models that include our previously discussed control variables (excluding the score difference at the end of the fourth quarter that determines the bandwidth).[48] Panel C contains estimates from fixed effects Poisson models without additional controls, and Panel D reports estimates from fixed effects Poisson models with consensus line,

[48]There are no offensive player injuries associated with bowl games in the RD sub-samples. In order to ensure that the models of offensive platoon injuries converge, we are forced to drop the bowl game indicator from our set of control variables for these models (the estimates reported in Columns (1) through (6) of Panel B).

game & weather, and within-game controls.

The coefficient of interest is the effect of a game going to overtime on injuries. As with our main specification results, coefficients are semi-elasticities. Thus the estimate in Column (1) of Panel A of 0.544 indicates that (the increased number of plays associated with) an overtime game results in a 54.4% increase in the number of injuries to offensive players. Note that our RD estimates are not directly comparable to the estimates from our main specification for three reasons. First, δ^k is the change in injuries due to a discrete jump in plays associated with the game going to overtime, whereas β^k is the effect of an increase in the number of plays run throughout the game. Thus, the range of plays examined is not comparable because overtime increases the number of plays at the end of the game when players may be fatigued or otherwise more susceptible to injury. Second, the $h\left(p_{jksg};\beta^k\right)$ and $\tilde{h}\left(p_{jksg},o_{jksg};\beta^k\right)$ functions are not the same. The latter allows for differential effects of plays in overtime relative to regulation games. Finally, the RD specifications use team (not team-by-year) effects to ensure adequate within variation.[49] Nevertheless, in Section 4.3, we report that games that go to overtime average between six and ten additional plays per game (depending on the bandwidth). Using the upper limit of this average for convenience, $10\hat{\beta} \approx \delta^k$. This means that the estimated RD effects represent much larger percent increases than those in the main specification which range from 3.0% to 9.0% increases for every additional 10 plays. We remind the reader that since these percent changes are against a baseline of 0.58 (0.47) injuries per offensive (defensive) platoon per game, in practically terms, both represent a similar increase in actual injuries.

The table shows that across all four specifications, there are no significant changes in injuries for offenses at the discontinuity. Defenses, however, experience large, often significant increases in injuries when the game goes to overtime and the number of plays increases. These increases are relatively robust: estimates are not very sensitive to changes in the bandwidth and are of similar magnitudes across all four specifications. The estimates in Panels A through C are significant in all cases, save when the bandwidth is small which limits the sample size. Given that the coefficient estimates in these cases are similar in size to those in specifications with larger bandwidths, but the

[49]Teams play less than 15 games a year and all games a team concludes with a large margin of victory/defeat are dropped from the sample when estimating the RD models.

Table 4: RD Count Models of the Effect of Pace on Injuries by Platoon, Bandwidth, and Model Specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Offense						Defense					
Bandwidth:	1 Pt.	2 Pts.	3 Pts.	4 Pts.	5 Pts.	6 Pts.	1 Pt.	2 Pts.	3 Pts.	4 Pts.	5 Pts.	6 Pts.
Variable	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.	Tot. Inj.
Panel A: GEE NB Models without Controls												
OT Discontinuity	0.544	0.241	-0.05	0.084	0.263	0.324	1.106	1.495*	1.837**	1.645**	1.663**	1.748**
	(1.074)	(0.905)	(0.821)	(0.804)	(0.780)	(0.771)	(1.027)	(0.892)	(0.824)	(0.818)	(0.779)	(0.777)
Controls												
Team Effects												
Number of Teams	133	137	149	154	158	162	133	137	149	154	158	162
Observations	622	830	1382	1686	1918	2120	622	830	1382	1686	1918	2120
Panel B: Negative Binomial GEE Models with Controls												
OT Discontinuity	0.239	-0.172	-0.236	-0.122	-0.009	-0.003	1.230	1.388	1.645*	1.504*	1.490*	1.552*
	(1.025)	(0.917)	(0.784)	(0.776)	(0.750)	(0.748)	(1.096)	(0.982)	(0.888)	(0.874)	(0.829)	(0.833)
Controls	x	x	x	x	x	x	x	x	x	x	x	x
Team Effects												
Number of Teams	133	137	149	154	158	162	133	137	149	154	158	162
Observations	622	830	1382	1686	1918	2120	622	830	1382	1686	1918	2120
Panel C: Fixed Effects Poisson Models without Controls												
OT Discontinuity	0.094	0.044	-0.485	-0.248	0.045	0.099	1.261	1.518	1.699**	1.445*	1.502*	1.555**
	(1.218)	(0.941)	(0.822)	(0.822)	(0.795)	(0.782)	(1.208)	(1.005)	(0.848)	(0.818)	(0.776)	(0.784)
Controls												
Team Effects	x	x	x	x	x	x	x	x	x	x	x	x
Number of Teams	98	112	120	121	121	121	94	106	110	115	115	117
Observations	542	780	1338	1630	1854	2046	511	740	1250	1570	1783	2000
Panel D: Fixed Effects Poisson Models with Controls												
OT Discontinuity	-0.707	-0.332	-0.429	-0.236	-0.103	-0.100	0.681	0.562	1.399	1.298	1.311	1.330
	(1.254)	(0.981)	(0.803)	(0.796)	(0.766)	(0.773)	(1.285)	(1.103)	(0.873)	(0.828)	(0.800)	(0.814)
Controls	x	x	x	x	x	x	x	x	x	x	x	x
Team Effects	x	x	x	x	x	x	x	x	x	x	x	x
Number of Teams	98	112	120	121	121	121	94	106	110	115	115	117
Observations	542	780	1338	1630	1854	2046	511	740	1250	1570	1783	2000
Bootstrap Reps.	N/A	N/A	N/A	N/A	N/A	1000	N/A	N/A	N/A	N/A	N/A	1000

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

All models contain controls for the effects of plays per game that are allowed to vary depending on whether the game went to overtime. Panels B and D also includes consensus line, game & weather, and within-game controls (excluding the score difference at the end of the fourth quarter that determines the bandwidth). The offensive platoon models in Panel B exclude the the bowl game indicator as a control variable ensure converge. The Simcoe (2007) variance-covariance matrices for the models in Panel D and a bandwidth greater than five are singular, so we calculate bootstrap standard errors. The fixed effects Poisson model drops teams that have fewer than two periods of data (FCS schools that only appear in the data when they play an FBS team) or do not report any injuries in the given RD subsample. This leads to differences in observation and team counts between the NB GEE and fixed effects Poisson models and between offensive and defensive fixed effects Poisson models.

estimated standard errors are on the order of 20 percent larger, we attribute the lack of significance in Columns (7) and (8) to an insufficient sample size. Additionally, while none of the defensive platoon estimates in Panel D are statistically significant at the conventional levels, we note that the estimates for bandwidths of three or more points are marginally insignificant with p-values of 10.9%, 11.7%, 10.1%, and 10.2%, respectively. Given the modest sample sizes in all of these specifications, we do not interpret the lack of traditional significance in our most saturated model relative to that in the other specifications as evidence that our RD design is flawed.

