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ESSAYS ON WORKER VALUE AND CONTRACTS IN TEAM ENVIRONMENTS

A dissertation submitted in partial satisfaction of the requirements for the degree of

Doctor of Philosophy in Economics

by

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June 2015

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ABSTRACT

ESSAYS ON WORKER VALUE AND CONTRACTS IN TEAM ENVIRONMENTS

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In many work environments, production occurs in teams. The value of that team's product, the productivities of workers' teammates, and workers' individual productivities help determine the value of workers to a firm, their wages, and the lengths of their contracts. This dissertation uses data from Major League Baseball to investigate these relationships. The first chapter estimates the relationship between team production and firm revenue and uses the estimated relationship to comment on the valuations of workers made previously in the literature. The second chapter analyzes wages and provides evidence that the young in the sample are willing to trade away wages to join more productive teams. The third and final chapter tests whether worker productivity uncertainty affects contract length and finds that length does increase with uncertainty. An abstract for each chapter is provided below.

Chapter 1: This study finds evidence that the absence of firm fixed effects from regressions of Scully's (1974) firm revenue equation leads to overestimating a player's marginal revenue product by at least 164%. This result is consistent across

two sets of seasons, is robust to two different measures of firm revenue and the most commonly controlled revenue sources, and occurs even in commonly used variations of Scully's (1974) revenue equation. This finding suggests that studies that have previously used the estimates of a baseball player's MRP or assume a conclusion drawn from such a study may need to be reexamined.

Chapter 2: This study finds that an average free agent trades away wages to join a team expected to be more productive. More importantly, the young in the sample drive this result: an average, young free agent trades roughly 25% of his wages to join a team with an expected productivity one standard deviation higher. In contrast, the wages of older free agents are unaffected by expected team productivity. These results are robust to a variety of wage-determinant controls, remain consistent across a set of robustness checks, and suggest that better teams provide an important human capital investment opportunity. High-quality measures of both workers' own past productivity and the expected productivity of a worker's future team provide key advantages to identifying these effects. This study is the first to show that the expected productivity of the team a worker will join produces a significant and negative compensating wage differential and may offer an opportunity to invest in human capital.

Chapter 3: This study finds evidence supporting worker productivity uncertainty as a contract-length determinant. This result is robust to a variety of worker- and firm-specific controls, is consistent across two different measures of uncertainty for two different types of worker productivity, and supports Danziger's

(1988) efficient risk-sharing hypothesis. This study improves upon previous studies that analyze the relationship between real uncertainty and contract length by using worker- and firm-specific data for the first time. This key advantage allows this study to control, at the finest level, for contract-length determinants that could complicate analysis when using more aggregated data, a common problem acknowledged in the literature. Specifically, it allows Danziger's (1988) hypothesis to be tested with real uncertainty measures derived from the productivity history of individual workers. Finally, the sensitivity of a third measure of uncertainty provides an initial look into a promising area of future research.

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CHAPTER 1

WHAT IS WRONG WITH MLB ESTIMATES OF A PLAYER'S MARGINAL REVENUE PRODUCT?

1.1 Introduction

In his seminal paper, Scully (1974) used worker and firm data from Major League Baseball (MLB) to estimate a worker's marginal revenue product (MRP) for the first time. His study compared these estimates to players' actual wages to determine the degree of monopsonistic exploitation in the MLB labor market, thereby launching a large and still-active literature that uses these estimates to analyze monopsony in MLB (e.g. Scully 1989; Zimbalist 1992; Bradbury 2010). Their use, however, grew beyond quantifying the exploitation of players. The literatures that have used the estimates of baseball players' MRPs include, but are not limited to, discrimination (e.g. Raimondo 1983; Hill and Spellman 1984), the winner's curse (e.g. Cassing and Douglas 1980; Burger and Walters 2008), market size-wage effects (e.g. Sommers and Quinton 1982; Burger and Walters 2003), bargaining power (e.g. MacDonald and Reynolds 1994), the superstar effect (e.g. Mullin and Dunn 2002), and the productivity-wage relationship (e.g. Blass 1992). Scully's (1974) MRP methodology has even penetrated the popular news press and allowed sports columnists to estimate draft values (e.g. Ball 2013) and the quality of offseason player trades (e.g. Silver 2005).

Although different variables have been included and some changes to the MRP methodology have been made, each study follows one particular feature of the third step in Scully's (1974) original methodology – the absence of firm fixed effects in the firm revenue equation, which estimates the revenue contribution from team performance. This is surprising. Over the past 40 years, fixed effects have become standard in empirical research and have even been used in Scully's (1974) second step, which estimates the contributions of team-aggregate production measures to team performance, to control for any team-specific effects on performance (e.g. Krautmann 1999; Krautmann et al. 2000). Furthermore, the properties of Scully's (1974) methodology have undergone extensive discussion, culminating in a back and forth between Krautmann and Bradbury (Krautman 1999, 2013; Bradbury 2013) that crystalized the universal value of Scully's (1974) methodology: "if one is interested in the efficiency consequences of a new long-term contract or whether a player "earned" his salary – then the Scully approach is warranted" (Krautmann 2013).

Because the MRP estimates from Scully's (1974) methodology have been so widely used, it is critical that the methodology properly estimates MRPs. This study argues that it does not. The absence of firm fixed effects from the firm revenue equation leads to systematically overestimating the revenue generated by team performance. Because this estimate affects the MRP of *every* player multiplicatively, a large upward bias can produce substantial effects on estimated MRPs, potentially affecting conclusions drawn from those estimates.

This study uses panel data from two sets of MLB seasons, 2007 to 2011 and 1995 to 1999, to demonstrate the substantial decrease in the estimated revenue generated by team performance, and, therefore, player MRPs, when including firm fixed effects. The remainder of this paper is organized as follows. Section 1.2 provides an overview of Scully's (1974) methodology and the relationship between team performance and firm revenue in MLB. Section 1.3 discusses the data and variables used in the analysis of that relationship. Section 1.4 presents the results and discusses their implications. Section 1.5 concludes the paper.

1.2 Performance-Revenue Relationship

Scully (1974) estimates a baseball player's MRP through a three-step process. First, each of player i's productivity measures in season t, $PROD_{i,f,t}^{m}$, where m denotes different productivities, is averaged with his teammates' to form a team-level value of each productivity, $\overline{PROD}_{f,t}^{m}$. Second, team f's performance, $PERF_{f,t}$, is regressed on the team-level values and a vector of controls, $Y_{f,t}$, to estimate the coefficient, $\hat{\gamma}^{m}$, on each productivity: $PERF_{f,t} = \sum_{m}\left(\hat{\gamma}^{m}\overline{PROD}_{f,t}^{m}\right) + Y_{f,t}'\hat{\gamma}^{Y}$. Third, firm f's revenue, $REV_{f,t}$, is regressed on team performance and a vector of time-varying controls, $X_{f,t}$:

$$(1.1.1)\ REV_{f,t} = \alpha_0 + \alpha_1 PERF_{f,t} + X_{f,t}'\alpha^{X} + \varepsilon_{f,t},$$

where $\varepsilon_{f,t}$ is the error. The estimate of a player's MRP is then calculated as the estimate on team performance, $\hat{\alpha}_1$, times the sum of the products of the estimate on each team-level productivity value, $\hat{\gamma}^{m}$, and the corresponding productivity measure of the player, $PROD_{i}^{m}$; therefore, $MRP_i = \hat{\alpha}_1(\sum_m \hat{\gamma}^{m} PROD_{i}^{m})$.

The estimate on team performance in equation 1.1.1, $\hat{\alpha}_1$, is critical to calculations of MRP. Because it affects every productivity measure of every player multiplicatively, any percentage change in $\hat{\alpha}_1$ produces a corresponding change in every player's MRP. An insignificant or relatively small estimate, therefore, would imply that a player's productivity is worth little, regardless of his contribution to team performance. A significant and meaningful $\hat{\alpha}_1$, however, has been consistently estimated in the MRP literature, leading to the consensus that a player's productivity generates considerable revenue for his firm. Such a consistent result is not surprising when observing the actual data. As seen in Figure 1.1, the data for the 2007 to 2011 seasons illustrate a significant relationship between total annual firm revenue, which is inflation adjusted to 2011 dollars, and winning percentage.

A key assumption to this observed relationship, and the structure of equation 1.1.1, is that each observation is independent. This assumption, however, is likely inappropriate. If each observation is instead assumed to be related by firm, the observed relationship between total annual firm revenue and winning percentage significantly diminishes, showing firm revenues that are relatively static and largely unaffected by team performance (Figure 1.2). Such a discrepancy in the observed relationship is not specific to just this time period. Total annual firm revenue, inflation adjusted to 1999 dollars, and winning percentage from the 1995 to 1999 seasons illustrate similar relationships (Figures 1.3 and 1.4).

Such a visual discrepancy in the relationship between firm revenue and team performance suggests a change to equation 1.1.1 may be necessary. In addition to time-varying controls, the revenue equation may need to include firm fixed effects, δ_f:

$$(1.2.1)\ REV_{f,t} = \beta_0 + \beta_1 PERF_{f,t} + X'_{f,t}\beta^X + \delta_f + \epsilon_{f,t}.$$

If the difference between $\hat{\alpha}_1$ and $\hat{\beta}_1$ is significant, or even if the difference in their magnitudes is sufficiently large, then the MRP literature and many of the studies that have used estimates of baseball players' MRPs may need to be reevaluated.

1.3 Data

The first and most common dependent variable used in equation 1.1.1 is the total annual revenue earned by each firm. Intuitively, this makes sense. Proper calculations of the MRP of a baseball player must account for the full revenue generated by his skills. This study uses the previously mentioned total annual firm revenue, as estimated by Forbes magazine, from the 2007 to 2011 seasons as a measure of $REV_{f,t}$.[1]

Several studies, however, have analyzed the accuracy of these estimates and have found that, although they are the best available data source of total firm revenue, the estimates contain nontrivial measurement error (e.g. Krautmann 1999; Krautmann 2013). Additionally, because these measures may contain centrally shared industry revenue, such as national television contracts, estimates of the revenue generated from team performance could be systematically too large (e.g. Krautmann 1999; Krautmann 2013). To avoid such measurement error and upward bias, a few studies have instead used observable measures that correlate with total firm revenue yet avoid centrally shared revenue sources, such as game attendance or turnstile revenue. The estimates from these various dependent variables still imply the same outcome: team

[1] http://bizofbaseball.com/index.php?option=com_wrapper&view=wrapper&Itemid=126

performance is a substantial contributor to revenue, implying player productivity is as well.

The previously mentioned revenues for the 1995 to 1999 seasons are unaffected by both problems. The revenue measures for these seasons are provided in the report released by the Independent Members of the Commissioner's Blue Ribbon Panel on Baseball Economics (Levin et al. 2000). These measures contain only the actual "local" revenue sources for each firm, eliminating both measurement error and the potential upward bias.

In addition to regressing total revenue on the variable of interest, winning percentage, each regression includes controls for the most commonly discussed revenue sources: market size, stadium characteristics, and time effects. This study follows the literature and uses the population estimate of the metropolitan area surrounding each MLB firm, $POP_{f,t}$, as a proxy for market size. The U.S. Census and Statistics Canada supply the estimates.[2] The measurement of the metropolitan areas differs between the two sets of seasons but is consistent within each. For metropolitan areas with two firms, such as New York, this study again follows the most common approach in the literature and matches each firm to 50% of the metropolitan area population (e.g. Scully 1974, Krautmann 1999, Burger and Walters 2003). Although no consistent measure of stadium characteristics is used in the literature, several papers characterize stadiums by a dummy variable that takes a value of 1 if the stadium is considered "new" (e.g. Mullin and Dunn 2002; Burger and Walters 2003;

[2] http://www.census.gov/population/metro/;
http://www5.statcan.gc.ca/cansim/a33?RT=TABLE&themeID=3433& spMode=tables&lang=eng.

Burger and Walters 2008). This study considers a stadium to be “new” if it has been used for five or fewer years. Finally, this study uses season dummy variables to control for any time effects on firm revenue. The summary statistics for each variable for the 2007 to 2011 and 1995 to 1999 seasons are provided in Tables 1.1 and 1.2, respectively.

1.4 Results

Two important questions need to be answered. First, is there a substantial difference in the estimated revenue generated by team performance between the regressions with and without firm fixed effects? Second, if there is, what does this difference imply? The first two subsections address the first question by investigating several common variations of the revenue equation discussed in Section 1.2. The third subsection focuses on the second question and additionally discusses how including firm fixed effects affects the estimates of the revenue controls.

1.4.1 Standard Linear Equation

The results when using the 2007 to 2011 seasons are provided in Table 1.3. The odd and even columns contain the estimates from the regressions of equations 1.1.1 and 1.2.1, respectively, and display a substantial difference between the estimates on team performance in equation 1.1.1, $\hat{\alpha}_1$, and equation 1.2.1, $\hat{\beta}_1$. The first two columns provide results for the univariate regressions of revenue on performance. These results quantify the team performance-revenue relationship illustrated in Figures 1.1 and 1.2. The third and fourth columns contain the results when controlling for market size, stadium characteristics, and time effects. The differences between $\hat{\alpha}_1$ and $\hat{\beta}_1$ in both pairs of columns are statistically significant at the 1% level. Perhaps even more important is that the magnitudes of $\hat{\beta}_1$ are at least 70% smaller than those of $\hat{\alpha}_1$, implying that MRPs from the regressions without fixed effects are overestimated by at least 236%. To put such a difference in perspective, for a one standard deviation increase in winning, roughly 11 games, $\hat{\beta}_1$ implies an increase in total revenue of only \$4.5 million (column 4) instead of the \$15.1 million implied by $\hat{\alpha}_1$ (column 3). Considering the average wage earned by a baseball player between the 2007 to 2011 seasons was \$3.3 million, this difference is analogous to the difference between hiring one-and-a-half and four-and-a-half baseball players, roughly the difference between paying for 17% and 50% of a team's lineup.[3]

These results are not unique to the Forbes revenue data or the 2007 to 2011 seasons. The estimates from the same respective regressions, but using the higher-quality revenue data from the 1995 to 1999 seasons, are provided in columns 1 through 4 of Table 1.4 and imply the same conclusions. In both pairs of columns, $\hat{\alpha}_1$ and $\hat{\beta}_1$ are again statistically different at the 1% level, but the differences in magnitudes are even larger. The magnitudes of $\hat{\beta}_1$ are at least 84% smaller than those of $\hat{\alpha}_1$ and imply a difference of \$13.0 million in 1999 dollars (columns 3 and 4). Such

[3] http://www.usatoday.com/sports/mlb/salaries/. The average salary includes all players, including both free agents and non-free agents, listed on the opening day team rosters and disabled lists. Only nine players can participate in a game at any one time.

a gap corresponds to hiring nine baseball players at the average salary ($1.4 million) for that timeframe, the equivalent of paying for a team's full lineup.[4]

1.4.2 Past Performance and Market-Size Interactions

Two additional variations of the revenue equation have been discussed and should be addressed. First, Zimbalist (1992) argues that a team's performance from the previous season, $PERF_{f,t-1}$, could affect revenue in the current season through season-ticket sales and broadcasting appeal, implying

$$(1.1.2)\ REV_{f,t} = \alpha_0 + \alpha_1 PERF_{f,t} + \alpha_2 PERF_{f,t-1} + X'_{f,t}\alpha^X + \varepsilon_{f,t}$$

$$(1.2.2)\ REV_{f,t} = \beta_0 + \beta_1 PERF_{f,t} + \beta_2 PERF_{f,t-1} + X'_{f,t}\beta^X + \delta_f + \epsilon_{f,t}.$$

In this context, the estimates of players' MRPs depend on the sum of the coefficients on the team performance variables, $\hat{\alpha}_1 + \hat{\alpha}_2$ and $\hat{\beta}_1 + \hat{\beta}_2$. Again, the relationship between the estimated revenue generated by team performance and a player's MRP is multiplicative, implying that any percentage change in the sum of the performance coefficients changes the MRPs of all players by that same percentage. For example, player i's estimated MRP that results from equation 1.1.2 is $MRP_i = (\hat{\alpha}_1 + \hat{\alpha}_2)(\sum_m \hat{\gamma}^m PROD_i^m)$. Despite the intuitiveness of Zimbalist's (1992) variation, only a few studies have adopted it (e.g. Krautmann 1999).

In contrast, the variation introduced by Sommers and Quinton (1982) has gained notable attention (e.g. Zimbalist 1992; Burger and Walters 2003, 2008). They use an interaction between team performance and market size and find that winning

[4] http://www.usatoday.com/sports/mlb/salaries/. The average salary includes all players, including both free agents and non-free agents, listed on the opening day team rosters and disabled lists.

in bigger markets has a larger impact on revenue than winning in smaller ones, suggesting

$$(1.1.3)\ REV_{f,t} = \alpha_0 + \alpha_1 PERF_{f,t} + \alpha_I PERF_{f,t} * POP_{f,t} + X'_{f,t}\alpha^X + \varepsilon_{f,t}$$

$$(1.2.3)\ REV_{f,t} = \beta_0 + \beta_1 PERF_{f,t} + \beta_I PERF_{f,t} * POP_{f,t} + X'_{f,t}\beta^X + \delta_f + \epsilon_{f,t}.$$

Like the two previous variations of the revenue equation, any change in the estimated revenue generated by team performance changes the MRP of all players; however, in this variation the market size of a firm also affects the overall change to the estimated MRPs of that firm's players. This effect can be observed in the estimated MRP that results from equation 1.1.3: $MRP_{i,f} = \left(\hat{\alpha}_1 + \hat{\alpha}_I POP_f\right)(\sum_m \hat{\gamma}^m PROD_i^m)$. For simplicity, the differences between the regression results for equations 1.1.3 and 1.2.3 will be discussed in the context of a firm with an average city population, $\overline{POP}$. Regression results for equations 1.1.2, 1.2.2, 1.1.3, and 1.2.3 for the 2007 to 2011 and 1995 to 1999 seasons are provided in columns 5 through 8 of Tables 1.3 and 1.4, respectively.

