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Three Essays in Labor Economics with Applications to Sports

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For my soon-to-be wife, Kelly.

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Abstract

This dissertation research focuses on the intersection of labor and sports economics. All three chapters seek to answer or explore labor economics questions in the context of sports-related data.

The first chapter addresses the substantial public debate regarding whether or not college athletes—specifically basketball and football players—should be compensated above the value of an athletic scholarship. In this paper, we estimate a panel data model with institution-level fixed effects and find that the annual value of a premium college basketball player at a major college program is approximately $380,000. These estimates are lower than those found in the previous literature, where yearly values often exceeded $1 million. Furthermore, we find that yearly changes in winning percentage have no effect on revenue at major programs which limits the ability of non-premium players to significantly contribute to team revenue.

The second chapter examines strategic bargaining which is an important tool used in business and employment settings. Participants in a bargaining situation can use a variety of strategies to maximize contract value. In this paper, we analyze the effect of delaying contractual agreement (or holding out) on NFL rookie contract values. Using an instrumental variables approach, we find that a player who delays agreement and signs a contract after the start of his team's training camp receives an increase in total contract value of nearly

$120,000, on average. We also find that this effect is substantially larger for players who are selected in early rounds of the draft and sign larger contracts, increasing to an average effect of around $430,000 for first round draft picks.

The third chapter studies the potential labor market impacts of sports teams and arenas. As professional sports continue to grow in popularity, the teams involved are competing for public funds in order to build the biggest and brightest stadiums and arenas. As this trend continues it is increasingly important to quantify the labor market impacts of sports teams and arenas to determine whether or not they are a sound public investment. This paper expands on the current literature in two ways. First, by evaluating labor market effects at a narrower geographic level (by ZIP code), the data allow for a more precise examination of the potential for differing effects by proximity to the stadium/arena. Second, this paper deviates from the previous literature by estimating the impact of sports arenas that do not serve a major professional team, and instead focusing primarily on arenas that host minor league hockey teams in the AHL and ECHL. The results mirror the previous literature in that we fail to find any convincing evidence which supports the notion that construction of a new sports arena improves local labor market conditions.

Chapter 1

Is Exploitation Real? Estimating the Marginal Revenue Product of Men's College Basketball Players Using Panel Data

1.1 Introduction

The National Collegiate Athletic Association (NCAA) and college athletic departments have come under intense scrutiny in recent years over the potential exploitation of college athletes as revenue producers instead of student-athletes. The NCAA prohibits paying college athletes, aside from the value of an athletic scholarship, and places restrictions on players who transfer between universities. These restrictions prevent universities from competing for athletes on a price basis and reduce competition for college athletes. The NCAA has effectively made all universities (within a single division) homogeneous buyers of student-athlete labor and has provided the framework for universities to extract economic rents from premier athletes by exploiting them on a wage basis.

Since colleges essentially act as monopsony employers in the player recruitment market, it is likely that players will not be paid their marginal revenue product. The economic rents generated by universities also result in an incentive to cheat. Cheating in this case can be monetary or non-monetary. universities could offer some form of compensation to premier student-athletes above the maximum set by the NCAA. Over the years there have been many instances of universities providing extra benefits to athletes (cash, gifts, apartments, etc.), but perhaps the most famous example involved the football program at Southern Methodist University (SMU). An investigation by the NCAA in 1985-86 revealed that SMU had maintained a slush fund for "under the table" payments to players from the mid-1970s through 1986. More recently, the University of Miami was caught in a major scandal in which an athletic booster (and eventual convicted felon) was providing improper benefits to at least 72 football and men's basketball players from 2002-2010. We have also seen cases of non-monetary cheating in the form of academic scandals where athletes get credit for fake courses or have tutors completing coursework for them. The most recent (and most well-known) of these cases has unfolded at the University of North Carolina where it was found that hundreds of athletes over many years were steered into bogus courses where they received high grades for doing little to no work.

To determine whether or not student-athletes are in fact being exploited, empirical estimates are needed to approximate their marginal revenue product. While these estimates will help to quantify how a college athlete's play provides value to his university, there is still much debate surrounding other aspects of student-athlete exploitation. In a July 2009 case, *Edward C. O'Bannon v. NCAA and Collegiate Licensing Company*, Ed O'Bannon, a former basketball player for UCLA, filed a lawsuit against the NCAA, Electronic Arts, and the Collegiate Licensing Company, alleging violations of the Sherman Antitrust Act and of actions that deprived him of his right of publicity. The lawsuit, filed on behalf of the NCAA's Division I football and men's basketball players, challenges the organization's use of the images of former student-athletes for commercial purposes. The suit argues that, upon graduation, a former student-athlete should become entitled to financial compensation for the NCAA's commercial uses of his image. Initially, O'Bannon agreed to a settlement with Electronic Arts and the Collegiate Licensing Company which could compensate college athletes for the use of their likenesses in video games and retail sales (McCann, 2013). Following that settlement, the courts ruled in favor of O'Bannon against the NCAA. As of this time, the ruling prevents the NCAA from capping the amount of a scholarship below the actual cost of attendance, and allows universities to create trust funds to pay student-athletes (upon graduation) equal shares for the use of their likenesses. While the result of this lawsuit (and other pending legal issues) is important to the general landscape of college basketball, this paper will focus on estimating the value of a player's on-court performance.

We attempt to build on the previous literature (described in detail in the following section) by providing a more accurate measure of the marginal revenue product of a men's college basketball player. First, we use an expanded panel dataset which includes nearly all Division I basketball programs observed over multiple years. Second, we control for time-invariant unobserved factors which affect revenue by using a panel data model with institution-level fixed effects. Third, we use a fixed-effects model to correct for potential endogeneity in the draft variable. Finally, we look at different methods to address and correct for the highly skewed nature of revenues for Division I men's basketball programs.

The main result we find is that a premium men's college basketball player (one who goes on to be drafted) is worth approximately $380,000 in annual revenue to his university. The estimated value from our model is far lower than previous literature suggests, where the annual value of a premium player was thought to be in excess of $1 million. Due to the structure of our model, we are also able to determine that premium players provide significant value only when they play for a college basketball program belonging to one of the six major conferences (during the years studied: Big East, Big Ten, ACC, SEC, Big 12, Pac 10). Furthermore, we argue that non-premium players at major conference programs have little ability to generate revenue by contributing to overall team success.

1.2 Literature Review

The idea of valuing athletes, amateur or professional, is not a new one. Scully (1974) pioneered the initial method for estimating the marginal revenue product of athletes when he evaluated the monopsonistic exploitation of major league baseball players. Scully's method distinguished individual player contributions to revenue through a two-equation model. The first equation estimated team revenue as a function of the team's win-loss record, controlling for other revenue-influencing characteristics. The second equation was essentially a team production function which related the team win-loss percentage to a number of measurable player statistics. The principal assumption necessary to justify this approach is that fans attend games (and therefore revenue is generated) to see teams win, not to witness individual statistical performances. Scully pointed out that it is impossible to ignore the fact that fans are also drawn to sporting events to see superstar players. This methodology is harder to justify when dealing with more "team-oriented" sports where cross-player effects are nearly impossible to quantify.

Another branch of literature attempts to estimate the marginal revenue product of athletes in a way which vastly differs from Scully's original work in both methodology and justification. Brown (1993, 1994) attempted to estimate player value in college football and basketball. Instead of estimating the effect of individual statistical metrics on revenue like Scully, Brown used the idea that a premium college athlete can be defined as one who is eventually drafted into a professional sports league. Scully assumed that individual statistical performances in baseball were separable and could be summed across the entire team, an assumption that is not likely to be the case in such sports as football and basketball, which rely more heavily on a team dynamic. Brown's method estimated team revenue as a function of the total number of players that are eventually drafted, while controlling for other revenue-generating characteristics.

There are advantages and disadvantages to each of these approaches as they relate to the goal of providing reliable estimates of player value in men's college basketball. Scully's approach provides more flexibility in that we can determine the worth of any player based on his specific statistical contributions during a season, which in turn allows us to create our own definition as to who qualifies as an elite or premium player. Brown's method requires us to make fewer assumptions about how team performance relates to winning, but it eliminates the flexibility provided by Scully. Under Brown's approach, we are only able to estimate the value of a player who goes on to be drafted in the NBA, and we are forced to define a premium player as such. However, much of the current debate about player exploitation in fact surrounds those elite players who go on to play in the NBA, so Brown's method will serve as the basis for our analysis.

Brown (1994) presented the first empirical estimates of the marginal revenue product for a premium Division I men's college basketball player using data from the 1988-89 season. The basic methodology regressed team revenues on the number of players drafted, controlling for market characteristics and opponents' team skill levels. This method provided an estimate of the marginal revenue product generated from acquiring an additional premium college basketball player. However, Brown also argued that the skill level of the players acquired by a college team is likely endogenous to its recruiting effort; and since recruiting effort is a function of a team's market and recruiting characteristics, the number of players drafted is likely correlated with the error term. To account for the endogeneity, Brown proposed a two-stage estimation procedure. The first stage is a Poisson regression using the number of drafted players as the dependent variable. In the second stage, team revenues are regressed on the fitted values of the number of drafted players estimated in the first stage, controlling for a team's market characteristics and opponents' team skill levels. One drawback to Brown's original work is that it uses a small sample of only 46 universities for which revenue data were obtained. Brown finds that recruiting an additional player who goes on to be drafted into the NBA is worth anywhere from $871,310 to $1,283,000 in annual revenue for his college team. The range of estimates is due to different specifications used for the second stage estimates.

Brown and Jewell (2004) provided an update of Brown (1994). The key change is the use of a larger and more recent revenue data set. The data was collected by the *Kansas City Star* in 1997, and provided revenue data from the 1995-96 season. Revenue information was collected for 95 Division I basketball programs, more than doubling Brown's original data set. Using nearly identical estimation procedures, they found the marginal revenue product of an additional player who goes on to be drafted into the NBA to be $1,194,469.

Brown and Jewell (2006) estimated the marginal revenue product of a women's college basketball player using revenue data from the 2000-01 season. The data were collected from Equity in Athletics Disclosure Act[1] forms reported in the *Chronicle of Higher Education*. They found that a women's college basketball player who goes on to be drafted into the WNBA generated revenues of $241,337 for her college team. An important point made in this paper, which reflects back on earlier estimates by Brown and Jewell, is that the marginal revenue product estimates should be thought of as an upper bound since many factors (coaching, facilities, etc.) which generate revenue are difficult to control for given data constraints.

Lane et al. (2014) utilized updated versions of the Scully and Brown approaches to estimate the marginal revenue product of men's college basketball players. Using a modified version of Scully's original approach where they incorporate a distribution of professional salaries to adjust for players with no performance data, they find about 60% of all men's college basketball players have a monetary contribution to the university that is greater than

[1] The Equity in Athletics Disclosure Act requires institutions of postsecondary education that participate in federal student financial assistance programs to prepare an annual report to the Department of Education on athletic participation, staffing, and revenues and expenses.

the value of a scholarship. Also, using an approach similar to Brown, they estimate that nearly 100% of players who go on to be drafted contribute more revenue to their university than the value of an athletic scholarship. Furthermore, universities' "profit" from the star players ranges from \$7,000 to \$1.8 million, with an average of about \$400,000.

We attempt to overcome some of the data constraints of these prior studies by utilizing an expanded panel data set. By using a panel data model with institution-level fixed effects, we can control for the time-invariant factors which can also drive revenue for college basketball programs. We also adjust for the extreme skewness in the revenue data, which could allow outliers in the data to drive the estimates.

1.3 Data Description

In a similar fashion to Brown (1994) and Brown and Jewell (2004), we assume that a college basketball team's yearly revenue is a function of its players' skill levels, coaching quality, the quality of opponents being played, market demand characteristics, and past success. Our sample uses data from the 2007-08, 2008-09, 2009-10, and 2010-11 seasons. Complete data for 326 Division I men's basketball teams were obtained, representing nearly all Division I programs. Teams were included in the dataset only if they were full, non-provisional members of Division I for the entire four-year period. Descriptive statistics for all data are found, by year, in Tables A-1 to A-4. The various categories of data are described below.

Team revenue information was obtained from the Equity in Athletics Disclosure Act (EADA) data provided by the U.S. Department of Education. The revenues include all sources, such as ticket sales, TV and broadcast revenue, concessions, donations, etc. All revenues are reported in thousands of dollars and have been adjusted to 2011 dollars using the Consumer Price Index to allow for consistent comparison across years. While this is the only comprehensive source of athletic revenue data available, it comes with certain limitations. The aggregated revenues used include sources like student fees, government aid, and endowment income which are likely invariant to current team performance or quality. To the extent that these additional revenues cannot be captured by the institution-level fixed effects, our estimates may overstate the value of current players.

We look at several measures of team and opponent quality. First, a simple winning percentage (wins/games) can be used to measure team success in a given year. Second, we use a dummy variable to control for whether or not a team is selected to participate in the NCAA tournament in a given year. Third, we utilize the rating percentage index (RPI), which measures team success along with the quality of opponents and which is commonly used to rank men's college basketball teams. The formula for determining the RPI is as follows:

$$\begin{aligned}\text{RPI} = (&\text{Winning Percentage [WP]} \times 0.25) + (\text{Opponents' WP} \times 0.50) \\ &+ (\text{Opponents' Opponents' WP} \times 0.25).\end{aligned}$$

For Division I men's basketball, the WP factor included in RPI was changed in 2004 to account for differences in home, away, and neutral site games. Instead of a traditional winning percentage, the formula values home wins less and road wins more.[2] In some specifications

[2]More specifically, in calculating the winning percentage, a home win now counts as 0.6 wins (instead of 1) and a road win counts as 1.4 wins.

we also include a measure for strength of schedule (SOS). The SOS calculations are similar to the RPI, but include no information about the team's own performance. SOS is a weighted average of the opponents' winning percentage (2/3 weight) and the opponents' opponents' winning percentage (1/3 weight). Alternatively, the simple winning percentage can be used along with SOS if we want to separate out the overall effects of RPI.

Premium players are defined as those who go on to be selected in the NBA draft. For example, a team playing in the 2007-08 season could have players drafted in the 2008 draft, but may also have players selected in the 2009-11 drafts. By accounting for four drafts following each season under study, we account for all players who go on to be drafted from a given team. For example, a freshman who plays during the 2007-08 season and stays in college for four years would not enter the draft until 2011. An alternate definition of a premium player is addressed in Section 1.4.3.

Along with player quality, we would also expect coaching quality to have an impact on team success and revenue production. We control for the quality of the coach by looking at the total years of experience of the head coach prior to the start of the season. We also control for the coach's overall winning percentage in all seasons prior to the season being studied.

In order to measure past team success and popularity, we include an average weekly ranking for a team over the three seasons prior to the season of study. The ranking assigns 25 points to a first-ranked team in a given week, 24 points to a second-ranked team, etc. These weekly point values are averaged over the three-year period. For example, when using data for the 2007-08 season, the average rank variable incorporates data from 2005-07. Since we do not want to make the assumption that the rank variable has a linear effect on revenue, we map the rankings into five categorical variables. Rank1 represents universities which were never ranked during the period. Rank2 includes universities with an average ranking between 0 and 1 (meaning they were on the fringe of the rankings; for example, a team that was ranked 25th over half the period and not ranked for the other half would have an average ranking of 0.5). Rank3 includes universities with an average ranking between 1 and 5. Rank4 includes universities with an average ranking of 5 to 10. Rank5 represents universities with an average ranking greater than 10, meaning that over the three-year period they were ranked in the top 15, on average. We chose to use the average ranking variable as a measure of past success and popularity, but not when looking at current team quality (and instead use RPI). The rankings are subjective in nature and do not necessarily reflect actual team quality, especially early in seasons when few games have been played. However, in looking at past success we are really trying to measure team popularity, which can lead to increased revenue in future seasons. A team which is ranked highly in prior seasons is likely to pick up additional fans and support, even if the ranking is not directly relatable to a measure such as RPI.

Market demand characteristics are specified in two ways. First, we use the total student population at each university, which provides a measure of the "captive" audience of potential fans. Second, we use the state population as a broader measure of market potential. Previous studies have used metropolitan statistical area (MSA) populations as another measure of market demand. The use of MSAs creates an issue about how to assign universities to the nearest MSA and has been shown to add little value in the form of explanatory power (Brown, 1994; Brown and Jewell, 2004). For these reasons, we chose to exclude MSA population data from our dataset.

We also account for the conference affiliation of each institution. The descriptive statistics (Tables A-1 to A-4) provide information about the six major conferences (Pac 10, SEC, Big 12, ACC, Big East, Big 10), although we have conference data for all universities. The period under study, 2007-11, is immediately prior to the recent shifts in conference alignment. Since 2011, all six of the major conferences have undergone changes in membership, but membership was consistent over the period being studied.

Table 1.1 provides a summary and description of the variables used throughout this paper, as well as source information for all data.

1.4 Models and Results

We undertake multiple estimation procedures in this paper to estimate the value of a premium college basketball player. First, we use our expanded panel data set to estimate a model with institution-level fixed effects. Second, we estimate a log-transformed model to determine if extreme skewness in the revenue data has an impact on the estimates.

1.4.1 Panel Data Model

By estimating a panel data model with institution-level fixed effects we are able to control for unobserved, time-invariant factors which could affect a university's revenue function, such as facilities and institutional athletic tradition, among others. The basic model is as follows:

$$\text{Revenue}_{it} = \alpha_i + \beta\text{DraftTotal}_{it} + \gamma X_{it} + \tau_t + \epsilon_{it}, \tag{1.1}$$

where $i = 1, ..., N$ denotes each institution in the sample and $t = 1, .., 4$ represents the four seasons under study. In this model, α_i represents the institution-level fixed effect and τ_t denotes the time fixed effect. The variable of interest is the number of players who go on to be drafted, DraftTotal_{it}, and X_{it} includes time-varying covariates, such as state population, student body size, coaching quality, NCAA tournament participation, RPI, SOS, winning percentage, and average rank (as described in Section 1.3).

The use of the fixed-effects model can also help to correct the potential endogeneity problem in the draft variable. We remain unconvinced that the method in Brown and Jewell (2004) (which uses a team's average ranking in previous seasons as an instrument) truly corrects for the endogeneity of the draft variable, since a team's ranking in previous years, while often correlated with the ability to attract top players, is likely to also directly affect current year revenue. While a strong correlation between premium players acquired and the team's average weekly ranking is an important factor in determining whether or not it is a suitable instrument, it is not the only necessary condition. More importantly, the team's average ranking should have no direct effect on revenue to be a valid instrument, which is unlikely in this case. Rather, it seems reasonable that ticket sales would be driven by a team's performance in previous years, which means that higher rankings would directly lead to higher revenues, regardless of the variation in premium players.

Lane et al. (2014) use a two-stage model similar to Brown, and use a series of excluded instruments to attempt to correct for potential endogeneity. The instruments include win-loss ratio, whether the team was a contender or loser in the previous season, and several statistical measures of performance. Again, while we agree that these factors are likely to be correlated with a university's ability to attract premium players, it is also likely that some (or all) of these factors also directly contribute to revenue earned. We choose to forego the two-stage models used previously and rely on the strength of the panel data model to potentially provide some relief for this issue. If we assume that the underlying characteristic driving the endogeneity (recruiting effort) is constant over the four year panel, then it will be absorbed by the institution-level fixed effect and will not bias our estimated coefficients. We argue that over our relatively short panel length, teams are unlikely to drastically alter their recruiting efforts (based on unobserved factors).