Overall, these RD estimates are a confirmation of our main specification results for defenses (increases in plays lead to an increase in injuries), but not for offenses (we do not find significant effects as with our main specifications). There are three possible explanations for the discrepancy in offensive results. First, our main specifications may not adequately control for important time varying (within season) effects that our RD specification is able to account for. If so, our main specification estimates are biased upwards, and the true effect of pace on offenses is zero. It is not clear what unobserved factors could cause such a bias. Alternatively, by definition, the increased internal validity associated with RD estimates comes with a cost in terms of external validity (Imbens and Lemieux 2008). The RD estimates are the local effect of an increase in plays at the end of a game as it goes to overtime, and not the global effect of plays on injuries. It may also be the case that this local effect is small, but does not extrapolate to the full range of plays well. If so, our main specification estimates for offenses may be unconfounded, despite not being parsimonious with their RD counterparts. While we suspect that the latter explanation is the case, we are unable to rule out that it is the former that causes the patterns we observe in our estimates.

5.3 Additional Results: Effects by Injury Type

In order to inform whether there is heterogeneity in the effects of plays by injury type or player position, we modify and re-estimate our main specification. In this section, we detail and report results from injury-specific models. Appendix Section A.6 does the same for position-specific models. For the injury-specific effects, we modify the model by restricting the definition of injuries to those in specific categories: concussion, upper body, lower body, miscellaneous, mixed, and undisclosed injuries.[50] We estimate the model separately for each combination of injury type and platoon. Columns (1) through (3) of Table 11 report results for injuries to offensive players, and the remaining columns report the same for defensive player injuries.[51]

Table 5: Count Models of the Effect of Pace on Injuries by Injury Type

	(1)	(2)	(3)	(4)	(5)	(6)
		Offense			Defense	
Variable	Concussion Injuries	Upper-Body Injuries	Lower-Body Injuries	Concussion Injuries	Upper-Body Injuries	Lower-Body Injuries
Plays per Game	0.004	0.000	0.003	0.010**	0.013***	0.007***
	(0.005)	(0.003)	(0.002)	(0.005)	(0.003)	(0.002)
Controls	x	x	x	x	x	x
Team-Season Effects	x	x	x	x	x	x
Number of Team-Seasons	270	460	577	208	411	557
Observations	3420	5828	7322	2641	5221	7068

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
All models include consensus line, game, weather, and within-game controls, as well as team-season fixed effects. The fixed effects Poisson model drops team-seasons that have fewer than two periods of data (FCS schools that only appear in the data when they play an FBS team) or do not report any injuries over the course of the season. This leads to differences in observation and team-by-season counts between models.

Columns (1) and (4) of the table show that the number of plays run has a strong and significant impact on the number defensive players who suffer from a concussion, but that the analogous estimate for offensive players is not statistically significant.[52] For every additional ten plays per game, there is a 10% increase in the number of defensive players who are concussed. The magnitude of this effect is large and a cause for concern given the evidence of a connection between concussions (and/or repeated sub-concussive injuries) and CTE (Stein et al. 2015, McKee et al. 2016).[53] The estimates in Columns (5) and (6) indicate that increasing the number of plays per game also causes

[50]Formally, we let $m \in M$ index injury type and model the conditional mean as

$$\mu_{jksgm} = \exp\left(\alpha_{js}^{km} + h\left(p_{jksgm}; \beta^{km}\right) + X_{jksgm}\gamma^{km}\right) \; for\ each\ k \in K\ \&\ m \in M,$$

where $M =${concussion, lower-body, upper-body, miscellaneous, mixed, undisclosed}.

[51]We also estimate the effects of plays on miscellaneous, mixed, and undisclosed injuries. Effects are rarely significant for either offensive or defensive platoons.

[52]For a synopsis of our current understanding of concussions, see Khurana and Kaye (2012).

[53]The Crisco et al. (2010) accelerometer study finds that college football players average 14.3 (6.3) head impacts per game (practice).

defensive players to suffer upper- and lower-body injuries at a much higher rate than their offensive teammates. The statistically significant upper- and lower-body estimates of 0.013 and 0.007 for defensive players are similar in magnitude to the effect on concussions. Overall, we do not find compelling evidence that any one type of injury is driving our results.

5.4 Economic Significance of Results

Our estimates indicate the causal effect pace of play has on injury rates. In order to provide additional context for the magnitude of these effects, we perform back-of-the-envelope calculations to determine the expected dollar cost of injuries resulting from an increase in plays per game. To calculate these costs, we adapt the methodology outlined by Fair and Champa (2017) to our context. Fair and Champa (2017) calculate the total reduction in injury costs to both NCAA and high school athletes from a hypothetical policy change that results in contact sports having injury rates similar to those in non-contact sports. Their methodology uses the years lost to disability (YLD) component of the disability-adjusted life-year (DALY) measure common to the public health literature and estimates of the value of a statistical life used for cost-benefit analysis and policy evaluation to place a dollar cost on each player injury.[54] Using our data and the estimates from our main specification, our back-of-the-envelope calculations indicate that every additional ten plays per game results in expected injury costs of $\$7,278$ per game for offenses and $\$5,870$ per game for defenses.[55] There were an average of 841 games per season in our dataset. If all NCAA teams ran an additional ten plays per game, collegiate football players would suffer from an increase of over $11 million worth of injury related costs per season.

[54]DALYs were created to quantify injury, disease, and death for the Global Burden of Disease Study (GBDS). They measure the deviation of a given population's health from an ideal state of where individuals live their complete life expectancy in full health. DALYs are calculated as the sum of two terms: years of life lost (YLLs) and YLDs. Note that YLDs are referred to as both years lost to disability and years lived with disability, depending on the source. See World Health Organization (2018) for a basic description of DALYs and/or Murray and Acharya (1997) for a more detailed overview. See US Burden of Disease Collaborators (2013) for an overview of the 2010 GBDS. The value of a statistical life is a measure of willingness to pay to reduce the risk of death. See Ashenfelter (2006) for an overview.

[55]For a detailed description of how we calculate these costs, see Appendix Section A.8.

6 Conclusions

While previous studies have documented correlations between observable factors and injuries, we develop strategies to plausibly recover causal estimates of any factor on athlete injuries. Specifically, we estimate the effects that pace of play has on injuries in college football with count models using two different identification strategies. First, we use team-season fixed effects, the spread, and other control variables to ensure that the number of plays run per game is conditionally random. Second, we use RD methods to exploit a plausibly exogenous increase in the number of plays due to the game going to overtime. In doing so, we believe we are the first to quantify the magnitudes of rates of injuries per play in NCAA or pro football. Across specifications, we find robust evidence that an increase in the number of plays causes an increase in the number of injuries for defensive players, and mixed evidence that the same holds true for offensive players.