Turning first to the estimates in columns 5 and 6 that use lagged winning percentage, the differences between the conventional equation (equation 1.1.2) and the equation with firm fixed effects (equation 1.2.2) are consistent with the regression results provided in columns 1 through 4. The sums of the estimates, $\hat{\alpha}_1 + \hat{\alpha}_2$ and $\hat{\beta}_1 + \hat{\beta}_2$, are significantly different at the 1% level for each pair of columns in both tables. The smallest difference is again observed in Table 1.3, which provides the results for the data from 2007 to 2011. For this set of seasons, the magnitude of $\hat{\beta}_1 + \hat{\beta}_2$ is 62% smaller than that of $\hat{\alpha}_1 + \hat{\alpha}_2$, implying the MRPs estimated from the regression of equation 1.1.2 are overestimated by at least 164%. Although the overestimation of the estimates is smaller than in columns 1 through 4, this is still a large discrepancy. In the context of the 2007 to 2011 MLB labor markets, $\hat{\alpha}_1 + \hat{\alpha}_2$ implies that winning 11 additional games increases total revenue by $22.9 million (column 5) over that and the subsequent season. For the same increase in wins, $\hat{\beta}_1 + \hat{\beta}_2$ implies an increase of only $8.3 million (column 6), which is equivalent to hiring four-and-a-half fewer players or paying for 50% of a team's lineup less than the amount implied by $\hat{\alpha}_1 + \hat{\alpha}_2$.

Finally, the results provided in columns 7 and 8 of Tables 1.3 and 1.4 yield similar conclusions. The differences in the relevant functions of the estimates, $\hat{\alpha}_1 + \hat{\alpha}_I\overline{POP}$ and $\hat{\beta}_1 + \hat{\beta}_I\overline{POP}$, are statistically significant at the 1% level for each pair of columns in both tables. Both sets of seasons imply that the estimated revenue generated by the winning of a team with an average sized market, and, therefore, each player's MRP that is on such a team, is at least 69% less when using firm fixed effects (equation 1.2.3) as opposed to not (equation 1.1.3), implying that MRPs estimated from equation 1.1.3 are overestimated by at least 228%. Translating this difference into the 2007 to 2011 MLB labor markets, $\hat{\alpha}_1 + \hat{\alpha}_I\overline{POP}$ implies that an increase of 11 wins increases revenue by $15.7 million (column 7, Table 1.3). This is $10.9 million more than what $\hat{\beta}_1 + \hat{\beta}_I\overline{POP}$ implies, corresponding to hiring three-and-a-half additional players or paying for 39% of a team's lineup.

1.4.3 Implications

Regardless of the controls, structure of the revenue equation, or seasons, the differences between using firm fixed effects or not are significant at the 1% level and imply sizeable percentage differences in the estimated revenue generated by team performance. As previously mentioned, these differences, in turn, imply equally sizeable percentage differences in the calculations of baseball players' MRPs. Estimates from the regressions that use firm fixed effects are at least 62% and 73% smaller for the 2007 to 2011 and 1995 to 1999 seasons, respectively. Of particular interest is that the higher-quality data from the 1995 to 1999 seasons consistently estimate larger differences between the regression with and without firm fixed effects, adding more creditability to these estimated differences. Such strong and consistent results imply that past studies have likely overestimated baseball players' MRPs substantially, potentially leading to incorrect conclusions in various literatures. Each of these studies and the conclusions they draw may need to be reexamined.

Such large differences in the team performance-revenue relationship are not the only important differences between the equations with and without firm fixed effects. The statistical significances of the controls have implications to studies outside the MRP literature and even to real-world decisions, such as building a new stadium; and the lagged winning percentage variable and the interaction between winning and population are important to calculating and understanding a player's value within MLB. Addressing the control variables first, the inclusion of firm fixed effects changes the conclusions drawn for each control variable in all three variations of the revenue equation for both sets of seasons. One exception does exist, however: the new-stadium variable in columns 5 and 6 of Table 1.3 is insignificant in both regressions. Such results suggest that the relevant literatures may need to reevaluate the importance of each of these potential revenue sources.

Interestingly, although significantly muted by the addition of fixed effects, lagged team performance is significant for both sets of seasons. Because the large bulk of the literature ignores this relationship, these results suggest that the literature may need to include previous team performance in the revenue equation. In contrast, the relationship between market size and team performance appears to be sensitive to the inclusion of firm fixed effects. For both sets of seasons, the regression without firm fixed effects supports a significant relationship between the two, but this support disappears when fixed effects are included. The latter result may be surprising. Firms with larger markets should benefit more from additional wins. Such intuition, however, assumes sufficient numbers of consumers are sensitive to winning (“fair-weather fans”). If enough consumers purchase only at elevated levels of winning, then more winning in larger markets would create a much larger increase in consumption than in markets with fewer potential consumers. The results from column 8 in Tables 1.3 and 1.4, however, suggest that such an assumption may be wrong and aggregate consumer demand is relatively insensitive to winning.

1.5 Conclusion

Do including firm fixed effects in the revenue equation of MRP calculations affect the estimated relationship between team performance and firm revenue in MLB? The evidence suggests it does; in fact, the results imply substantial decreases to the estimated revenue generated by team performance when including fixed effects in the firm revenue regression. These findings are consistent across two separate time series with two different measures of total firm revenue and are robust to the revenue sources and variations of Scully's (1974) revenue equation that are commonly found in the literature.

The substantially smaller estimates, at least 62% and 73% smaller for the 2007 to 2011 and 1995 to 1999 seasons, respectively, are particularly worrisome. Many studies that use MRP estimates rely on these magnitudes to form their conclusions. For example, studies in the discrimination literature compare the estimates of baseball players' MRPs to their wages and test whether these gaps, or lack thereof, are significantly different between different ethnicities. Because the revenue generated from team performance affects *every* player's MRP multiplicatively, the conclusions in each of these studies are drawn from MRP estimates that may be systematically and substantially too large. This study finds that regressions without firm fixed effects may overestimate MRPs by at least 164%. This suggests that the results from each study that either uses MRP calculations or assumes a conclusion drawn from such a study may need to be reevaluated.

TABLES

Table 1.1: 2007 – 2011
Summary Statistics

Variable	Obs	Mean	St. Dev	Min	Max
Total Season Revenue [$100 million]	150	2.057	.525	1.382	4.631
Winning %	150	.500	.066	.346	.636
City Population [1 million]	150	4.335	2.094	1.545	9.878
Stadium Age [years]	150	14.953	22.643	0	99

Revenue and salaries are inflation adjusted to 2011 dollars.

Table 1.2: 1995 – 1999
Summary Statistics

Variable	Obs	Mean	St. Dev	Min	Max
Total Season Revenue [$100 million]	144	.592	.319	.120	1.759
Winning %	144	.500	.070	.327	.704
City Population [1 million]	144	3.093	1.063	1.460	4.955
Stadium Age [years]	144	20.125	20.344	0	87

Revenue and salaries are inflation adjusted to 1999 dollars.

Table 1.3: 2007 – 2011
Winning-Revenue Relationship: No Firm Fixed Effects Vs. Firm Fixed Effects

Dependent Variable: Revenue [$100 million]	No FE (1)	FE (2)	No FE (1.1.1) (3)	FE (1.2.1) (4)	No FE (1.1.2) (5)	FE (1.2.2) (6)	No FE (1.1.3) (7)	FE (1.2.3) (8)
Winning %	3.546*** (.747)	.539** (.254)	2.228*** (.478)	.663*** (.216)	1.420*** (.523)	.613*** (.232)	-2.870** (1.329)	-.233 (.781)
Lagged Winning %					1.802*** (.514)	.608*** (.204)		
City Pop [1 million]			.142*** (.023)	-.128 (.216)	.137*** (.026)	-.395* (.233)	-.476*** (.173)	-.234 (.240)
Winning x Population							1.197*** (.344)	.217 (.195)
New Stadium			-.078 (.065)	.124** (.058)	-.051 (.071)	.135** (.063)	-.069 (.060)	.118** (.057)
Season Dummies			YES	YES	YES	YES	YES	YES

***** denotes significance at the 10% level, ****** denotes significance at the 5% level, and *** denotes significance at the 1% level. The regressions in columns 5 and 6 use 114 observations; the rest use 150. Heteroskedasticity-robust standard errors are provided in parentheses.

Table 1.4: 1995 – 1999

Winning-Revenue Relationship: No Firm Fixed Effects Vs. Firm Fixed Effects

Dependent Variable: Revenue [$100 million]	**No FE (1)**	**FE (2)**	**No FE (1.1.1) (3)**	**FE (1.2.1) (4)**	**No FE (1.1.2) (5)**	**FE (1.2.2) (6)**	**No FE (1.1.3) (7)**	**FE (1.2.3) (8)**
Winning %	2.434*** (.358)	.382 (.293)	2.247*** (.324)	.329 (.231)	1.804*** (.362)	.515** (.241)	.421 (1.013)	-.443 (.766)
Lagged Winning %					1.833*** (.372)	.492* (.252)		
City Pop [1 million]			.067*** (.019)	-.111 (.271)	.053*** (.019)	-.319 (.340)	-.207 (.153)	-.279 (.340)
Winning x Population							.563* (.323)	.252 (.262)
New Stadium			.124*** (.047)	.033 (.039)	.077 (.050)	.066 (.044)	.132*** (.047)	.035 (.039)
Season Dummies			YES	YES	YES	YES	YES	YES

***** denotes significance at the 10% level, ****** denotes significance at the 5% level, and *** denotes significance at the 1% level. The regressions in columns 5 and 6 use 114 observations; the rest use 150. Heteroskedasticity-robust standard errors are provided in parentheses.

FIGURES

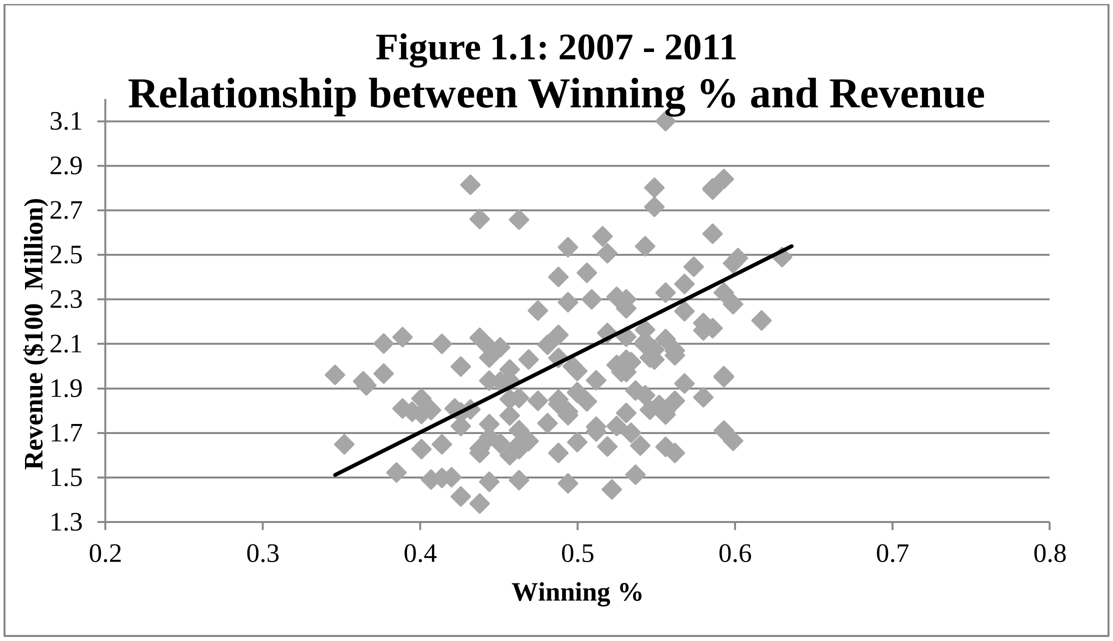
Figure 1.1: 2007 - 2011
Relationship between Winning % and Revenue
Revenue ($100 Million)
3.1
2.9
2.7
2.5
2.3
2.1
1.9
1.7
1.5
1.3
0.2
0.3
0.4
0.5
0.6
0.7
0.8
Winning %

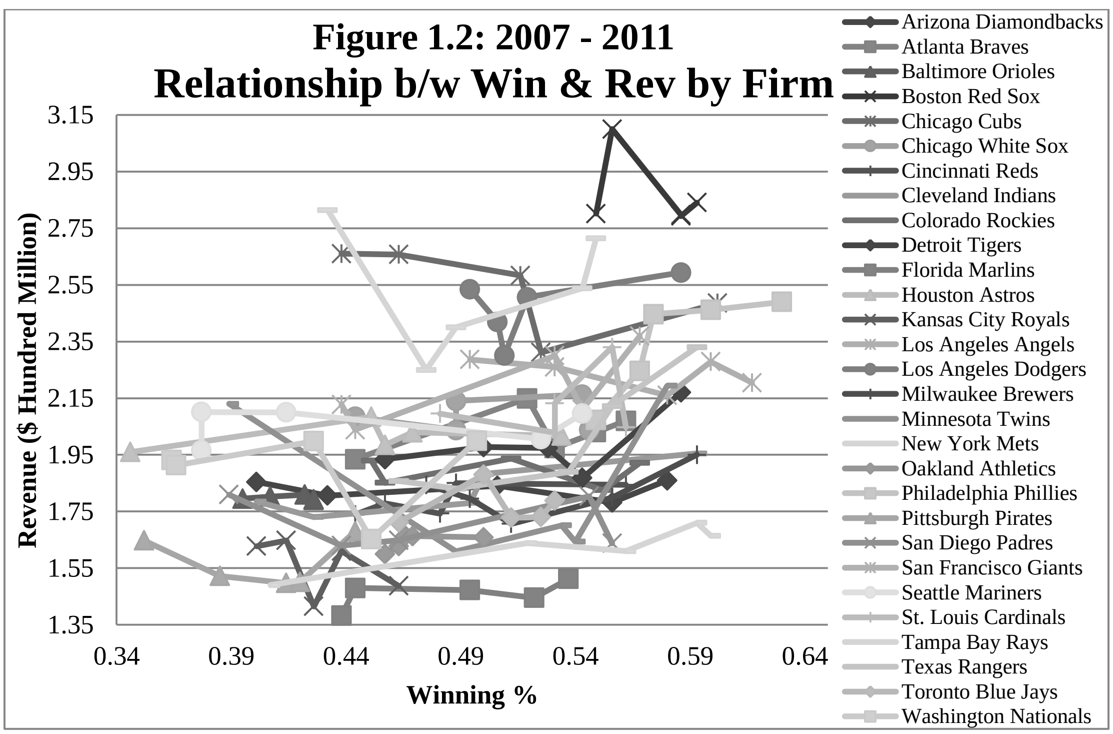
Figure 1.2: 2007 - 2011
Relationship b/w Win & Rev by Firm
Revenue ($ Hundred Million)
3.15
2.95
2.75
2.55
2.35
2.15
1.95
1.75
1.55
1.35
0.34
0.39
0.44
0.49
0.54
0.59
0.64
Winning %
Arizona Diamondbacks
Atlanta Braves
Baltimore Orioles
Boston Red Sox
Chicago Cubs
Chicago White Sox
Cincinnati Reds
Cleveland Indians
Colorado Rockies
Detroit Tigers
Florida Marlins
Houston Astros
Kansas City Royals
Los Angeles Angels
Los Angeles Dodgers
Milwaukee Brewers
Minnesota Twins
New York Mets
Oakland Athletics
Philadelphia Phillies
Pittsburgh Pirates
San Diego Padres
San Francisco Giants
Seattle Mariners
St. Louis Cardinals
Tampa Bay Rays
Texas Rangers
Toronto Blue Jays
Washington Nationals

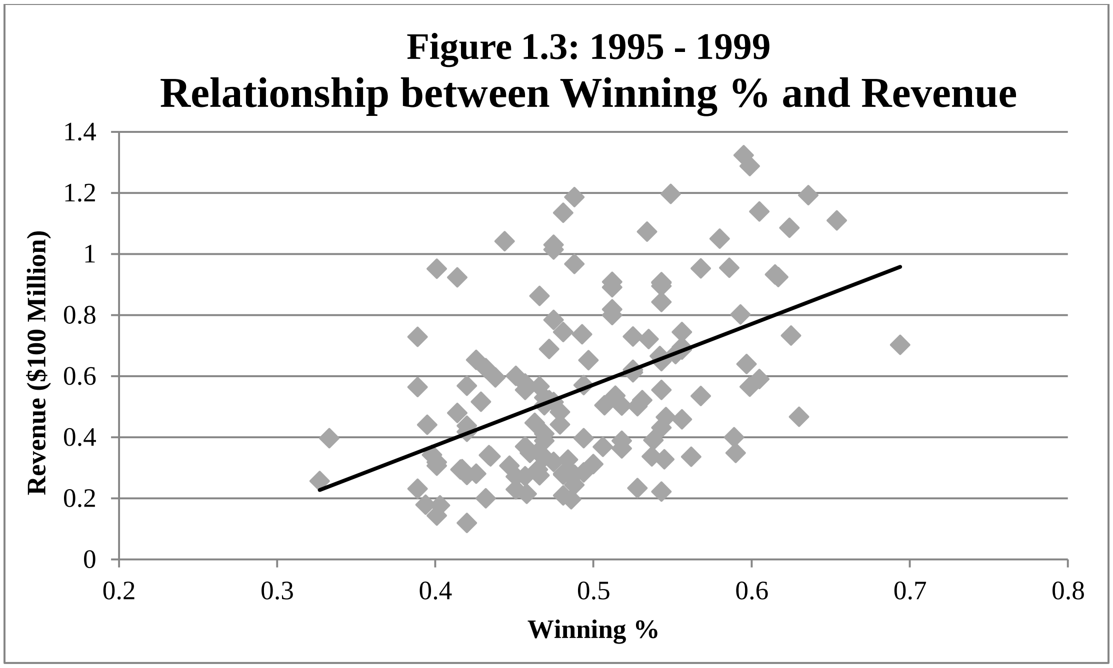
Figure 1.3: 1995 - 1999
Relationship between Winning % and Revenue
Revenue ($100 Million)
Winning %
1.4
1.2
1
0.8
0.6
0.4
0.2
0
0.2
0.3
0.4
0.5
0.6
0.7
0.8

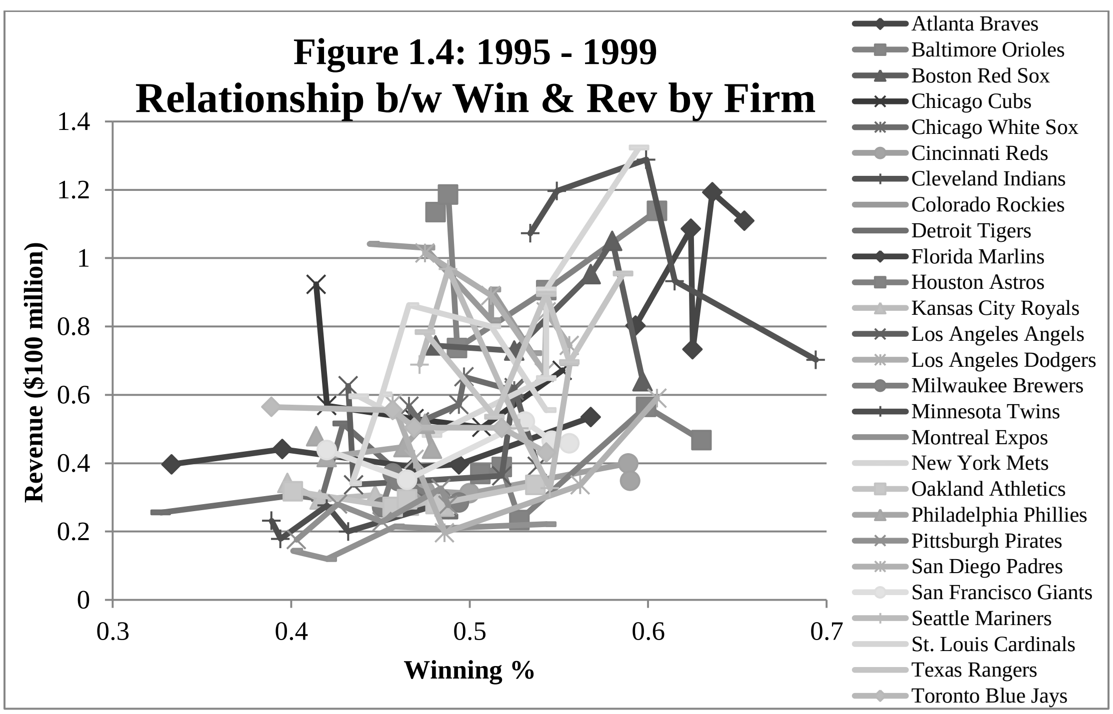
Figure 1.4: 1995 - 1999
Relationship b/w Win & Rev by Firm
Revenue ($100 million)
Winning %
1.4
1.2
1
0.8
0.6
0.4
0.2
0
0.3
0.4
0.5
0.6
0.7
Atlanta Braves
Baltimore Orioles
Boston Red Sox
Chicago Cubs
Chicago White Sox
Cincinnati Reds
Cleveland Indians
Colorado Rockies
Detroit Tigers
Florida Marlins
Houston Astros
Kansas City Royals
Los Angeles Angels
Los Angeles Dodgers
Milwaukee Brewers
Minnesota Twins
Montreal Expos
New York Mets
Oakland Athletics
Philadelphia Phillies
Pittsburgh Pirates
San Diego Padres
San Francisco Giants
Seattle Mariners
St. Louis Cardinals
Texas Rangers
Toronto Blue Jays

CHAPTER 2

ARE WORKERS WILLING TO PAY TO JOIN A BETTER TEAM?