Several variations of the fixed-effects model were run (using different sample sizes) and results can be found in Table 2. We show three specifications of the model using the full sample of Division I programs (326), only those universities which also compete in the Football Bowl Subdivision (FBS, 118), and only those belonging to one of the six major basketball conferences (73). While in basketball there is no explicit distinction made amongst Division I teams, we use the corresponding football team's classification and/or membership in one of the six major conferences as a proxy to classify men's college basketball teams into major vs. non-major programs. In general, universities which compete in the FBS or a major conference have the largest and most well-funded athletic departments, the effects from which are likely to carry over to basketball programs as well.

Table 1.2 shows that, regardless of specification, the estimated marginal revenue product of a premium men's college basketball player is statistically significant and ranges from about

\$300,000 - \$440,000. Interestingly, when we limit the sample to FBS programs or the six major conferences, the measure of team and opponent quality (RPI) becomes insignificant. This implies that controlling for a variable such as RPI is important in trying to distinguish between the samples, but the coefficient becomes insignifcant when looking only at the major programs. The coefficients on the time fixed effect show an interesting result in all samples. Since the revenues have all been adjusted to constant 2011 dollars, the significant time effects in the final two years (2009-10 and 2010-11) should not be related to inflation. In early 2010, the NCAA agreed to a new contract for the broadcast rights for the men's basketball tournament (Wolverton, 2010). The deal included an increase in the annual payout to college teams as a result of the tournament, which could explain the significant increase in revenue for the 2010-11 season. It is less clear as to why we see a significant increase in revenue for the 2009-10 season.

It should come as no surprise that premium players at higher-revenue-producing universities generate more value than players at universities which generate little revenue, but the question is how much more. In order to get a better grasp of the difference between the samples, we next run alternate versions of the model in which we introduce an interaction term:

$$\text{Revenue}_{it} = \alpha_i + \beta \text{DraftTotal}_{it} + \delta \text{DraftTotal}_{it} * \text{FBS}_{it} + \gamma X_{it} + \tau_t + \epsilon_{it}, \qquad (1.2)$$

where $i = 1, ..., N$ denotes each institution in the sample and $t = 1, .., 4$ represents the four seasons under study. As before, α_i represents the institution-level fixed effect and τ_t denotes the time fixed effect. Similar to the previous model, the variable of interest is the number of players who go on to be selected in the NBA draft, DraftTotal_{it}. Additionally, we allow for an interaction term, $\text{DraftTotal}_{it} * \text{FBS}_{it}$, where FBS_{it} is a dummy variable equal to one if the institution also plays major college football (we also do the same with an interaction denoting that a team belongs to one of the six major conferences). This term allows us to separate out the differing effects that premium players have on revenue at major vs. non-major college basketball programs. As before, X_{it} includes time-varying covariates, such as state population, student body size, coaching quality, RPI, SOS, winning percentage, and average rank. By utilizing the interaction term, we are able to take advantage of the full sample size while still distingushing between the value of players at major college programs vs. the remainder of Division I programs (Table 1.3).

In a somewhat surprising result, players at non-major college programs provide *no* significant value to their university in terms of increased revenue. In Table 1.3 [column (1)], we see that the coefficient on DraftTotal is insignificant (-$47,000), while the coefficient on the interaction term is significant ($439,000). Because the interaction term only applies to players who play at FBS programs, our estimates therefore suggest that premium players provide no significant value if they play for a non-major basketball program. For premium players at FBS programs we must consider both terms discussed above. A player who goes on to be drafted provides approximately $392,000 ($439,000-$47,000) of value to his team. However, if we ignore the insignificant term, the value increases to nearly $440,000. We also see that, once again, when using the full sample of universities the coefficient on RPI becomes significant.

We find very similar results when using teams in the six major conferences as the interaction instead of FBS programs. Table 1.3 [column (2)] shows that only players at the major programs generate any significant value. In this case the estimates suggest that premium players at a major program provide approximately $424,000 ($404,000+$20,000) of value.

1.4.2 Panel Data Model Addressing Extreme Skewness in Revenues

The revenues of Division I college basketball programs are highly skewed with a small number of teams earning massive revenues, while the majority of programs do not. Figure 1.1 shows revenue for all observations in the sample (326 teams, 4 seasons per team), and the extreme skewness is evident. Nearly 25% of all revenue observations fall at $1 million or lower, while the highest single year revenue total exceeds $40 million. This skewness can have a large impact empirically as we focus on estimating conditional mean values.

The previous research on this topic has largely ignored the issue, and we attempt to address it here. The most common method for dealing with a highly right-skewed dependent variable is to use a log-transformation. In our situation, the mechanics of the model remain unchanged; we simply exchange the raw revenue values with logged values in the models we used previously, and obtain the following results:

Table 1.4 shows the results from the log-transformed models (which mimic the results shown in Tables 1.2 and 1.3). It becomes immediately evident that the skewness in revenue was driving some of the significant results shown previously. After using the log-transformation, the coefficient on the number of players drafted only remains significant in the iteration of the model where we reduce the sample to only those universities in the largest six conferences. In all other iterations (including those with interactions) none of the relevant coefficients remain significant. This leads to the conclusion that premium players only provide significant value to their university when playing for a large, high-revenue-producing basketball program.

The complications from this approach arise when we attempt to interpret the results. The coefficients produced from the new model can be interpreted directly as semi-elasticities, which are of very little use in our context. We are concerned with finding a dollar value to attribute to the marginal revenue product of men's college basketball players. In order to do this we must re-transform the dependent variable, and we use a method from the health economics literature [see (Duan et al., 1983) and (Manning, 1998)].

Following (Duan et al., 1983), we use the "smearing factor" approach where the marginal effect of having an additional player go on to be drafted is calculated by:

$$\frac{\partial E(Y|X)}{\partial x} = \phi_s \exp(x'\beta)\beta, \tag{1.3}$$

where ϕ_s is the estimated sample average of the exponentiated residuals. This approach is only valid under the assumption that homoskedasticity holds; however, we can allow for heteroskedasticity by defining subgroups in the sample within which the error term has constant variance and compute separate smearing factors for each subgroup. We calculate the marginal effect under both assumptions, using conference affiliation to define the subgroups in the case of heteroskedasticity.

Under the assumption of homoskedasticity we find the value of a premium player to be \$397,000, and under heteroskedasticity we find a value of \$382,000. These results are very similar to previous results found in Tables 1.2 and 1.3. We find that using the logged model has very little impact on the previously estimated marginal effect for a premium player at a major conference university, but the adjustment has eliminated the significant results found in the broader sample. One failure of this approach is that it ignores the potential contributions of non-premium players, which we address in the following section.

1.4.3 Panel Data Model Addressing Non-Premium Players

The benefit of Scully's original approach and additional work using similar methods is that it allowed for the calculation of individual player contributions based on statistical performance. In this section we consider a model where premium players contribute to revenue, but we also examine whether the team's overall performance has a concurrent impact (instead of examining these effects separately, as has been done previously). Following closely from the results of the previous section, we will focus the analysis on universities playing in one of the six major conferences (where we have already shown premium players to have a significant impact on revenue).

The modification to the model is simple. Instead of using RPI to capture both the team's own performance as well as the strength of its opponents, we use the team's winning percentage in a given season, along with the strength of their schedule, to examine the effects independently. If winning percentage has a significant effect on revenue, then we must allow for the possibility that non-premium players contribute to revenue through helping the team succeed.

Table 1.5 [column (1)] shows the results of the model where we examine the simultaneous impact of premium players and a team's winning percentage on revenue. We see that the coefficient estimate for drafted players is nearly identical to that of the previous model, and the coefficient on winning percentage is insignificant. The calculated marginal effect of an additional premium player is nearly identical to the previous section, where the annual value was found to be $380,000 under the assumption of heteroskedasticity.

An easy criticism of this result would be to argue that the effects of the two variables of interest are conflating each other, causing the insignificance of winning percentage. As a check of this concern we run the same regression and exclude the DraftTotal variable which should, if anything, overvalue the impact of a team's winning percentage on revenue. Table 1.5 [column (2)] shows these results, and we see that even when the number of drafted players is excluded from the model, winning percentage still has no explanatory power in determining revenue.

A possible explanation for this result relies on the fact that in this limited sample we are mainly dealing with prominant universities, which have longstanding athletic histories and traditions. In large part, these major programs derive revenue from relatively stable sources like broadcast rights and donations. Furthermore, while changes in ticket sales can be a large driver of revenue, many of these programs have such engrained fan bases that yearly fluctuations in team performance will have little to no effect on attendance. The results shown indicate that at major college basketball programs overall team performance does little to move the revenue needle, while the addition of star players can still have some revenue implications – most likely due to increased national attention and merchandise sales.

A modified definition of premium players

Our methodology comprises a modest departure from the previous literature in the way in which we define premium players. Brown and Jewell (2004) used only three years of draft data when determining which players went on to be drafted into the NBA. This means that a freshman player in a given season who was drafted following his senior year would not be included as a premium player during estimation. Their justification was that any player who was highly productive as a freshman would likely leave college early to enter the NBA draft and therefore would still be included under this methodology. We decided to forego this assumption knowing that some players, even highly productive ones, decide to stay in college through their senior season. We therefore investigated whether or not the change in methodology had any meaningful effect on our estimates. We redefine the draft total variable in the same method as Brown, and re-run the preferred estimates (Table 1.4 [column (3)]). We see that with this change the coefficient on the variable of interest is still significant and has a value very similar to previous estimates. It does not appear as though the difference in methodology has a large impact on the final results.

1.5 Discussion

While our estimates of marginal revenue product are informative on their own, it is useful to frame them in terms of the current public debate on college athlete pay. We have shown that premium men's basketball players at major programs provide substantial value to their universities, but this is obviously not true for all players in all programs. A simple exercise can be a useful tool in putting this value in perspective (see Table 1.7).

The NCAA allows Division I basketball programs to retain 13 full scholarship players each year.[3] Using our sample of 73 teams belonging to one of the six major conferences, this means that there are approximately 949 players on scholarship in any given year at these institutions. Next, consider the number of premium players (those who go on to be drafted in the following year or another subsequent draft) on a college roster in a particular year. In our data, this quantity is stable and averages about 141 players per season; however, when considering only players on major conference teams, the average falls to 105 players per season. This means that just over 11% of college players at major programs in a given season will go on to be drafted (after the current season or in a future season). If we use our estimate for the value of a premium player ($380,000) we find the total value of drafted players in a given season at the 73 major programs is approximately $40 million. Conversely, we can look at the total value provided to all players in the form of scholarships in a given year. Using a median estimate for scholarship value ($27,923),[4] we find that overall men's college basketball players (at those 73 universities) are provided with approximately $27 million in scholarship value every year.

The previous example uses a median scholarship value to derive the value provided to basketball players in a given year, but arguments have been made that the total value is actually significantly higher. For example, a report released by *USA Today* estimates the

[3]The 13 scholarship limit for Division I men's college basketball is current as of the 2013-14 NCAA Manual - Bylaw 15.5.5.1.

[4]Median grant-in-aid based on Weiner and Berkowitz (2011).

full value of an athletic scholarship to be \$120,000 annually when including factors beyond tuition and room and board – factors such as elite coaching, academic counseling, strength and conditioning training, media relations assistance, etc. (Weiner and Berkowitz, 2011). If this larger scholarship value were used in the above example, it would show that the benefits provided to players would actually exceed the value provided to their institutions, in total. We would expect these additional factors to be even more pronounced for players at major conference programs as they get more exposure in the form of nationally televised games and intense pro scouting. We show in Table 1.7 that if we assume the total annual value of a scholarship is \$50,000 or more (instead of \$28,000), the value provided to all players begins to exceed the value that premium players generate.

There are also arguments to be made that the true value of an athletic scholarship may in fact be lower than previously discussed. The cost of a scholarship to the university is the marginal cost of adding another student in class, which could range from zero to the full cost of tuition depending on whether or not the scholarship athlete is replacing another student. However, if we focus on the *value* of the scholarship to the athlete (not the cost to the university), it makes sense to use the full tuition cost in this discussion. If a potential college basketball player chooses to forego playing basketball and attend college anyway, they will incur the full cost of tuition (not the marginal cost to the university of adding an additional student). There are also counterpoints to the idea that the value of a scholarship may reach as high as \$120,000 in some cases. The main argument is that the additional factors that increase a scholarship's value only benefit athletes to the extent that they increase future earnings. Therefore, these benefits are unlikely to provide significant value to college players who do not go on to play in the NBA. For this reason, we do not use the full $120,000 annual value in our simple example, and instead present a scenario where the total scholarship value is $50,000.

While this may be a simplified example, the intuition is straightforward. Instead of the usual assumption that universities collect additional profit by extracting economic rents from premium players, it may well be the case that universities extract rents in order to cover the scholarship costs of non-premium players. If, in fact, the only players who generate value for their university are those who go on to be drafted, then much of their value may simply be redistributed amongst the team's other players. This example only considers players and teams which participate in one of the major six conferences, since most of the debate around player compensation and exploitation focuses on players at major programs which bring in significant annual revenue.

1.6 Conclusion

The goal of this paper was to review previous estimates of the marginal revenue product of men's college basketball players and attempt to improve upon that work using panel data and different empirical methods. We have shown that some of the value previously attributed to players is absorbed in a fixed-effect model, meaning that it is due to some time-invariant institution-level effects. We argue that this is likely due to factors such as historical athletic tradition, facilities, etc. that universities provide. We also show that the inherent skewness in the revenue data could lead to spurious significant estimates for a larger sample of universities if left uncorrected. Accounting for these factors, we estimate that the yearly value of a premium college player at a major conference university is approximately \$380,000, as opposed to previous estimates in excess of \$1 million. Another interesting result of our paper comes from the realization that revenue can be generated by different factors simultaneously. By accounting for premium players and a team's success at the same time, we are able to account for the possibility that both star players and role players generate revenue for a program through different channels. Our results show that a team's winning percentage in any given year has no effect on revenue given that it plays in a major conference. Therefore, we conclude that only premium players are able to effect revenue at their universities in a significant way.

A limitation we face in this study is related to the definition of a premium player. By considering only those players who are eventually selected in the NBA draft (the highest threshold of player quality) we only account for players with a high enough productivity level to be drafted. It is possible that players who just miss out on being drafted are only marginally less productive than those who are, meaning they could generate revenues very close to the least productive drafted players. Unfortunately, we are unable to observe the productivity level of undrafted players in this way. However, since much of the current debate on this topic focuses on the potential exploitation of superstar college players, (and not of their less-well-known teammates) we do not see this as a major drawback.

We use these estimates to make a case that major college basketball programs may not be exploiting premium players for the reasons typically believed. Instead of extracting economic rents to bolster athletic department profits, these universities may simply be using that extra value in order to cover the cost of scholarships to the non-premium players on the team.[5] One concern with this rational is the fact that it seems clear that overall revenue in men's college basketball has increased substantially in recent years. Since the number of players is generally fixed one would expect that the marginal revenue product of players should be increasing as well. The easiest explanation of this phenomenon is that the additional revenues accrued in recent years are unrelated to a team's on-court performances. A main driver of the revenue boom has been increasing rights fees for television broadcasts. The revenue derived from these sources is consistent and will be largely unaffected by premium players and team performance in any given season. Those particular revenue streams have more to do with the overall popularity of the sport, which can be rooted in team (university) loyalty, and not necessarily player driven productivity.

By no means do we expect that these new estimates will put an end to the debate surrounding student-athlete compensation. However, we hope to narrow the debate by providing more accurate estimates of the current value of men's college basketball players.

[5]The model used in this paper can capture the value created by players directly for their universities, but we do not address the issue at stake in the O'Bannon lawsuit, which remains a completely separate one. The O'Bannon lawsuit argues that players are exploited by the NCAA when their likenesses and images are used to sell merchandise, video games, etc.

Fig. 1.1 Revenue Distribution for All Teams from 2008-2011

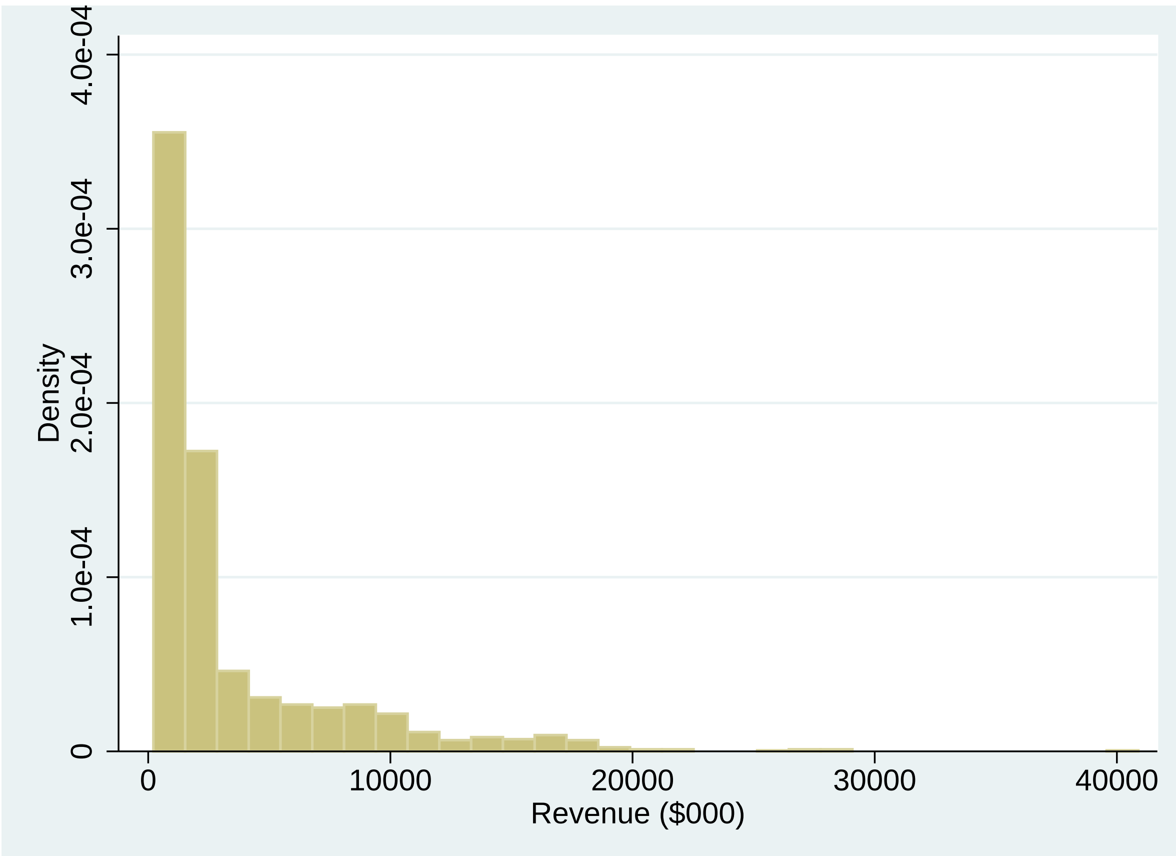

Table 1.1 Summary and Description of Variables

Variable	Description	Source
Revenue	Inflation-adjusted men's basketball revenue per season (thousands)	http://ope.ed.gov/athletics/
DraftTotal	Number of players drafted from a given season's roster	http://www.nbadraft.net/
DraftTotal*FBS	Interaction of DraftTotal and dummy variable for FBS team	FBS designation from: http://ope.ed.gov/athletics/
Rank	Average weekly ranking over prior three seasons	http://www.collegepollarchive.com/mbasketball/ap/
CoachExp/ CoachWinPct	Years of experience for head coach and career winning percentage prior to season	http://www.ncaa.org/championships/statistics/ncaa-mens-basketball-records-books
RPI	Rating percentage index	http://statsheet.com/mcb/rankings/RPI
WinPct	Winning percentage	http://web1.ncaa.org/stats/StatsSrv/rankings ?doWhat=archive&rpt=archive&sportCode=MBB
SOS	Strength of schedule	http://www.cbssports.com/collegebasketball/ bracketology/sos/
StatePop	State population	http://www.census.gov/popest/data/intercensal/state/
StudentCount	Total student population	http://ope.ed.gov/athletics/

Table 1.2 Fixed Effect Models Using Samples of Division I Teams

VARIABLES	(1) Revenue	(2) Revenue	(3) Revenue
DraftTotal	305.4**	408.6**	441.2*
	(132.8)	(175.0)	(238.8)
CoachExp	0.145	-3.041	3.810
	(11.01)	(20.18)	(22.60)
CoachWinPct	65.62	247.7	631.3
	(155.1)	(365.5)	(828.9)
Tournament	-40.80	-114.1	-17.93
	(98.61)	(189.3)	(186.7)
Rank2	114.6	-15.99	6.664
	(138.5)	(228.6)	(304.6)
Rank3	426.8*	403.9	546.8
	(231.7)	(303.3)	(384.3)
Rank4	140.4	45.79	112.0
	(337.2)	(317.6)	(464.9)
Rank5	365.4*	169.3	188.2
	(209.2)	(144.2)	(508.9)
StatePop	-0.000173	-0.000384	-0.000677
	(0.000120)	(0.000254)	(0.000613)
RPI	810.9***	492.1	-1,110
	(294.2)	(926.1)	(1,773)
2008-09 Season	58.57	3.550	-69.09
	(54.90)	(129.2)	(176.1)
2009-10 Season	455.7***	750.6***	1,163***
	(102.7)	(182.7)	(236.9)
2010-11 Season	407.8***	634.5**	951.5**
	(121.6)	(257.8)	(425.6)
Constant	4,313***	9,122**	15,360**
	(1,426)	(3,256)	(6,770)
Observations	1,304	471	292
R-squared	0.093	0.129	0.174
Number of teams	326	118	73

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the conference level in parentheses. In the full sample model, there are 32 clusters used to calculate the standard errors. All revenues are reported in thousands ($000s).