Our analysis is motivated by a recently proposed rule change that was said to limit the deleterious effects of new HUNH offenses that execute plays in rapid succession in order to gain an advantage over their opponents. According to Solomon (February 13, 2014), proponents of the rule argued that fatigue caused by that rapid succession of plays causes poor technique and positioning that lead to injuries. Opponents of the rule argued that it was actually a form of gamesmanship by coaches who run traditional offenses, and it would have only a minimal effect on player health.[56] All agreed that there was neither data nor evidence to support or refute either side's claims. Our study fills this gap.

While our findings suggest that the proposed policy would reduce injuries in NCAA football, we caution that that our results do not suggest that the HUNH directly causes injuries. We find no evidence that this is the case. Rather, there is a constant risk of injury on every play. The HUNH indirectly leads to an increase in injuries through an increase in the number plays. We note that any policy that limits plays (shorter seasons, shorter games, fewer clock stoppages, etc.) would have the same effect, likely with less pronounced distributional effects.[57] If the NCAA Football Rules

[56] Most of the experts Solomon (February 13, 2014) interviewed suggested that the rule would reduce injuries only by reducing the number of plays per game. Randy Cohen, the head athletic trainer at the University of Arizona (a team that runs a HUNH offense), provided anecdotal evidence that traditional offenses result in more injuries.

[57] We suspect that reducing the amount of allowable practice time or limiting the amount of physical contact during practice would also have the same effect, although data limitations do not allow us to directly test this hypothesis.

Committee wishes enact policies that reduce athlete injuries, it should evaluate the costs of all such play limiting policies and choose the policy (or combination of policies) that reduces injuries with the minimal cost.

We conclude by offering a caveat and a call for improved data collection. The long-term viability of the sport of football depends on finding creative ways to reduce injury risk while still preserving the character of the game. These solutions can only be found after careful analysis of a transparent accounting of the events surrounding and details of player injuries. Our study is only as good as the available data, and while our novel dataset is an improvement over the official NCAA injury (ISS) data, it is still imperfect for studying the causes of player injuries. Particularly, we are unable to link player injuries to the actual events that caused those injuries (and the history of plays preceding those injuries). These limitations are due to the NCAA's decision not to collect complete participation and injury histories for all players. More precise results and discoveries would be possible with the existence of such granular data and a policy that grants researchers access that data for approved projects without compromising player privacy. Given the importance of player health issues and the volume of all other types of sports data available, this seems both an important and feasible undertaking for the NCAA.

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A Appendix

A.1 Injuries and Injury Categories

Table 6 lists the unique set of injuries observed in our dataset and how they map to five general injury categories.

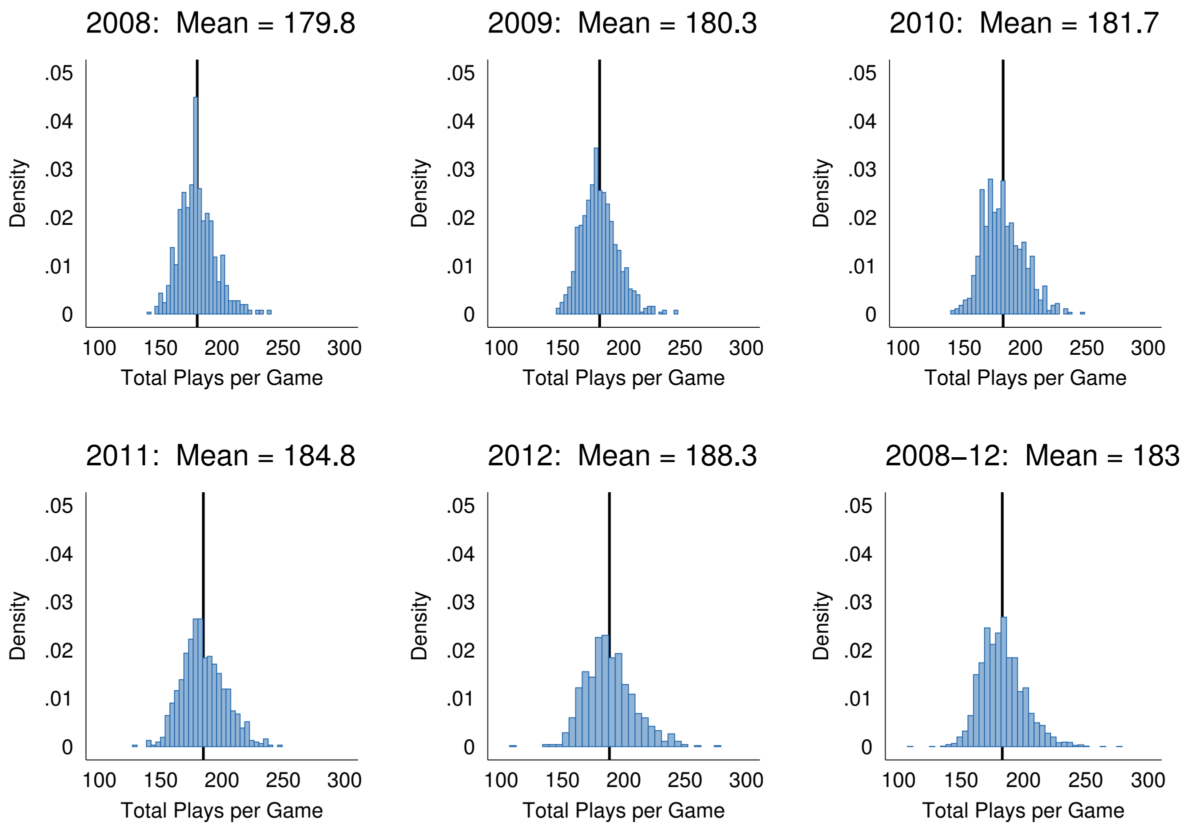
Table 6: List of Injuries by Injury Category