2.1 Introduction

The idea that firms with undesirable characteristics must pay a wage premium to compensate workers, or that workers accept lower wages from firms with desirable characteristics, goes back at least to Adam Smith (1776). A large literature has attempted to measure the size of such compensating wage differentials (CWDs) for various firm characteristics across numerous industries.[5] Despite the size of the literature, little research has studied compensating differentials for the expected productivity of the firm's team a worker will join, or "team quality". This should perhaps surprise academic economists, who seem to place great emphasis on the quality of their prospective colleagues when choosing which department to join.

Three main data challenges are likely responsible for this lack of attention. First, reliable measures of workers' productivities are rarely observed. Even if they are, because of the team-production environment, such measures are likely affected by spillovers from teammates' effort (e.g. Falk and Ichino 2006; Mas and Moretti 2009) and complementarities among teammates' skillsets (e.g. Lazear 1999; Hamilton et al. 2003).. Both of these factors temporarily modify observed worker productivity through peer effects specific to the team, thereby introducing team-specific measurement error and producing positive bias on any estimates of negative CWDs (Hwang et al. 1992). Second, each worker must be matched not only with the firm hiring him but also with the specific team he joins. This is a particularly difficult challenge considering many industries comprise firms that employ multiple teams.

[5] Please see Borjas (2005) for a quick overview of CWDs.

Some of these teams may even share workers between them. Third, given that workers can be suitably matched, a measure of team quality, the *expected* productivity of the team a worker joins, is also rarely observed. For industries with static team production, current or previous team production, if observed, can provide a reasonable proxy for team quality; however, if team production is unstable, such a proxy may introduce sufficient measurement error to attenuate estimates of team quality-wage effects.

These three challenges may explain why Michaelides (2010) is possibly the only other study that has tested for team quality-wage effects. His study used wage data from the National Basketball Association (NBA) to test CWD theory across an array of possible wage determinants, one being team quality. Although data from the NBA match workers and teams, the data from the NBA is not ideal for identifying team quality-wage effects. Evidence of complementarities in team-oriented sports, specifically the National Hockey League and NBA, was found by Idson and Kahane (2000, 2004), implying estimation bias from measurement error in the worker productivity measures used by Michaelides (2010). His study also used previous team winning percentage to measure team quality despite instability in the variable across seasons. Furthermore, he restricted team winning percentage to be binary (either a winning or losing season), losing much of the information available in the variable. Subsequently, the estimates of the team quality-wage effects were largely insignificant, and some estimates were even positive – the opposite of what CWD theory predicts.

In contrast, this study uses data from the Major League Baseball (MLB) free-agent market. Like the data used by Michaelides (2010), individual free agents are easily paired with their contracting firm and team, and a substantial set of firm and worker controls are available.[6] Unlike the data used by Michaelides (2010), this study uses worker productivity data with minimal spillovers and complementarities and constructs a continuous and credible team-quality variable. Collectively, these properties provide a dataset ideal for analyzing team quality-wage effects, and allow this study to identify a significant, negative team quality-wage effect that supports the CWD theory of Rosen (1986). This is likely the first study to do so. This study also finds that the young drive this effect, suggesting that team quality may provide a human capital investment opportunity.

The remainder of this paper is organized as follows. Section 2.2 discusses spillovers and complementarities in the context of MLB. Section 2.3 introduces free agents and the free agent-market data. Section 2.4 details the data and variables used to control for several important wage determinants that may correlate with team quality, and Section 2.5 discusses the construction of the main team-quality variable used in this study. Section 2.6 presents the main results. It discusses the evidence supporting CWD theory and some evidence suggesting that the opportunity for human-capital investment may be a key mechanism guiding young workers' apparent preference for better teams. Section 2.7 discusses several factors that complicate MLB wage contracts and provides robustness checks for each. Section 2.8 concludes

[6] Professional sports data mitigate several other commonplace data problems that can confound team-quality analysis in other industries. Please see Appendix A.1for more information.

the paper, discussing the potential for detecting team-quality effects in other industries and several mechanisms that may produce the estimated team quality-wage effects found in this study. This section also offers some anecdotal evidence from the history of MLB consistent with the team quality-wage effects estimated in this study.

2.2 Spillovers and Complementarities

Properly chosen player productivity statistics from MLB should be unaffected by spillovers from and complementarities with teammates. In general, each player puts forth top effort, regardless of spillovers from teammates, because individual productivity is easily measured and heavily monitored and replacement players are readily available for each team. Similarly, choosing proper productivity statistics can minimize complementarities between teammates. Unlike other team sports, such as basketball, individual tasks, like getting a hit, making a catch, or throwing a ball, contribute to team production through individualized pathways and are, therefore, largely independent of other teammates' skillsets. For example, a baseball player hitting a ball is unaffected by the actions of his teammates on the bench or those currently on the bases; however, a basketball player shooting a shot is affected by his location relative to the basket and the defense of his guard, both of which are directly affected by the movements and abilities of the shooter's teammates. As long as the chosen productivity statistics only measure a MLB player's individual tasks, the observed production values will be largely independent from teammate complementarities.

Spillover and complementarity effects from opponents should also not affect player productivity statistics systematically. MLB hitting statistics generally exclude hits earned from opponents' fielding mistakes; likewise, fielding statistics generally ignore hits that are not hit within a reasonable range of the fielder. A batter's hitting production is, however, determined jointly with the production of the opposing pitcher; but the way firms handle player fatigue, the competition schedule among teams, and the large number of different opponents that each player competes against suggest that each batter and pitcher, on average, competes against similar distributions of pitcher and batter skill, respectively, implying minimal systematic effects from spillovers or complementarities.

Because batters don't fatigue, teams reuse the best hitting lineup for every game, minimizing any correlation between batting lineups and the opposing pitcher's productivity. Pitchers, conversely, do fatigue, so teams use pitching rotations to rest pitchers with high inning counts. For example, "starting pitchers", who pitch between three and six innings in a game, usually receive at least three games of rest. Teams also compete in "series", a group of three to four games between the same two teams. Fixed rest and competition schedules prevent a team from only using certain pitchers against certain teams. Theoretically, however, teams could adjust their pitching rotations by shrinking rest periods for certain pitchers. Such an adjustment would increase the risk of pitcher fatigue and injury and, if used at all, would be used sparingly; but if teams do systematically alter their pitching rotations, batters on better teams would face better pitching opposition. Consequently, because observed productivity affects wages significantly, batters would prefer playing for worse teams. The results in Section 2.6 contradict this. Collectively, these properties suggest that, on average, pitching rotations are uncorrelated with batter productivity.

Finally, each team competes against 18 to 20 different teams over a 162-game season and regularly uses a general stock of seven to nine pitchers and at least nine batters. The large quantity of opponents each player competes against, coupled with the lack of endogenous changes to batting lineups and pitching rotations, ensures each batter and pitcher competes against similar distributions of opponent skill, resulting in minimal systematic spillovers and complementarities between batters and pitchers.

In summary, the high visibility and availability of replacement players and the unique nature of team production in MLB suggest limited spillovers and complementarities between a player and his teammates in certain contexts. Likewise, the properties of the player productivity statistics and the organization of the competition schedule suggest minimal spillovers and complementarities between a player and the opposing team. Collectively, these characteristics imply that productivity statistics from MLB can be largely independent of spillover and complementarity effects, providing accurate measures of worker productivity and limiting the positive bias described by Hwang et al. (1992). The productivity statistics chosen for this study are described in more detail in Section 2.4.2.

2.3 Free-Agent Market

The MLB free-agent market begins in late October, immediately following the World Series, and concludes by February. Each player becomes eligible for the free-agent market through one of three methods: 1) the player has completed at least six years in MLB's major league when his contract expires; 2) the player's 10-year contract with the Japanese major leagues, purchased by a MLB firm, expires; or 3) the player's contract expires and no contract was offered to him by the tender deadline. The majority of free agents spend at least eight years in MLB and its minor league affiliates before entering the market through method (1), yielding an older and far more experienced sample relative to the MLB population.

Rule changes and poor player evaluations by firms limit the amount of clean data. New rules governing the free-agent market were introduced in the 2006 market, which finished phasing in during 2007, and modifications to these rules were introduced in the 2011 market. As Hakes and Sauer (2006) point out, the majority of MLB teams did not evaluate batters effectively until around the middle of the 2000's decade. These two factors result in three years of high-quality data: the 2008, 2009, and 2010 free-agent markets. ESPN supplies the list of free agents and their contracting firm for each of these three years.[7]

[7] http://espn.go.com/mlb/freeagents/_/year

2.4 Data

This study compiles data over the three-year period (2008 – 2010 free-agent markets) into a repeated cross-section. Like many analyses using MLB data, this study focuses on non-pitchers. Pitchers are generally divided into two very different subgroups, “starting pitchers” and “relief pitchers”, which raise analytical problems when using smaller datasets. Each subgroup is trained differently, used differently, and valued differently, requiring starting- and relief-pitcher data to be analyzed separately; however, too few observations of each pitching subgroup exist for reliable analysis. The rest of this section discusses the data on position-player contracts, worker characteristics, and firm characteristics. The summary statistics for each variable discussed below are presented in Table 2.1.

2.4.1 Worker Contracts

The wage regressions in this study use the inflation-adjusted, total guaranteed wages a free agent collects throughout his contract as the dependent variable and control for contract length to differentiate contracts with similar wages but different durations of payment.[8] A second purpose for a contract-length control is addressed by Christofides (1990), who suggests that contract length affects but is not affected by wages. The average contract length observed in the sample is roughly 1.5 years, meaning players

[8] Signing bonuses are guaranteed and, therefore, included, but team re-contracting options are not and, therefore, are excluded. Using annualized wages does not change the results or conclusions discussed in Sections 2.6 and 2.7.

generally negotiate a new contract every couple of years. The contract data are supplied by ESPN.[9]

As previously mentioned, the same rules regulate all contracts from the 2008, 2009, and 2010 free-agent markets; however, each market contains different skill distributions and quantities of free agents, potentially producing some year-specific effects on free agent contracts. All regressions use year-specific dummy variables to control for any corresponding effects.

Unlike most other professional sports leagues, no firm salary caps or free agent-salary restrictions exist. A "luxury tax", a wage floor, unobserved performance and playing-time incentives, and restrictions on trading type-A and B free agents, however, do exist.[10] These factors might systematically distort the wage data for certain free-agent types. The contract data from ESPN are used to construct dummy variables to control for free-agent type, and robustness checks for each potential distortion are discussed in Section 2.7.

2.4.2 Worker Characteristics

Sports Reference supplies the player productivity statistics used in the sample.[11] Fielding percentage, fld, measures defensive productivity, and on-base percentage, obp, and slugging ratio, slg, measure offensive productivity.[12] These statistics were chosen because they only measure individual tasks, limiting the exposure to spillover

[9] http://espn.go.com/mlb/freeagents/_/year
[10] MLB contracts Elias Sports Bureau to determine the free agents in the top 20% for their position (type A) and those within the top 21-40% (type B). The algorithm is proprietary and not available to the public.
[11] http://www.baseball-reference.com
[12] Please see Table A.1 in Appendix A.2 for the formula for each performance statistic.

and complementarity effects. fld measures a free agent's ability to catch and throw a ball. It excludes any hits that were not hit within a reasonable distance of the free agent or were fielded by other players. On some rare occasions, good fielders can artificially increase the statistic of a free agent by catching a thrown ball that typically would not have been caught, but this does not happen often and can affect only a small part of the statistic. Both offensive productivity statistics measure a free agent's individual hitting ability exclusively. obp measures his ability to get safely to first base, and slg measures the total number of bases he reaches from a hitting opportunity. The hitting production of other players affects neither statistic, and actions determined by managers that influence observed production, such as bunting, are excluded from both statistics. Because future free-agent productivity is unknown, the previous season's productivity provides a proxy for expected productivity (Quirk and Fort 1992).

This study also controls for free agents moving to different cities and their position, age, experience, and fame. Roughly 25% of the free agents in the sample re-sign with their previous firm. The pecuniary and nonpecuniary costs of moving or, alternately, working away from home may provide an incentive for free agents to re-sign with their previous team or with a team based in their current city. All regressions include a dummy variable that controls for free agents moving to a new city. The data are supplied by ESPN and Sports Reference.

Free agents, as previously mentioned, are generally older than their non-free agent counterparts because they must accrue at least six years in the major league of

MLB. The average age of 33.5 and experience of 10.3 years illustrate this characteristic of the dataset. Furthermore, because baseball players' athleticism naturally degrades with time, the elevated age of free agents could suggest that free agents may care about maintaining current levels of productivity, not just increasing them. The position data, along with age and experience, are also provided by Sports Reference.

Finally, fame appears to affect wages in professional sports, even when controlling for worker productivity and market size (e.g. Mullin and Dunn 2002; Franck and Nuesch 2012). This study uses the prices of Topps baseball cards that were printed in the previous season as a proxy variable for fame (Nardinelli and Simon 1990; Mullin and Dunn 2002). Becket Media published these prices in their annual price guides (edited by Brian Fleischer**)** in the first quarter of 2009, 2010, and 2011. The close proximity of the publication date to the conclusion of the 2008, 2009, and 2010 free-agent markets suggests that the published prices capture relevant fame levels. Each price guide supplies the prices of cards from the preceding year, so the price guide printed in 2010 contains the prices for the 2009 series of Topps baseball cards. The price guides categorize prices into bins, each corresponding to a different level of consumer demand for a player's card, or "fame". Due to inflation and variations in aggregate demand for baseball cards, the bin values change between years. Each card price from 2009 and 2010 is adjusted to match the appropriate 2011 price bin.[13] The typical baseball card price is about 15 cents, the value associated with just enough consumer demand to warrant Topps printing a card.

2.4.3 Firm Characteristics

The heterogeneity in free-agent productivities provides advantages to firms with larger budgets that can, and do, contract the more productive players, thereby correlating higher-budget firms with increased team-quality measures. Estimates of previous season firm revenue and net operating income, supplied by Forbes, control for any budget effects that may correlate with team quality.[14] These variables also control for two potential wage determinants, market size and rent sharing.

Market size-wage correlation exists in MLB (Burger and Walters 2003). Estimates of previous season firm revenue provide a proxy for the expected market size of each MLB firm. On a firm-by-firm basis, revenue estimates change, on average, by roughly \$8.40 million per year between 2008 and 2011, which is roughly 4% of an average firm's annual revenue (\$206.29 million). Such small year-to-year changes imply that the previous season's revenue estimate supplies a credible proxy for the proceeding season's revenue and market size.

Even when controlling for worker and firm characteristics, firm profit-wage correlation can exist (e.g. Blanchflower et al. 1996; Hildreth and Oswald 1997). Estimates of net operating income control for any rent sharing-wage effects. Because Blanchflower et al. (1996) found significant evidence that lagged ability-to-pay

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[13] For specifics on price adjustments, please see Table A.2 in Appendix A.2.

[14] http://www.forbes.com/lists/2009/33/baseball-values-09_The-Business-Of-Baseball_Rank.html; http://www.forbes.com/lists/2010/33/baseball-valuations-10_The-Business-Of-Baseball_Rank.html; http://www.forbes.com/lists/2011/33/baseball-valuations-11_The-Business-Of-Baseball_Rank.html.

variables most strongly capture rent sharing-wage effects, this study uses net operating income from the preceding season.

In addition to market-size and rent-sharing effects, correlation between firm size and worker wages can exist even with worker and firm controls (e.g. Mellow 1982; Brown and Medoff 1989). Firms in MLB, however, produce teams consisting of exactly 40 players; furthermore, the inherent competitiveness and flow of information among MLB firms should imply near identical numbers and types of trainers, coaches, scouts, and managerial staff. To insure against any omitted variable bias from firm size-wage effects, all regressions use firm fixed effects, which also control for any other static firm characteristic that may influence free-agent wages.