Table 1.3 Fixed Effect Models Using Interaction Terms

VARIABLES	(1) Revenue	(2) Revenue
DraftTotal	-47.25	19.78
	(122.8)	(104.4)
DraftTotal*FBS	439.0**	
	(186.0)	
DraftTotal*Big6		403.6**
		(192.7)
CoachExp	-1.665	-0.497
	(11.15)	(11.35)
CoachWinPct	127.5	116.5
	(148.7)	(144.5)
Tournament	-49.53	-44.85
	(100.4)	(101.7)
Rank2	76.37	98.80
	(142.6)	(135.8)
Rank3	401.0*	444.9*
	(235.3)	(238.0)
Rank4	174.6	185.4
	(345.4)	(359.7)
Rank5	208.2	425.1*
	(229.8)	(229.0)
StatePop	-0.000172	-0.000155
	(0.000120)	(0.000116)
RPI	894.8***	914.9***
	(306.8)	(304.2)
2008-09 Season	64.66	57.73
	(55.99)	(58.21)
2009-10 Season	458.8***	451.8***
	(102.4)	(103.6)
2010-11 Season	414.5***	400.6***
	(120.3)	(123.0)
Constant	4,250***	4,058***
	(1,428)	(1,404)
Observations	1,304	1,304
R-squared	0.100	0.100
Number of teams	326	326

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the conference level in parentheses. In the full sample model, there are 32 clusters used to calculate the standard errors. All revenues are reported in thousands ($000s).

Table 1.4 Fixed Effect Models Using Logged Dependent Variable

VARIABLES	(1) ln(Revenue)	(2) ln(Revenue)	(3) ln(Revenue)	(4) ln(Revenue)	(5) ln(Revenue)
DraftTotal	0.00875	0.0208	0.0276**	-0.0283	-0.0250
	(0.0176)	(0.0128)	(0.0133)	(0.0588)	(0.0453)
DraftTotal*FBS				0.0461	
				(0.0581)	
DraftTotal*Big6					0.0477
					(0.0453)
CoachExp	0.000526	0.000236	0.00175	0.000335	0.000449
	(0.00219)	(0.00244)	(0.00213)	(0.00220)	(0.00219)
CoachWinPct	-0.0760	-0.0308	0.0490	-0.0695	-0.0700
	(0.0519)	(0.101)	(0.107)	(0.0509)	(0.0492)
Tournament	0.0196	-0.0272	-0.00146	0.0187	0.0191
	(0.0310)	(0.0260)	(0.0212)	(0.0300)	(0.0306)
Rank2	0.0135	-0.00893	0.00851	0.00951	0.0117
	(0.0222)	(0.0363)	(0.0345)	(0.0219)	(0.0216)
Rank3	0.0738**	0.0682	0.0759*	0.0711**	0.0760**
	(0.0280)	(0.0389)	(0.0436)	(0.0284)	(0.0296)
Rank4	0.0838**	0.0542	0.0357	0.0874**	0.0891**
	(0.0377)	(0.0370)	(0.0500)	(0.0385)	(0.0402)
Rank5	0.0692*	0.0526	0.0473	0.0527	0.0763**
	(0.0405)	(0.0361)	(0.0570)	(0.0494)	(0.0370)
StatePop	7.11e-08	-1.23e-08	-7.72e-08	7.13e-08	7.33e-08
	(4.43e-08)	(8.42e-08)	(6.59e-08)	(4.49e-08)	(4.46e-08)
RPI	0.372***	0.397*	-0.0196	0.381***	0.384***
	(0.128)	(0.191)	(0.169)	(0.127)	(0.129)
2008-09 Season	0.0556***	0.0128	0.00302	0.0562***	0.0555***
	(0.0189)	(0.0172)	(0.0157)	(0.0195)	(0.0193)
2009-10 Season	0.149***	0.140***	0.121***	0.149***	0.148***
	(0.0228)	(0.0326)	(0.0255)	(0.0230)	(0.0228)
2010-11 Season	0.152***	0.123***	0.0847***	0.153***	0.152***
	(0.0252)	(0.0369)	(0.0306)	(0.0255)	(0.0253)
Constant	6.625***	8.283***	9.591***	6.619***	6.595***
	(0.475)	(0.885)	(0.686)	(0.482)	(0.482)
Observations	1,304	471	292	1,304	1,304
R-squared	0.127	0.121	0.225	0.128	0.129
Number of teams	326	118	73	326	326

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the conference level in parentheses. In the full sample model, there are 32 clusters used to calculate the standard errors. All revenues are reported in thousands ($000s).

Table 1.5 Model Allowing for Contributions of Non-Premium Players

VARIABLES	(1) ln(Revenue)	(3) ln(Revenue)
DraftTotal	0.0272*	
	(0.0137)	
WinPct	-0.00675	0.0200
	(0.0593)	(0.0629)
CoachExp	0.00175	0.00239
	(0.00208)	(0.00201)
CoachWinPct	0.0473	0.0234
	(0.108)	(0.108)
Tournament	-0.000791	0.00485
	(0.0229)	(0.0233)
SOS	-0.175	-0.321
	(0.483)	(0.454)
Rank2	0.00866	0.00712
	(0.0350)	(0.0360)
Rank3	0.0772*	0.0732*
	(0.0438)	(0.0435)
Rank4	0.0380	0.0329
	(0.0505)	(0.0496)
Rank5	0.0491	0.0410
	(0.0577)	(0.0603)
StatePop	-7.78e-08	-7.95e-08
	(6.68e-08)	(7.41e-08)
2008-09 Season	0.00235	0.00351
	(0.0151)	(0.0148)
2009-10 Season	0.121***	0.123***
	(0.0253)	(0.0249)
2010-11 Season	0.0848***	0.0861***
	(0.0306)	(0.0310)
Constant	9.694***	9.858***
	(0.805)	(0.866)
Observations	292	292
R-squared	0.226	0.205
Number of teams	73	73

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors in parentheses. All revenues are reported in thousands ($000s).

Table 1.6 Modified Estimates Using 3 Years of Draft Data

VARIABLES	ln(Revenue)
DraftTotal	0.022*
	(0.012)
Observations	292
Number of teams	73
R-squared	0.215

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors in parentheses. All revenues are reported in thousands ($000s).

Table 1.7 Example of Overall Player Valuation

Number of scholarships allowed per team (Division I)	13
Number of Big Six conference teams	73
Total scholarship players per season (on 73 teams)	949
Avg. number of premium players per year on those teams	105
Percentage of players drafted (from Big Six teams)	11.1%
Total value of drafted players, based on our estimates	≈ $40 million
Total value of scholarships given to all Big Six players *(assuming a yearly scholarship value of $27,923)*	≈ $27 million
Total value of scholarships given to all Big Six players *(assuming a yearly scholarship value of $50,000)*	≈ $47 million

Table A-1 Descriptive Statistics for 2007-08 Season

	Full Sample (N=326)				FBS (N=117)			
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**	**Mean**	**Std. Dev.**	**Min**	**Max**
Revenue ($000s)	3,345.8	4,174.1	214.4	26,000.2	6,519.7	5,309.1	347.0	26,000.2
Quality of Team:								
Winning Percentage	0.510	0.174	0.100	0.950	0.558	0.176	0.100	0.950
Ratings Percentage Index	0.500	0.068	0.346	0.676	0.537	0.064	0.387	0.676
Strength of Schedule	0.499	0.049	0.400	0.620	0.540	0.038	0.446	0.620
Avg. Rank (2005-07)	0.992	2.937	0.000	19.202	2.459	4.356	0.000	19.202
NCAA Tournament	0.199	0.400	0.00	1.00	0.316	0.467	0.00	1.00
Premium Players	0.429	0.908	0.000	6.000	0.983	1.239	0.000	6.000
Market Characteristics:								
Full-time Students	11,182	7,882	1,341	36,835	17,450	7,965	2,812	36,835
State Population (000s)	10,800	9,343	535	36,300	10,400	9,178	535	36,300
Coaching Quality:								
Years of Experience	9.60	8.12	0.00	41	11.14	8.99	0.00	41
Career Winning Percentage	0.503	0.181	0.00	0.818	0.566	0.163	0.00	0.818
Conference Affiliation:								
Pac 10	0.03	0.17	0.00	1.00	0.09	0.28	0.00	1.00
SEC	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
Big 12	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
ACC	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
Big East	0.05	0.22	0.00	1.00	0.08	0.27	0.00	1.00
Big 10	0.03	0.18	0.00	1.00	0.09	0.29	0.00	1.00

Table A-2 Descriptive Statistics for 2008-09 Season

	Full Sample (N=326)				FBS (N=118)			
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**	**Mean**	**Std. Dev.**	**Min**	**Max**
Revenue ($000s)	3,390.2	4,078.9	241.3	26,514.7	6,414.3	5,201.8	320.5	26,514.7
Quality of Team:								
Winning Percentage	0.512	0.171	0.069	0.895	0.536	0.166	0.069	0.861
Ratings Percentage Index	0.500	0.069	0.340	0.668	0.519	0.071	0.352	0.668
Strength of Schedule	0.500	0.045	0.393	0.604	0.536	0.034	0.465	0.604
Avg. Rank (2006-08)	0.992	3.028	0.000	19.200	2.306	4.463	0.000	19.200
NCAA Tournament	0.196	0.398	0.00	1.00	0.347	0.478	0.00	1.00
Premium Players	0.439	0.964	0.000	6.000	0.958	1.323	0.000	6.000
Market Characteristics:								
Full-time Students	11,068	7,869	1,404	43,026	17,194	8,085	2,847	43,026
State Population (000s)	10,900	9,445	546	36,600	10,500	9,270	546	36,600
Coaching Quality:								
Years of Experience	10.04	8.15	0.00	36	11.22	8.80	0.00	36
Career Winning Percentage	0.520	0.165	0.00	0.882	0.564	0.162	0.00	0.807
Conference Affiliation:								
Pac 10	0.03	0.17	0.00	1.00	0.08	0.28	0.00	1.00
SEC	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
Big 12	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
ACC	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
Big East	0.05	0.22	0.00	1.00	0.08	0.27	0.00	1.00
Big 10	0.03	0.18	0.00	1.00	0.09	0.29	0.00	1.00

Table A-3 Descriptive Statistics for 2009-10 Season

	Full Sample (N=326)				FBS (N=118)			
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**	**Mean**	**Std. Dev.**	**Min**	**Max**
Revenue ($000s)	3,772.0	4,609.3	221.5	28,000.4	7,136.6	5,863.7	398.7	28,000.4
Quality of Team:								
Winning Percentage	0.519	0.176	0.033	0.921	0.577	0.159	0.125	0.921
Ratings Percentage Index	0.504	0.068	0.349	0.679	0.543	0.060	0.398	0.679
Strength of Schedule	0.501	0.048	0.393	0.609	0.539	0.040	0.424	0.609
Avg. Rank (2007-09)	0.992	3.105	0.000	23.400	2.318	4.584	0.000	23.400
NCAA Tournament	0.196	0.398	0.00	1.00	0.339	0.475	0.00	1.00
Premium Players	0.426	0.976	0.000	8.000	0.958	1.374	0.000	8.000
Market Characteristics:								
Full-time Students	11,332	8,019	1,451	45,490	17,532	8,239	2,911	45,490
State Population (000s)	11,000	9,552	560	37,000	10,600	9,387	560	37,000
Coaching Quality:								
Years of Experience	10.39	8.25	0.00	37	11.69	8.81	0.00	37
Career Winning Percentage	0.513	0.166	0.00	0.848	0.570	0.152	0.00	0.811
Conference Affiliation:								
Pac 10	0.03	0.17	0.00	1.00	0.08	0.28	0.00	1.00
SEC	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
Big 12	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
ACC	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
Big East	0.05	0.22	0.00	1.00	0.08	0.27	0.00	1.00
Big 10	0.03	0.18	0.00	1.00	0.09	0.29	0.00	1.00

Table A-4 Descriptive Statistics for 2010-11 Season

	Full Sample (N=326)				FBS (N=118)			
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**	**Mean**	**Std. Dev.**	**Min**	**Max**
Revenue ($000s)	3,709.6	4,709.8	346.9	40,887.9	6,989.8	6,225.6	461.7	40,887.9
Quality of Team:								
Winning Percentage	0.514	0.172	0.125	0.921	0.576	0.162	0.125	0.921
Ratings Percentage Index	0.502	0.068	0.345	0.671	0.541	0.064	0.373	0.671
Strength of Schedule	0.502	0.048	0.389	0.620	0.539	0.037	0.418	0.609
Avg. Rank (2008-10)	0.997	3.063	0.000	19.400	2.293	4.424	0.000	19.400
NCAA Tournament	0.209	0.407	0.00	1.00	0.347	0.478	0.00	1.00
Premium Players	0.362	0.917	0.000	7.000	0.805	1.335	0.000	7.000
Market Characteristics:								
Full-time Students	11,538	8,123	1,510	46,894	17,844	8,269	2,943	46,894
State Population (000s)	11,100	9,663	565	37,300	10,700	9,507	565	37,300
Coaching Quality:								
Years of Experience	10.44	8.40	0.00	38	12.03	8.59	0.00	38
Career Winning Percentage	0.507	0.176	0.00	0.856	0.584	0.136	0.00	0.798
Conference Affiliation:								
Pac 10	0.03	0.17	0.00	1.00	0.08	0.28	0.00	1.00
SEC	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
Big 12	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
ACC	0.04	0.19	0.00	1.00	0.10	0.30	0.00	1.00
Big East	0.05	0.22	0.00	1.00	0.08	0.27	0.00	1.00
Big 10	0.03	0.18	0.00	1.00	0.09	0.29	0.00	1.00

Chapter 2

Do Holdouts Pay? Estimating the Impact of Delayed Contractual Agreement on NFL Rookie Contract Values

2.1 Introduction

Contract negotiations and strategic bargaining play a pivotal role in business decisions worldwide. Some of the most well-known and highly publicized of these negotiations take place frequently in professional sports leagues. Among them, the National Football League (NFL) often has the most high-profile, prolonged contract disputes in part due to the fact that players sign non-guaranteed contracts. The nature of these contracts often causes players to try to get larger initial contract values, or attempt to renegotiate a new contract before their current one ends.

While strategic negotiations happen amongst all variety of players in the NFL, up until recently some of the best known instances of delayed contract agreement occurred with newly drafted NFL players trying to come to terms on their first contract.[1] A common negotiating tactic used in these scenarios was for a player to "hold out" or refuse to sign a contract, and thereby refuse to participate in team activities, in an attempt to receive a better offer. In these cases, both sides have leverage in negotiation. The team owns the rights to the player after drafting them, so if the player refuses to sign a contract they will not be able to play in the NFL at all that season. The player holds power because (presumably) the team wants the player to contribute on the field and there may be pressure from fans who want to see a highly touted new player participating. The question which remains, and that we will answer, is whether or not the player yields enough leverage in negotiation in order for the holdout strategy to be successful.

The collective bargaining agreement (CBA) agreed to by the NFL and the NFL Players Association (NFLPA) dictates the terms and conditions under which NFL teams and players are allowed to negotiate and come to terms on contracts. After a series of legal battles between the NFL and NFLPA in the early 1990s, a new collective bargaining agreement was reached in 1993 which allowed for more open free agency and the implementation of a salary cap or a limit on the total amount of money a team can spend on player salaries.[2] That CBA was extended several times and continued through the period under study (2001-07). Article XVII of the CBA addressed contract controls for rookies, in part, by setting up the "Entering Player Pool."[3] This pool served as a league-wide limit on the total amount of salary NFL

[1] JaMarcus Russell was selected by the Oakland Raiders with the first overall pick in the 2007 draft. Contract negotiations stalled and Russell missed all of training camp, not signing a contract until the season had already started (Pasquarelli, 2007).

[2] Based on ESPN's recording of NFL labor history: http://sports.espn.go.com/nfl/news/story?page=nfl_labor_history

[3] A copy of the CBA which was effective beginning in 2006 can be found at: http://static.nfl.com/static/content/public/image/cba/nfl-cba-2006-2012.pdf

teams could use in signing newly drafted players. Each team received an allocation from the pool which was calculated based on the number, round, and position of the team's picks in the draft. The CBA did not dictate how this allocation was to be divided amongst all of a team's draft picks, and it also only controlled the amount of money allotted to rookie contracts in the first year. This allowed for loopholes in which teams could manipulate their allocation using creative signing bonus payouts. These facts led to increasing salaries for players at the top of the draft, which provided a strong incentive for players to hold out or delay contractual agreement in order to continue getting a bigger piece of the pie.