Injury Category	Injuries
Concussion/Head	Concussion, head, head/hand, head/neck, head/wrist, neck/concussion
Upper Body	Abdominal, arm, arm/elbow, arm/wrist, back, back/ribs, bicep, body, broken collarbone, chest, clavicle, collarbone, elbow, finger, forearm, hand, hand/personal, hand/wrist, jaw, midsection, mouth, neck, neck/back, neck/larynx, neck/shoulder, neck/spine, nose, oblique, pectoral, pulmonary contusion, rib, rib/lat, ribs, shoulder, shoulder blade, shoulder/elbow, shoulder/neck, shoulder/personal, sore neck, spine, sternum, stinger, throat, thumb, thumb/shoulder, torn labrum, torso, transient quadriplegia, triceps, upper body, wrist
Lower Body	Achilles, ankle, ankle/hamstring, ankle/sick, ankle/toe, arch, broken leg, bruised thigh, calf, calf/ankle, feet, fibula, fibula/torn acl, flu/foot, foot, foot/ankle, foot/elbow, foot/leg, gluteus, groin, groin/hamstring, groin/leg, hamstring, heel, hernia, high ankle sprain, hip, hip/rib, knee, knee/ankle, knee/transfer, leg, leg/ankle, leg/knee, lower body, lower extremity, lower leg, lower leg/foot, mcl, pelvis, plantar fasciitis/heel, quad, right knee, shin, tailbone, thigh, tibia, toe, torn achilles, torn acl, torn mcl, upper leg
Miscellaneous	Abrasions, contusion, eye, face, fatigue, heart, heat issues, illness, internal organ, irregular heartbeat, kidney, laceration, lung, migraines, muscle pull, pinched nerve, pulled muscle, rest, soft tissue, spleen, staph infection, stomach, stress fracture, stroke, tooth, vision
Mixed	Ankle/shoulder, elbow/knee, groin/shoulder, hand/knee, hand/thigh, leg/shoulder, multiple, shoulder and foot, shoulder/knee, shoulder/leg, various
Undisclosed	Medical, uncertain, undisclosed

A.2 Density of Plays per Game by Year

Figure 5 plots the empirical density of total plays per game by team (offense and defense) for each season in our analysis, with vertical bars indicating the mean. The figure illustrates a slight increase in the plays per game over time as the distribution shifts to the right: from an average of 180 total plays per game in 2008 to 188 total plays in 2012.

Figure 5: Histograms of Plays per Game for NCAA Football Teams by Year



A.3 Additional Tests of RD Validity

Formal tests of the continuity of observable factors are traditionally done by performing an analogous RD analysis on each of the control variables. Failure to find evidence of discontinuities in these variables is consistent with a valid RD design. We do so by modeling the effect of the fourth quarter score differential on rushes and passes per play with both linear and quadratic forms. Formally, we estimate

$$X_{jksg} = \alpha^k + \delta^k o_{jksg} + \grave{h}\left(d_{jksg}, \beta^k\right) + \varepsilon_{jksg} \; \forall \, k,$$

as a two equation SUR system where X_{jksg} is a vector of controls (rushes, passes per play), o_{jksg} is a vector of indicators equal to one if the game went into overtime, d_{jksg} is a vector of fourth score differentials, $\grave{h}\left(d_{jksg}, \beta^k\right)$ is a function of the score differential in the given game (in practice, either linear or quadratic), and ε_{jksg} is a vector of error terms. Table 7 presents estimates of the δ^k parameter for both platoons across multiple bandwidths. The results in Panel A model $\grave{h}\left(d_{jksg}, \beta^k\right)$ as a linear function and the results in Panel B model it as quadratic. Coefficient estimates show little evidence of discontinuities across these multiple specifications.[58]

A.4 Exploratory Analysis

Table 8 presents the results of three different specifications of basic count models to establish general patterns in the data. The models also explore how the effects of the number of plays per game vary with our three injury definitions: "First," "Week Gap," and "All." Panel A presents the results for offensive player injuries, and Panel B contains estimates from analogous specifications for defensive players.

Each of the first three columns contain estimates from a pooled Poisson model corresponding to one of our three injury definitions. A concern with this specification is that the pooled Poisson model assumes that injury counts are independent. To address this issue, the fourth through sixth columns contain estimates from population averaged (PA) or generalized estimating equation (GEE) negative binomial (NB) models (Liang and Zeger 1986). These specifications allow for temporal (in our case, seasonal) correlation in the model errors for each team and result in estimates that are more efficient than those produced by standard pooled estimation. Estimating a GEE NB is even more efficient than its Poisson analog if there is evidence of overdispersion in the data, as there is in our case.[59] A possible shortcoming of the GEE models is that they average out, or are marginal with respect to, the team-by-year effects we include in our final specification. Thus, the GEE estimates are inconsistent if team-by-year unobservables are correlated with plays per game (Cameron and Trivedi 2005). To address this concern, the last three columns contain

[58]Chi-squared goodness of fit tests fail to reject the null of no discontinuities in any of control variables in two thirds of our specifications. We suspect that the jointly significant findings in the remaining specifications are spurious and the result of models that are too restrictive. Given that our running variable takes on only one value beyond the cutoff (the fourth quarter score differential is always zero in games that go to overtime), we are unable to allow the effect of that running variable to vary on both sides of the cutoff. In order to further test for discontinuities in our control variables, we estimate our main specifications with and without control variables (see Section 5.2). Results are consistent with the other graphical and empirical tests we present and support the validity of our RD design.

[59]We also estimate GEE Poisson versions of the models. Results are qualitatively similar.

Table 7: RD SUR Models of the Effect of Overtime on Given Control Variables by Platoon, Bandwidth, and Model Specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Offense						Defense					
Bandwidth:	1 Pt.	2 Pts.	3 Pts.	4 Pts.	5 Pts.	6 Pts.	1 Pt.	2 Pts.	3 Pts.	4 Pts.	5 Pts.	6 Pts.
	SUR	SUR	SUR	SUR	SUR	SUR	SUR	SUR	SUR	SUR	SUR	SUR
Variable	Sys.	Sys.	Sys.	Sys.	Sys.	Sys.	Sys.	Sys.	Sys.	Sys.	Sys.	Sys.
Panel A: Linear Specification												
OT Disc. (Rush)	0.009	0.013	0.017*	0.014	0.009	0.012	0.009	0.013	0.017	0.014	0.009	0.012
	(0.007)	(0.012)	(0.010)	(0.009)	(0.008)	(0.008)	(0.009)	(0.014)	(0.011)	(0.010)	(0.009)	(0.009)
OT Disc. (Pass)	-0.005	-0.012	-0.008	-0.007	-0.004	-0.005	-0.005	-0.012	-0.008	-0.007	-0.004	-0.005
	(0.008)	(0.012)	(0.010)	(0.009)	(0.008)	(0.008)	(0.009)	(0.014)	(0.012)	(0.010)	(0.010)	(0.009)
χ^2	2.884	1.239	7.311**	5.449*	3.333	5.885*	3.144	0.967	6.671**	5.527*	3.676	6.428**
Panel B: Quadratic Specification												
OT Disc. (Rush)	0.009	0.011	0.008	0.021	0.023**	0.013	0.009	0.011	0.008	0.021	0.023	0.013
	(0.007)	(0.009)	(0.021)	(0.015)	(0.011)	(0.011)	(0.009)	(0.010)	(0.024)	(0.017)	(0.014)	(0.013)
OT Disc. (Pass)	-0.005	-0.007	-0.016	-0.012	-0.013	-0.007	-0.005	-0.007	-0.016	-0.012	-0.013	-0.007
	(0.008)	(0.009)	(0.022)	(0.016)	(0.012)	(0.011)	(0.009)	(0.010)	(0.024)	(0.017)	(0.014)	(0.013)
χ^2	2.884	1.954	0.860	3.601	7.141**	2.968	3.144	1.900	0.877	3.040	5.495*	2.720
Observations	622	830	1382	1686	1918	2120	622	830	1382	1686	1918	2120