2.5 Team Quality

In MLB, a team's previous season winning percentage, $\tilde{Q}$, provided by Sports Reference and summarized in Table 2.1, provides an obvious and accessible measure for team quality. On a firm-by-firm basis, team winning percentage changes in absolute value, on average, by roughly .055 per season between the 2008 and 2011 seasons. Because approximately 90% of teams in this timeframe finish with a winning percentage between .4 and .6, .055 represents roughly 28% of the available difference among teams' own winning percentages between seasons, suggesting that any team quality-wage estimate that uses $\tilde{Q}$ will be attenuated from measurement error. Additionally, some unobservable variables that help determine winning percentage, such as coaching decisions, may increase free-agent wages, further attenuating any negative team quality-wage estimates.

To better measure free agent and firm expectations about a team's future productivity, a new team-quality variable, $\hat{Q}$, is constructed using Scully's (1974) methodology for estimating winning percentage in MLB. The general equation describing the winning percentage of team f in period t is

$$(2.1)\ Winning\ \%_{f,t} = \alpha_0 + \eta_{f,t-1}^{h,l,p} + Z'_{f,t}\delta + \epsilon_{f,t},$$

where η_{t-1} represents a polynomial for average team hitting (h), fielding (l), and pitching (p) that uses player statistics from the preceding season and Z_t is a vector of team characteristics. A three-step process constructs the team-quality variable, $\hat{Q}$, used in this study: 1) an "expected team" is built for every firm for the season following each free-agent market, 2) equation 2.1 is estimated using each expected team's average statistics, and 3) $\hat{Q}$ is predicted for each team using the estimates from the second step. The final result is a more precise team-quality variable constructed from exogenous team-performance measures and team characteristics. The rest of this section follows these steps and concludes with a discussion of two measurement-error issues that could result from the $\hat{Q}$ construction process.

2.5.1 Step (1): Expected Team

Each "expected team" comprises the best available position player for each of the eight fielding positions, plus a designated hitter for teams in the American League, and eight pitchers comprising the best available five starting pitchers and three relief pitchers.[15] This composition was chosen because it provides a full batting order, complete fielding positions, and because roughly 70% of a season's pitching is done by the most frequently used five starting pitchers and three relief pitchers.

The previous season obp, slg, and fld determine the "best" position players, and strikeouts per walk, $sobb$, and strikeouts per batter, $sobf$, from the previous season determine the "best" pitchers. Similar to the position players productivity statistics, $sobb$ and $sobf$ were chosen because they measure the individual task of pitching, specifically strikeouts and walks. The fielding abilities of a pitcher's teammates have no effect on these measures.

In order to minimize the potential for small sample bias, each position player and pitcher must also have at least 100 plate appearances (PA) or batters faced (BF), respectively, to be eligible. In addition, each position player must play that fielding position at some point during the previous or subsequent seasons.[16] Pitcher selection differs from the position player-selection rule: each pitcher must pitch the majority of his innings at that pitching position. The difference in position eligibility between position players and pitchers reflects the differences in skillsets required to play multiple positions. Differences among most fielding positions are negligible, so position players commonly fill multiple roles, whereas pitchers train as either a starting pitcher or a relief pitcher, rarely both.

[15] Teams in the American League use a designated hitter to replace the pitcher in a team's batting order. This slot is left unfilled in the expected-team roster for National League teams because pitchers are never chosen for hitting ability, and they are uniformly poor batters.

[16] The subsequent season is also used because free agents and firms would know which contracted worker would be playing which position. For example, if a firm already has a great shortstop and contracts another, but has a terrible second baseman, it would be obvious that the new shortstop would replace the remedial second baseman in the following season.

The expected position-player rosters of Bleacher Report, a professional online sports magazine, were published in articles in February, 2010 (Cappetta 2010) and March, 2011 (Trueblood 2011) and provide a metric to measure the credibility of the constructed expected teams. Although Cappetta (2010) and Trueblood (2011) likely use different information to form their expected rosters, e.g. spring-training performance statistics, the position players from the expected teams match 85% of the rosters printed by Bleacher Report, suggesting the expected teams are reasonable. Bleacher Report supplies neither an expected position-player roster for 2009 nor any expected pitching roster for comparison.

2.5.2 Steps (2) and (3): Estimating $\hat{\mathbf{Q}}$

η is a second-order polynomial comprising all the possible linear and quadratic terms of the average fld, obp, slg, $sobb$, and $sobf$ of each expected team; and dummy variables for the division a team plays within compose Z. Pooled OLS is used to estimate equation 2.1, and given these estimates, $\hat{Q}$ is constructed such that $\hat{Q}_{f,t} = \hat{\alpha}_0 + \hat{\eta}^{h,l,p}_{f,t-1} + \hat{\delta}Z_{f,t}$.[17] The resulting values of $\hat{Q}$ differ in absolute value from the actual winning percentages by an average of .036, roughly a 33% improvement over the previous season's winning percentage, $\tilde{Q}$. Such an improvement is sizeable and explains the stronger team quality-wage effects, which are discussed in Section 2.6, when using $\hat{Q}$ instead of $\tilde{Q}$. Table 2.2 shows the summary statistics for $\hat{Q}$ and the corresponding statistics for each actual season.

[17] Please see Table A.3 in Appendix A.2 for the regression estimates of equation 2.1.

2.5.3 Potential Measurement-Error Issues

Because employment decisions after the season begins may occur too far in the future for free agents and firms to anticipate, each expected team is created from the best players contracted to each firm when the season begins. This selection process can create two possibilities for measurement error. The first can occur because free agents and firms sign contracts before the season begins and, therefore, must create values for team quality from estimations of future team compositions. Such free-agent and firm estimations cannot be measured, potentially introducing measurement error into $\hat{Q}$ by differentiating those free-agent and firm estimations from the actual teams used for constructing $\hat{Q}$. Two properties, however, suggest this to be unlikely: 1) $\hat{Q}$ is constructed from expected team-productivity *averages*, and 2 substantial insight into player-team matching is readily available to free agents and firms through professional sports agents; the considerable experience of free agents and firm administrators; the extensive information network among free agents, their sports agents, and firm administrators; the wide media coverage of the free-agent market and inter-firm trades; and the availability of professional market analysis (e.g. ESPN, CBS Sorts, Fox Sports, Elias Sports Bureau, SABR, and countless others). Collectively, these suggest that minimal measurement error exists because free agents and firms should be reasonably able to estimate the *average* team productivities of their future teams.

The second possibility for measurement error can exist if free agents don't include themselves when evaluating potential teams. Many of the better free agents in the sample are used in their respective expected teams, potentially increasing the productivity of those expected teams above those free agents' expectations. A robustness check is provided in Section 2.7.

2.6 Results

This study uses the free market-returns approach, introduced by Krautmann (1999), to construct the following equation:

$$(2.2)\ \log(total\ wages)_{i,f,t} = \beta_0 + \beta_1 \hat{Q}_{f,t} + P'_{i,t-1}\theta_P + X'_{i,t}\theta_X + Y'_{f,t-1}\theta_Y + \theta_f + \theta_t + \varepsilon_{i,f,t},$$

where $\log(total\ wages)_{i,f,t}$ is the logarithm of the total wages free agent i will receive from firm f for the contract signed before the start of season t; $\hat{Q}_{f,t}$ represents team quality for firm f in season t; $P_{i,t-1}$ is a vector of free agent i's performance statistics from season $t-1$, specifically obp, slg, and fld; $X_{i,t}$ is a vector of free agent i's characteristics going into season t, specifically contract length, dummies for free-agent type and position, age and age squared, experience and experience squared, card price, and whether the free agent is moving to a new city; $Y_{f,t-1}$ is a vector containing firm f's revenue and net operating income from season $t-1$; θ_f are firm fixed effects; θ_t are year dummies; and $\varepsilon_{i,f,t}$ is the error.

Table 2.3 reports regression results for equation 2.2 in column 1 and two alternative specifications, which are described in the following subsection, in columns 3 and 5. Columns 2, 4, and 6 contain results for regressions identical to those presented in columns 1, 3, and 5, respectively, except they instead use $\tilde{Q}$ as a measure of team quality. As previously mentioned, $\tilde{Q}$ likely suffers from measurement error, explaining its attenuated estimates and smaller levels of significance. Overall the parallels between $\tilde{Q}$ and $\hat{Q}$ discussed in this section bolster credibility: the attenuation behaves as expected, and both variables imply similar conclusions.

The negative coefficients on $\hat{Q}$ and $\tilde{Q}$ in columns 1 and 2, respectively, which are significant at the 5% level, support CWD theory by suggesting that free agents trade wages for improved team quality. Expected utility gain from increased expected winning and/or investment in human capital could produce such an effect. The first subsection below discusses age in the context of differentiating the two mechanisms, and the second discusses the influence of each mechanism on the team quality-wage relationship.

2.6.1 Mechanism Differentiation

Age effects create an exploitable difference between the team quality-wage relationship expected of younger free agents and their elders. To begin, if team quality notably benefits human capital, significant differences in remaining career lengths between younger and older free agents (Witnauer et al. 2007) should result in team quality-wage effects diminishing as the free agent ages. Conversely, if utility from winning largely drives the results, then the expected utility gain from an investment in team quality should affect younger and older free agents uniformly.

If, however, contracts are "sticky", meaning contracting to a firm increases the chance of re-contracting with that firm in the future, and assuming better teams are generally better in the following seasons, younger free agents will acquire more utility from winning than their elders from the same team-quality investment. Such an age effect would imitate the age effect hypothesized under human capital accumulation, preventing the identification of each mechanism's effect on the team quality-wage relationship. The data, however, suggest contracts are not sticky. As seen in Figure 2.1, regardless of team winning percentage, free agents contract with a *different* team roughly 70% of the time. Furthermore, younger free agents contract with a different team even more frequently, roughly 85% of the time.

2.6.2 Influence of Potential Mechanisms

To test the importance of each mechanism to the team quality-wage relationship, the estimates reported in columns 3 and 4 of Table 2.3 include the interaction between both $\hat{Q}$ and $\tilde{Q}$, respectively, and a free agent's age, allowing team quality-wage effects to adjust by age. In order to allow the coefficients on $\hat{Q}$ and $\tilde{Q}$ to capture the team quality-wage effect for a 28-year-old free agent, instead of a 0-year-old free agent, the age in the interaction term is rescaled by subtracting 28.[18]

The coefficients on $\hat{Q}$ and $\tilde{Q}$ again show a negative and significant relationship between team quality and wages, and, more interestingly, the positive coefficients on the interaction terms suggest that the team quality-wage relationship diminishes as free agents age. This result could imply that free agents lose interest in team quality as they age, supporting the hypothesis that team quality supplies a human capital investment option. The insignificance of the interaction terms,

[18] For more information on the age distribution, please see Figure A.1 in Appendix A.3.

however, could signal that no real difference in the team quality-wage relationship exists among free agents of different ages, supporting utility from winning as a motivation behind the observed team quality-wage effect.

To see whether there exists a difference in sensitivity to team quality between the young and their elders, equation 2.2 is modified to include discrete age categories. Three such categories are constructed, each comprising roughly a third of the data: free agents 31 and younger (D_{Young}), free agents aged 32 to 34 (D_{Middle}), and free agents 35 and older (D_{Old}). Each age-category dummy is interacted with team quality, either $\hat{Q}$ or $\tilde{Q}$, thereby measuring the team quality-wage effect for its specific age category. All three interaction terms replace the team-quality variable, and D_{Young} and D_{Old} replace the age quadratic in Y, to form the equation

$$(2.3)\ \log(total\ wages)_{i,f,t} = \delta_1 D_{Young} Q_{f,t} + \delta_2 D_{Middle} Q_{f,t} + \delta_3 D_{Old} Q_{f,t} + D_{Young} + D_{Old} + \Gamma_{i,f,t} + \epsilon_{i,f,t},$$

where Q corresponds to $\hat{Q}$ in column 5 and $\tilde{Q}$ in column 6, Γ contains all other relevant variables and parameters from equation 2.2, and ϵ is the error.

The negative team-quality estimates for the younger free agents have large magnitudes and are significant at the 1% level in both columns 5 and 6. The estimates for the more mature free agents, in contrast, are relatively small in magnitude and insignificant, even at the 10% level. Such results indicate that the young are much more sensitive to team quality than their elder counterparts, an age effect consistent with an underlying human capital investment mechanism and contradictory to a mechanism strictly determined by utility from winning. Additionally, the coefficient on the young in column 5 is statistically different from the corresponding coefficient on the middle aged at the 10% level, and the coefficients on the young in both 5 and 6 are close to being statistically different from each of the other corresponding age categories (the largest p-value is .167). Such results are consistent with team quality affecting young and elder free agents differently.

The negative sign on the estimates across all ages, however, suggest utility from winning may have some small effect on the team quality-wage relationship. Furthermore, the failure to reject that the estimates on the young and old are statistically different at the 10% level in either column may provide evidence that some older free agents do desire to win before they retire, an interesting phenomenon observed in many sports.

The estimates in column 5 correspond to a young free agent trading roughly 25% of his total wages to increase his expected winning percentage by .05, a change of roughly one standard deviation. Such a trade may seem large, but this investment may be appropriate for young free agents who should work another 4 to 5 years in MLB (Witnauer et al. 2007), the equivalent of roughly two to three additional contracts. Preventing a decrease in or, less likely, increasing obp, slg, and card price by one standard deviation each, roughly .035, .073, and .106, respectively, prevents a decrease in total wages by roughly 30%, 16%, and 28%. Assuming a young free agent maintains this human capital difference into two or three subsequent contracts, preventing a decrease in each measure by 19% or 13% of one standard deviation, respectively, is enough to repay his initial investment.

It is important to note that this break-even calculation considers neither fld nor any expected utility gain from increased winning. Although the estimates on fld are insignificant, fielding ability likely has some small positive effect on wages, potentially decreasing the amount of human capital accumulation in each attribute needed to break even. As previously mentioned, utility gain from winning may contribute to the team quality-wage effect, potentially implying that young free agents invest less than 25% of their wages in accumulating human capital. The likelihood that either factor could affect wages suggests that the above break-even calculations form upper bounds on the human capital accumulation needed to justify the estimated investment in team quality.

2.7 Robustness Checks

This section tests whether the "luxury tax", wage floor, or unaccounted contract incentives mentioned at the beginning of Section 2.4.1 or the potential for measurement error in $\hat{Q}$ discussed in Section 2.5.3 drive the results in Table 2.3. Each issue is addressed using equation 2.3 and is contrasted against the coefficient estimates on $\hat{Q}$ from column 5 in Table 2.3, which are provided in column 1 of Table 2.4 for convenience. The results discussed below are reported in columns 2 through 8 in Table 2.4, and the concluding subsection summarizes the correlation between of all the robustness checks and the main results.

The luxury tax is a tax on the total money spent on player salaries exceeding a predetermined level. Of the 90 teams composing the 2009, 2010, and 2011 seasons, only 5 (created by the New York Yankees and Boston Red Sox firms) paid any luxury tax, and no tax exceeded 6% of any team's revenue.[19] Column 2 in Table 2.4 reports estimates from equation 2.3 that exclude free agents contracting with these 5 teams. If the luxury tax does create a wage distortion, it should lower the wages of the free agents whose wages cause their teams to be taxed. This prospect, combined with the high team quality of teams built by the Yankees and Red Sox, suggests that the estimates of team quality-wage effects in column 1 should have larger magnitudes than the estimates excluding free agents from the 5 qualified teams. The opposite occurs, most likely explained by both firms' relative propensity to overspend on free agents, particularly the infamous New York Yankees.[20]

The wage floor was set at $400,000 for each of the three seasons. The only time this wage floor binds occurs when a team has an open roster spot with no available, appropriately priced free agents to fill it. In such a situation, a firm will adhere to the wage floor and overpay a lower value free agent to fill the vacancy on its team. The large pool of players, both in MLB and its minor league affiliates, should ensure this rarely happens. The dataset contains only two examples, and these free agents may be worth the wage. Estimates from a Tobit regression are provided in column 3, and the unchanged coefficient estimates suggest that the wage floor produces no wage distortions in the data.

[19] Please see Table A.4 in Appendix A.2 for more information on the percentage of revenue paid in luxury taxes.

[20] Comparing the Yankee and Red Sox workers' predicted log wages against their actual log wages suggests that, on average, the Yankees and Red Sox overpay their workers by .620 log-dollar units, a large amount considering the average log wage is 6.499.

The contract data from ESPN do not contain information on performance or playing-time bonuses. Only the very top performing free agents receive any performance incentives in their contracts and only the oldest free agents receive playing-time bonuses. The small number of contracts containing these provisions and the small bonus amounts suggest insignificant distortions on wages. For example, winning the MVP award, a silver slugger award, *and* being selected as an All-Star in 2010 would have earned David Ortiz a total bonus of $250,000, less than 2% of his guaranteed wage for that season. Columns 4 through 7 in Table 2.4 report estimates excluding roughly the top 10% of free agents in obp, slg, fld, and age.[21] The estimates in column 7 suggest that playing-time bonuses create limited, if any, wage distortions; but the attenuation of the estimates in columns 4 through 6 of Table 2.4 could imply that wage distortions from performance bonuses exist in the data. This, as previously mentioned, is considerably unlikely. The more likely explanation is data loss. The most productive free agents have the most to lose and may value team quality disproportionately more than their less productive coworkers, so eliminating them could be reducing the average effect of team quality on wages. Regardless, the estimates in columns 4 through 7 support team quality as a CWD and imply that the young are the principal free agents affected.

As previously mentioned, if free agents do not include themselves when evaluating potential teams, $\hat{Q}$ could overstate team quality for each free agent who is

[21] The samples used in columns 4 – 7 exclude all free agents with an obp of .374 or higher, roughly 10.22% of the sample; a slg of .497 or higher, roughly 10.22% of the sample; a fld of 1, roughly 9.68% of the sample; and an age of 39 or above, roughly 7.53% of the sample, respectively.

used in his own expected team. Such measurement error, however, is unlikely for two reasons: 1) firms that actively pursue a given free agent are pursuing that free agent to attain a set level of team productivity; and 2) given the requisite resources to actively pursue such a free agent, those firms have many options available to achieve that same level of team productivity, either through alternative free agents and/or inter-firm trades. In such a context, each free agent that is included in his own expected team can be interpreted as a proxy for the various alternatives available to that firm to achieve its team-productivity goal, implying the need to include that free agent in his expected team. Regardless, to account for the potential for measurement error, new $\breve{Q}$ values are created from team averages that exclude each free agent from his own expected team. Team quality-wage estimates using $\breve{Q}$ are provided in column 8 in Table 2.4. Contrary to the estimates using $\hat{Q}$, the coefficient on the old is positive and, therefore, does not support utility from winning as a mechanism that contributes to the negative team quality-wage relationship. Aside from that one exception, however, the estimates imply the same conclusions as those drawn from the estimates using $\hat{Q}$.