All of this changed with the formation of a new collective bargaining agreement in 2011, which came into existence after a months-long player lockout shut down league operations. There were many topics at issue during the negotiation for a new CBA, including the salary cap, player safety, revenue sharing, and, most importantly for this paper, rookie salaries. In the years leading up to the lockout and eventual agreement on a new CBA, the salaries for top draft picks had started to grow rapidly and out of proportion to the extent that highly drafted rookies were signing larger contracts than established, successful players. As part of the new CBA,[4] the NFLPA agreed to a rookie wage scale which laid out specific contract values based on when a player is selected in the draft. In an instant, newly drafted players had essentially lost their ability to hold out in the hopes of receiving a better contract offer. Even still, the question remains, did player holdouts actually work to systematically increase contract values? While this exact scenario no longer plays itself out every summer following the NFL draft, the wealth of information available regarding the outcome of player contract

[4] A copy of the CBA which went into effect in 2011 can be found at: https://nfllabor.files.wordpress.com/2010/01/collective-bargaining-agreement-2011-2020.pdf

negotiations still makes it relevant in the broader context of strategic bargaining between two parties. Also, while rookie NFL players no longer have the ability to hold out, this strategy is still used by veteran players in the NFL looking for improved contract terms.[5]

Strategic bargaining remains prominent in many business and employment settings, whether it be a labor union bargaining over new contract terms with management or recent college graduates negotiating their first employment contract. In this paper we set out to develop the first causal estimates of the effect of a contract holdout on the value of an NFL rookie contract. Using an instrumental variables model we find that a player who delays contractual agreement, and signs a contract after the start of his team's training camp, receives an increase in total contract value of nearly $120,000, on average. We also find that this effect is substantially larger for players who are selected in early rounds of the draft, increasing to an average effect of around $430,000 for players selected in the first round. Lastly, we evaluate the impact that holdout duration has on a player's total contract value. Using the distribution of player holdout lengths we find that for each additional day a player delays agreement, total contract value increases by approximately $10,000, on average.

The next section discusses some of the relevant theoretical and empirical research on the topic of strategic bargaining, focusing specifically on the tactic of delaying contractual agreement. In Section 2.3 we describe the data used for our empirical analysis. Section 2.4 outlines a preliminary model to estimate the effect of a contract holdout. Section 2.5 discusses the potential endogeneity problems in the model and addresses the need for instrumental

[5]Two recent examples involve the Seattle Seahawks. In 2014, running back Marshawn Lynch did not report to training camp on time in the hopes of agreeing to a new, larger contract (Breech, 2014). In 2015, safety Kam Chancellor went so far as to miss all of training camp along with two regular season games in an attempt to get more money (Brinson, 2015).

variables estimation. Section 2.6 discusses the results of the instrumental variables model and presents a simulation exercise. In Section 2.7 we conduct robustness checks related to our choice of an instrumental variable.

2.2 Background & Conceptual Model

A wealth of research exists which is broadly related to strategic negotiation and bargaining, as well as some specifically dealing with NFL contracts.[6] A sampling of the relevant work that forms a basis for this paper is discussed below as we examine literature dealing with the timing of offers, private information, and specifically with NFL contracts.

Admati and Perry (1987) study a variation on the standard alternating offers bargaining model. Using the model they show that endogenous time between offers can be an important strategic variable in bargaining with incomplete information. In characterizing the equilibrium outcomes of the game they show that endogenous time between offers will be used in equilibrium by players to signal their relative strength, which may then cause a delay in reaching agreement. Also, the delay does not vanish as the minimal time between offers becomes arbitrarily small. Their results also support the casual observation that, in many bargaining situations, some delay between offers exists. The idea that a player in a bargaining situation delays agreement in order to signal their relative strength reinforces later, NFL specific, work in Conlin (1999).

[6]See Roth and Murnighan (1982), Samuelson (1984), and Roth et al. (1988) for a primer on the roles that information and deadlines play in bargaining models.

Ma and Manove (1993) note that anecdotal and experimental evidence suggests that parties who engage in bargaining under a deadline exhibit a complex behavioral pattern, and not only are delays in agreement common, but parties may fail to reach an agreement before the deadline. Another observation is that cheap talk and stalling are frequently seen in the early stages of negotiations. In this paper, they construct a complete-information model with an equilibrium that reflects the following stylized facts: initial delay, moderate offers, rejected offers, agreement near the deadline, and some failures in reaching agreement. In order to obtain these equilibria they add two additional features to the finite-horizon bargaining model: strategic delay and imperfect player control over the timing of offers during the bargaining session. The premise of bargaining under a deadline fits nicely in the context of NFL contract negotiations where we examine whether players reach agreement prior to the start of training camp, which serves as the deadline. The results of this model in which agreements either happen near the deadline or fail to be made mirror what we see in the contract data being studied.

Forsythe et al. (1991) conduct an experimental study of two-player pie-splitting games in which one player has private information about the size of the pie. The bargaining in the experiment occurs under a deadline where failure to reach agreement results in both sides getting nothing. Even operating under this severe penalty (which is much harsher than failing to reach agreement in our case) they observe some games resulting in "strikes" or failures to reach agreement.

Kennan and Wilson (1993) also examine the outcome of bargaining games with private information. They review theoretical and empirical studies of bargaining and conclude that delays and other costly actions may be required to convey private information credibly, as they provide convincing evidence. Further, they find that in economic transactions, inefficient delays in agreement can occur due to the informed party's attempt to signal. The idea that a player with private information may use delayed agreement as a signal in negotiation forms a basis for our model.

Conlin (1999) provides the foundation for the empirical analysis found in this paper. He develops a conceptual model for a separating equilibrium in the bargaining of NFL contracts. The basis of the model is that while there is substantial public information available about players at the time of the NFL draft, players may still maintain some private information about their ability, skill level, or risk preferences. This private information can be either positive (perhaps the player knows they did not perform to their abilities in workouts) or negative (a player could have an undisclosed injury). The separating equilibrium predicts that a player who delays contractual agreement signs a more lucrative contract and has private information on his ability level at the time of contract negotiations. The empirical results support the implications of the separating equilibrium. Using contract data for 1,873 players selected in the 1986-1991 drafts, the empirical results show that players of high ability levels sign after longer contract negotiations and that players who delay contractual agreement sign more lucrative contracts.

There are several empirical issues with the analysis in Conlin (1999) that we address in this paper. First, we attempt to correct for the endogeneity in the decision to hold out. It seems likely that there are unobserved factors which affect the decision for a player to hold out, as well as the eventual contract value. We implement an instrumental variables regression to correct for this problem, which allows us to calculate causal estimates of the monetary value of delaying contractual agreement in NFL contract negotiations. We also address the fact that rookie contract values are highly right-skewed which can distort regression results when estimating conditional means.

2.3 Contract Values & Empirical Characteristics

The data used for our analysis originates from contracts signed by newly drafted players in the 2001-2007 seasons. Teams are awarded one draft pick in each of seven rounds in the current draft format with the possibility of extra compensatory picks being awarded.[7] Information is available for 1,776 contracts that were agreed to, and had details reported to the NFLPA, during the period of study between rookie players and one of the 32 NFL teams (31 in 2001).[8] Table 2.1 presents descriptive statistics for the relevant variables in the dataset.

The dataset provides information on the length of contract signed, base salaries in each contract year, and the signing bonus paid. From these values, we calculate two dependent variables that are used in our analysis. First, total contract value is simply the sum of all base year salaries plus the signing bonus. Second, we calculate an average prorated value as the total contract value divided by the length of the contract such that the signing bonus is divided over the life of the contract. In both cases, all contract amounts are converted

[7]The NFL can assign as many as 32 compensatory draft picks based on the number and value of free agents lost by each team. The picks are assigned at the ends of rounds 3-7 and are determined by a proprietary formula developed by the NFL Management Council: http://operations.nfl.com/the-players/the-nfl-draft/the-rules-of-the-draft/.

[8]We are grateful to Michael Conlin for providing the data, which was originally furnished by the NFLPA. The data provided includes information on contract terms, players, agents, and teams. We added state-level characteristics to the existing data. All data used are described in detail throughout the paper.

into constant 2007 dollars, and then any future base salaries are discounted to develop a net present value.[9] Figures 2.1 and 2.2 show that, regardless of which measure we use, the contract values are right-skewed. This is due to the fact that high draft picks sign lucrative contracts and then there is a sharp drop-off for contracts of lower selections in the draft.

In the case of NFL rookie contracts, we can define holdouts in a specific way. After a player is selected in the NFL draft (typically held in late April for the period under study), he begins negotiating a contract with the selecting team. The player is typically represented by an agent during this process who negotiates on his behalf. Ideally, players come to an agreement at some point between the draft and the start of the team's training camp. Training camp is the first mandatory team activity leading up to the regular season, and typically begins in late July or early August. We define a holdout as a situation where a player fails to sign a contract before the start of training camp. The duration of a holdout can be calculated by setting the training camp start date for a given team as the zero point. Any player who signs prior to the start of training camp will have a negative holdout duration describing how many days early he signed, while a player who signs after the start of camp will have a positive holdout duration which describes the number of days after training camp began that the contract was signed.

In one model specification we define a holdout dummy variable which is equal to one for any player who signs a contract after the start of training camp and zero otherwise. Figure 2.3 shows the distribution of time over which players sign their contracts. It becomes clear

[9]Net present values are calculated using the 3-year, constant maturity U.S. Treasury securities rate for the year in which the contract was signed as the discount rate. The results given throughout the paper are not materially different than the results obtained either by using a constant five percent discount rate or without using a net present value calculation for contract values.

that a majority of players sign contracts in the days leading up to the start of training camp. In addition, there are players who fail to meet the deadline and do not agree to a contract until after training camp has started (this type of result was suggested by the experimental evidence in Ma and Manove (1993)).

In a second model specification we account for the duration of a holdout by defining a left-censored version of the holdout variable (Figure 2.4). In this variation all players who sign a contract prior to the start of training camp are assigned a zero value, and those who delay agreement are assigned a value equal to the duration of the holdout, in days. The justification for using a left-censored variable instead of the entire continuous distribution of player signings (both positive and negative) stems from the underlying model assumptions. In Conlin (1999), the model that is developed assumes that players choose to hold out as a signal that they have some type of positive, private information. The reasons why players sign well in advance of the training camp deadline are less clear. Oftentimes signing early could be for reasons unrelated to contract negotiation and maximizing contract values. In some cases first overall picks sign early because they were already negotiating a contract prior to the draft. In other cases players may sign quickly due to extreme risk aversion. In any case, since the reasons are not clear, we censor those observations (and give them all a value of 0).

2.4 Ordinary Least Squares Model & Results

We specify a model intended to test whether or not delaying contractual agreement does indeed lead to more lucrative contract terms. Our dependent variable ($Y_{i,n}$) takes several forms as we try to account for different measures of contract value. As discussed in the previous section, the different contract measures include the total contract value and the average prorated value. Regardless of which measure of contract value is being studied, we use a log transformation to account for the right-skewed distribution:

$$ln(Y_{i,n}) = \alpha Holdout_i + \beta(P_i, A_i, T_n, S_n) + \tau_t + \eta_n + u_i, \tag{2.1}$$

where $Y_{i,n}$ represents the value of a contract agreed to between player, i, and team, n. In addition $Holdout_i$ represents the holdout dummy variable and P_i, A_i, T_n, S_n are the control variables related to the player, agent, team, and state, respectively. Also, τ_t represents time fixed effects and η_n represents team fixed effects.

Contract values are determined by several factors relating to: the player; the agent who negotiates the contract; the team that signs the player; and the state where the team is located. First, we must control for the ability and skill level of the player being drafted (P_i). The results of the NFL draft provide the best look at the public information available regarding player skill at the time of the draft. The position where a player is selected in the draft is largely representative of how NFL teams view his skills. In the model we include the player's overall selection number as well as a squared term to allow for a non-linear effect. Also, the position played can significantly affect a player's value to a given team, so we include dummy variables controlling for the different position groups that players are categorized into, which include: quarterback, running back, wide receiver, tight end, offensive lineman, defensive lineman, linebacker, defensive back, kicker, and punter. Finally, we include a dummy variable denoting whether or not a drafted player attended a Division I-A (Football Bowl Subdivision) university. Teams may place value on the fact that a player has competed against the highest levels of competition in college football.

Our next set of controls (A_i) relate to the agent a player hires to negotiate a contract on his behalf. It is likely that an agent with more experience is better suited to negotiate a more favorable contract, with either higher total value or better terms (Conlin et al., 2013). We use data on the number of years an agent has been certified with the NFLPA and the number of other players represented as measures of agent experience.

Next, we consider team characteristics (T_n) which could affect player contract values. First, a team's win/loss record in the previous season largely determines where in the draft order a team selects, and it may also alter the team's willingness to offer favorable contract terms early in the negotiating process. The intuition is that successful teams are less likely to be in dire need of signing new rookie players compared to teams who have been performing poorly. Second, as a proxy for revenue and popularity, we control for a team's attendance in the previous season with the ratio of empty seats in their stadium during home games. Additionally, we account for the length of tenure for the team's current head coach. A long-tenured head coach may have more influence in player selection and contract negotiation than a newly hired coach. We also include a set of team fixed effects to control for the team participating in the negotiation, as different teams may approach this strategic setting differently. In doing this, the variation in the model is determined across players over time within a given team.

Finally, we use controls for various state-level characteristics (S_n) which could impact contract value and negotiation including, state per capita income, the unemployment rate, and a dummy variable indicating whether or not a state has passed the Uniform Athlete Agent Act (UAAA) which requires the registration of all agents at the state level.

Results of the initial model are shown in Table 2.2. Separate models are estimated using either the total contract value or the average prorated contract value as the dependent variable. Both of the models contain team fixed effects, as well as time fixed effects to control for macroeconomic factors which affect contracts uniformly across players and teams (for brevity, they are withheld from the tables).[10] The variable of interest is the dummy variable representing whether or not a player chose to hold out or delay contractual agreement until after the start of training camp.

The results show that choosing to holdout has a positive effect on both total contract value and average contract value.[11] In particular, holding out has the effect of increasing total contract by nearly 2 percent. While the effect is only statistically significant when looking at the prorated average contract value, we explore a different model specification in the next section which allows us to control for endogeneity. Also of note, based on the R-squared values given in Table 2.2, our set of control variables capture (in large part) the factors that cause variation in contract values due to the selection number in the draft being a significant determinant of contract values.

[10]For this model, and throughout the paper, we also produced an alterntate version using state fixed effects instead of team fixed effects (not shown). In all cases, this change did not have a meaningful impact on the estimates.

[11]Also, as one might expect, a quantile regression model confirms that players who sign higher-valued contracts enjoy larger benefits from holding out compared to those who sign relatively smaller contracts.

2.5 Instrumental Variables Model

As discussed in Section 2.2, the previous literature fails to address the potential endogeneity in the decision to hold out made by a player. Some possible sources of endogeneity in the previous model are unobservable aspects of performance confounding the estimates[12] or differing risk preferences, whereby better players have more concern for injury which leads to higher holdout rates. To address this issue we must find a valid instrument for the holdout dummy variable or the censored holdout variable. In order for our chosen instrument to be valid it must satisfy the following conditions. First, the instrument must be relevant, meaning that it has a significant effect in the prediction of the endogenous variable. Second, the instrument itself must be exogenous meaning that it can impact our dependent variable only through its effect on the decision to hold out, and not through a direct effect. Finally, the instrument should not be correlated with any unobservable control variables which would confound the relationship.

2.5.1 Potential Instrument & First-Stage Results

We examine whether state income tax rates function as a valid instrument. First, we can test whether the instrument is relevant through a first-stage regression where we consider the decision to hold out as the dependent variable, and we see if state income tax rates have a significant impact. A player who is negotiating with a team located in a high-tax state may be more likely to hold out for better contract terms since a larger portion of their contract

[12] We would expect positive unobservable performance characterisitcs to be positively correlated with holdout rates. Further, contract values by round are offered based on average expected performance, so a high-ability unobservable would be negatively correlated with contract value. This type of endogeneity could lead to underestimation or a downward bias in the OLS estimates.

value is allocated to paying the tax. Second, we must argue that state tax rates should not have a direct effect on contract values. The logic behind this argument is that, since NFL teams face a hard salary cap in the period being studied, teams in high income tax states cannot systemically offer more lucrative contracts to all players to compensate for the higher tax rates.

Table 2.3 shows the results from the first-stage estimation where the decision to hold out or the censored duration of a holdout is the dependent variable and the state income tax rate is the instrument. The state tax rates used in this section correspond to the highest state income tax rate imposed if the state uses a progressive taxing system, or the flat tax rate in other states.[13] The set of control variables remains the same from the previous section. We use a probit model for the binary holdout decision, and a tobit model for the censored duration of a holdout. The results in Table 2.3 show that state tax rates have a significant impact on a player's decision to delay contractual agreement.

The reasoning as to why this would be the case merits further discussion. The effect found here may seem unusual since most employees are taxed by their place of residence. So, a player could simply establish residence in a state with a lower income tax rate and be unaffected by the tax rate where his team is located; however, the payment of state income tax for professional athletes is a more complicated matter. Athletes are beholden to a so-called "jock tax" which is sometimes a separate tax law but usually just an aggressive extension of a regular income tax applied to selected nonresidents by a city or state (Hoffman

[13]State income tax rates for 2001-2007 were compiled from data available through the Tax Foundation: http://taxfoundation.org/article/state-individual-income-tax-rates.

and Hodge, 2004).[14] The basic methodology determines a visiting athlete's daily income in that state by dividing the number of "duty days" into his annual salary. Most states consider a duty day to be any day in which team activities are being held, such as regular season games, preseason games, postseason games, practices, etc. For example, if an NFL player has 150 duty days in a season, and they spend 15 of those days working in a given state, 10 percent of their annual salary will be considered taxable income in that state.

For a player who lives in the state in which his team plays, the majority of his salary will be taxed at that state's tax rate. Similarly, a player who lives in a different state will still have the majority of his income taxed at his team's home state tax rate since the majority of his duty days will be in that state. In summary, regardless of where an NFL player chooses to live, his tax liability will be most affected by the tax rate in his team's home state. One caveat is that signing bonuses are generally taxed only in a player's home state provided the bonus meets certain criteria.[15] Therefore, a player could limit his tax exposure by setting up residence in a lower tax state; however, it would require them to maintain two separate residences (one year-round and one during the season) which often is not cost-effective, especially for rookie players.

2.5.2 Maximum Likelihood IV Model & Results - Binary Holdout

In this section we combine the empirical improvements of this paper and estimate a full maximum likelihood linear regression model with an endogenous binary treatment variable,

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[14] The first instance of a jock tax was retaliation by the state of California when they taxed Chicago Bulls players following their defeat of the Los Angeles Lakers in the 1991 NBA Finals. The tax strategy became widespread from that time through 1995 (DiMascio, 2007).

[15] Based on a conversation with Sean Packard, CPA, who is Director of Tax at OFS Wealth. He specializes in tax planning and the preparation of tax returns for professional athletes.

which represents the decision of whether or not to hold out. The model was originally derived by Maddala (1983), and combines the previously described linear model for the outcome with a probit first stage, estimated by maximum likelihood:

$$ln(Y_{i,n}) = \alpha Holdout_i + \beta(P_i, A_i, T_n, S_n) + \tau_t + \eta_n + u_i \tag{2.2}$$

and $Holdout_i$ is a binary-treatment variable defined as

$$Holdout_i = \begin{cases} 1, & \text{if } \ w_i\gamma + \epsilon_i > 0 \\ 0, & \text{otherwise} \end{cases} \tag{2.3}$$

where w_i are the covariates used to model the decision to hold out, and the error terms u_i and ϵ_i are assumed to be bivariate normal with mean zero.