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 8: Simple Count Models of the Effect of Pace on Injuries by Model Specification, Injury Definition, and Platoon

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pooled Poisson			GEE Negative Binomial			FE Poisson		
Injury Definition:	First	Week Gap	All	First	Week Gap	All	First	Week Gap	All
Variable	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.
Panel A: Offense									
Plays per Game	0.004***	0.004***	0.000	0.004***	0.005***	0.002**	0.005***	0.005***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Team-Season Effects							x	x	x
Number of Team-Seasons	-	-	-	977	977	977	600	600	600
Observations	8140	8140	8140	8140	8140	8140	7615	7615	7615
Panel B: Defense									
Plays per Game	0.003*	0.003*	0.000	0.003**	0.004**	0.001	0.005***	0.005***	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Team-Season Effects							x	x	x
Number of Team-Seasons	-	-	-	977	977	977	587	587	587
Observations	8140	8140	8140	8140	8140	8140	7447	7447	7447

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
The fixed effects Poisson model drops team-seasons that have fewer than two periods of data (FCS schools that only appear in the data when they play an FBS team) or do not report any injuries over the course of the season. This leads to differences in observation and team-by-season counts both between the NB GEE and fixed effects Poisson models and between the offensive and defensive fixed effects Poisson models.

estimates from fixed effects Poisson models.[60]

In all three specifications, the coefficients are semi-elasticities, so a one unit increase in plays-per-game can be interpreted as being associated with a $\left(100 \times \hat{\beta}\right)\%$ change in the number of injuries.[61] In other words, the coefficient of 0.004 in Column (1) of Panel A indicates that the model predicts that for every additional 10 plays run, there is a 4 increase in the number of injuries. Results are relative to an average of 0.53 (0.44) injuries per offensive (defensive) platoon per game based on the "First" injury definition (see Table 1).

We estimate all three models with robust standard errors. The Poisson model results report cluster-robust standard errors that cluster on team-seasons to control for both overdispersion and correlation over the course of a season for a given team.[62] The GEE model robust standard errors have an exchangeable correlation structure that also allows for correlation over the course of a season for a given team.[63]

In looking at the estimates overall, we see that there is a positive, generally significant relationship between plays and injuries. While this is consistent with the Saban hypothesis (as teams run more plays, there is a greater chance of an injury occurring), we do not draw any conclusions about the effects of plays-per-game on injuries from these simple, exploratory models. We provide a more thorough analysis of the hypotheses in Section 5.1.

Instead, we turn to a comparison of the different injury definitions, the relationship between plays and injuries is positive and significant in all specifications for the "First" and "Week Gap" injury definitions. Additionally, the coefficient estimates and standard errors of our variable of interest are almost all identical within each model specification based on these two injury definitions. Compared to the other injury definitions, the "All" injury definition estimates are always

[60]The fixed effects Poisson is more commonly used than its fixed effects NB analog since it is consistent under weaker distributional assumptions (Cameron and Trivedi 2013). Additionally, Allison and Waterman (2002) show that time-constant effects are not identified in a panel NB model (although they perform simulations that suggest that one can simply use dummy variables to control for those effects).

[61]Despite estimating a nonlinear model, we do not report marginal effects because a) as semi-elasticities, the coefficients have a natural interpretation; and b) fixed effects are not estimable in the fixed effect Poisson model. Since marginal effects are a function of those fixed effects, they cannot be calculated in all count model settings. This makes cross equation comparisons of marginal effects impossible (Cameron and Trivedi 2005).

[62]Simcoe (2007) provides a method to calculate robust standard errors for our fixed effects Poisson models based on Wooldridge (1999).

[63]Estimating the negative binomial model GEE model addresses the efficiency losses related to overdispersion.

smaller in magnitude and insignificant in five of the six specifications. We attribute this to the measurement error we know to be present in this definition attenuating the estimates. Our analysis leads us to conclude that the "First" and "Week Gap" definitions of injuries are very similar, but the "All" definition is a poor measure of actual injuries. As the "Week Gap" definition is the more conservative measure, and we find it to be the most intuitively appealing definition of injuries, we use this measure for our subsequent analyses.

Next, we focus our attention on determining our preferred count data model specification. In doing so, we face a potential trade-off between consistency and efficiency. Cameron and Trivedi (2005) note that the approach used often depends on the convention in discipline of the authors. Statisticians prefer the efficiency gains from GEE models, particularly in contexts with randomized treatment and control groups that address potential endogeneity concerns (Gardiner et al. 2009, Hubbard et al. 2010). Conversely, the econometric literature for panel data count models favors fixed effects Poisson models as they address concerns related to time (in our case, game) invariant unobservable characteristics.[64] In our context, the number of plays run each game is not determined at random, so the fixed effects Poisson model is necessary to help ensure that we recover consistent estimates. Although the magnitudes of the changes are not large, that the coefficient estimates sometimes increase (from 0.004 to 0.005 for defenses based on the "Week Gap" injury definition) as we add additional team-by-season controls suggests that these effects are important. The similarity of the GEE NB and fixed effects Poisson standard error estimates indicates that our cluster-robust approach is appropriate for addressing efficiency concerns, so we treat the fixed effects Poisson as our preferred specification for our subsequent analyses.

A.5 Full Saban Hypothesis Model Estimates

Table 9 contains the full set of estimates for the models that correspond to those presented in Table 2. Columns (2) and (6) add our key game level control: the consensus line (point spread).

[64] For instance, this would be a concern if HUNH coaches who run many plays each week also practice their players hard all week. If those rough practices lead to injuries, since we can neither observe nor control for how demanding practices are, we will recover an estimate that overstates the true effect of pace on injuries. By including team-by-season fixed effects, we can control for the unobserved characteristics of the team that year, including how hard they practice, which will address the issue.

The estimated coefficients are significant and indicate that for each additional 10 points a team is expected to win by, the number of injuries the offense (defense) sustains increases by 6% (8%). This result is somewhat surprising, but may indicate that players become complacent when they expect to win, use poor form, and are more susceptible to injury.