2.7.1 Summary

The magnitudes and significances of the estimates and the relationships between the estimates on the young and the more elder free agents in columns 2 through 8 collectively support the conclusions derived from the main findings discussed in the previous section. Specifically, the results support team quality as a CWD. Additionally, they are also consistent with a human capital investment mechanism and, with the exception of the estimates in column 8, could also suggest a small effect from the utility-from-winning mechanism. Even accounting for hypothetical wage distortions and measurement error, the estimates in Table 2.4 imply that young free agents trade at least 19% of their wages to join a team with an expected winning percentage one standard deviation higher.

2.8 Conclusion

Does membership in a productive team matter to workers? The results of this study indicate that workers do indeed trade wages for team quality, even when controlling for worker productivity, firm fixed effects, and a set of time-varying worker and firm characteristics. This is likely the first study ever to do so. The results are insensitive to a set of robustness checks and are supported by two measures of team quality. The analysis in this study improves on previous studies of compensating differentials for team quality by using firm fixed effects to control for permanent firm features correlated with team quality and by matching workers with their future team, allowing for controls of time-varying worker and firm characteristics that correlate with team quality. In addition, spillovers and complementarities among teammates are minimal in the chosen industry, thereby minimizing estimation bias from measurement error in worker productivity and improving the quantification of team quality relative to other industries.

The significant team quality-wage estimates for young free agents, coupled with the insignificance of the estimates for older free agents, suggest that young free agents drive the negative team quality-wage effect, a result consistent with workers preferring better teams because those teams offer some benefit to human capital. Whereas skills learned from abler teammates may, in principle, be firm-specific (Becker 1962), occupation-specific (Shaw 1984; Kambourov and Manovskii 2009), or task-specific (Gibbons and Waldman 2004; Gathman and Schönberg 2010), it seems likely that task-specific human capital may be the most appropriate investment for MLB free agents: fielding and hitting skills are universally sought at all non-pitching positions; however, occupation-specific skills are also required for each specific position.

The presence of such learning-related wage effects can be tested for indirectly in industries other than MLB, even if the data are deficient. Specifically, do firms with higher team quality pay younger workers less than their value to the firm, while more mature workers receive wages more aligned with their value? Studying team-quality effects in graduate school enrollment, corporate internship programs, or entry-level jobs may provide insight into the matching mechanisms between worker and firm.

In addition to productivity-related human capital, at least two alternative mechanisms might help account for younger free agents' willingness to accept lower wages to join better teams: membership on a high performing team may increase the fame of a worker, and/or being part of a superior team may provide an improved signal for firms hiring in the future. Turning first to fame, if team productivity increases worker fame, free agents may invest in team quality to collect additional future wages from improved fame. Not all increases in fame may be universal;

instead, fame may be market-specific, therefore firm-specific. For example, a position player with the Boston Red Sox may accrue vastly more fame in the Boston market than in the more universal MLB market. Changing firms would reduce the firm-specific fame capital linked to the Boston Red Sox. Distinguishing between universal and firm-specific fame effects on wages, particularly when accounting for team quality-wage effects, may illuminate more about the superstar effect and team quality-investment decisions.

Alternatively, free agents may believe that membership in better teams improves their productivity signal. Productivity signaling, however, is unlikely to be the sole mechanism: the high availability of objective productivity measures imply that free-agent productivity should be readily identifiable. As the signaling theory of Spence (1973) argues, discernibility eliminates the need for signals. Many unobservable skills that correlate with team productivity may exist, however, such as being "clutch" or instilling confidence in teammates.[22] Being associated with a highly productive team may signal firms that a free agent possesses these valuable yet unobservable skills.

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[22] This study does not control for these unobservable skills, nor does it need to. These types of skills would increase wages (otherwise free agents would not invest in them) and would correlate positively with team quality; therefore, any bias would attenuate negative team quality-wage effects toward zero. If they do exist, then the estimates in Table 2.3 represent a lower bound on the negative relationship between team quality and wages.

If, as the estimates suggest, workers are willing to accept lower wages to join firms with more productive teams, then some interesting implications for the competitiveness of labor and product markets follow. If firms that start with a small productivity advantage can attract good workers more cheaply than other firms, clustering of high-skill workers could lead to productivity stratification across teams. Ultimately, this may limit competition and reduce product variety in an industry. Interestingly, the history of MLB provides anecdotal evidence supporting this idea. All competition for professional baseball has either been eliminated or absorbed by MLB since its founding in 1869. Player productivity is also highly clustered: the skill of a player dictates participation in a specific set of teams, or “league”, whether the major league or one of the six different levels of minor leagues owned by MLB. Recently, MLB may have (partially) countered the negative externalities to competitiveness that good teams impose on bad ones by instituting regulations on amateur player recruitment that benefit the less competitive teams from previous seasons.

TABLES

Table 2.1
Summary Statistics

Variable	Position Players				
	Obs	**Mean**	**St Dev**	**Min**	**Max**
Log of Total Wages [$\log_{10}$($)]	186	6.499	.567	5.615	8.276
Contract Length [years]	186	1.516	1.150	1	8
On-Base %, obp	186	.332	.035	.244	.439
Slugging Ratio, slg	186	.408	.073	.200	.601
Fielding %, fld	186	.982	.017	.857	1.000
Baseball Card Price [$]	186	.149	.106	0	1.00
Age [years]	186	33.532	3.200	26	43
Experience [years]	186	10.301	3.315	3	21
Moving to a New City	186	.757†	---	---	---
Type-A Free Agent	186	.113†	---	---	---
Type-B Free Agent	186	.129†	---	---	---
No-Type Free Agent	186	.758†	---	---	---
Position: Outfield	186	.328†	---	---	---
Position: Infield	186	.468†	---	---	---
Position: Catcher	186	.204†	---	---	---
Free Agents in 2008 Market	186	.280†	---	---	---
Free Agents in 2009 Market	186	.382†	---	---	---
Free Agents in 2010 Market	186	.339†	---	---	---
Firm Revenue [$ millions]	90	206.286	54.097	144.56	463.050
Firm Net Operating Income [$ millions]	90	17.540	14.254	-30.975	48.405
Previous Season Winning %, $\tilde{Q}$	90	.500	.068	.352	.636

The sample comprises 186 free agents contracting to 90 teams. Wages, revenue, and net operating income are inflation adjusted to 2011 dollars. Card prices are matched to 2011 price categories to insure consistency across years (see Table A.2 in Appendix A.2). † "Mean" values correspond to the proportion of free agents in the sample that fall into each category.

Table 2.2
Constructed Team Quality & Actual Winning %

Season	Season Averages	
	$\widehat{Q}$	Actual Winning %
2009	.500 (.049)	.500 (.069)
2010	.502 (.048)	.500 (.067)
2011	.498 (.057)	.500 (.069)

Table 2.3
Main Results

Dependent: log($total\ wages$)	No Interaction		Age Interaction		Dummy-Age Interaction	
	$\widehat{Q}$ (1)	$\widetilde{Q}$ (2)	$\widehat{Q}$ (4)	$\widetilde{Q}$ (3)	$\widehat{Q}$ (5)	$\widetilde{Q}$ (6)
Team Quality (Q)	-1.503** (.700)	-1.208** (.578)	-2.572*** (.866)	-1.698** (.826)		
Team Quality (Q) x Age			.244 (.168)	.081 (.093)		
Team Quality (Q) x D_{Young}					-2.526*** (.689)	-2.130*** (.780)
Team Quality (Q) x D_{Middle}					-1.013 (.824)	-.955 (.726)
Team Quality (Q) x D_{Old}					-.616 (1.321)	-1.115 (.708)
On-Base % (obp)	3.388*** (.920)	3.630*** (.943)	3.523*** (.930)	3.628*** (.955)	3.2139*** (.905)	3.338*** (.927)
Slugging Ratio (slg)	.702 (.445)	.662 (.452)	.700 (.442)	.680 (.455)	.860* (.446)	.857* (.457)
Fielding % (fld)	2.154 (1.395)	2.119 (1.524)	1.751 (1.287)	1.938 (1.495)	1.838 (1.313)	1.707 (1.540)
Card Price	1.012*** (.210)	1.020*** (.218)	.937*** (.214)	1.007*** (.222)	1.016*** (.220)	1.057*** (.231)

***** denotes significance at the 10% level, ****** denotes significance at the 5% level, and *** denotes significance at the 1% level. All regressions control for contract length, free-agent productivity, type, position, age, age squared, experience, experience squared, fame, moving to a new city, market size, rent sharing, firm fixed effects, and year effects. Each regression uses 186 observations. Columns 1 and 2 have 132 d.f., 3 and 4 have 131 d.f., and 5 and 6 have 130 d.f. Regressions presented in columns 5 and 6 use dummies for D_{Young} and D_{Old} instead of a quadratic in age. Heteroskedasticity-robust standard errors are given in parentheses.

Table 2.4

Robustness Checks

Dependent Variable: log(*total wages*)	Comparison Regression (Table 2.3, Column 5)	Excluding Free Agents on Luxury-Tax Teams	Tobit	Excluding Top Performers			Excluding Oldest	Excluding Free Agents from Expected Team
				obp	slg	fld		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Team Quality ($\hat{Q}$) x D_{Young}	-2.526*** (.689)	-2.727*** (.683)	-2.516*** (.590)	-2.036** (.790)	-1.885** (.746)	-2.180*** (.750)	-2.718*** (.725)	-2.281*** (.714)
Team Quality ($\hat{Q}$) x D_{Middle}	-1.013 (.824)	-1.32* (.798)	-.991 (.707)	-1.462 (.943)	-1.266 (.910)	-.896 (.956)	-1.258 (.862)	-.497 (.770)
Team Quality ($\hat{Q}$) x D_{Old}	-.616 (1.321)	-1.163 (1.270)	-.606 (1.132)	-1.175 (1.280)	-1.100 (1.359)	-.129 (1.599)	-.229 (1.532)	.409 (1.380)

* denotes significance at the 10% level, **denotes significance at the 5% level, and *** denotes significance at the 1% level. All regressions control for contract length, free-agent productivity, type, position, age, experience, experience squared, fame, moving to a new city, market size, rent sharing, firm fixed effects, and year effects. Column 2 uses 175 observations and has 125 d.f, columns 3 and 8 use 186 observations and have 135 d.f., columns 4 and 5 use 167 observations and have 116 d.f., column 6 uses 168 observations and has 117 d.f., and column 7 uses 172 observations and has 121 d.f. Heteroskedasticity-robust standard errors are provided in parentheses.

FIGURES

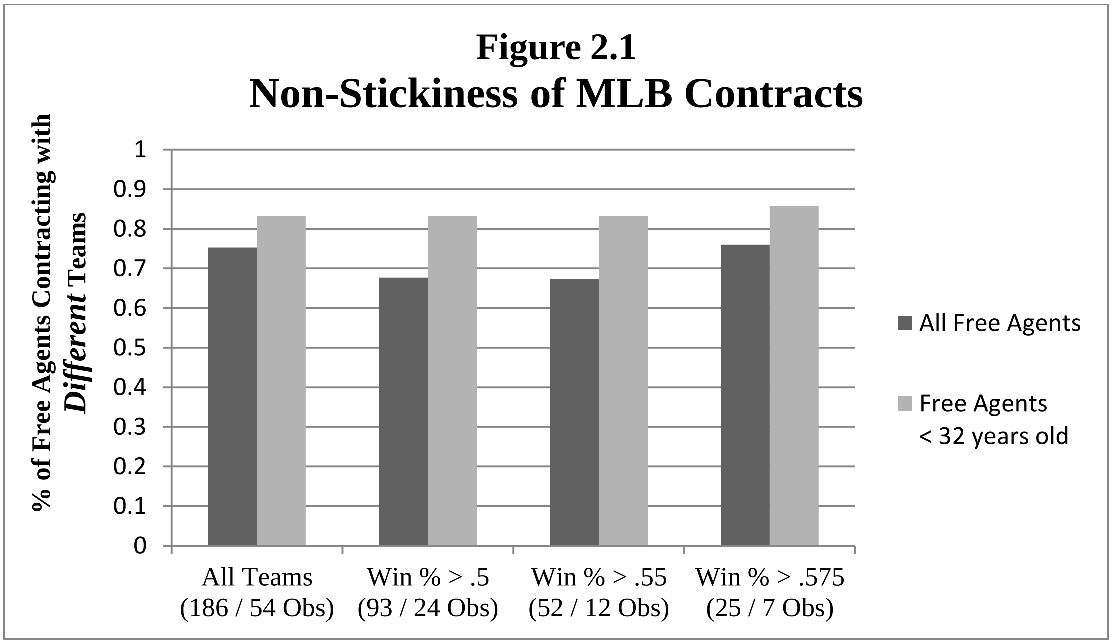
Figure 2.1
Non-Stickiness of MLB Contracts
% of Free Agents Contracting with *Different* Teams
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
All Teams (186 / 54 Obs)
Win % > .5 (93 / 24 Obs)
Win % > .55 (52 / 12 Obs)
Win % > .575 (25 / 7 Obs)
All Free Agents
Free Agents < 32 years old

CHAPTER 3

THE EFFECTS OF WORKER PRODUCTIVITY UNCERTAINTY ON THE LENGTH OF CONTRACTS

3.1 Introduction

Although a number of studies examine the determinants of wages, benefits, and other features of labor contracts, relatively few investigate the determinants of contract length. One topic within this literature, uncertainty, has received a large share of the theoretical and empirical attention. The initial theoretical hypothesis, the efficient-production hypothesis, suggests that increases in uncertainty, regardless of their sources, increase contract length (Gray 1978). Generally, unanticipated nominal or real shocks, i.e. uncertainty, create deviations between wages and their market-clearing levels, causing increasing efficiency losses over time and incentivizing workers and firms to shorten the length of contracts. It was not until Danziger's (1988) efficient risk-sharing hypothesis that increases in the real uncertainty of worker productivity were hypothesized to increase contract length. Danziger's (1988) model theorizes that more risk-neutral firms supply risk-averse workers with wage insurance in the form of longer contracts, associating higher levels of productivity uncertainty with longer contract length.

This study tests the contract-length effects of real uncertainty and the positive relationship implied by Danziger's (1988) efficient risk-sharing hypothesis. Unlike nominal uncertainty, real uncertainty has received little attention with little consensus among the empirical findings. Kanago (1998) and Rich and Tracy (2004) find evidence of a negative relationship between real uncertainty and contract length. Conversely, Murphy (2000) reports results that real uncertainty increases contract length, supporting Danziger's (1988) hypothesis. Wallace and Blanco (1991) and

Wallace (2001) find mixed results that depend upon the industry being studied, suggesting that some industries may have multiple mechanisms affecting contract length simultaneously (Harris and Holmstrom 1987).

This is likely an important problem. Apart from the efficient-production hypothesis, five other factors may complicate the empirical analysis of the efficient risk-sharing hypothesis. Gray (1978) first theorized that both contracting costs and indexation, such as a cost of living adjustment, should increase contract length. Contracting costs increase the costs of re-contracting, providing an incentive to minimize such occurrences; indexation mitigates the efficiency loss from increased uncertainty, thereby lengthening contracts. Similarly, investment by a supplier and/or purchaser in characteristics of their relationship can increase the value of that relationship, thereby also increasing contract length (e.g. Joskow 1987; Crocker and Masten 1988; Brickley et al. 2006; Bandiera 2007). In contrast, Harris and Holmstrom (1987) theorize that increased information costs (i.e. renegotiating) may decrease contract length. As uncertainty increases, the multi-period value of information decays, decreasing the value of new, costly information and shortening contract length. Finally, the bargaining-power differential between a contracting supplier and purchaser can produce various contract-length effects (e.g. Hendricks and Kahn 1983; Murphy 1992).

Controlling for each of these mechanisms may be infeasible when using union- and industry-level datasets. Wallace and Blanco (1991) describe this problem perfectly: “The sensitivity of contract length to firm-specific shocks cannot be directly investigated because data limitations prevent an approach more disaggregated than the [industry] level." It may come as a surprise then that no previous studies use individual-level contracts with worker and firm controls. This study is the first analysis of the real uncertainty-contract length relationship to do so.

This should be particularly surprising because Danziger (1988) characterizes his efficient risk-sharing hypothesis in the context of worker productivity uncertainty. Such a deficiency in the literature may be explained by a problem inherent in most worker productivity measures. Workers commonly work in teams, affecting each other through spillovers in effort levels (e.g. Falk and Ichino 2006; Mas and Moretti 2009) and complementarities among skillsets (e.g. Lazear 1999; Hamilton, Nickerson, and Owan 2003). Each of these team production factors modifies a worker's observed productivity through peer effects specific to the observed team, thereby introducing team-specific measurement error and limiting the quality of any derived measure of uncertainty.

This study uses data from the 2008 to 2010 Major League Baseball (MLB) free-agent market. The remainder of this paper is organized as follows. Section 3.2 discusses the characteristics of the free-agent market that minimize the competing-mechanism and spillover-complementarity problems and, therefore, provide an ideal environment to test Danziger's (1988) efficient risk-sharing hypothesis. Section 3.3 introduces the free-agent market. Section 3.4 describes the data and covariates used in the analysis of contract length. Section 3.5 details the construction of the uncertainty measures used in this study. Section 3.6 introduces the empirical strategy, discusses the results from the real uncertainty-contract length analysis, and addresses the robustness of those results. Section 3.7 concludes the paper.

3.2 Suitability of MLB

Danziger's (1988) efficient risk-sharing hypothesis requires several key environmental factors, each present in the MLB free-agent market, to hold. Additionally, several characteristics of the free-agent market either eliminate or control for the competing mechanisms described above. Finally, the data provides productivity measures that should be minimally affected by either effort spillovers from or skillset complementarities among teammates. The following subsections address each of these characteristics of the data.

3.2.1 Efficient Risk-Sharing Hypothesis

Three environmental conditions are necessary in order for the efficient risk-sharing hypothesis to hold: 1) a sufficient gap in risk aversion exists between worker and firm, 2) uncertainty exists in worker productivity, and 3) wages depend sufficiently on worker productivity. Without the first condition, firms may not be sufficiently risk neutral – relative to their workers – to insure their workers against wage uncertainty. The two subsequent conditions ensure that workers face the necessary wage uncertainty to want insurance.