Table 2.4 shows that the estimated coefficients on the holdout dummy variable are statistically significant (at the 1% level) when looking at the total contract value or the average prorated value of the contract.[16] This implies that a player's decision to delay contractual agreement until after the beginning of training camp is rewarded in the form of a more lucrative contract. Given that our dependent variable in both models is log-transformed, the magnitude of the treatment effect is directly interpreted as a semi-elasticity with the effect calculated as $[100 * (e^{\beta} - 1)]$ since the variable of interest is binary (Halvorsen and

[16] While the estimated coefficients found in this model are similar in magnitude to those found using a traditional (linear) two-stage least squares (2SLS) estimation, 2SLS estimation lacks the efficiency necessary to achieve significant results due to the small sample size.

Palmquist, 1980). Given that, the decision to hold out leads to about a 4.6 percent increase in total contract value or just over a 5 percent increase in the average prorated contract value.

In order to calculate the marginal effect of interest in dollar terms, we must re-transform the dependent variable, and we do so using a method based on Duan et al. (1983) and Manning (1998). Following Duan et al. (1983), we use the "smearing factor" approach where the marginal effect of holding out is calculated by:

$$\frac{\partial E(Y|X)}{\partial x} = \phi_s \exp(x'\beta + \alpha) - \phi_s \exp(x'\beta), \tag{2.4}$$

where the smearing factor, ϕ_s, is the estimated sample average of the exponentiated residuals and α is the estimated coefficient on the holdout dummy variable. This approach is only valid under the assumption of homoskedasticity; however, we can allow for heteroskedasticity by defining subgroups in the sample within which the error term has a constant variance and computing separate smearing factors for each subgroup. Under the assumption of heteroskedasticity, using the round in which a player was selected in the draft as the subgroup, the average marginal effect on the total contract value resulting from holding out is approximately \$119,000. Using the same method we also calculate the average marginal effect of a holdout on the prorated average contract value and find the effect to be approximately \$30,000.

2.5.3 Maximum Likelihood IV Model & Results - Censored Holdout

In the previous section, we examined the impact of the decision to hold out and treated it as a binary decision, a player either holds out or he signs on time. The question remains as to whether or not all holdouts are equal, or if there might be varying strength and intensity amongst holdouts. Logically, it would seem to make sense that a player who holds out for one day generates less bargaining power (and sends a less emphatic signal) than a player who chooses to hold out for 5, 10, or even 15 days. Since we have data available on the exact date in which a player signs a contract, we can also examine what impact the duration of a holdout might have.

We specify a similar maximum likelihood model to the previous section, but in this case we estimate a tobit first stage. Since we are using a censored distribution of holdout duration we can rewrite Equation 2.3 as:

$$Holdout_i = \begin{cases} w_i\gamma + \epsilon_i, \text{ if } \; w_i\gamma + \epsilon_i > 0 \\ \\ 0, \;\; \text{otherwise} \end{cases} \tag{2.5}$$

The model is estimated as a recursive mixed-process model according to Roodman (2011), which allows for consistent estimation of recursive systems, of any form, where all endogeneous variables are explanatory and observed. The first-stage estimates from this model are identical to the tobit estimates found earlier in Table 2.3, column (2).

Table 2.5 shows the model results and the estimated coefficients on holdout duration are positive and significant, as expected, for both the total contract value and average prorated value. This implies that as the duration of a holdout increases, so does the eventual contract value. As in the previous model, given that our dependent variable is log-transformed, the magnitude of the treatment effect is directly interpreted as a semi-elasticity where a one day increase in holdout duration leads to about a 0.38 percent increase in total contract value or to about a 0.45 percent increase in the average prorated contract value. As before, the results are more useful and intuitive in level form, so we re-transform the marginal effects.

Following Duan et al. (1983), we again use the "smearing factor" approach where the marginal effect of holding out is calculated by:

$$\frac{\partial E(Y|X)}{\partial x} = \phi_s \exp(x'\beta)\beta_1 \tag{2.6}$$

where the smearing factor, ϕ_s, is the estimated sample average of the exponentiated residuals and β_1 is the estimated coefficient of the holdout duration variable. Under the assumption of heteroskedasticity, the average marginal effect on the total contract value resulting from each additional day of a holdout is approximately \$10,300. Given that a player chooses to holdout, the average duration in the sample is 5.8 days. Therefore, for a holdout of average duration, the estimated increase in total contract value is equal to roughly \$60,000.

2.6 Discussion & Simulation

For simplicity, in this section we focus on the effects related to the total value of a contract. The aim is to ensure that the estimated values in both stages of the instrumental variables model are compatible with the presumed economic incentives of drafted NFL players.

In order to determine the feasibility of the estimates, we consider a simple simulation in which there is a one percentage point increase in state income tax rates. First, an increase in tax rates causes a change in total tax liability for players. Second, based on our first-stage estimation results, we showed that an increase in state tax rates leads to an increased probability of a player holding out, which in turn increases the expected value from a holdout. The questions to be answered are whether or not our estimated values are consistent in this framework, and would these effects cause any unexpected incentives to arise for players (such as delayed agreement having such overwhelming value that all players would hold out).

We first conduct the simulation using average marginal effects across all players, and then examine any differences when evaluating the results by the round in which a player was drafted.

Table 2.6 shows the simulation results. The average total contract value in the sample is \$2,746,000 which implies that a one percentage point increase in state tax rates increases a player's tax liability by \$27,460. The estimated average marginal effect of a holdout is \$119,000 in the full sample; however, we are interested in finding the change in the expected value of holding out. Based on the first-stage model results, a one percentage point increase in state tax rate leads to about a six percent increase in the probability of holding out. Therefore, a one percentage point increase in state tax rates increases a player's expected value from holding out by \$7,140 (\$119,000*0.06). The estimated values are in line with expectations. While we expect that the decision to hold out provides additional value to the player, if our simulation had shown an increase in expected value that was equal to or larger than the additional tax liability it would imply that all players should choose to hold out, which we know is not the case. The simulation results seem to align with reality and generate proper incentives in that the additional value from holding out is high enough to encourage some players to do so, but not so high as to incentivize everyone to hold out.

Table 2.6 also shows simulation results where we breakdown the sample by the round in which players were drafted. As expected, early round draft picks have higher contract values which lead to higher marginal effects from holding out. The key result persists in that the increases in the expected value from holding out are reasonably sized and do not imply that all players should ultimately decide to hold out.

2.7 Robustness Check

When possible, it is useful to conduct further analysis to confirm and support the results shown. In this section we discuss alternate results which further support our use of state income tax rates as an instrumental variable.

We argued in Section 2.5 that state tax rates are a valid instrumental variable in our model. The justification was based, in part, on the fact that since states enforce so-called "jock taxes" a player will always face the largest tax burden from his team's home state, regardless of where he chooses to live. Therefore, it is logical that the state tax rate where his team resides impacts his decision of whether or not to hold out for a better contract. Since the enforcement of jock taxes only began in late 1991, we can use earlier NFL rookie contract data to verify that the change in tax policy is important. In this section we examine rookie contract data from the 1986-91 seasons.[17] We run similar first-stage regressions to our previous analysis.[18] Since the entire period occurs prior to the enforcement of state jock taxes, we should expect to see that a team's home state income tax rate should have a more limited (or no) impact on a player's contract decisions since he will only be taxed based on where he chooses to live.

Table 2.7 shows the first stage results using the earlier contract data. We see that a team's tax rate in their home state now has no significant impact on a player's decision to delay contractual agreement, independent of whether we are modeling the holdout as a binary decision or as a censored continuous variable. This provides further justification for our use of state tax rates as a valid instrument in our model.

2.8 Summary & Conclusion

This paper provides the first estimates of the impact of a rookie NFL player's decision to hold out on their contract value that control for endogeneity in the holdout decision. The estimated value of a holdout on the total contract value (in 2007 dollars) is approximately $120,000, on average, across all players in the sample. Naturally, the value is greater for players who are selected earlier in the draft and sign larger contracts, with the average effect rising to nearly $430,000 for a first round draft pick. We also examine the differing effects of

[17] As before, this data was originally compiled by the NFLPA. Again, we are grateful to Michael Conlin for providing access to the data.

[18] There are minor differences in the models due to differences in data availability. The Uniform Athlete Agent Act only came into existence in 2000, so it is not relevant in the earlier set of data. Also, in the earlier data we do not know how many other players a given agent represents. However, if we re-estimate the original first-stage estimates without those two controls, it does not effect the significance of the results.

holdout duration or intensity. For each additional day that a holdout continues, the effect on the total contract value is $10,000, on average.

We set out to empirically analyze a strategic bargaining setting in which a player uses a delay in contractual agreement as a negotiating strategy. In the specific context studied, NFL rookie players are able to extract additional contract value by employing a strategy where they delay agreement until after a deadline (in this case the start of the team's training camp). We face certain limitations in this study, primarily related to data availability. Ideally, we would examine a larger sample of players, including both veteran and rookie players. Also, having information available about the subsequent contracts signed by each player would be useful in testing the private information aspect of the model. It is impossible to observe whether or not, at the time of negotiation, a player holds some positive private information, but we could test this theory by evaluating player performance in the years following a player being drafted/signed. By observing whether a player over or underperformed statistically, relative to his draft position, in the years following the draft we could infer whether or not that player may have had positive private information regarding his ability.

Even given these constraints, it is still useful to broaden this result and think about whether the same strategy could be useful and effective in other bargaining situations. In all likelihood, NFL rookie players had less leverage in their contract negotiations than a typical employee since they are prevented from signing with another NFL team, although their skills are likely more scarce than in many other professions. This situation would be comparable to an employee trying to negotiate a new employment contract after having signed a non-compete agreement, which would prevent the employee from signing a contract with any direct competitors. Even with the somewhat minimal leverage that NFL rookie players possess, we find that delaying agreement still had value in negotiating better contract terms, and likely acted as a signal to an NFL team that the player holds some positive private information about his skill or ability level.

Fig. 2.1 Distribution of Total Contract Values ($000s)

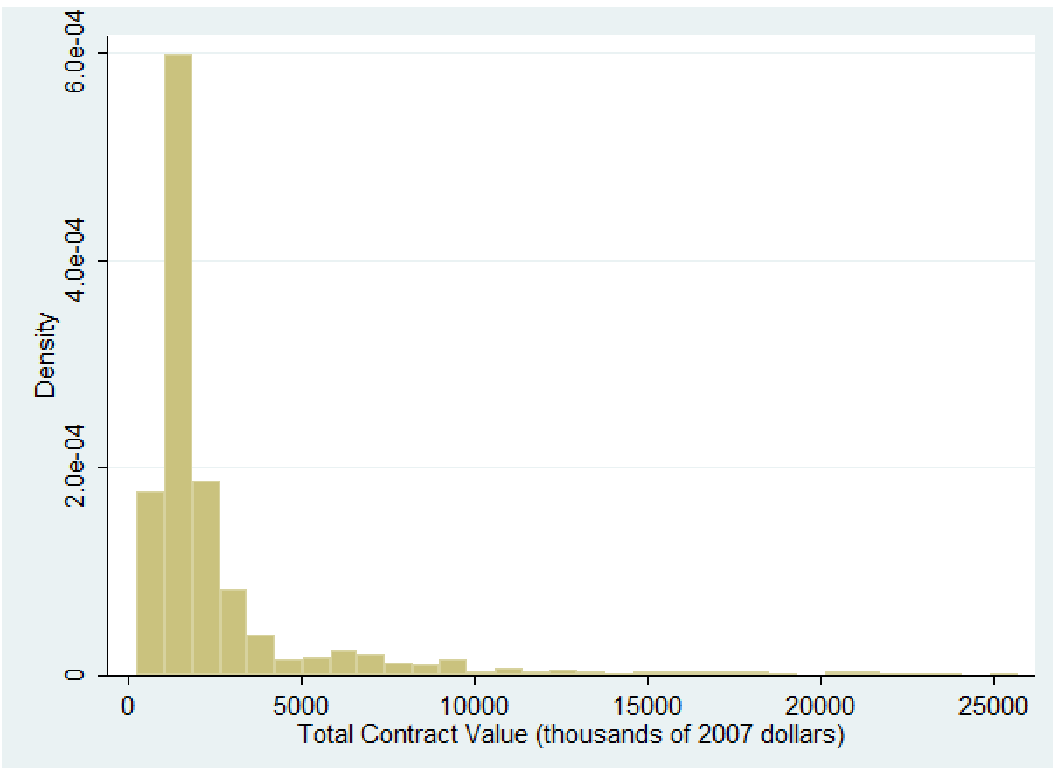

Fig. 2.2 Distribution of Average Contract Values ($000s)

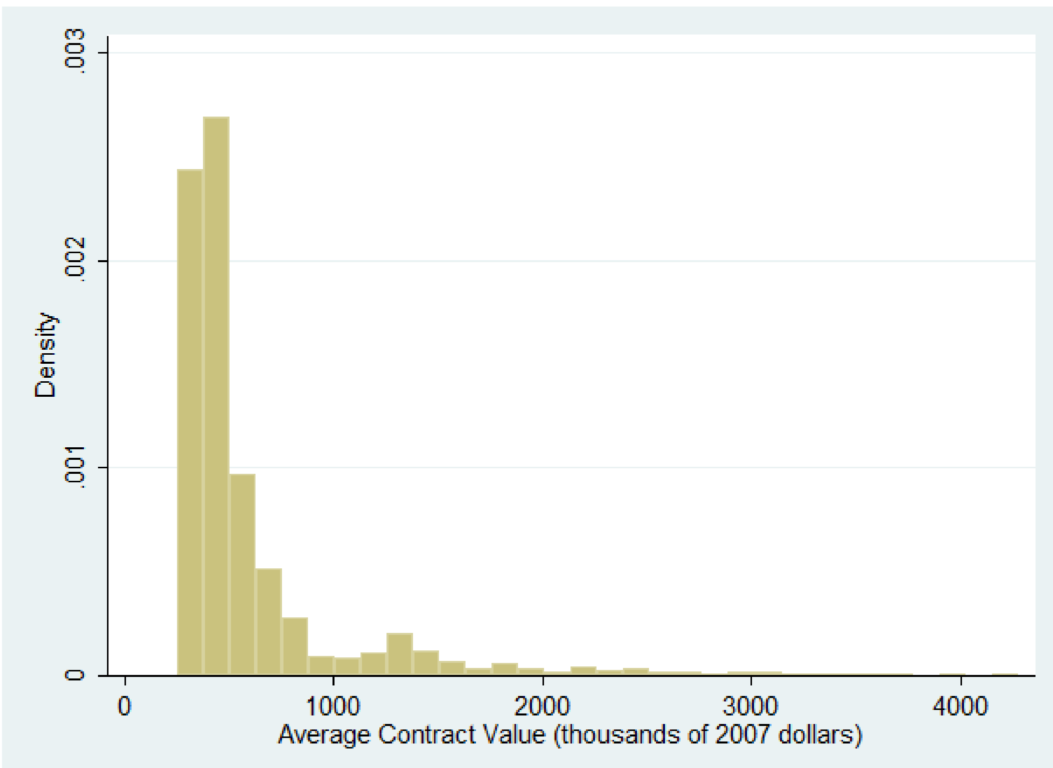

Fig. 2.3 Distribution of Holdouts

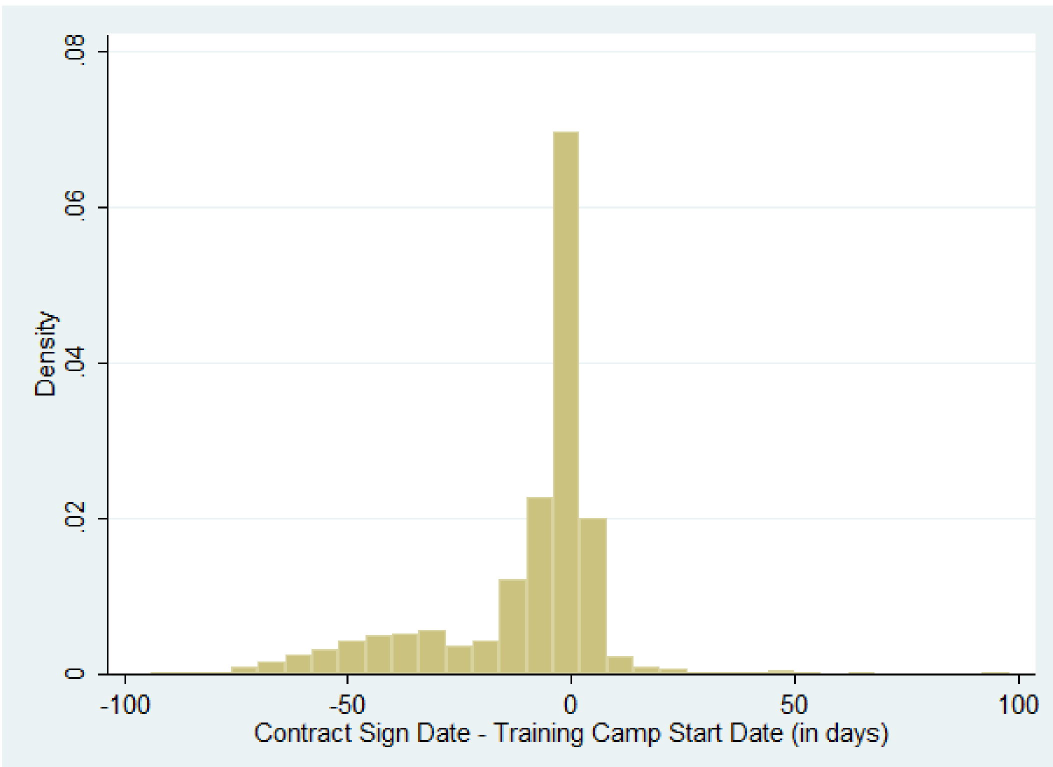

Fig. 2.4 Distribution of Holdouts (Censored)