Table 9: Count Models of the Effect of Pace on Total Injuries by Control Variables (All Estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Offense				Defense			
Variable	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.
Pace								
Plays per Game	0.005***	0.004***	0.004**	0.003*	0.005***	0.007***	0.008***	0.009***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Game								
Consensus Line		-0.006***	0.001	0.000		-0.008***	-0.001	-0.001
		(0.002)	(0.002)	(0.002)		(0.002)	(0.002)	(0.002)
Dummy = 1 if Home Team			0.012	0.009			-0.025	-0.024
			(0.040)	(0.040)			(0.045)	(0.045)
Dummy = 1 if Neutral Site			0.006	0.003			-0.017	-0.002
			(0.098)	(0.098)			(0.119)	(0.120)
Dummy = 1 if Bowl Game			-3.133***	-3.141***			-3.115***	-3.105***
			(0.465)	(0.465)			(0.467)	(0.468)
Dummy = 1 if Turf Field			0.165***	0.170***			0.160***	0.164***
			(0.045)	(0.045)			(0.051)	(0.051)
Cumulative Plays per Season			-0.001***	-0.001***			-0.001***	-0.001***
			(0.000)	(0.000)			(0.000)	(0.000)
Days Since Last Game			0.034***	0.034***			0.026***	0.026***
			(0.008)	(0.008)			(0.008)	(0.008)
Weather								
Daily Max. Temp. (F)			-0.002	-0.002			0.002	0.003
			(0.003)	(0.003)			(0.003)	(0.003)
Daily Min. Temp. (F)			0.000	0.000			-0.004	-0.004
			(0.003)	(0.003)			(0.003)	(0.003)
Daily Precipitation (in)			0.067*	0.069*			0.011	0.010
			(0.039)	(0.040)			(0.054)	(0.055)
Within-Game								
Q1 Score Differential (abs val)				-0.008**				0.000
				(0.004)				(0.004)
Q2 Score Differential (abs val)				0.005				0.001
				(0.004)				(0.004)
Q3 Score Differential (abs val)				-0.001				-0.001
				(0.003)				(0.004)
Q4 Score Differential (abs val)				0.001				0.001
				(0.002)				(0.003)
Dummy = 1 if Overtime				0.057				0.021
				(0.091)				(0.111)
Dummy = 1 if Game Called Early				0.262				-0.066
				(0.467)				(0.718)
Duration of Game (Minutes)				0.002*				-0.001
				(0.001)				(0.001)
Rush Attempts per Play				1.351***				0.684
				(0.513)				(0.543)
Pass Attempts per Play				1.169**				0.374
				(0.469)				(0.527)
Team-Season FE	x	x	x	x	x	x	x	x
Number of Team-Seasons	600	600	600	600	587	587	587	587
Observations	7615	7615	7615	7615	7447	7447	7447	7447

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The fixed effects Poisson model drops team-seasons that have fewer than two periods of data (FCS schools that only appear in the data when they play an FBS team) or do not report any injuries over the course of the season. This leads to differences in observation and team-by-season counts between the offensive and defensive fixed effects Poisson models. We include dummy variables that are equal to one if the consensus line, days since the last game, daily maximum/minimum temperature, or daily precipitation (in) measures are missing as controls. Coefficient estimates for these variables are available by request.

Next, Columns (3) and (7) add additional game and game-day level controls. Although the estimates on the spread coefficient become insignificant, the full set of game-day estimates are jointly significant. We include controls for the location of the game (home or neutral vs. away) to control for potentially different injury effects based on whether the team is playing on its home field or not. We believe that the negative, significant bowl game effect is driven by teams being less likely to report injuries that occur at the end of the season. Alternatively, our injury data source, which exists to provide information for gamblers, may be less diligent in collecting injury information when players have an entire off-season to heal. The starkest coefficient estimate is that of the effect of playing on a turf (as opposed to natural grass) field. Turf field games result in an 16.5% (16.0%) increase in injuries to offensive (defensive) players. We also include a measure of cumulative plays per season to determine whether the effect of plays run accumulates over the season. The negative, significant estimate is puzzling, but small in magnitude. The positive, significant estimates of the effect the number of days between games is possibly the result of injuries sustained in practice. While such injuries are problematic for our study, we remind the reader that the only data that exists that more precisely links injuries to a particular activity does not include information on the player or team. This negates the ability to control for other factors which is an important trade-off. Finally, none of our local, game-day weather measures are individually significant at the 5 level.

We add within-game measures to control for the intensity of the game in Columns (4) and (8). The first quarter score differential, duration of the game, and the rush/pass play call measures are significant for offenses, but none of these effects are significant for defensive platoons.

A.6 Additional Results: Effects by Position

In order to inform whether there is heterogeneity in the effects of plays on injuries by position, we re-estimate our main specification seven times. Each time, we restrict the definition of injuries to those experienced by players at a different position.[65] Analogous to Table 1, Table 10 provides

[65]Formally, we index positions with $\ell \in L = \{QB,\ RB,\ WR,\ OL,\ DL,\ LB,\ DB\}$ and model the conditional mean as

$$\mu_{j\ell sg} = \exp\left(\alpha_{js}^{\ell} + h\left(p_{j\ell sg};\beta^{\ell}\right) + X_{j\ell sg}\gamma^{\ell}\right) \ for\ each\ \ell \in L.$$

summary statistics for the position-specific injury variables in order to provide context for the magnitudes of our estimates. Table 11 reports those estimates for offensive player positions in Columns (1) - (4) (quarterbacks, running backs, wide receivers, and offensive linemen) and defensive player positions in Columns (5) - (7) (defensive linemen, linebackers, and defensive backs).

Table 10: Sample Moments of Position-Specific Injury Variables

	(1)	(2)	(3)	(4)
	Offense		Defense	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Injuries by Position (Week Gap)				
QB Injuries	0.075	0.277		
RB Injuries	0.148	0.413		
WR Injuries	0.190	0.488		
OL Injuries	0.161	0.458		
DL Injuries			0.144	0.427
LB Injuries			0.137	0.409
DB Injuries			0.182	0.485
Observations	8140		8140	

Table 11: Count Models of the Effect of Pace on Injuries by Position

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Offense				Defense		
Variable	QB Inj.	RB Inj.	WR Inj.	OL Inj.	DL Inj.	LB Inj.	DB Inj.
Plays per Game	0.006*	-0.002	0.004*	0.005	0.007**	0.012***	0.008***
	(0.004)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Controls	x	x	x	x	x	x	x
Team-Season Effects	x	x	x	x	x	x	x
Number of Team-Seasons	377	500	528	499	469	468	517
Observations	4770	6349	6703	6340	5953	5946	6565

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
All models include consensus line, game, weather, and within-game controls, as well as team-season fixed effects. The fixed effects Poisson model drops team-seasons that have fewer than two periods of data (FCS schools that only appear in the data when they play an FBS team) or do not report any injuries over the course of the season. This leads to differences in observation and team-by-season counts between models.

The table shows that an increases in the number plays run has a statistically significant effect on the propensities that quarterbacks, wide receivers, defensive linemen, linebackers, and defensive backs are injured. Results are strongest, both in terms of magnitudes and significance, for defensive players, particularly linebackers.[66] This is intuitive given that these players physically engage both runners and receivers on most plays. However, we do not find statistically significant effects for running backs who frequently carry or receive the ball and are often tackled forcefully when they do.[67] That the effects for offensive/defensive linemen and wide receivers/defensive backs are not more similar given the related but opposing natures of their positions is also counterintuitive, but not implausible.