All three of these conditions are interrelated in MLB. Turning first to the firms, despite high variance in team winning percentage between 2007 and 2011, firms' revenues are largely stationary over the same time frame (see Chapter 1), implying short-term changes in aggregate player productivity, i.e. team winning percentage, produce little revenue uncertainty, and therefore risk, for firms. This is discussed further in the following subsection. Free agents, conversely, should be highly risk averse with respect to their productivity because their outside wage option(s) is likely substantially lower than the minimum major-league wage ($400,000 per season) and small dips in performance decrease future pay substantially (e.g. see Chapter 2; Krautmann 1999). The latter characteristic is particularly salient because, as discussed in Section 3.5, free-agent productivity is reasonably volatile. Taken together, the characteristics of both firms and free agents provide the conditions necessary for the efficient risk-sharing mechanism to affect MLB contracts.

3.2.2 Competing Mechanisms

If not adequately addressed, several alternative mechanisms – the efficient-production mechanism, bargaining-power differentials, indexation, information costs, contracting costs, and relationship investment – could complicate the analysis of the efficient risk-sharing hypothesis. Turning first to the efficient-production mechanism, two conditions are necessary for it to hold: 1) uncertainty exists in worker productivity, and 2) firm revenue depends sufficiently on worker productivity over the length of the contract. The first condition introduces risk into the system; the second condition ensures that this risk affects firms. Without the second condition, firms' contracting decisions will be unaffected by the uncertainty in production, resulting in contract length being unaffected. MLB fails the second condition. As discussed in Section

3.4.1, free agent contracts are relatively short, so only the short-term relationship between firm revenue and productivity is relevant; however, there is little relationship between the two (see Chapter 1). Following the method discussed in Section 1.2 and regressing revenue from the 2008 to 2011 seasons on winning percentage, season dummies, and firm fixed effects yields an estimate of .410 on winning percentage.[23] Although the estimate is significant at the 10% level, .410 corresponds to an increase of roughly \$2.79 million, which is roughly 1.34% of average firm revenue, for a full standard deviation increase in winning percentage, roughly .068. Considering each free agent produces, at most, during roughly 11% of his team's available plate appearances, variations in his productivity should affect firm revenue minimally (see Chapter 1).[24] This result suggests that productivity uncertainty likely does not affect contract length through the mechanism described by Gray (1978).

Such a small impact on firm revenue could still affect the relationship between uncertainty and contract length if firms have significant bargaining power over free agents. This, however, is also not a characteristic supported in the MLB free-agent market. Firms generally pay free agents either more (see Chapter 1) or close to their marginal revenue product (e.g. Zimbalist 1992; Krautmann 1999; Mullin and Dunn 2002). Likewise, neither indexation nor information costs should affect free-agent contracts. Indexation does not exist in MLB contracts, and as discussed in the

[23] These years cover each of the seasons that either precedes or follows the free-agent markets used in this study.
[24] Each non-pitching player, who is currently playing in a game, occupies one of nine positions on a team's batting order. Assuming a free agent plays every inning in every game of a season, he will occupy roughly $\frac{1}{9}$ of all potential plate appearances.

following subsection, information costs are homogenous and essentially zero among firms and free agents. Finally, controls exist for both contracting costs and relationship investment. These are discussed in Section 3.4.

3.2.3 Spillover and Complementarity Problem

Worker productivity is readily observable in MLB. Productivity statistics are collected by MLB and a collection of amateur enthusiasts and media professionals (e.g. Fox Sports, USA Today, Baseball Prospectus, ESPN), resulting in many accurate and publically available measures of productivity. Countless studies that use MLB productivity data assume that worker productivity measures are minimally affected by spillovers and complementarities. Although not specifically testing such assumptions, Section 2.2 does posit and defend four main points that collectively imply such a property for the productivity measures used by this study: 1) each player puts forth top effort independently of spillovers from teammates because individual productivity is heavily monitored and replacement players are readily available (e.g. Krautmann 1990; Maxcy et al. 2002); 2) the skillset of each worker comprises individual-only tasks, such as throwing a ball, implying independence from complementarities from teammates' skillsets; 3) available performance statistics generally exclude batter and fielder actions, such as a fielding error, that artificially inflate or deflate the opposing fielder's or batter's productivity measure; and 4) despite joint production between batters and pitchers, each team's rest schedule for its players, the competition schedule for a season, and the large number of different opponents that each player competes against suggest minimal systematic effects from production spillovers or complementarities between batters and pitchers.

3.2.4 Summary

The MLB free-agent market contains the three environmental conditions necessary for the efficient risk-sharing hypothesis to hold. The dataset should also be independent of the efficient-production mechanism as well as free from indexation, information-cost, and bargaining-power effects. Controls for both contracting costs and relationship-investment effects are also available. Finally, the productivity measures, which are discussed in Section 3.4.2, should be minimally affected by spillovers and complementarities. Collectively, these characteristics provide a dataset well suited to test Danziger's (1988) efficient risk-sharing hypothesis.

3.3 Free-Agent Market

The MLB free-agent market begins immediately following the World Series in late October and mostly concludes by February. Each player becomes eligible for the free-agent market through one of three methods: 1) the player has completed at least six years in MLB's major league when his contract expires; 2) the player's 10-year contract with the Japanese major leagues, purchased by a MLB firm, expires; or 3) the player's contract expires and no contract was offered to him by the tender deadline. The large majority of free agents spend at least eight years in MLB and its minor league affiliates before entering the market through the first method, yielding an older and far more experienced sample relative to the MLB population.

Rule changes and poor player evaluations by firms limit the amount of usable data. New rules governing the free-agent market were introduced into the 2006 market and finished phasing in during 2007, and modifications to these rules were introduced into the 2011 market. As Hakes and Sauer (2006) point out, the majority of MLB teams did not evaluate batters effectively until around the middle of the 2000's decade. These two factors result in three years of high-quality data: the 2008, 2009, and 2010 free-agent markets. ESPN supplies the list of free agents and their contracting firm for each of these three years.[25]

3.4 Data

This study compiles 182 observations from the 2008 to 2010 free-agent markets into a repeated cross-section. Like many analyses using MLB data, this study focuses on non-pitchers. Pitchers are generally divided into two very different subgroups, "starting pitchers" and "relief pitchers", raising analytical problems when using smaller datasets. Each subgroup is trained differently, used differently, and valued differently, requiring starting- and relief-pitcher data to be analyzed separately; however, too few observations of each pitching subgroup exist for reliable analysis. The rest of this section discusses the data on worker-firm contracts, worker characteristics, and firm characteristics. The summary statistics for each variable discussed below are presented in Table 3.1.

[25] http://espn.go.com/mlb/freeagents/_/year

3.4.1 Worker-Firm Contracts

The contract-length regressions in this study use the number of seasons specified in each free agent's contract as the dependent variable. Contracts are guaranteed and cannot be broken or changed once signed. The average contract length observed in the sample is roughly 1.5 years with a standard deviation of a little more than a year, meaning players generally negotiate a new contract every couple of years. A histogram showing the contract-length profile of the data, which are supplied by ESPN, is provided in Figure 3.1.[26]

Unlike most other professional sports leagues, no firm salary caps or free agent-wage restrictions exist, minimizing the possibility that firms substitute extended contracts for wages. Each market, however, contains different skill distributions and quantities of free agents, potentially producing some year-specific effects on free-agent contracts. All regressions use year-specific dummy variables to control for any corresponding effects.

Finally, costs on trading type-A and B free agents may affect contracts. Free agents are segregated into "types" by their productivity: type A, those in the top 20% for their position; type B, those within the top 21-40%; and those with no type in the bottom 60%. MLB contracts Elias Sports Bureau to determine which free agents are in the top 20% for their position (type A) and which are within the top 21-40% (type B). The algorithm is proprietary and not available to the public. Dummy variables controlling for free-agent type are included in each regression.

[26] http://espn.go.com/mlb/freeagents/_/year

3.4.2 Worker Characteristics

Sports Reference supplies the player productivity statistics used by this study.[27] On-base percentage, obp, which measures the likelihood that a batter successfully gets on base, and slugging ratio, slg, which measures the power of the batter, measure free-agent production.[28] Defensive production is not included because wages do not depend sufficiently on such productivity (see Chapter 2), thereby violating the third environmental condition necessary to support the efficient risk-sharing hypothesis. Each statistic was chosen for two reasons: 1) as argued in Section 2.2, each is determined by individual player productivity and should be minimally affected by production spillovers and complementarities, and 2) each has become a standard productivity measure in analyses that use MLB data. Because future productivity is unknown, the previous season's productivity is used (Quirk and Fort 1992).

This study uses data from ESPN and Sports Reference to control for free agents moving to different cities and their position, age, and experience. The nonpecuniary value of maintaining a continuous residence may provide an incentive for free agents to negotiate longer contracts lengths with their previous team or with a team based in their current city. Roughly 24% of the free agents in the sample sign with a firm in their current city. All regressions include a dummy variable that controls for free agents moving to a new city.

Free agents may require different levels of firm investment in order to adequately produce in different fielding positions. For example, a catcher must learn

[27] http://www.baseball-reference.com

[28] Please see Table B.1 in Appendix B.1 for the formula for each performance statistic.

to read his pitchers' abilities and those of the opposing teams' batters to choose appropriate pitches. Evidence from the National Football League (Tang 2013) suggests that increasing such relationship-investment costs for specific positions correlates positively with increased contract length. Each regression includes position dummies to control for any investment costs associated with fielding position.

Free agents, as previously mentioned, are generally older and more experienced than their non-free-agent counterparts. The approximate average age of 32 and experience of 10 years show this characteristic of the dataset. Furthermore, age and experience could be associated with productivity uncertainty in MLB. Players, as they age, generally get weaker and slow down, potentially leading to more sporadic production; conversely, as they accrue more experience, they may improve their capabilities and become more consistent. Both are included as controls.

3.4.3 Firm Characteristics

Market size is a potential contract-length determinant. Free agents may want to extend contracts with firms based in larger markets, possibly to accrue fame or otherwise enjoy the advantages of a larger fan base. This study uses estimates of previous season firm revenue, supplied by Forbes, as a proxy for the expected market size of each MLB firm.[29] As illustrated in Figure 3.2, on a firm-by-firm basis, revenue estimates change, on average, by roughly $8.40 million per year between 2008 and 2011, which is roughly 4% of an average firm's annual revenue ($206.29 million).

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[29] http://www.forbes.com/lists/2009/33/baseball-values-09_The-Business-Of-Baseball_Rank.html; http://www.forbes.com/lists/2010/33/baseball-valuations-10_The-Business-Of-Baseball_Rank.html; http://www.forbes.com/lists/2011/33/baseball-valuations-11_The-Business-Of-Baseball_Rank.html.

Such year-to-year changes imply that the previous season's revenue estimate supplies a reasonable proxy for the proceeding season's revenue and market size. In addition to controlling for market size, all regressions use firm fixed effects, which control for any static firm characteristic, such as weather or stadium features, that may influence contract length.

3.5 Productivity Uncertainty

The chosen productivity measures, obp and slg, are inherently noisy. Over a free agent's previous three seasons, the average standard deviation of his own obp and slg is .024 and .047, which correspond to changes of roughly 19% and 10% of an average free agent's wages, respectively. Such volatility should provide sufficient uncertainty to incentivize the more unpredictable free agents to seek longer contracts.

This study uses two measures of uncertainty for each of the two measures of productivity. Because contracts are formed before the next season begins, all measures of uncertainty for season t are formed from information available at the conclusion of season $t-1$. Each measure is derived from the magnitude of the residual, $\hat{\varepsilon}_{t-1}^{P}$, that results from the productivity forecast

$$(3.1)\ P_{i,t-1} = \alpha_0 + \alpha_1 P_{i,t-2} + \alpha_2 P_{i,t-3} + I_{i,t-1}'\alpha_I + \delta_i + \varepsilon_{i,t-1}^{P},$$

where $P_{i,t-1}$ is the chosen productivity measure (obp and slg) of free agent i in season $t-1$, $I_{i,t-1}$ comprises age and experience, δ_i are free-agent fixed effects, and $\varepsilon_{i,t-1}^{P}$ is the error. Such a residual has been commonly used in the literature to construct measures of uncertainty.

The seven preceding seasons of P and I were collected for each season that a free agent enters the dataset, creating a total of 686 free agent-season "observations", i.e. usable triplets of data $(t-1, t-2, t-3)$. Because some free agents did not play every season and because some entered the market multiple times between 2008 and 2010, some free agents may have between one and seven observations. This discrepancy between free agents may systematically affect the accuracy of the forecast, and therefore $\hat{\varepsilon}$, through the precision of δ_i. The data, however, do not support any relationship between the number of observations and the precision of the forecast. Regressions of the magnitudes of $\hat{\varepsilon}_i^{obp}$ and $\hat{\varepsilon}_i^{slg}$ on the total number of observations for free agent i yield small and highly insignificant estimates, suggesting that any effect from the precision of δ_i on the magnitude of $\hat{\varepsilon}$ is too small to affect analysis.[30]

Productivity uncertainty enters Danziger's (1988) model through two channels: 1) the *probability* that a shock to productivity will occur in the following period, and 2) the *magnitude* of that productivity shock. The first description is explicitly discussed by Danziger (1988), and its theorized effects on contract length form his efficient risk-sharing hypothesis. The second description is less important to the hypothesis, but no less intuitive, and may provide some interesting secondary support. The following two subsections each develop and discuss the corresponding measures of uncertainty used in this study. Because each measure functions as a proxy for the expected uncertainty of a free agent, each subsection discusses the

[30] Please see Table B.2 in Appendix B.1 for regression results.

strength of the relationship between the measure and its future value. The summary statistics for each measure are included in Table 3.1.

3.5.1 Probability of a Real Productivity Shock

In developing the efficient risk-sharing hypothesis, Danziger (1988) uses a simple model that assumes productivity shocks either occur or do not with some specific probability. Worker productivity shocks in MLB, however, always occur to some degree.[31] To bridge this discrepancy between theory and data, this study borrows from Wallace (2001) and defines a "large" shock as occurring whenever the magnitude of a free-agent's residual from equation 3.1, $\hat{\varepsilon}$, is greater than the magnitudes of 55% of all free agents in the main sample, roughly .070 for $\hat{\varepsilon}^{obp}$ and .079 for $\hat{\varepsilon}^{slg}$. The sensitivity of this threshold is tested in Section 3.6.1. Let the number of observed large shocks to P that have occurred from $t-7$ to $t-1$ for free agent i be $S_{i,t-1}^{P}$ and the total number of observations in the same timeframe be $N_{i,t-1}$. Define the proportion of $S_{i,t-1}^{P}$ to $N_{i,t-1}$ to be $PR_{t-1}^{P} = \frac{S_{i,t-1}^{P}}{N_{i,t-1}}$, which can be interpreted as the past probability that a free agent experienced a large shock to P.

Plotting PR_{t-1} against the occurrence of a large productivity shock in period t demonstrates a significant relationship between both. As illustrated in Figure 3.3, the percentage of free agents who experience a large shock increases with increasing

[31] Although some players in the sample are extremely close to their predicted productivity measures, $\hat{\varepsilon}_i \neq 0 \ \forall i$.

values of PR.[32] Only about 14% and 22% of free agents with $PR = 0$ receive a large shock to their obp and slg, respectively, in the following period. The percentages increase to 45% and 47% for free agents whose PR values are close to .5, and the percentages increase to roughly 86% and 82% for free agents whose $PR = 1$. Such a relationship suggests that PR values provide a strong proxy for the expected probability that a significant productivity shock will occur to either obp or slg.

3.5.2 Magnitude of a Real Productivity Shock

Studies that use worker performance data from MLB use the previous season's productivity measures consistently as a proxy for the expectation of future production (Quirk and Fort 1992). Intuitively then, the magnitude of the previous season's productivity shock, i.e. the magnitude of the residual from equation 3.1, $MAG_{t-1} = |\hat{\varepsilon}_{t-1}|$, should provide a proxy for the expected magnitude of the productivity shock a free agent receives in the following period. But does it? The direct relationship between MAG_{t-1} and MAG_t provides such insight and is illustrated in Figure 3.4.[33] The strong clustering of observations around the dashed $Y = X$ line indicates a strong correlation between magnitudes from consecutive time periods. Such a correlation suggests that MAG_{t-1} does provide an effective proxy.

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[32] Because not all 686 observations match to a future value, only 546 observations could be included in Figure 3.3.

[33] Because not all 686 observations match to a future value, only 546 observations could be included in Figure 3.4.

3.6 Results

As previously mentioned, the available data allows a unique look into the efficient risk-sharing mechanism at the worker-firm level. This study regresses the length of the contract, $CL_{i,f,t}$, which was negotiated between free agent i and firm f and begins in season t, on each uncertainty measure and a set of controls:

$$(3.2)\ CL_{i,f,t} = \beta_0 + UNCERT_{i,t-1}^{P'}\beta_{UNCERT} + P'_{i,t-1}\beta_P + X'_{i,t}\beta_X + \beta_{REV}REV_{f,t-1} + \theta_f + \theta_t + u_{i,f,t},$$

where $UNCERT_{i,t-1}^{P}$ is a vector containing either the PR or MAG uncertainty measure of both obp and slg; $P_{i,t}^{'}$ is a vector containing obp and slg from the previous season; $X_{i,t}^{'}$ is a vector comprising free-agent age, age squared, experience, experience squared, position, type, and whether he is moving to a new city; $REV_{f,t-1}$ is the revenue of firm f from the previous season; θ_f and θ_t are firm fixed effects and time dummies, respectively; and $u_{i,f,t}$ is the error.

Table 3.2 reports the regression results for equation 3.2 in column 1, which contains the estimates when using PR, and column 2, which contains the estimates when using MAG. Before turning to the estimates on the uncertainty measures, another result should be acknowledged. The joint significance of the productivity measures, obp and slg, at the 1% level in both columns provides rare evidence that supports productivity as a contract-length determinant. Only a few studies using individual-level data even use productivity measures in contract-length regressions (Kahn 1993). The more common industry-level studies, which use union contracts, do not have access to such data and generally ignore worker productivity. This seldom creates identification problems, however, because many of those contracts are not contingent on worker productivity or productivity is assumed to be homogenous.