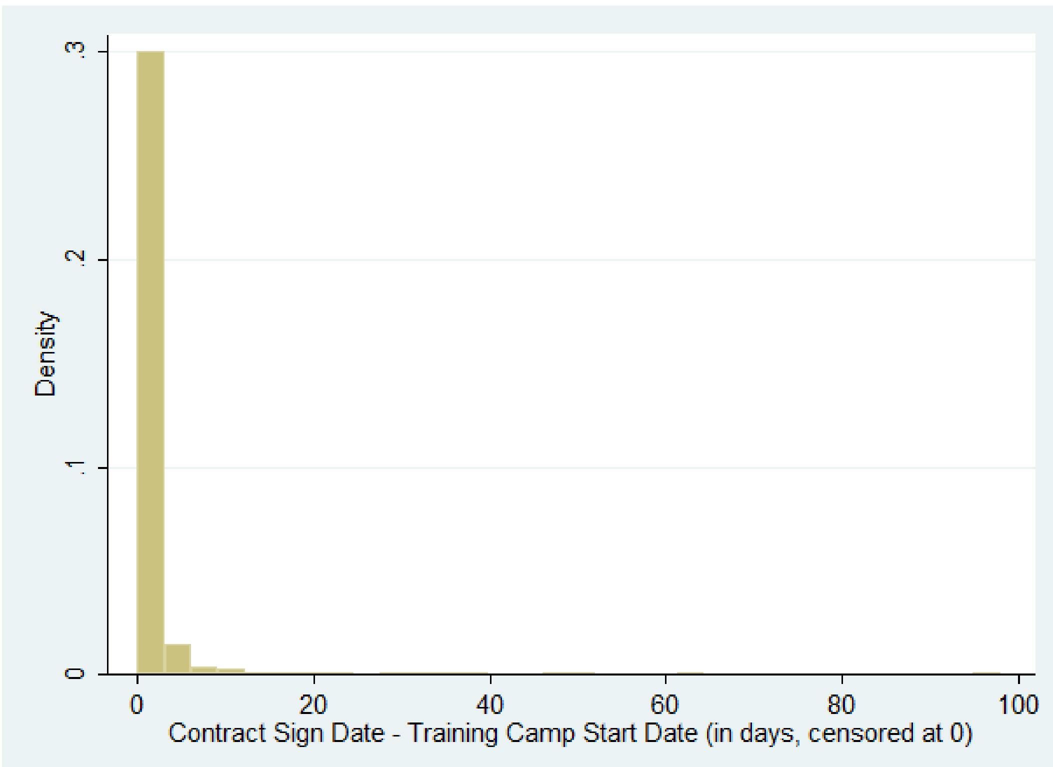

Table 2.1 Descriptive Statistics for 2001-07 Contract Data

Variable	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
Total contract value ($000s)	1776	2,746.01	3,388.16	253.00	25,718.22
Avg. prorated value ($000s)	1776	620.41	503.31	253.00	4,276.42
Signing bonus ($000s)	1776	612.63	1,126.09	0.00	10,920.00
Contract length (years)	1776	3.81	1.03	1.00	7.00
Holdout duration (days)	1776	-10.62	19.26	-94.00	98.00
Holdout (0 or 1)	1776	0.19	0.40	0.00	1.00
Division I-A (FBS) School	1776	0.91	0.28	0.00	1.00
Quarterback	1776	0.05	0.22	0.00	1.00
Running back	1776	0.09	0.28	0.00	1.00
Wide receiver	1776	0.13	0.34	0.00	1.00
Tight end	1776	0.06	0.23	0.00	1.00
Offensive lineman	1776	0.17	0.38	0.00	1.00
Defensive lineman	1776	0.18	0.38	0.00	1.00
Linebacker	1776	0.11	0.32	0.00	1.00
Defensive back	1776	0.19	0.40	0.00	1.00
Kicker	1776	0.01	0.0.07	0.00	1.00
Punter	1776	0.01	0.12	0.00	1.00
Team win/loss (prev. season)	1776	7.83	3.11	0.00	15.00
Head coach tenure	1776	3.97	3.05	1.00	15.00
Ratio empty seats (prev. season)	1776	0.05	0.08	0.00	0.53
# players agent represents	1776	19.47	14.11	1.00	67.00
Agent experience in years	1776	9.98	6.40	0.00	24.00
State income tax rate	1776	4.79	3.19	0.00	10.30
State per capita income ($000s)	1776	34.91	4.96	25.28	50.26
State unemployment rate	1776	5.20	0.89	3.00	7.40
UAAA (0 or 1)	1776	0.41	0.49	0.00	1.00

Table 2.2 OLS Regression Results

VARIABLES	(1) ln(TotalValue)	(2) ln(AveragePay)
Holdout	0.0175	0.0185*
	(0.0119)	(0.0107)
Division I-A School	-0.000227	0.000644
	(0.00656)	(0.00703)
Selection Number	-0.0131***	-0.0137***
	(0.000581)	(0.000554)
Selection Number^2	3.34e-05***	3.56e-05***
	(1.81e-06)	(1.76e-06)
Agent - Players represented	0.000126	0.000138
	(0.000149)	(0.000176)
Agent - Years certified	-3.75e-05	6.71e-05
	(0.000482)	(0.000470)
Team - Head coach tenure	-0.000440	-0.000630
	(0.00136)	(0.00113)
Team - Ratio of empty seats	0.0364	0.0378
	(0.0471)	(0.0388)
Team - Win/loss record	-0.00275*	-0.00310**
	(0.00145)	(0.00130)
State - Per capita income	5.35e-06	7.11e-06*
	(4.67e-06)	(3.84e-06)
State - UAAA	-0.00944	-0.00466
	(0.0116)	(0.0105)
State - Unemployment Rate	0.00117	0.00562
	(0.00811)	(0.00640)
Constant	6.862***	6.454***
	(0.250)	(0.190)
Observations	1,776	1,776
R-squared	0.971	0.939

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the state level in parentheses. Team and year fixed effects are also included in the model, but not shown. Controls for player position are also included and not shown, but have the expected pattern where coefficients for all positions are negative, relative to quarterbacks.

Table 2.3 First-Stage Regression Results

VARIABLES	(1) Probit Holdout (0 or 1)	(2) Tobit Holdout (censored)
State Tax Rate	0.292***	3.387***
	(0.0758)	(0.828)
Division I-A School	0.0486	1.013
	(0.136)	(1.428)
Selection Number	-0.0213***	-0.201***
	(0.00323)	(0.0383)
Selection Number^2	5.51e-05***	0.000521***
	(1.07e-05)	(0.000120)
Agent - Players represented	0.00458	0.0292
	(0.00304)	(0.0367)
Agent - Years certified	0.00590	0.125
	(0.00753)	(0.0902)
Team - Head coach tenure	0.0499	0.426
	(0.0489)	(0.484)
Team - Ratio of empty seats	0.668	2.288
	(1.405)	(12.33)
Team - Win/loss record	0.0377	0.186
	(0.0247)	(0.253)
State - Per capita income	-9.99e-05	-0.00109
	(0.000100)	(0.00103)
State - UAAA	0.366	4.494*
	(0.254)	(2.487)
State - Unemployment Rate	0.0204	0.125
	(0.208)	(1.904)
Constant	1.956	10.84
	(4.940)	(48.39)
Observations	1,776	1,776

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the state level in parentheses. Team and year fixed effects are also included in the model, but not shown. Controls for player position are also included and not shown, but have the expected pattern where coefficients for all positions are negative, relative to quarterbacks.

Table 2.4 Maximum Likelihood Binary Endogenous Variable Results

VARIABLES	(1) Outcome ln(TotalValue)	(2) First Stage Holdout	(3) Outcome ln(AveragePay)	(4) First Stage Holdout
Holdout	0.0445***		0.0490***	
	(0.0158)		(0.0146)	
State Tax Rate		0.292***		0.292***
		(0.0758)		(0.0758)
		[0.059]		[0.059]
Division I-A School	-0.000564	0.0486	0.000263	0.0486
	(0.00671)	(0.136)	(0.00725)	(0.136)
Selection Number	-0.0129***	-0.0213***	-0.0135***	-0.0213***
	(0.000550)	(0.00323)	(0.000539)	(0.00323)
Selection Number^2	3.30e-05***	5.51e-05***	3.51e-05***	5.51e-05***
	(1.72e-06)	(1.07e-05)	(1.71e-06)	(1.07e-05)
Agent - Players rep.	9.76e-05	0.00458	0.000105	0.00458
	(0.000147)	(0.00304)	(0.000169)	(0.00304)
Agent - Years certified	-6.13e-05	0.00590	4.03e-05	0.00590
	(0.000469)	(0.00753)	(0.000446)	(0.00753)
Team - Coach tenure	-0.000623	0.0499	-0.000837	0.0499
	(0.00147)	(0.0489)	(0.00122)	(0.0489)
Team - Ratio empty seats	0.0346	0.668	0.0358	0.668
	(0.0461)	(1.405)	(0.0417)	(1.405)
Team - Win/loss record	-0.00289**	0.0377	-0.00326**	0.0377
	(0.00142)	(0.0247)	(0.00129)	(0.0247)
State - Per capita income	5.67e-06	-9.99e-05	7.48e-06*	-9.99e-05
	(4.82e-06)	(0.000100)	(4.12e-06)	(0.000100)
State - UAAA	-0.0106	0.366	-0.00594	0.366
	(0.0118)	(0.254)	(0.0110)	(0.254)
State - Unemp. rate	0.00109	0.0204	0.00553	0.0204
	(0.00812)	(0.208)	(0.00657)	(0.208)
Constant	6.876***	1.956	6.436***	1.956
	(0.254)	(4.940)	(0.203)	(4.940)
Observations	1,776	1,776	1,776	1,776

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the state level in parentheses. Relevant marginal effects are included in [brackets]. Team and year fixed effects are also included in the model, but not shown. Controls for player position are also included and not shown, but have the expected pattern where coefficients for all positions are negative, relative to quarterbacks.

Table 2.5 Maximum Likelihood Censored Endogenous Variable Results

VARIABLES	(1) Outcome ln(TotalValue)	(2) First Stage Holdout	(3) Outcome ln(AveragePay)	(4) First Stage Holdout
Holdout Duration	0.00375*		0.00455**	
	(0.00222)		(0.00197)	
State Tax Rate		3.387***		3.387***
		(0.828)		(0.828)
Division I-A School	-0.000661	1.013	7.03e-05	1.013
	(0.00620)	(1.428)	(0.00659)	(1.428)
Selection Number	-0.0130***	-0.201***	-0.0136***	-0.201***
	(0.000540)	(0.0383)	(0.000473)	(0.0383)
Selection Number^2	3.32e-05***	0.000521***	3.53e-05***	0.000521***
	(1.66e-06)	(0.000120)	(1.51e-06)	(0.000120)
Agent - Players rep.	0.000146	0.0292	0.000158	0.0292
	(0.000140)	(0.0367)	(0.000160)	(0.0367)
Agent - Years certified	-0.000138	0.125	-5.78e-05	0.125
	(0.000425)	(0.0902)	(0.000423)	(0.0902)
Team - Coach tenure	-0.000398	0.426	-0.000597	0.426
	(0.00141)	(0.484)	(0.00123)	(0.484)
Team - Ratio empty seats	0.0425	2.288	0.0449	2.288
	(0.0431)	(12.33)	(0.0358)	(12.33)
Team - Win/loss record	-0.00241*	0.186	-0.00269**	0.186
	(0.00136)	(0.253)	(0.00119)	(0.253)
State - Per capita income	5.37e-06	-0.00109	7.17e-06*	-0.00109
	(4.78e-06)	(0.00103)	(3.98e-06)	(0.00103)
State - UAAA	-0.0108	4.494*	-0.00641	4.494*
	(0.0119)	(2.487)	(0.0108)	(2.487)
State - Unemp. rate	0.00137	0.125	0.00585	0.125
	(0.00851)	(1.904)	(0.00691)	(1.904)
Constant	6.906***	10.84	6.467***	10.84
	(0.254)	(48.39)	(0.196)	(48.39)
Observations	1,776	1,776	1,776	1,776

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the state level in parentheses. Team and year fixed effects are also included in the model, but not shown. Controls for player position are also included and not shown, but have the expected pattern where coefficients for all positions are negative, relative to quarterbacks.

Table 2.6 Simulation Results - Binary Holdout

Round	Average Contract Value	Max. Change in Tax Liability from 1% Increase in Tax Rate	Estimated Marginal Effect from Holding Out	Change in Expected Value of Holdout
–	$2,746,000	$27,460	$119,000	$7,140
1	$10,140,000	$101,400	$434,000	$26,040
2	$3,200,000	$32,000	$142,000	$8,520
3	$1,870,000	$18,700	$83,000	$4,980
4	$1,600,000	$16,000	$71,000	$4,260
5	$1,350,000	$13,500	$59,000	$3,540
6	$1,250,000	$12,500	$55,000	$3,300
7	$1,170,000	$11,700	$51,000	$3,060

Table 2.7 Robustness Check Using 1986-91 Contract Data

VARIABLES	(1) Probit Holdout (0 or 1)	(2) Tobit Holdout (censored)
State Tax Rate	-0.0293	-0.635
	(0.0230)	(0.625)
Division I-A School	0.0131	1.649
	(0.0300)	(1.024)
Selection Number	-0.00272***	-0.210***
	(0.000450)	(0.0170)
Selection Number^2	3.46e-06***	0.000439***
	(1.19e-06)	(4.41e-05)
Agent - Years Certified	0.0124**	0.432*
	(0.00503)	(0.252)
Team - Head coach tenure	0.00697	0.0749
	(0.00475)	(0.112)
Team - Ratio of empty seats	-5.38e-07*	-3.13e-06
	(3.03e-07)	(1.14e-05)
Team - Win/loss record	0.00925	0.0254
	(0.00857)	(0.250)
State - Per capita income	-1.12e-06	0.000607
	(4.31e-05)	(0.00122)
State - Unemployment rate	0.0351**	1.294**
	(0.0132)	(0.511)
Constant	1.167	-0.185
	(0.923)	(0.962)
Observations	1,872	1,872

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the state level in parentheses. Team and year fixed effects are also included in the model, but not shown. Controls for player position are also included and not shown.

Chapter 3

Labor Market Impacts of Sports: Evaluating the Effect of Lower Tier Professional Sports Arenas on Local Communities

3.1 Introduction

The "benefits" of sports stadiums and arenas to local economies have been touted for years as public funds are sought to subsidize the ever-growing cost of constructing these facilities. We often see owners of sports franchises lobbying for state and/or local funding to build new arenas as their respective teams generate millions of dollars in profit. We also see these same state and local governments giving in to what sometimes appear to be outrageous demands under threat that teams will move on to a new city if the funding is not provided. In an effort to avoid public criticism, government leaders sometimes argee to provide exorbitant funding which can cost the public dearly in the long-run.

Most of the publicity in this area is generated by teams participating in one of the four major professional sports leagues in the U.S. Teams operating in Major League Baseball

(MLB), the National Football League (NFL), the National Basketball Association (NBA), and the National Hockey League (NHL) often face pressure to continue playing in the newest, most advanced, fan-friendly venues in order to continue to draw fans to games amongst increasing competition for entertainment dollars. In order to assuage taxpayer and legislator concerns teams have taken the step of commissioning economic impact studies in order to prove the value of a new stadium, either in their current city or a potential re-location city, and justify the cost.[1] Seeing as though these studies are often requested and paid for by the very teams who stand to benefit from public stadium finanacing, they should be taken with a grain of salt.

Due to this inherent conflict of interest, when looking at previous literature on the subject it is important to distinguish between "the promotional literature versus the economists" as Coates and Humphreys (2008) call it. Within the promotional literature, work done largely by consulting firms, it is argued that public subsidies are warranted because of the local economic development benefits of building a stadium or arena. The economic development benefits which are typically of most interest in these studies are income and job creation, along with increased tax revenue (Coates and Humphreys, 2008). In this paper we focus on the previous *economic* literature, which has largely attempted to verify the claims made by the promotional literature with little success.

While most of the publicity and attention has centered around the financing and economic benefits of stadiums and arenas designed for major professional sports teams, there are substantial amounts of money (both public and private) being used to construct arenas for

[1] A recent example is an economic impact analysis jointly commissioned by the NFL's San Diego Chargers and Oakland Raiders to evaluate a "Los Angeles" stadium in Carson, CA (Cooper et al., 2015)

lower tier professional teams.[2] In this paper we study whether or not these smaller, less well-known arenas are able to generate any localized economic benefits and more specifically, labor market benefits. While the size, impact, and cost of the arenas to be studied in this paper differ substantially from their more popular counterparts, it should still be of vital interest as to whether or not they provide any proven economic benefits for the local communities which commit to building them. We also expand the analysis to the more common major arenas in order to see if they produce different results, and to see if our findings differ from the previous literature.

3.2 Background

A wealth of academic literature exists related to the economic impacts of different types of sports facilities. The research is generally of two types: case study analysis which focuses on a single stadium or arena project, or broader analysis looking for economic impacts generated by sports facilities of a certain type, usually related to the NFL, NBA, MLB, or NHL. For our purposes we will focus on the broader analysis, and not single arena case studies. In a survey of the literature, Coates and Humphreys (2008) find consistency in that professional sports franchises and facilities generally have had no measurable economic impact on local/regional economies. The studies reviewed range in data, estimators and model specifications used, as well as using different cities or geographical areas.[3]

[2]Throughout the remainder of this paper, the term "arena" is used to describe an indoor sports facility often used to host basketball and hockey games and other events.

[3]Some of the relevant work surveyed by Coates and Humphreys (2008) is discussed below. For additional literature on the economic impact of sports, see Baade and Dye (1998); Hudson (1999); Lertwachara and Cochran (2007)

Baade and Sanderson (1997) estimated employment created by sports facilities using data for ten cities in the years 1958 through 1993. They found very little effect on employment shares from new facility construction. In general, they could find no overall link between professional sports teams and job creation. Coates and Humphreys (1999) followed by studying the impact of professional sports on per capita income. Included in their data are all 37 metropolitan statistical areas (MSAs) that had an NFL, MLB, or NBA team from 1967-1994. They concluded that professional sports teams had no positive effect on per capita income within a metropolitan statistical area.

In later work, Coates and Humphreys (2003) used the same methodology to analyze wages and employment in two specific sectors of the economy: service and retail. They found evidence of stadiums and arenas having positive effects on earnings in one sector, but being counterbalanced by negative effects on earnings and employment in other sectors. Similarly, Gius and Johnson (2001) examined the impact of professional sports teams on per capita income in metropolitan areas. They included data from cities with populations greater than 25,000 from the 1988 and 1994 City and County Data Books. Again, they were unable to find any significant impact on wages.

Siegfried and Zimbalist (2000) reviewed some of the relevant academic and promotional literature and found the same stark contrast in results. They offered three reasons why professional sports teams may not generate positive economic impacts. First, consumers largely have an inflexible leisure budget, so any additional money that is spent on sporting events is simply shifted away from another form of leisure. Second, much of the increased income generated from a professional sports team is likely to leak out of the local economy due to taxation, savings, and out-of-town residents. Finally, new sports facilities typically have a negative budgetary impact on local governments, since financing is often provided with little to show for it in terms of increased tax collections.

Jasina and Rotthoff (2008) broke the geographical mold by estimating potential impacts that professional sports franchises have on county employment and wages. They found mixed results on employment in the clothing, drinking, food, hotel, and liquor industries when a sports franchise is present, and mostly negative effects on payroll.

Recently, Coates (2015) revisited the previous work by Coates and Humphreys (1999) and incorporated an additional 17 years of data along with a number of new stadiums, arenas, and franchises. Using data on all metropolitan statistical areas from 1969-2011, he finds little change from the original study in which the presence of teams and arenas frequently indicate harmful effects on per capita income, wages, and wages per job.

A consistent theme throughout the literature is a focus on sports facilities associated with top-level professional sports franchises and estimating different economic impacts at relatively broad geographic levels, such as metropolitan statistical areas along with one additional study at the county level.[4] In contrast, this paper focuses on the growing set of arenas that are utilized for events and athletics below the top-tier professional level. In a 2001 article published by the Minneapolis Federal Reserve it is noted that, "Given the relatively small to nonexistent benefits from team sports at higher levels, one should not expect anything

[4]There has been some case study-type analysis done for individual facilities which host lower-tier professional sports teams. For example, Hodur et al. (2006) studied the economic impact of the FARGODOME in Fargo, ND.

or very, very little from minor league activities" (Wirtz, 2001). In the following sections we analyze whether or not that preconceived notion is indeed accurate.