A.7 Robustness Check: Within Variation

While both offensive and defensive platoons try to impose their style of play on their opponent, by the nature of the game, offenses are more able to determine the pace at which the game is played (e.g., traditionally vs. up-tempo). Given that the fixed effects in main specifications exploit within variation (over games in a season for a given team and platoon) to identify the β^k parameters, including team-by-season fixed effects has different implications for our offensive and defensive estimates.

For offenses, since the team's offensive strategy is generally fixed from game to game, by including team-by-season fixed effects, our coefficients of interest are identified from randomness in the way the game proceeds (conditional on our controls absorbing the relevant variation in differences between opponents). This is likely a relatively narrow range (on the order of 75-85 plays for traditional offenses and 95-105 plays for up-tempo offenses).

In contrast, the identifying variation for defenses is likely due to randomness in both how the game unfolds and the opponent's offensive strategy. A defense may defend 80 plays against a traditional offense one week and 100 plays against a HUNH offense the next. In other words, our defensive estimates are based on a potentially broader range of plays. Our concern is that the

[66] Our findings are broadly consistent with those of Crisco et al. (2010). The authors use data from accelerometers placed in the helmets of players on three college football teams to determine the frequency of head impacts. They find that these frequencies vary by position with linemen and linebackers incurring the most impacts in both games and practices.

[67] The Martini et al. (2013) study based on accelerometer data finds that players in run-first offenses suffer more impacts, but those in pass-first offenses suffer impacts of greater magnitudes. This provides a possible, but not definitive, explanation for our finding for running backs.

offensive estimates in our main specification may not be based on variation relevant to our research question. If this is the case, our estimates can still be interpreted as the conditional propensity for injury per play in college football, but applying them to the increase in plays when a team switches from a traditional to HUNH offense may be extrapolating outside of the bounds of the data.

To address this concern, we conduct robustness tests to determine whether differences in the identifying variation used affects our results. Table 12 reports the results of estimating our preferred fixed effects Poisson with all controls specification (Columns (4) and (8) from Table 2), but with different fixed effects. Offensive results are reported in the first three columns, and the remaining columns contain defensive results.[68] The first and fourth columns repeat estimates from models that contain team-by-season fixed effects for comparison.

Table 12: Count Models of the Effect of Pace on Injuries by Platoon and Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Offense			Defense	
Variable	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.	Total Inj.
Plays per Game	0.003*	0.004**	0.001	0.009***	0.009***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Controls	x	x	x	x	x	x
Team-Season Effects	x			x		
Time Effects		x			x	
Team Effects		x			x	
Opponent-Season Effects			x			x
Number of Groups	600	124	679	587	124	654
Observations	7615	7677	7798	7447	7677	7631

Notes: Robust standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
All models include consensus line, game, weather, and within-game controls. The Simcoe (2007) variance-covariance matrix for the model in Column (6) is singular, and we are unable to bootstrap a sufficient number of estimates to calculate robust standard errors because of our inclusion of an indicator equal to one if the game was called early due to weather. Four games in our sample ended prematurely for this reason. Dropping this control neither results in a measurable change to the point estimate of our coefficient of interest, nor its non-robust standard error, so we report the robust standard error from a model without this control in Column (6). The fixed effects Poisson model drops groups that have fewer than two periods of data (FCS schools that only appear in the data when they play an FBS team) or do not report any injuries over the course of the season. This leads to differences in observation and group counts between the fixed effects Poisson model specifications.

[68] While our defensive estimates do not suffer from the same extrapolation concerns as our offensive estimates, we estimate a similar specifications nonetheless. As the subsequent paragraphs detail, the results are informative, albeit for different reasons than their offensive analogs.

To address the potential lack of relevant variation in our offensive estimates, we perform two robustness checks that rely on different within variation for identification. The estimates in the second and fifth columns are from models with team fixed effects and season effects. While these models are less able to control for season-varying, team-specific factors that may bias results, they identify the coefficients of interest using variation over games across multiple seasons for a given team and platoon. For offenses, this adds potential variation due to changes in offensive strategy over time (for instance, when a new coach or coordinator is hired). For defenses, this merely results in potentially greater variation in the offensive strategies of their opponents. Estimates for both platoons are similar to those from our main team-by-season fixed effect model specification. These results suggest that our results are robust to different sources of identifying variation.[69] They are also consistent with our findings from Table 3 that suggest that the risk of injury is constant for each play.

The third and sixth columns of the table report estimates from models with opponent-by-season fixed effects. These models identify the plays-per-game coefficients using a different kind of within variation than our main model does: variation over games in a season for a given *opponent* and platoon. For a given team's offense, this variation comes from the team's offensive strategy relative to the other offensive strategies the opponent faces in the season. This is in addition to randomness in how the game unfolds. As such, the estimates in Column (3) are identified using variation in the number of plays potentially on par with the shift from a traditional to HUNH offense. In contrast to our previous estimates, the effect of additional plays on injuries is not statistically different from zero in this specification. This is consistent with there being no, or a very small true effect, of running a HUNH offense on the injury propensity of offensive players. Alternatively, we cannot rule out that this result is driven by team specific confounding factors: offenses that run a HUNH conditioning themselves to be able to handle the stress of additional plays, recruiting athletes better suited to their pace of play, or substituting players more frequently to mitigate the deleterious effects of pace of play. All of these factors are controlled for in specifications that include team

[69] While we cannot rule out that this similar result is the due to the combined effects of the greater variation available and a roughly equal, opposite bias due to season-varying, team-specific omitted factors, we find this to be an unlikely explanation for the pattern of both offensive and defensive results in the table.

or team-by-season specific effects. In other words, exploiting this more relevant variation in the number of offensive plays comes at a cost. By excluding team (or team-by-season) fixed effects, we are no longer able to control for unobserved, (season specific,) team-level factors that may bias our estimates down.[70] Although this specification is unable to better illuminate the effects of switching to a HUNH on player injuries, it does underscore the importance of controlling for team-level omitted variables. The RD model design discussed in Section 4.3 is an alternative way to estimate the model that does just that.

In contrast to the offensive result in a model with opponent-by-season fixed effects, the defensive estimate of our coefficient of interest in Column (6) is very similar to the estimates from previous specifications. This estimate is identified using a potentially narrower range of variation in the number of plays. For a given team's defense, the opponent's offensive strategy is fixed, so variation relative to the other defenses the opponent faced is primarily driven by randomness in the way the game proceeds, conditional on our controls. That our defensive estimates are so similar across specifications suggests both that defenses primarily react to offensive strategies and that our controls absorb the relevant variation in differences between teams the opponent faces. Together, this is support for our identifying assumption that the conditional variation is "as good as random," at least for defenses.