Turning back to the estimates on the uncertainty measures, they support the efficient risk-sharing hypothesis if three conditions are satisfied: 1) they are statistically significant determinants of contract length, 2) they are positive, and 3) their magnitudes are economically meaningful. All three conditions are satisfied. First, the pair of estimates on PR and MAG are each jointly significant at the 5% level, suggesting that productivity uncertainty is a significant determinant of contract length in the free-agent market. Apart from the relevance of this result to the efficient risk-sharing hypothesis, this result is additionally important for two reasons: it is the first time that any uncertainty has been shown to affect the length of individual-level contracts, and more specifically, it is the first time that real uncertainty in a worker's productivity has been shown to be a significant determinant of contract length.

Second, the estimates for each uncertainty measure are positive, supporting the positive relationship between real uncertainty and contract length implied by Danziger (1988). Third, the magnitudes of each estimate are meaningful. For a one standard deviation increase in PR^{OBP} and PR^{SLG}, roughly .407 and .378, respectively, the corresponding estimates in column 1 imply an increase in contract length of .126 and .249 years, roughly 8% and 16% of an average free agent's contract length. These increases are relatively consistent with the increases to contract length implied by the estimates on MAG in column 2. A one standard deviation increase in MAG^{OBP} and

MAG^{SLG}, roughly .063 and .064, corresponds to an increase in contract length of .107 and .162 years, roughly 7% and 11% of an average free agent's contract length. Collectively, such results provide strong support for Danziger's (1988) efficient risk-sharing hypothesis.

3.6.1 Sensitivity of PR Estimates to the Definition of a "Large" Shock

Recall from Section 3.5.1 that $PR = \frac{\#\ of\ past\ large\ shocks}{\#\ of\ past\ observations}$, where a "large" shock is defined as occurring whenever the magnitude of a free agent's residual from equation 3.1, $|\hat{\varepsilon}|$, is sufficiently large. This study uses the thresholds $|\hat{\varepsilon}^{OBP}| > .070$ and $|\hat{\varepsilon}^{SLG}| > .079$, which yield estimates in column 1 of Table 3.2 that support the efficient risk-sharing hypothesis. These results, however, could just be lucky. The choice of threshold is arbitrary, and different thresholds could yield results that do not support Danziger's (1988) hypothesis.

The sensitivity of the results in column 1 to changes in threshold is tested using four different pairs of threshold values: 050 and .059, .060 and .069, .080 and .089, and .090 and .099. The lowest pair of thresholds is larger than 38% and 44% of the magnitudes of $\hat{\varepsilon}^{OBP}$ and $\hat{\varepsilon}^{SLG}$ in the main sample, respectively; the highest pair is correspondingly larger than 67% and 65%. The resulting estimates are provided in columns 1, 2, 4, and 5 of Table 3.3, respectively. Column 3 provides the results from column 1 of Table 3.2 for comparison.

Although the point estimates do change as the thresholds change, the estimates on PR^{OBP} and PR^{SLG} are jointly significant at the 5% level for columns 1 and 2 and at the 10% level for columns 4 and 5. Furthermore, all the point estimates are again positive and economically meaningful: the lowest estimate on $PR^{OBP} = .240$ corresponds to a 6% change in contract length for a one standard deviation increase, and the lowest estimate of $PR^{SLG} = .374$ corresponds similarly to a 9% change. The relative insensitivity of the results to threshold value suggests that the data generally support the efficient risk-sharing hypothesis through the PR measure.

3.6.2 Alternative Measure of Real Uncertainty (SEE)

Recall that the decision to use PR, and less so MAG, derives from Danziger's (1988) treatment of real uncertainty. In contrast, the majority of the literature (e.g. Christofides 1990; Wallace and Blanco 1991; Wallace 2001; Rich and Tracy 2004) follows Christofides and Wilton (1983), who use the standard error of the estimate (SEE), in adopting Gray's (1978) definition of uncertainty – forecast variance. To test the robustness of the efficient risk-sharing hypothesis to Gray's (1978) definition of uncertainty, equation 3.2 is estimated with SEE measures. The residuals, $\hat{\varepsilon}$, from past time periods are used to create an aggregate measure similar to the standard error of P, specifically $SEE_{t-1} = \sqrt{\frac{\sum_j \hat{\varepsilon}^2_{t-1-j}}{N}}$, where, in this study, $j \in [0,6]$ and N is the number of observations used. The summary statistics are provided in Table 3.1.

Although PR and SEE are quite correlated, $\rho^{OBP}_{PR,SEE} = .83$ and $\rho^{SLG}_{PR,SEE} = .85$, a Wald test for joint significance of the SEE estimates, which are provided in column 1 of Table 3.4, is highly insignificant. Such a result can be supported by either of two explanations: 1) there exists some characteristic in MLB that creates measurement error in the SEE measures of uncertainty, or 2) Gray's (1978) definition of uncertainty is not relevant to the relationship between productivity uncertainty and contract length in MLB. Although identifying which explanation causes the insignificance and why is beyond the scope of this study, a simple test can provide some useful information. If the free agents with uncorrelated PR and SEE measures are removed from the data and the resulting estimates become significant, then the relationship between productivity uncertainty and contract length is sensitive to how uncertainty is measured. Although this result cannot identify which explanation drives the original insignificance of the SEE measures, it does imply that either the first explanation holds or the second holds with SEE acting as a proxy for PR. If, however, the estimates remain insignificant, then the second explanation is likely correct.

The relationship between PR and SEE is illustrated in Figures 3.5 and 3.6. The observed dispersion of SEE values at each level of PR, particularly for slg, suggests that the correlation between the two measures is smallest for the free agents that have high values of SEE at lower relative values of PR. Ten such observations are easily identifiable in Figures 3.5 and 3.6: one with $SEE^{OBP} \approx .198$ and $PR^{OBP} = .8$, seven with $SEE^{SLG} > .167$ and $PR^{SLG} \in (.666, .834)$, and two with $SEE^{SLG} \in (.117, .133)$ and $PR^{SLG} = .4$.[34] Removing just these observations yields estimates, provided in column 2 of Table 3.4, that are jointly significant at the 10% level. Furthermore, the estimates on both SEE measures are positive and economically

[34] Eliminating further observations reduces sample size without significantly strengthening the relationship between SEE and PR.

meaningful. An increase of one standard deviation in SEE^{OBP} and SEE^{SLG}, roughly .060 and .059, respectively, corresponds to an increase in contract length of .139 and .136 years, both roughly 9% of an average free agent's contract length and consistent with the results from PR and MAG. These results support Danziger's (1988) efficient risk-sharing hypothesis and suggest that the relationship between real uncertainty and contract length is sensitive to whatever causes the divergence between PR and SEE for those free agents removed from the data.

But what could cause certain free agents' SEE measures to be "too" high while their PR measures are not? This can only occur in the data when a predictable free agent has one or two spectacularly unexpected performances, an event that makes sense in the context of MLB. Large single season shocks to productivity, like a bad injury (negative) or an uncharacteristically productive year (positive), are not particularly rare (Krautmann 1990) and could explain how relatively predictable free agents deviate significantly from expectations once or twice. Just as interesting, such shocks likely affect the power of a batter (slg) far more than his accuracy (obp), potentially explaining why SEE^{slg} has more outlier observations than $SEEE^{OBP}$. Such a story would support the first explanation and, therefore, would be consistent with Gray's (1978) definition of uncertainty affecting contract length in MLB.

3.7 Conclusion

Does uncertainty generated from real shocks to worker productivity affect the length of a worker's contract? If so, does the relationship between real uncertainty and contract length support Danziger's (1988) efficient risk-sharing hypothesis? The evidence suggests they do. Not only do the results of this study indicate that productivity uncertainty is a significant contract-length determinant, but they also support the efficient risk-sharing hypothesis. The results are consistent across two measures of uncertainty (three with modifications) for two types of worker productivity and are robust to a variety of contract-length determinants, including, but not exclusive to, contracting-cost, indexation, relationship investment, information-cost, and bargaining-power effects. In addition, these results should be independent of Gray's (1978) efficient-production mechanism.

The individual-level data and observed matching between free agents and firms allow worker- and firm-specific controls, a key advantage over previous studies that rely on aggregate controls for union contracts. As such, this study introduces into the literature the first contract-length analysis of real uncertainty that uses individual-level data; more specifically, it provides the first individual-level analysis of the efficient risk-sharing hypothesis.

The three measures of productivity uncertainty used in this study provide different insights into the relationship between productivity uncertainty and contract length. The first, which measures the probability that a worker's productivity will deviate sufficiently from his expected level, tests the efficient risk-sharing hypothesis with Danziger's (1988) own treatment of uncertainty. The second measure, which is derived from Danziger's (1988) model, tests whether the magnitude of that deviation affects contract length. Finding that both measures significantly increase contract length suggests that both the expected frequency and magnitude of shocks matter when negotiating contract lengths. Finally, the third measure, which is the standard error of the estimate, tests whether Danziger's (1988) hypothesis is robust to Gray's (1978) definition of uncertainty. The sensitivity of the measure may suggest that the characteristics of different uncertainty measures may make certain measures better at measuring uncertainty in different environments. Such a finding could help explain why real uncertainty has been found to affect contract length differently in various industries; however, far more research is needed before any conclusions can be drawn.

TABLES

Table 3.1
Summary Statistics

Variable	Obs	Mean	St. Dev	Min	Max
Contract Length [years]	182	1.527	1.160	1	8
On-base Percentage, obp	182	.333	.035	.244	.439
Slugging Ratio, slg	182	.411	.074	.241	.627
Probability of a "Shock", PR^{OBP}	182	.474	.407	0	1
Probability of a "Shock", PR^{SLG}	182	.477	.382	0	1
Magnitude of Residual, MAG^{OBP}	182	.080	.063	.001	.336
Magnitude of Residual, MAG^{SLG}	182	.084	.064	.000	.361
Standard Error of Estimate, SEE^{OBP}	182	.084	.060	.003	.339
Standard Error of Estimate, SEE^{SLG}	182	.095	.059	.009	.326
Age [years]	182	31.648	3.285	24	41
Experience [years]	182	10.456	3.471	4	21
Moving to a New City	182	.764†			
Type-A Free Agent	182	.111†			
Type-B Free Agent	182	.126†			
No-Type Free Agent	182	.763†			
Position: Outfielder	182	.326†			
Position: Infielder	182	.316†			
Position: 1st Baseman	182	.142†			
Position: Catcher	182	.200†			
Position: Designated Hitter	182	.016†			
Free Agents in 2008 Market	182	.269†			
Free Agents in 2009 Market	182	.379†			
Free Agents in 2010 Market	182	.352†			
Firm Revenue [$100 million]	90	2.0629	.5410	1.4456	4.6305

Firm revenue is inflation adjusted to 2011 dollars. † "Mean" values correspond to the proportion of free agents in the sample that fall into each category.

Table 3.2
Contract Length & Productivity Uncertainty

Dependent Variable: Contract Length	**Probability of Shock,** PR **(1)**	**Magnitude of Shock,** MAG **(2)**
Uncertainty in obp	.311	1.701
	(.188)	(1.116)
Uncertainty in slg	.660***	2.530*
	(.247)	(1.352)
On-base % (obp)	4.072	4.151
	(2.815)	(2.823)
Slugging Ratio (slg)	3.952**	3.201**
	(1.537)	(1.427)
Age	-.547	-.471
	(.498)	(.503)
Age^2	.008	.007
	(.008)	(.008)
Experience	.095	.094
	(.182)	(.195)
Experience^2	-.007	-.006
	(.007)	(.008)
New City	-.165	-.157
	(.189)	(.200)
Type-A Free Agent	1.392***	1.498***
	(.407)	(.423)
Type-B Free Agent	.492**	.442*
	(.225)	(.236)
First Baseman	.086	.120
	(.249)	(.245)
Infielder	.241	.193
	(.212)	(.218)
Catcher	.117	.026
	(.244)	(.242)
Designated Hitter	.378	.215
	(.520)	(.546)
Firm Revenue	-1.797	-1.960
	(1.251)	(1.302)

***** denotes significance at the 10% level, ****** denotes significance at the 5% level, and *** denotes significance at the 1% level. In addition to the above variables, all regressions control for season and firm fixed effects. Each regression uses 182 observations and has 134 d.f. Heteroskedasticity-robust standard errors are provided in parentheses.

Table 3.3
Sensitivity of PR to the Definition of a "Large" Shock

Dependent: Contract Length	Large Shock: $\lvert\hat{\varepsilon}^{obp}\rvert \geq .050$ $\lvert\hat{\varepsilon}^{slg}\rvert \geq .059$ (1)	Large Shock: $obp \geq .060$ $slg \geq .069$ (2)	Large Shock: $obp \geq .070$ $slg \geq .079$ (3)	Large Shock: $obp \geq .080$ $slg \geq .089$ (4)	Large Shock: $obp \geq .090$ $slg \geq .099$ (5)
PR^{OBP}	.242	.240	.311	.397*	.378*
	(.206)	(.202)	(.188)	(.211)	(.193)
PR^{SLG}	.611**	.747***	.660***	.464*	.374
	(.240)	(.263)	(.247)	(.239)	(.238)

***** denotes significance at the 10% level, ****** denotes significance at the 5% level, and *** denotes significance at the 1% level. All regressions additionally control for age, age squared, experience, experience squared, moving to a new city, free-agent type, position, market size, season, and firm fixed effects. Each regression uses 182 observations and has 134 d.f. Heteroskedasticity-robust standard errors are provided in parentheses.

Table 3.4
Contract Length and SEE

Dependent: **Contract Length**	**Full Sample** (1)	**10 Observations Removed** (2)
SEE^{OBP}	1.574	2.311*
	(1.254)	(1.340)
SEE^{SLG}	1.209	2.311
	(1.398)	(1.605)

***** denotes significance at the 10% level. All regressions additionally control for age, age squared, experience, experience squared, moving to a new city, free-agent type, position, market size, season, and firm fixed effects. The regression provided in column 1 uses 182 observations and has 134 d.f.; the regression in column 2 uses 172 observations and has 124 d.f. Heteroskedasticity-robust standard errors are provided in parentheses.

FIGURES

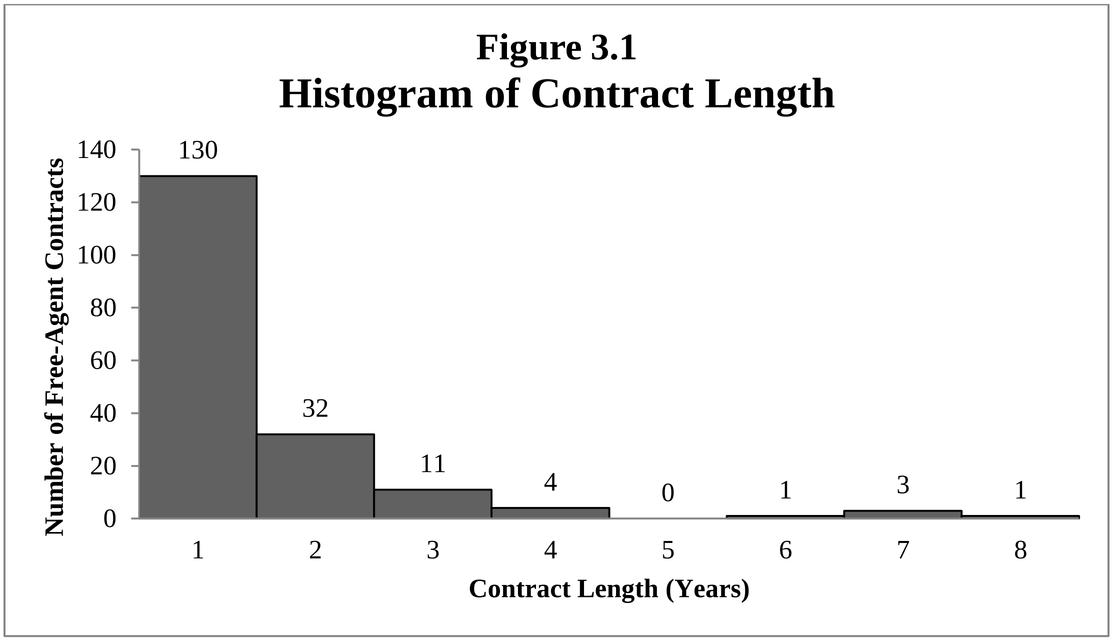


Figure 3.1
Histogram of Contract Length
Number of Free-Agent Contracts
140
120
100
80
60
40
20
0
130
32
11
4
0
1
3
1
1
2
3
4
5
6
7
8
Contract Length (Years)

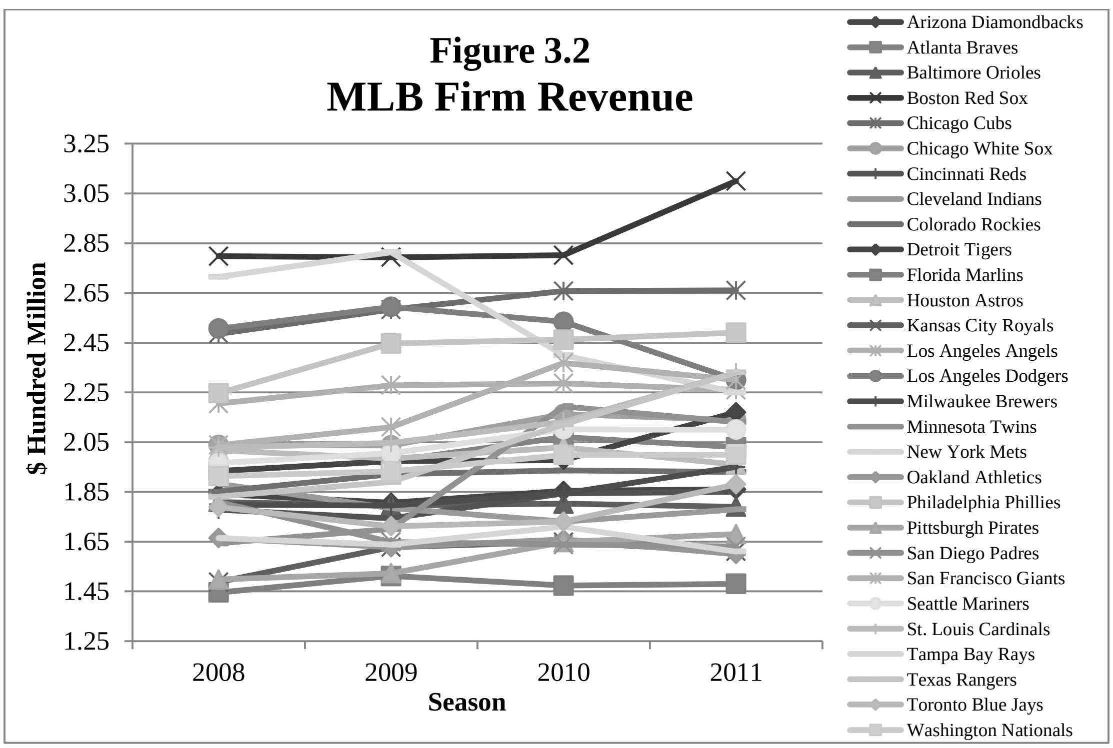


Figure 3.2
MLB Firm Revenue
$ Hundred Million
3.25
3.05
2.85
2.65
2.45
2.25
2.05
1.85
1.65
1.45
1.25
2008
2009
2010
2011
Season
Arizona Diamondbacks
Atlanta Braves
Baltimore Orioles
Boston Red Sox
Chicago Cubs
Chicago White Sox
Cincinnati Reds
Cleveland Indians
Colorado Rockies
Detroit Tigers
Florida Marlins
Houston Astros
Kansas City Royals
Los Angeles Angels
Los Angeles Dodgers
Milwaukee Brewers
Minnesota Twins
New York Mets
Oakland Athletics
Philadelphia Phillies
Pittsburgh Pirates
San Diego Padres
San Francisco Giants
Seattle Mariners
St. Louis Cardinals
Tampa Bay Rays
Texas Rangers
Toronto Blue Jays
Washington Nationals