3.3 Data Availability & Arena Sample

In this paper, we advance the current research in two ways: by analyzing the potential economic impacts of arenas which have not previously been studied, and by narrowing the geographic regions being analyzed. We create a dataset of 24 sports arenas, built since the mid-1990's, that *do not* host any teams which play in the NFL, NBA, MLB, or NHL. To develop this list we focus largely on teams which participate in the American Hockey League (AHL) and the ECHL (formerly the East Coast Hockey League). While these are still professional sports leagues, they operate below the top-level sports leagues, and in this case function as developmental leagues for the NHL. Also included in the dataset are arenas which function as a home for developmental league (D-League) NBA teams, or for arena football teams. Some arenas in the dataset are the primary host for teams in more than one of these leagues, and there is also a fair amount of movement of teams coming and going from a given arena. For this reason, we focus on estimating the economic impact from the arena itself, detached from whichever particular team is playing there in a given year.

There are notable differences between the arenas and locations identified in our sample versus that of the previous literature. As shown in Table 3.1, the average population for the cities in our sample is about 165,000 and the average arena capacity is around 10,500. By contrast, the majority of cities which host a professional sports franchise in the NFL, NBA, MLB, or NHL have populations in excess of one million people. While the seating capacity of stadiums and arenas vary wildly by sport, as a comparison, arenas that host an NHL team have capacities ranging from 15,000 to 21,000, with an average capacity just over 18,000.[5] Not surprisingly, on average, top-tier professional sports teams are located in significantly larger cities and play in much larger stadiums and arenas. Due to these differences, it is important to evaluate these arenas on a different scale. While it may make sense in the previous literature to estimate labor market effects at the MSA level, it seems unlikely that smaller arenas, which host lower level teams would be able to move the needle on that scale.

In order to focus our attention on more localized economic impacts we use ZIP Code Business Patterns (ZBP) data published by the Census Bureau. The data are published on an annual basis and provide employment, income, and business establishment counts by ZIP code. The data are available from 1998 through 2013, and for this reason we limit our analysis to arenas built during this time period. These data allow us to analyze potential economic impacts at a more precise geographic level than the previous literature which used metropolitan statistical areas or, in one case, counties.

The previous work of Jasina and Rotthoff (2008) moved in the direction of estimating more localized labor market effects by shifting their analysis from the MSA level to the county level. While the county level may seem like a fairly narrow geographic scope, the numbers tell a different story. Many of the largest cities in the United States, where professional sports franchises are often located, are found in counties with populations in excess of one million.[6] To give a broad comparison of size between MSAs, counties, and ZIP codes, consider that

[5] A listing of the 30 current NHL arenas along with their capacities can be found at: http://statshockey.homestead.com/info/nhlarenas.html

[6] For example, based on 2010 populations, Los Angeles County has nearly 10 million residents. In addition, Cook County (Chicago), San Diego County, Miami-Dade County, and Dallas County all have populations in excess of 2 million.

the average MSA has a population just over 700,000. Counties are significantly smaller, but across the more than 3,000 counties in the U.S. the average population exceeds 100,000. ZIP codes, however, are established at a much finer geographic level. There are in excess of 40,000 ZIP codes in the U.S. and the average population is under 10,000 residents. It is for these reasons that we choose to focus our analysis on potential labor market effects at the ZIP code level.

Figures 3.1 & 3.2 provide a visual representation of the difference in scope between county-level and ZIP-code-level analysis. The example shown is the Intrust Bank Arena located in Wichita, Kansas. Figure 3.1 shows the location of the arena in ZIP code 67202, surrounded by four neighboring ZIP codes. Figure 3.2 shows an expanded map of Sedgwick County, Kansas, of which ZIP code 67202 is a part. As seen in the figure, Sedgwick County contains all (or part) of more than 20 ZIP codes.

Table 3.2 provides summary statistics regarding employment and wages for the 24 ZIP codes included in the sample, which correspond to the arenas listed in Table 3.1. Each ZIP code has 16 years of annual data, from 1998-2013. The averages across all ZIP codes for each labor market measure are just under 15,000 employees, about $604 million in annual payroll, nearly $38,000 in annual pay per employee, and around 820 establishments.

3.4 Empirical Models

In order to estimate the potential labor market impacts of a new arena, we need to identify an appropriate control method for identification. Previous literature provides some basis for estimating these effects, and we explore several different methods of analysis in the sections that follow.

3.4.1 Pre/Post Analysis

The simplest solution is a pre/post analysis where we include in the sample only the ZIP codes in which a stadium was constructed. The dynamic panel model is expressed as:

$$y_{it} = \alpha Arena_{it} + \gamma y_{it-1} + \eta_i + \tau_t + \epsilon_{it} \tag{3.1}$$

where y_{it} is a given labor market measure (employment, total annual payroll, average pay per employee, or number of establishments), $Arena_{it}$ is a dummy variable denoting whether or not an arena exists in a ZIP code in a given year, y_{it-1} is the lagged labor market measure, η_i is a ZIP code fixed effect, and τ_t is a time fixed effect. In this model, the estimated effect of an arena is based on comparing the employment (or wages) in a given ZIP code prior to and after the construction of an arena. We also present results from a static version of the model which drops the lagged labor market measure as an independent variable.

The estimation procedure depends on whether we are using the dynamic or static version of the model. The static model is estimated as a linear model with ZIP code and time fixed effects. The dynamic model cannot be estimated in the same way due to the presence of dynamic panel bias from the inclusion of the lagged dependent variable (Nickell, 1981). The issue of achieving consistent estimates in dynamic panel models has been frequently discussed in the literature.[7] In this paper, we use a system generalized method of moments (GMM) estimator which allows for all available further lags of the dependent variables to serve as instruments for the dynamic term in the model. Roodman (2009) describes the system GMM procedure in great detail and provides insight into the empirical application.

Table 3.3 shows the results of this analysis from both models. The coefficients of interest are those on the dummy variable for whether or not an arena was constructed. There is a clear tradeoff in the use of the two models presented. The static version of the model requires fewer assumptions about the data, but the data being used lack sufficient control variables so we are left to rely on ZIP code and time fixed effects. We find positive coefficients on the arena variable for each labor market measure and a significant result when looking at the effect of arena construction on the total annual payroll in the ZIP code. The obvious concern is that the positive results are due to unobserved heterogeneity that we are unable to control for given the current data.

The dynamic version of the model allows us to better control for current period labor market conditions by using a lagged dependent variable. As discussed previously, this creates additional issues in trying to produce consistent estimates. The system GMM model being used is designed in a way that a large number of instrumental variables are included, and in such a small sample size there is a concern that the results shown could be an artifact of the model design. In the dynamic model, we see negative, significant effects on employment and annual payrolls as a result of an arena being constructed. If this result were to hold it would be a departure from the expectation that sports arenas have positive labor market impacts.

[7] The estimation technique used in this paper is grounded in the work of Anderson and Hsiao (1982), Holtz-Eakin et al. (1988), and Arellano and Bond (1991).

The main issue with the use of the pre/post analysis is that we are relying on each individual ZIP code, along with the other ZIP codes which have had arenas built, to act as controls. The problem arises because we have no way of observing the counterfactual outcome of what would have happened to employment and wages in a given ZIP code had an arena never been constructed. There is also no reason to believe that two ZIP codes have similar labor market dynamics just because they each have had an arena constructed during the period of study. In order to provide a better control group for identification it is necessary to find untreated ZIP codes, or ones which have never had an arena constructed, which are similar to the ZIP codes in our sample. A secondary issue with the model in this form is that we assume time period fixed effects are constant across all ZIP codes included in the sample, which rules out the possibility of heterogeneous time trends. The next section of this paper identifies an alternate method of identification and control for the model.

3.4.2 Contiguous ZIP Code Group Analysis

The second empirical strategy borrows from Dube et al. (2010) who estimated the labor market impact of minimum wages by using contiguous county pairs as a better way to control for spatial heterogeneity. In that paper, adjacent counties from neighboring states were paired based on the fact that the two states had different minimum wage levels. By pairing the contiguous counties, each pair can be used to control for heterogeneity under the assumption that neighboring counties would be affected by the same exogeneous shocks.

In order to apply a similar procedure in this paper we not only identify the ZIP code in which an arena is located, but we also identify the surrounding ZIP codes (ones which border the area of interest). This allows for the estimation of any differing employment or wage effects between the home ZIP code and surrounding areas. If there are any positive effects found in the local ZIP code, this approach should allow us to identify whether those effects are the result of a simple shift of employment from outer areas into the home area, or if there is actual job creation happening as a result of the new arena.

Table 3.4 lists the same 24 arenas used in the previous sample, and includes the ZIP code for the arena itself, as well as any contiguous, border ZIP codes.[8] Each arena (ZIP code) in the sample has between two and seven surrounding ZIP codes matched to it.

Table 3.5 provides summary statistics regarding employment and wages for the 24 ZIP codes where an arena was constructed and the 112 surrounding ZIP codes included in the sample. Each ZIP code has 16 years of annual data, from 1998-2013. The average level of employment across the sample is just under 11,000, and the average annual pay per employee is around $34,600. While different, this sample is similar to the original arena-only sample in terms of labor market characteristics.

In this preferred specification, we deviate from Dube et al. (2010) in that, instead of individually pairing the arena ZIP code with each neighboring ZIP code, we form ZIP code groups. Since each ZIP code in our sample belongs to a single grouping of ZIP codes (a home ZIP code and the neighboring ZIP codes), it makes sense to allow the time fixed effects in the model to vary by these groups. Since the ZIP code is such a narrow geographic area, any exogenous shock which were to affect the labor market in a given pairing of ZIP codes could

[8]The ZIP code in which an arena is located can be identified by the mailing address. The surrounding ZIP codes are identified using a ZIP code mapping tool, such as http://www.usnaviguide.com. A handful of neighboring ZIP codes were excluded from the analysis due to a lack of data availability in the ZBP Census data.

reasonably be expected to affect the other surrounding ZIP codes in the group. We can write the new model specification as:

$$y_{it} = \alpha Arena_{it} + \gamma y_{it-1} + \eta_i + \tau_{gt} + \epsilon_{igt} \tag{3.2}$$

where we allow for group-specific time effects (τ_{gt}), which uses only the variation within each grouping of ZIP codes. Similar to the previous section, we also estimate a static version of the model where the lagged dependent variable is excluded from the right-hand-side variables.

Table 3.6 shows the results of the analysis for both versions of the model. Since we are allowing for differing time trends at the group level, it is logical to estimate standard errors clustered at the group level as well. We find a similar pattern to the previous results. When looking at the static model, there are positive coefficients on the arena dummy variable for all labor market factors. Additionally, the positive effect on annual payroll is statistically significant at the 5% level, and the impact on average pay is significant at the 10% level. However, as with the previous static model there is a concern for uncontrolled heterogeneity since we are limited by the data to only ZIP-code and group-specific time fixed effects. The results from the dynamic model again show largely negative coefficients, implying a decrease in labor market factors as the result of a new arena being constructed. The only statistically significant result is a decrease of nearly 10 establishments, which in context represents a relatively small average decrease of about 1.5% of total establishments in a given ZIP code as the result of a new arena opening.

There is an additional concern when analyzing the results from the model as it is currently specified. By using a binary variable to represent the presence of an arena, we are estimating the average labor market impact over the entire length of the sample. This could work to lessen any immediate measurable impact that the arena might have by spreading the effect out over 5, 10 or even 15 years depending on when the arena was constructed within the sample period. The solution to this issue is to estimate separate effects for each year after an arena was constructed, and to limit the window in which we expect to see an impact.[9] In the following analysis we create five separate dummy variables representing one year after arena opening, two years after arena opening, and so on. We limit the window to five years because it seems likely that any expected labor market impact should be observable within five years after the arena opens, and it limits the exposure to unobserved factors affecting the labor market as we increase the timeframe.

Table 3.7 presents the results of the analysis using the preferred dynamic model specification.[10] A cursory look at the results shows that none of the new arena variables generates statistically significant results. One interesting pattern that emerges in the estimated coefficients is that they are largely negative in the first and second years after an arena opens, but in the third year after opening the coefficients turn positive for all four labor market measures. It may be possible that the arena construction disrupts the surrounding labor market and causes a negative impact initially, only to have the labor market recover and improve later on. However, given the lack of significant and convincing results, it is purely speculation.

[9]The concept of estimating variable treatment effects over time is not a novel one. Chandra et al. (2010) provide one example in which a similar procedure is used.

[10]An alternative way to estimate a similar variable treatment effect is to change the observation window in the data. For example, we could cut the data to only include five years of sample after each arena was constructed. Using this approach does not materially change the results shown in this section.

3.5 Comparison with Top-Tier Professional Arenas

As discussed previously, most of the empirical analysis on this subject has focused on the economic impact of stadiums and arenas which host top-tier professional sports teams. In order to more fully understand the results presented in this paper and how they compare to the previous literature, it seems worthwhile to evaluate the effects of those arenas as well. To facilitate the comparison, we consider the same time period and use the same ZIP code data used thus far. Since the original analysis in this paper focused on arenas which primarily hosted lower tier professional hockey and basketball teams, we extend the analysis here to examine arenas which host NHL and NBA teams.

Table 3.8 provides a listing of arenas built during the period 1998-2013, and which primarily hosted a team that plays in the NHL, NBA, or both. There are 20 arenas included in this sample, all of which were constructed between 1999-2012. In comparison with the previous sample, these arenas are much larger in terms of seating capacity with an average of just over 18,000 seats compared to about 10,500 in the lower-tier arenas. There is also a significant difference in terms of the cost to build the different types of arenas. In our previous sample, the average construction cost was around $72 million. The sample of arenas included here range in cost from $220-550 million.[11] Does that extra cost and increased capacity lead to a different result in terms of economic or labor market impact? To conduct this analysis we use the preferred specification (from Section 3.4.2) where surrounding ZIP codes are identified and used as controls for the home ZIP code in which the arena is located.

[11] Costs associated with the construction of these more recent NHL and NBA arenas can be found at: http://www.macleans.ca/authors/amanda-shendruk/the-20-most-expensive-nhl-arenas.

Table 3.9 provides summary statistics regarding the labor market conditions of the home and surrounding ZIP codes for this new sample. In comparison with the previous sample, the ZIP codes used here are larger, on average, with more establishments, higher levels of employment and wages, and higher average pay per employee. This is no surprise since NHL and NBA teams tend to locate in larger cities with higher population densities compared to their lower tier league counterparts. For completeness, Table 3.10 shows the breakdown of the ZIP codes being used in this section.

Table 3.11 reports the results from the estimation of labor market effects due to the construction of an NBA or NHL arena. Similar to the results in previous sections, we estimate both a dynamic and a static version of the model. As before, it is likely that the static model is overstating any possible effects due to inadequate control variables in the data, and similar to previous results we see some positive labor market impacts from that model. Both annual payroll and average annual pay show positive, significant increases as a result of an arena being constructed. The only negative coefficient is on the number of establishments, but that impact is very imprecisely estimated. The dynamic model pulls back on some of those positive results, but still shows agreement with a statistically significant increase in average annual pay as the result of a new NHL or NBA arena.

Once again there is concern that the results are influenced due to the specification used where the dummy variable for an arena being present is set to one for every year after construction. This could cause an immediate labor market impact to be washed out when averaging over a long time period, or it could cause a spurious significant impact to show in the data due to a long-term change in labor market conditions. In either case, we follow the same approach used in Section 3.4.2 to estimate the variable effect of an arena by year.

Table 3.12 shows the results of the analysis. We find a fairly significant deviation from the previous model. This may indicate that some other long-term factors may have been influencing the previous estimates but do not impact the results in this shorter five-year window. We see a significant increase in the number of establishments in year one following the arena opening, and the positive coefficients continue in subsequent years. There is a similar pattern in employment, where a decrease in the first year is followed by three years of positive coefficients. Interestingly, in contrast to the original dynamic model, the results show nearly all negative results for annual payrolls and, in turn, average annual pay. As with many of the results presented in this paper, since we lack consistent, significant results we can only speculate about whether the results may be meaningful.

3.6 Discussion

The results found in Section 3.5 are in line with the previous literature in that there appears to be little, if any, significant labor market impact as a result of the addition of a stadium/arena in a given area. The approach used in this paper is similar to Coates and Humphreys (1999) and Coates (2015), but the models are estimated at different geographic levels. The previous work studied labor market impacts at the MSA level and found little effect. In this paper, we hypothesized that the lack of significant results could have been a product of the large geographic scope. However, after estimating similar effects at the much narrower ZIP code level, we still fail to find much in the way of convincing, positive results.

The lack of positive results in the previous section seem to further foreshadow our initial insignificant results when analyzing lower tier teams and arenas. It certainly seems as though the prediction of nonexistent benefits from these arenas in Wirtz (2001) is more a reality than a preconceived notion, as we initially thought. The only positive outlook given the lack of results found here would be that we have estimated positive coefficients in our models, just with a lack of significance. It is possible that the effects could be more precisely estimated given a larger sample of arenas and more years of data.

3.7 Conclusion

The question of whether or not sports stadiums and arenas generate economic benefits for local communities is certainly not a new one. For nearly 30 years, economists have been analyzing sports facilities and attempting to measure what, if any, economic impact they generate. Even with what seems like mounting evidence against any tangible, positive benefits, the question continues to be posed time and time again. So, why is it that cities and municipalities continue to support the construction of new stadiums and arenas with the use of public funding? The answer may simply be that it was never really about the numbers. A 2001 piece by Adam Zaretsky makes the observation that "the reasons...include many intangible variables, such as civic pride and political self-interest. Moreover, cities generally justify these decisions–and convince taxpayers of their virtue–with analyses that many economists consider suspect because the studies generally overlook some basic economic realities" (Zaretsky, 2001).

The reality of our current sports environment in which fans are more passionate, diehard, and loyal than ever is that no politician or decision maker wants to be seen as the reason for a city's favorite sports team packing up and leaving town in the middle of the night. As much as citizens complain about the abuse of taxpayer funding used to construct new sports facilities, it has not been enough to shift the balance of power away from the sports teams who demand public funds. So, while most economic evidence to date, including this paper, fails to find any meaningful impact that sports facilities have on local labor markets, it remains unclear as to whether that will have any effect moving forward. It is likely that stadiums and arenas will continue to be touted as vehicles of urban revitalization and economic growth by politicians and supporters, while civic pride and the love of sports will prevent the economic realities of the situation from having any impact.