Overall, these robustness checks provide support for our main estimates for defenses, but the implications for offenses are unclear. Regarding the latter, the first of our two modified specifications yields estimates consistent with our main model, despite allowing for greater variation in the number plays run. While the second specification yields a result that is no longer statistically significant, we believe that this may be due to bias introduced by omitted variables that are correlated with the number of plays run. For the defensive estimates, our results are very robust across all specifications.

[70] In addition to the equilibrium factors mentioned previously, there may also be differences in the injury reporting procedures of different coaching regimes. Failure to account for these differences in reporting would introduce measurement error that would attenuate our estimates. This is also consistent with our pattern of results.

A.8 Injury Cost Calculations

We modify the methodology used by Fair and Champa (2017) to our context in order to perform the back-of-the-envelope calculations that provide additional context for the magnitude of our estimates. YLDs are calculated by summing the product of disease or injury counts in a population and disability weights corresponding to each condition. A weight of zero can be interpreted as perfect health and a weight of one is akin to death. We use data on disability weights that correspond to the injuries in our dataset from Salomon et al. (2012). These weights are used in the calculation of the 2010 GBDS which corresponds to the median year in our data. We calculate a college football injury specific YLD by combining the Salomon et al. (2012) disability weights with our data. To do so, we first match injuries to the closest available sequela listed in the Salomon et al. (2012) study. Our matching algorithm breaks ties between multiple plausible sequela or levels of severity by choosing the most conservative weight. Table 13 contains a crosswalk that provides the results of our matching algorithm.

Table 13: Disability Weights by Injury

Sequela	Weight	Injuries
Crush injury: short or long term, with or without treatment	0.145	Internal organ, kidney, lung, spleen, pulmonary contusion
Disfigurement: level 1	0.013	Eye, face
Dislocation of shoulder: long term, with or without treatment	0.080	Shoulder and foot, shoulder/knee, shoulder/leg, broken collarbone, shoulder, shoulder/elbow, shoulder/neck, shoulder/personal
Distance vision: mild impairment	0.004	Vision
Fracture of clavicle, scapula, or humerus: short or long term, with or without treatment	0.053	Clavicle, collarbone, shoulder blade
Fracture of foot bones: short term, with or without treatment	0.033	Ankle, ankle/hamstring, ankle/sick, ankle/toe, feet, flu/foot, foot, foot/ankle, foot/elbow, foot/leg, heel, toe, ankle/shoulder
Fracture of hand: short term, with or without treatment	0.025	Hand/knee, hand/thigh, finger, hand, hand/personal, hand/wrist, thumb, thumb/shoulder, wrist
Fracture of patella, tibia or fibula, or ankle: short term,with or without treatment	0.087	Broken leg, fibula, fibula/torn acl, knee, knee/ankle, knee/transfer, leg, leg/ankle, leg/knee, right knee, shin, tibia, stress fracture, leg/shoulder
Fracture of pelvis: short term	0.390	Hip, pelvis, tailbone, torn labrum
Headache: migraine	0.433	Migraines
Heart failure: mild	0.037	Heart, irregular heartbeat
Infectious disease: acute episode, mild	0.005	Illness, staph infection
Injured nerves: short term	0.065	Pinched nerve, back, neck, neck/back, neck/larynx, neck/shoulder, neck/spine, spine, stinger, transient quadriplegia
'Multiple'/'various' injury: coded as twice the minimum muscle or tendon injury weight	0.018	Multiple, various
Open wound: short term, with or without treatment	0.005	Abrasions, laceration
Other injuries of muscle and tendon (includes sprains, strains, and dislocations other than shoulder, knee, or hip)	0.009	Achilles, arch, bruised thigh, calf, calf/ankle, gluteus, groin, groin/hamstring, groin/leg, hamstring, hernia, high ankle sprain, lower body, lower extremity, lower leg, lower leg/foot, mcl, plantar fasciitis/heel, quad, thigh, torn achilles, torn acl, torn mcl, upper leg, contusion, fatigue, heat issues, muscle pull, pulled muscle, rest, soft tissue, stomach, tooth, elbow/knee, groin/shoulder, medical, uncertain, undisclosed, abdominal, arm, arm/elbow, arm/wrist, bicep, body, elbow, forearm, jaw, midsection, mouth, nose, oblique, pectoral, sore neck, throat, torso, triceps, upper body
Severe chest injury: short term, with or without treatment	0.352	Hip/rib, back/ribs, chest, rib, rib/lat, ribs, sternum
Stroke: long-term consequences, mild	0.021	Stroke
Traumatic brain injury: long-term consequences, minor, with or without treatment	0.106	Concussion, head, head/hand, head/neck, head/wrist, neck/concussion

Next, we take the average over all injuries to calculate the average years of healthy life lost due to college football injuries. Formally, let w_m represent the disability weight for injury-type m and recall that the dependent variable modeled in Section 4.1 is y_{jksg} (the count of injuries that occur to the players on platoon k of team j during and following game g of season s). Define $Y_{km} = \sum_{j,s,g} y_{jksgm}$. Finally, let $\bar{d}_{mks}$ represent the platoon-specific average YLD which is calculated as

$$\bar{d}_k = \frac{\sum_m w_m Y_{km}}{\sum_m Y_{km}}.$$

Next, we follow Fair and Champa (2017) and use Aldy and Viscusi's (2008) estimate of the value of a statistical life year for 20 year old individuals of approximately $\$192,000$.[71] We refer to this value as v.

Finally, since the $\hat{\beta}^k$ estimates are semi-elasticities, our back-of-the-envelope estimate of the per game injury cost of an increase in ten plays per game is

$$c_{kg} = \left(1 + 10\hat{\beta}^k\right) \bar{y}_{kg} \bar{d}_k v,$$

where $\bar{y}_{kg}$ is the average number of injuries per game to platoon k (reported in Table 1). Using estimates of $\hat{\beta}^k$ from our preferred specifications of our main results table (Columns (4) and (8) from Table 2) and estimates of $\bar{d}_k$ (reported in Table 14), we estimate that an additional ten plays per game results in expected injury costs (c_{kg}) of $\$7,278$ per game for offenses and $\$5,870$ per game for defenses. Given the average of 841 games per season in our dataset, if all NCAA teams ran an additional ten plays per game, injury related costs per season would increase by over \$11 million.

[71] Specifically, we use the cohort-adjusted estimate from Figure 2 of Aldy and Viscusi (2008) of approximately $\$150,000$. This measure is in year 2000 dollars, so we convert it to 2010 dollars and round to the nearest thousand dollars.

Table 14: Sample Moments of Disability Weights

	(1)	(2)	(3)	(4)
	Offense		Defense	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Weight	0.064	0.062	0.060	0.056
Observations	4671		3770	

Notes: Disability weights obtained from Salomon et al. (2012).