Figure 3.3
Percentage of Each Range of PR_{t-1} Values that Matches to a Shock / No Shock in Period t

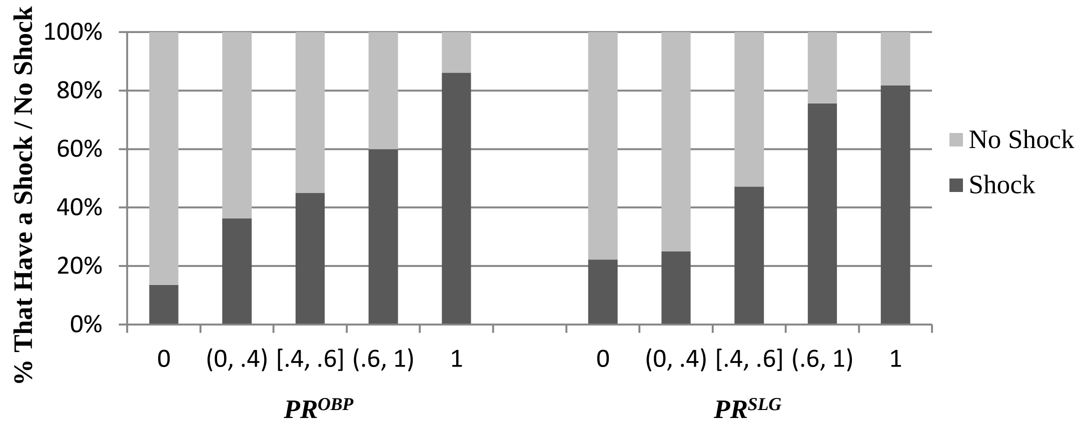
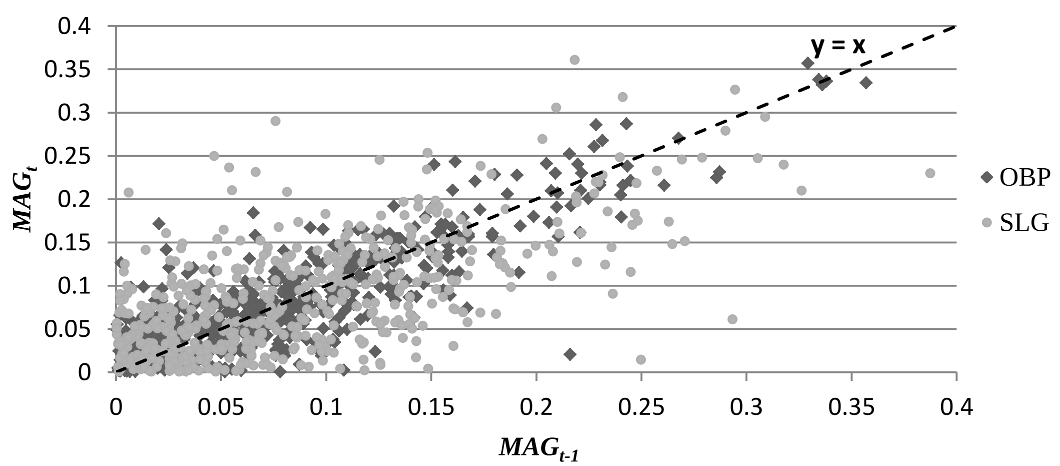


Figure 3.4
Relationship between the Magnitudes of the Residuals in Periods *t-1* and *t*



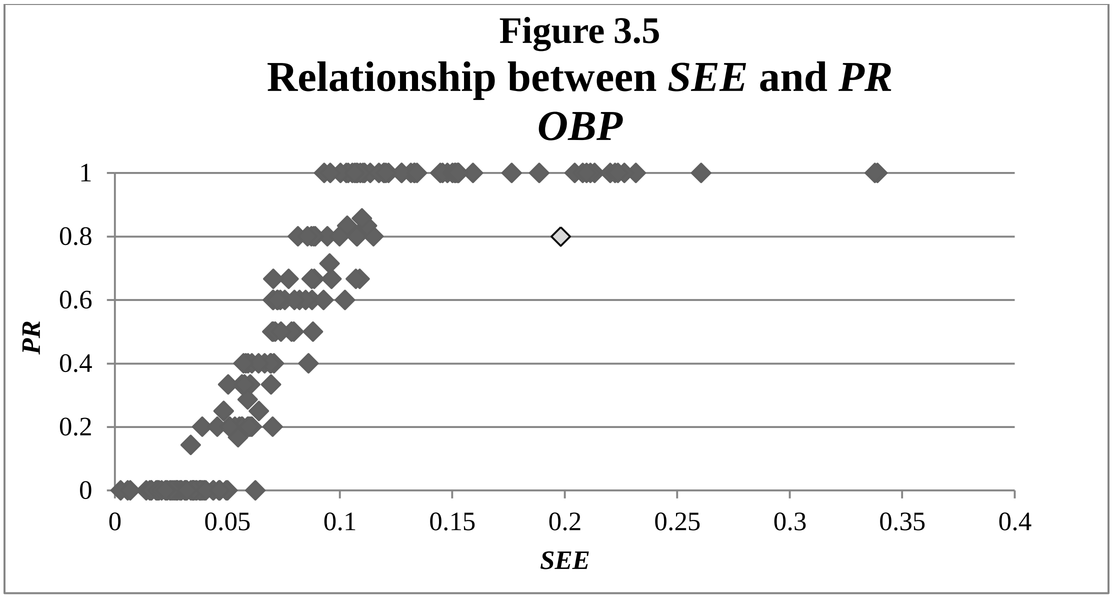


Figure 3.5
Relationship between *SEE* and *PR*
OBP
1
0.8
0.6
0.4
0.2
0
PR
0
0.05
0.1
0.15
0.2
0.25
0.3
0.35
0.4
SEE

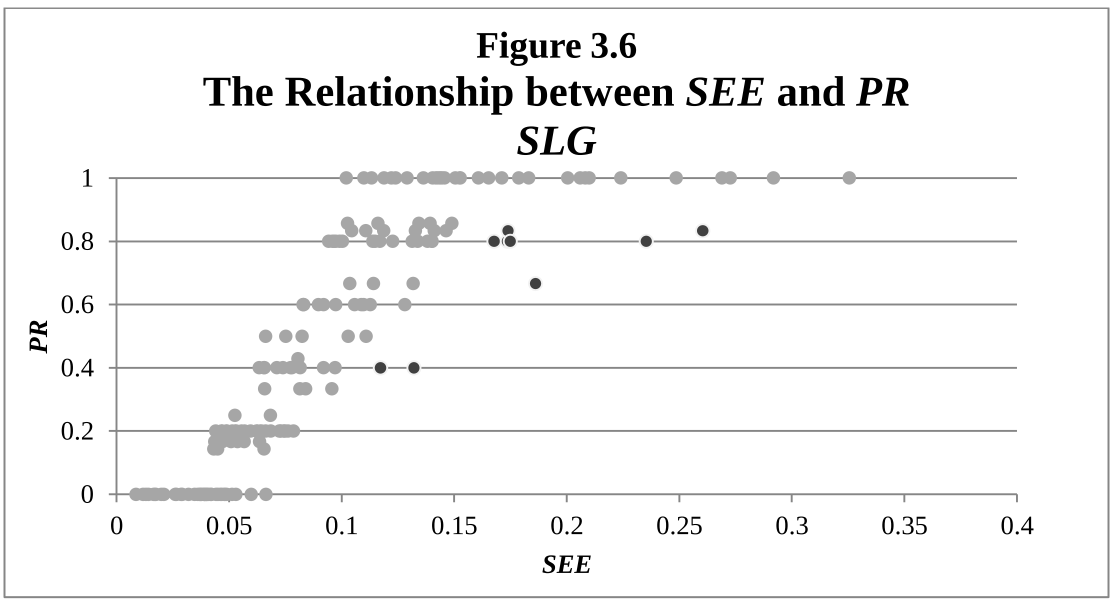


Figure 3.6
The Relationship between *SEE* and *PR*
SLG
1
0.8
0.6
0.4
0.2
0
PR
0
0.05
0.1
0.15
0.2
0.25
0.3
0.35
0.4
SEE

CHAPTERS 1, 2, AND 3

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APPENDIX A

APPENDICES TO CHAPTER 2

A.1: Additional Advantages of Professional Sports Data

Industry, firm, and team characteristics in professional sports leagues remove potential effects from team and firm structure, firm entry and exit, and fluctuations in consumer demand common to other industries that may confound team-quality measurement or influence the value of team quality endogenously. Team structure within a firm is not always well defined in a dataset. Incorrectly matching workers to their respective teams pairs each worker with an incorrect value of team quality, thereby introducing measurement error into the team quality variable. The more teams a firm has, the more exacerbated the matching problem. Generally, each firm in a professional sports league contains a single team, implicitly identifying the worker-team match. An industry of single-team firms also removes cross-team spillovers and complementarities within a firm. The presence of other teams may create spillovers through between-team competition or provide production complementarities, such as a worker in an architecture team benefitting from working alongside a good construction team. These factors may bias team-quality estimates in the same way as the within-team spillovers and complementarities discussed in Section 2.2.

Many industries experience fluid firm entrance and exit and/or comprise firms that routinely construct and disband teams. The fluctuating supply of available teams in these industries may affect the market value of team quality to workers. For example, if a worker believes more teams will soon exist, the value of immediately contracting to a good team may diminish. Fortunately, the number of firms in most professional sports leagues is fixed (e.g. MLB has exactly 30 firms), removing any fluctuations in team supply.

In many industries, firms match team production to fluctuations in consumer demand for its product. If sufficient hiring and firing frictions exist, then increasing or decreasing team production requires parallel changes to worker productivity, creating measurement error in worker productivity measures. In contrast, a professional athlete will produce at his skill level regardless of current consumer demand.

A.2: TABLES

Table A.1
Performance Statistics

	Statistic	Formula	Description
Fielding	fld	$\frac{PO + A}{TC}$	PO counts the number of outs made, A counts the number of assisted outs, and TC totals all fielded balls and equals $PO + A + errors$.
Hitting	obp	$\frac{W + 1B + 2B + 3B + 4B}{PA^{-}}$	W counts the total number of walks; $1B$, $2B$, $3B$, and $4B$ count the number of each type of hit that goes the respective number of bases (1, 2, 3, or 4); and PA^{-} counts the number of total plate appearances (minus catcher interferences and bunts).
	slg	$\frac{1B + 2 \times 2B + 3 \times 3B + 4 \times 4B}{AB}$	The numerator counts the total number of bases reached. AB counts the total at bats, which equals PA^{-} minus W. Please see obp for additional detail.
Pitching	$sobb$	$\frac{SO}{BB}$	SO counts the total number of strikeouts, and BB counts the total number of walks.
	$sobf$	$\frac{SO}{BF}$	SO counts the total number of strikeouts, and BF counts the total number of batters faced.

Table A.2
Corrected Bins for Card Prices

Bin #	2008	2009	2010
0	0 (No Card)	0 (No Card)	**<u>0</u>** (No Card)
1	.12	.15 (1) & .12 (2)	**<u>.15</u>** (1) & .12 (2)
2	.20 .30	.25 (1) & .20 (2)	**<u>.25</u>**
3	.40	.40	**<u>.40</u>**
4	.50	.50	**<u>.50</u>**
5	.60	.60	**<u>.60</u>**
6	.75	1.0	**<u>1.0</u>**

Collapsing 2008's seven bins into six does lose information, but it keeps card prices consistent across years. Bin #2 was linked across years by meticulously matching many players and their card values. Column 1 corresponds to prices for the first of two prints in a card series, whereas column 2 corresponds to prices from the second. Only seven free agents in the data receive their card price from the second series, and removing them has no measureable effect on the results. Both 2008 prints (used for the 2009 season) have the same prices for both series, unlike the proceeding years in bins #1 and #2. **<u>Bold and underlined</u>** prices correspond to the actual prices assigned to each bin. For example, a 2008 card worth .30 in the February 2009 price guide is assigned a price of .25, or a 2009 card from the second print worth .12 in February 2010 is assigned a price of .15.

Table A.3
Estimates of Winning-Percentage Regression

Variable	Coefficient
Average Team obp	187.667 (118.018)
Average Team obp^2	38.201 (29.215)
Average Team slg	-62.600 (75.043)
Average Team slg^2	-7.218 (11.243)
Average Team fld	-678.582** (336.065)
Average Team fld^2	354.610** (170.080)
Average Team $sobb$	-13.584*** (2.947)
Average Team $sobb^2$	-.102*** (.028)
Average Team $sobf$	104.040 (77.716)
Average Team $sobf^2$	-40.573** (19.546)
Interaction (obp x slg)	-35.120 (35.387)
Interaction (obp x fld)	-197.157* (115.263)
Interaction (obp x $sobb$)	3.470** (1.606)
Interaction (obp x $sobf$)	-61.863** (25.860)
Interaction (slg x fld)	79.816 (76.262)
Interaction (slg x $sobb$)	-2.069* (1.180)
Interaction (slg x $sobf$)	41.746** (20.779)
Interaction (fld x $sobb$)	13.394*** (3.188)
Interaction (fld x $sobf$)	-95.308 (80.324)
Interaction ($sobb$ x $sobf$)	3.514** (1.336)

* indicates significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level. The regression also controls for the division within which a team plays.

Table A.4
Percentage of Revenue Spent on Luxury Tax

Team	2009	2010	2011
New York Yankees	5.83%	4.22%	3.16%
Boston Red Sox	0%	.55%	1.10%

No other MLB firms paid luxury tax for the 2009, 2010, or 2011 seasons. The percentages are calculated from luxury tax data provided by The Associated Press and Forbes.

A.3: FIGURES

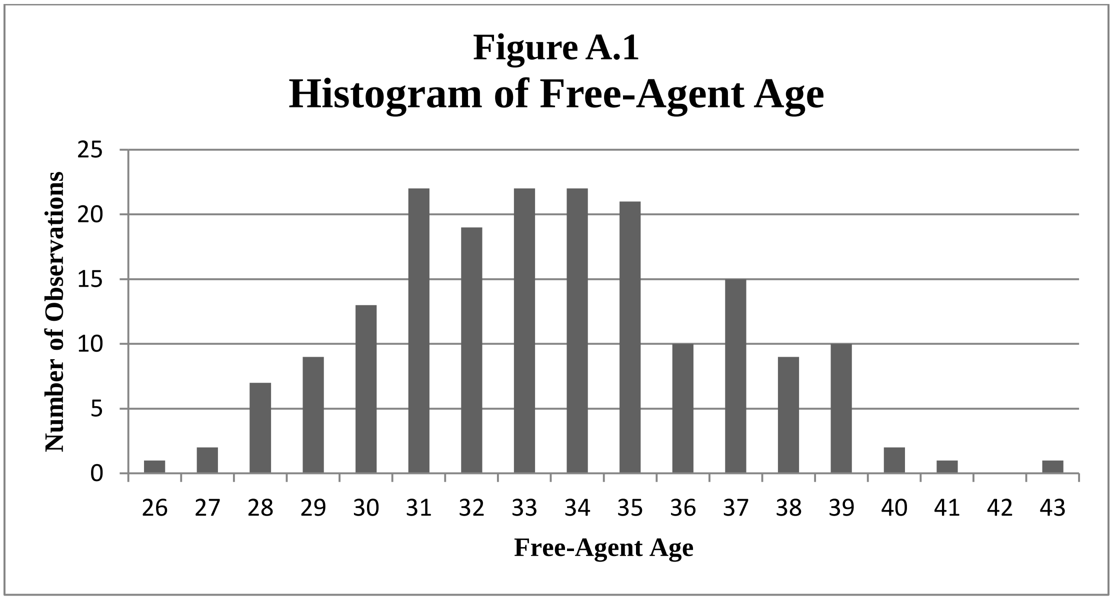
Figure A.1
Histogram of Free-Agent Age
Number of Observations
0
5
10
15
20
25
26 27 28 29 30 31 32 33 34 35 36 37 38 39 40 41 42 43
Free-Agent Age

APPENDIX B

APPENDICES TO CHAPTER 3

B.1: TABLES

Table B.1
Performance Statistics

Statistic	Formula	Description
obp	$\frac{W + 1B + 2B + 3B + 4B}{PA^{-}}$	W counts the total number of walks; $1B$, $2B$, $3B$, and $4B$ count the number of each type of hit that goes the respective number of bases (1, 2, 3, or 4); and PA^{-} counts the number of total plate appearances (minus catcher interferences and bunts).
slg	$\frac{1B + 2 \times 2B + 3 \times 3B + 4 \times 4B}{AB}$	The numerator counts the total number of bases reached. AB counts the total at bats, which equals PA^{-} minus W. Please see obp for additional detail.

Table B.2
Regression of the Magnitude of $\hat{\varepsilon}$ on the Number of Observations (i.e. Usable Triplets)

Dependent Variable: Magnitude of Shock, $\lvert\hat{\varepsilon}\rvert$	**Magnitude of ε^{obp}**	**Magnitude of ε^{slg}**
# of Usable Triplets	.0015	.0015
$(t, t-1, t-2)$	(.0022)	(.0024)
constant	.0719***	.0803***
	(.0119)	(.0131)

*** denotes significance at the 1% level. Each regression used 686 observations. Heteroskedasticity-robust standard errors are provided in parentheses.