Fig. 3.1 Intrust Bank Arena - Wichita, KS (67202)

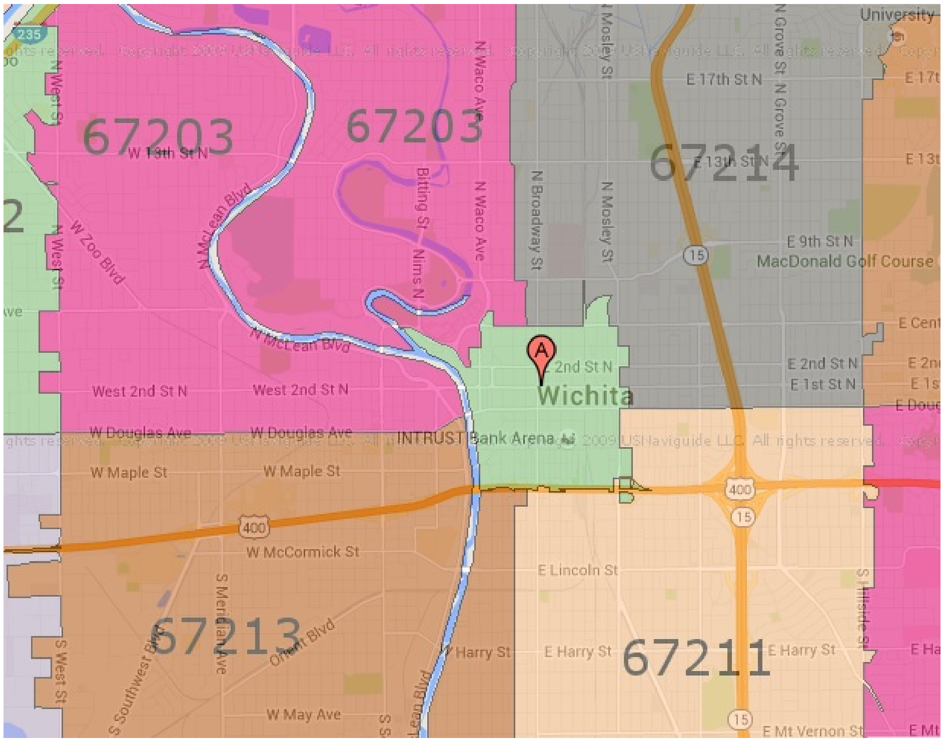

Fig. 3.2 Intrust Bank Arena - Wichita, KS (Sedgwick County)

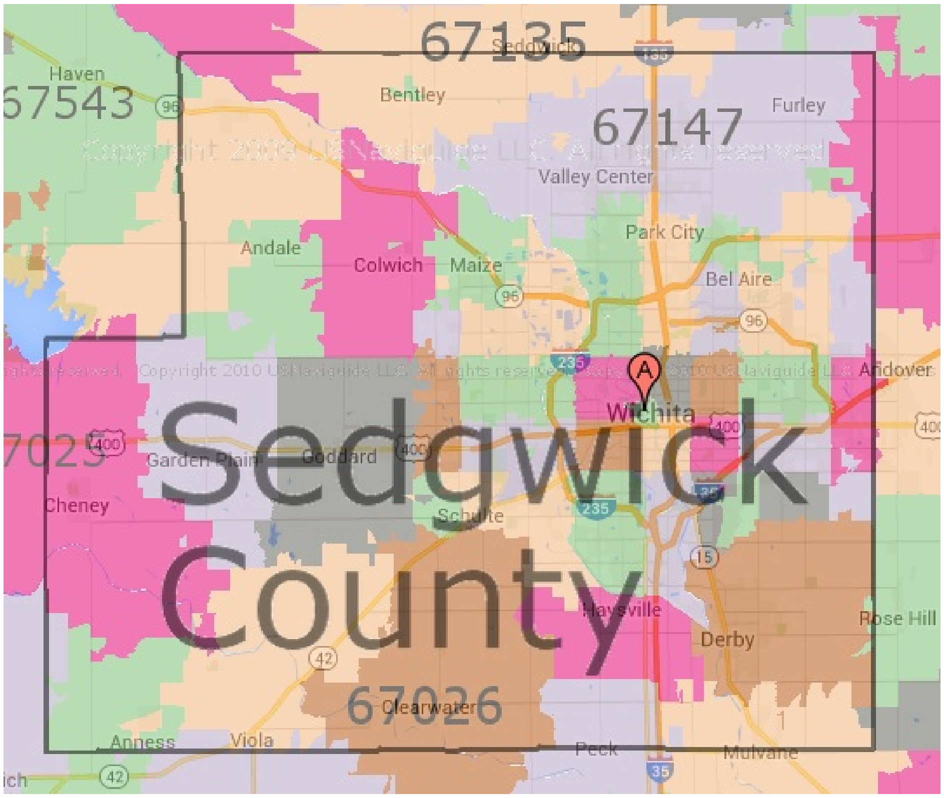

Table 3.1 Sample of Arenas Built from 1998-2013

Location	Arena	Opened	Population	Capacity	Cost (mil)	Primary Use
Evansville, IN	Ford Center	2011	117,429	11,000	127.5	ECHL
Wichita, KS	Intrust Bank Arena	2010	382,368	15,750	205.5	ECHL
Cedar Park, TX	Cedar Park Center	2009	61,238	8,700	55.0	AHL, D-League
Toledo, OH	Huntington Center	2009	287,208	9,341	105.0	ECHL
Allen, TX	Allen Event Center	2009	84,246	8,600	52.6	ECHL
Independence, MO	Independence Events Center	2009	116,830	7,000	60.0	ECHL
Tulsa, OK	BOK Center	2008	391,906	19,199	196.0	ECHL, WNBA
Des Moines, IA	Wells Fargo Arena	2005	203,433	16,980	117.0	AHL, D-League
Reno, NV	Reno Events Center	2005	233,294	7,000		D-League
Duluth, GA	The Arena at Gwinnett Center	2003	26,600	13,100	91.5	ECHL, AFL
Loveland, CO	Budweiser Events Center	2003	66,859	7,200	28.0	ECHL
Jacksonville, FL	Jacksonville Veterans Mem. Arena	2003	821,784	15,000	130.0	AFL
Frisco, TX	Dr Pepper Arena	2003	145,035	7,000	27.0	D-League
Hidalgo, TX	State Farm Arena	2003	11,198	6,800	23.0	D-League
Hershey, PA	Giant Center	2002	14,257	12,500	65.0	AHL
Bridgeport, CT	Webster Bank Arena	2001	144,229	10,000	56.3	AHL
Manchester, NH	Verizon Wireless Arena	2001	109,565	11,770	68.0	AHL, ECHL
Reading, PA	Santander Arena	2001	89,893	9,000	42.5	ECHL
Elmira, NY	First Arena	2000	29,200	3,784	16.0	ECHL
Wilkes-Barre, PA	Mohegan Sun Arena at Casey Plaza	1999	41,200	10,000	44.0	AHL
Lowell, MA	Tsongas Center at Umass Lowell	1998	108,522	7,800	24.0	AHL
Estero, FL	Germain Arena	1998	18,176	8,284	22.0	ECHL
Greenville, SC	Bon Secours Wellness Arena	1998	101,214	15,951	63.0	ECHL
Bakersfield, CA	Rabobank Arena	1998	363,630	10,400	38.0	ECHL, AHL
Average			165,388.1	10,506.6	72.0	

Table 3.2 Summary Statistics for Arena Sample

Variable	Obs	Mean	Std. Dev.	Min	Max
Establishments	384	823.77	398.62	118.00	2214.00
Employment	384	14933.60	8242.71	819.00	41508.00
Annual Payroll ($000s)	384	604216.80	443239.80	18928.00	2023152.00
Average Annual Pay ($000s)	384	37.70	12.76	15.14	102.27
Arena (=1 if yes)	384	0.59	0.49	0.00	1.00

Table 3.3 Pre/Post Analysis Using Arena Sample

VARIABLES	(1) Employment	(2) Annual Payroll	(3) Average Pay	(4) Establishments
Static Model				
Arena	1,217	80,180*	1.399	68.12
	(1,071)	(40,987)	(1.052)	(54.20)
Observations	384	384	384	384
R-squared	0.821	0.884	0.906	0.823
Dynamic Model				
Arena	-640.2**	-28,459**	0.151	-2.490
	(326.6)	(12,790)	(0.665)	(8.900)
L.Employment	0.932***			
	(0.0286)			
L.AnnualPayroll		0.877***		
		(0.110)		
L.AveragePay			0.586***	
			(0.104)	
L.Establishments				0.928***
				(0.0138)
Observations	336	336	336	336

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. ZIP code and time fixed effects are included in both models, but not shown. Standard errors are in parentheses and are clustered at the ZIP code level. There are 24 clusters used. Annual payroll and average pay are reported in thousands of dollars ($000s).

Table 3.4 Contiguous ZIP Codes for Arena Sample

Location	Arena	ZIP code	Zip1	Zip2	Zip3	Zip4	Zip5	Zip6	Zip7
Evansville, IN	Ford Center	47708	47713	47711	47712				
Wichita, KS	Intrust Bank Arena	67202	67203	67211	67213	67214			
Cedar Park, TX	Cedar Park Center	78613	78641	78681	78717	78750			
Toledo, OH	Huntington Center	43604	43611	43608	43620	43607	43609		
Allen, TX	Allen Event Center	75002	75069	75013	75074	75094	75098		
Independence, MO	Independence Events Center	64055	64136	64133	64052	64050	64057	64064	
Tulsa, OK	BOK Center	74103	74119	74127	74106	74120			
Des Moines, IA	Wells Fargo Arena	50309	50314	50316	50315	50321	50312	50317	
Reno, NV	Reno Events Center	89501	89510	89506					
Duluth, GA	The Arena at Gwinnett Center	30097	30024	30043	30096	30092	30022	30005	
Loveland, CO	Budweiser Events Center	80538	80512	80526	80525	80528	80550	80534	80537
Jacksonville, FL	Jacksonville Veterans Mem. Arena	32202	32206	32209	32204	32207			
Frisco, TX	Dr Pepper Arena	75034	75068	75078	75035	75024	75056		
Hidalgo, TX	State Farm Arena	78557	78503	78577					
Hershey, PA	Giant Center	17033	17036	17078	17057	17022	17545		
Bridgeport, CT	Webster Bank Arena	06604	06608	06606	06605				
Manchester, NH	Verizon Wireless Arena	03101	03104	03106	03045	03110	03103		
Reading, PA	Santander Arena	19602	19601	19604	19606	19508	19611		
Elmira, NY	First Arena	14901	14904	14903	14861	14816	14894		
Wilkes-Barre, PA	Mohegan Sun Arena at Casey Plaza	18702	18701	18705	18704	18706	18707	18640	18651
Lowell, MA	Tsongas Center at Umass Lowell	01852	01826	01850	01854	01851	01824	01876	
Estero, FL	Germain Arena	33928	33913	33908	34134	34135	34120	34142	
Greenville, SC	Bon Secours Wellness Arena	29601	29609	29611	29605	29607			
Bakersfield, CA	Rabobank Arena	93301	93308	93305	93304	93309			

Table 3.5 Summary Statistics for Contiguous ZIP Code Sample

Variable	Obs	Mean	Std. Dev.	Min	Max
Establishments	2,176	628.24	486.90	5.00	2,713.00
Employment	2,176	10,963.24	10,102.32	14.00	76,222.00
Annual Payroll ($000s)	2,176	433,398.90	525,784.40	291.00	5,493,521.00
Average Annual Pay ($000s)	2,176	34.65	10.99	7.17	102.27
Arena (=1 if yes)	2,176	0.10	0.31	0.00	1.00

Table 3.6 Contiguous ZIP Code Group Analysis

VARIABLES	(1) Employment	(2) Annual Payroll	(3) Average Pay	(4) Establishments
Static Model				
Arena	1,016	91,541**	2.292*	77.85
	(1,226)	(40,135)	(1.199)	(56.47)
Observations	2,176	2,176	2,176	2,176
R-squared	0.933	0.891	0.889	0.934
Dynamic Model				
Arena	-282.6	-1,455	0.948	-9.717**
	(499.1)	(24,310)	(0.828)	(4.942)
L.Employment	0.808***			
	(0.0897)			
L.AnnualPayroll		0.945***		
		(0.0727)		
L.AveragePay			0.464***	
			(0.0744)	
L.Establishments				0.892***
				(0.0159)
Observations	1,904	1,904	1,904	1,904

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. ZIP code fixed effects and group-specific time fixed effects are included in both models, but not shown. Standard errors are in parentheses and are clustered at the ZIP code group level. Annual payroll and average annual pay are reported in thousands of dollars ($000s).

Table 3.7 Contiguous ZIP Code Group Analysis - Variable Treatment

VARIABLES	(1) Employment	(2) Annual Payroll	(3) Average Pay	(4) Establishments
Arena (t+1)	-241.4	-10,930	0.0329	-12.51
	(302.6)	(21,048)	(0.728)	(9.401)
Arena (t+2)	-112.3	-18,148	0.390	-5.116
	(270.7)	(17,821)	(0.903)	(8.400)
Arena (t+3)	275.5	20,263	1.188	4.354
	(349.1)	(13,311)	(1.168)	(6.950)
Arena (t+4)	76.94	-809.2	0.261	2.119
	(317.2)	(25,932)	(0.842)	(11.45)
Arena (t+5)	-407.7	-11,731	0.980	1.547
	(383.6)	(26,249)	(0.680)	(4.435)
L.Employment	0.816***			
	(0.0806)			
L.AnnualPayroll		0.945***		
		(0.0743)		
L.AveragePay			0.469***	
			(0.0720)	
L.Establishments				0.888***
				(0.0154)
Observations	1,904	1,904	1,904	1,904

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. ZIP code fixed effects and group-specific time fixed effects are included the model, but not shown. Standard errors are in parentheses and are clustered at the ZIP code group level. Annual payroll and average annual pay are reported in thousands of dollars ($000s).

Table 3.8 Sample of NHL and NBA Arenas Built from 1998-2013

Location	**Arena**	**Opened**	**Capacity**	**Primary Use**
New York City, NY	Barclays Center	2012	17,732	NBA, NHL
Orlando, FL	Amway Center	2010	18,846	NBA
Pittsburgh, PA	Consol Energy Center	2010	18,387	NHL
Newark, NJ	Prudential Center	2007	17,625	NHL
Charlotte, NC	Time Warner Cable Arena	2005	19,077	NBA
Memphis, TN	FedExForum	2004	18,119	NBA
Houston, TX	Toyota Center	2003	18,055	NBA
Glendale, AZ	Gila River Arena	2003	17,125	NHL
Oklahoma City, OK	Chesapeake Energy Arena	2002	18,203	NBA
San Antonio, TX	AT&T Center	2002	18,418	NBA
Dallas, TX	American Airlines Center	2001	19,200	NBA, NHL
Miami, FL	American Airlines Arena	2000	19,600	NBA
Saint Paul, MN	Xcel Energy Center	2000	17,954	NHL
Columbus, OH	Nationwide Arena	2000	18,144	NHL
Los Angeles, CA	Staples Center	1999	19,000	NBA, NHL
New Orleans, LA	Smoothie King Center	1999	16,867	NBA
Atlanta, GA	Philips Arena	1999	18,118	NBA
Denver, CO	Pepsi Center	1999	19,155	NBA, NHL
Indianapolis, IN	Bankers Life Fieldhouse	1999	18,165	NBA
Raleigh, NC	PNC Arena	1999	18,680	NHL

Table 3.9 Summary Statistics for NHL/NBA Sample

Variable	Obs	Mean	Std. Dev.	Min	Max
Establishments	1,904	871.18	615.05	28.00	3,371.00
Employment	1,904	18,305.31	16,189.69	223.00	92,855.00
Annual Payroll ($000s)	1,904	880,613.60	1,177,300.00	3,087.00	12,700,000.00
Average Pay ($000s)	1,904	41.04	15.60	13.60	140.25
Arena (=1 if yes)	1,904	0.12	0.32	0.00	1.00

Table 3.10 Contiguous ZIP Codes for NHL/NBA Arena Sample

Location	**Arena**	**ZIP code**	**Zip1**	**Zip2**	**Zip3**	**Zip4**	**Zip5**	**Zip6**	**Zip7**
New York City, NY	Barclays Center	11217	11201	11231	11215	11238	11205		
Orlando, FL	Amway Center	32801	32803	32804	32805	32806			
Pittsburgh, PA	Consol Energy Center	15219	15203	15222	15213	15201			
Newark, NJ	Prudential Center	07102	07104	07103	07108	07114	07105	07029	
Charlotte, NC	Time Warner Cable Arena	28202	28203	28204	28206	28208			
Memphis, TN	FedExForum	38103	38105	38126	38106	38104			
Houston, TX	Toyota Center	77002	77007	77019	77006	77004	77003	77020	77009
Glendale, AZ	Gila River Arena	85305	85345	85307	85037	85303			
Oklahoma City, OK	Chesapeake Energy Arena	73102	73103	73104	73109	73106			
San Antonio, TX	AT&T Center	78219	78218	78234	78244	78220	78202	78208	
Dallas, TX	American Airlines Center	75219	75205	75204	75201	75207	75235	75209	
Miami, FL	American Airlines Arena	33132	33137	33136	33130	33128	33139		
Saint Paul, MN	Xcel Energy Center	55102	55107	55118	55116	55105	55104	55103	55101
Columbus, OH	Nationwide Arena	43215	43201	43203	43205	43206	43222	43212	
Los Angeles, CA	Staples Center	90015	90006	90007	90011	90021	90017	90014	
New Orleans, LA	Smoothie King Center	70113	70112	70130	70115	70125			
Atlanta, GA	Philips Arena	30303	30308	30312	30313				
Denver, CO	Pepsi Center	80204	80211	80202	80203	80223	80219	80212	80214
Indianapolis, IN	Bankers Life Fieldhouse	46204	46202	46225	46222				
Raleigh, NC	PNC Arena	27607	27513	27612	27608	27511	27606		

Table 3.11 Contiguous ZIP Code Group Analysis - NHL/NBA Arena Sample

VARIABLES	(1) Employment	(2) Annual Payroll	(3) Average Pay	(4) Establishments
Static Model				
Arena	211.9	431,224*	7.137***	-7.333
	(1,352)	(223,821)	(2.414)	(24.56)
Observations	1,904	1,904	1,904	1,904
R-squared	0.977	0.946	0.891	0.985
Dynamic Model				
Arena	-2,072	84,830	5.692***	5.058
	(1,279)	(76,946)	(1.504)	(9.305)
L.Employment	0.541***			
	(0.110)			
L.AnnualPayroll		0.920***		
		(0.0582)		
L.AveragePay			0.759***	
			(0.0364)	
L.Establishments				0.876***
				(0.0562)
Observations	1,666	1,666	1,666	1,666

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. ZIP code fixed effects and group-specific time fixed effects are included in both models, but not shown. Standard errors are in parentheses and are clustered at the ZIP code group level. Annual payroll and average annual pay are reported in thousands of dollars ($000s).

Table 3.12 ZIP Code Group Analysis - NHL/NBA Arena Sample - Variable Treatment

VARIABLES	(1) Employment	(2) Annual Payroll	(3) Average Pay	(4) Establishments
Arena (t+1)	-374.1	-145,407	-1.021	36.33**
	(864.3)	(151,572)	(1.712)	(14.36)
Arena (t+2)	391.9	-88,010	-1.990	13.83
	(526.5)	(99,291)	(1.794)	(9.190)
Arena (t+3)	994.4	-21,649	-1.625	11.62
	(649.8)	(88,943)	(1.121)	(11.43)
Arena (t+4)	878.8	19,488	0.840	31.87**
	(923.9)	(65,069)	(1.023)	(15.80)
Arena (t+5)	-493.8	-208,749**	-1.464	12.28
	(715.8)	(101,638)	(1.693)	(12.34)
L.Employment	0.576***			
	(0.104)			
L.AnnualPayroll		0.925***		
		(0.0434)		
L.AveragePay			0.795***	
			(0.0421)	
L.Establishments				0.866***
				(0.0577)
Observations	1,666	1,666	1,666	1,666

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. ZIP code fixed effects and group-specific time fixed effects are included in the model, but not shown. Standard errors are in parentheses and are clustered at the ZIP code group level. Annual payroll and average annual pay are reported in thousands of dollars ($000s).

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