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INTERACTIONS BETWEEN WORKERS AND THE TECHNOLOGY OF PRODUCTION: EVIDENCE FROM PROFESSIONAL BASEBALL

by

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INTERACTIONS BETWEEN WORKERS AND THE TECHNOLOGY OF PRODUCTION: EVIDENCE FROM PROFESSIONAL BASEBALL

Eric D. Gould and Eyal Winter*

Abstract—This paper shows that workers can affect the productivity of their coworkers based on income maximization considerations, rather than relying on behavioral considerations such as peer pressure, social norms, and shame. We show that a worker's effort has a positive effect on the effort of coworkers if they are complements in production, and a negative effect if they are substitutes. The theory is tested using a panel data set of baseball players from 1970 to 2003. The results are consistent with the idea that the effort choices of workers interact in ways that are dependent on the technology of production.

I. Introduction

THIS paper examines how the effort choices of workers within the same firm interact with each other, and how this interaction depends on the technology of production. In contrast to the existing literature, we focus on showing how the effort choice of one worker can affect the effort choices of his coworkers based purely on income maximizing considerations, rather than relying on behavioral explanations such as peer pressure and shame. In addition, we break from the existing literature by showing that the effort choice of one worker could have a positive or negative effect on his coworkers. For example, a mechanism based on behavioral considerations like peer pressure or shame predicts that a high level of effort by one worker will induce other workers to increase their effort level, or that a lower effort by one worker causes other workers to follow suit. We refer to both of these cases as a "positive interaction" in the sense that a change in effort by one worker causes others to change their effort in the same direction. However, we show that a "negative interaction" between workers is also possible, in the sense that a change in effort by one worker causes other workers to change their effort in the opposite direction.

Therefore, this paper contributes to the existing literature by showing that the interaction of effort choices could work in both directions, even within the same firm at the same time. In particular, we show that a "positive interaction" should exist between complementary workers, while workers who are substitutes may free ride off the effort of each other, and thus generate a "negative interaction" in the effort choices of coworkers.

The theory is tested using panel data on the performance of baseball players from 1970 to 2003. The game of baseball provides a clear case where pitchers and nonpitchers can safely be defined as substitutes for each other in team performance—since preventing runs and scoring runs are perfect substitutes in the team's goal of scoring more runs than the opposing team. In addition, players who are not pitchers are often complements with each other since it usually takes more than one player to get a hit in order to score a run for the team. The empirical analysis shows that a player's batting average significantly increases with the batting performance of other players on the team, but decreases with the quality of the team's pitching. Furthermore, a pitcher's performance increases with the pitching quality of the other pitchers, but is unaffected by the batting output of the team. These results are inconsistent with behavioral explanations for how one worker affects the performance of other workers, since a typical behavioral response should cause workers to change their effort in the same direction regardless of the other player's role or function. Thus, psychological considerations are unlikely to explain our findings that players respond differentially to the actions of their coworkers according to their role and function on the team. Overall, the results are more consistent with an interaction of effort choices within the team that are based on a rational response to the technology of production.

Our empirical findings are robust to controlling for individual fixed effects, experience, year effects, team, home ballpark characteristics, and managerial quality. The inclusion of individual fixed effects means that the results cannot be explained by assortative matching between complementary or substitutable players at the team level, since the analysis is exploiting variation over time within a given player's performance. In addition, the results are robust to using a first-differences specification, as well as restricting the sample to only those workers who change teams (changing all of their coworkers), or using a sample of only those workers who remain with the same team, manager, and home ballpark in consecutive years. Furthermore, in order to control for unobserved yearly shocks that may affect the performance of the whole team, we instrument the yearly performance of one's teammates with the lifetime performance of his teammates. Yearly variation in this instrument stems only from changes in the composition of one's coworkers, since each player's lifetime performance is constant for each year. Results using this instrument are very similar to the OLS estimates.

There is a growing literature that stresses the importance of the environment in determining the outcomes of individuals. Most of this literature is concerned with examining how peers and environmental factors affect youth behavior regarding their educational achievements, health, criminal

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involvement, work status, and other economic variables.[1] This paper differs by looking at the interaction of adult behavior in the workplace. The literature on the interaction of workers within a firm is not extensive. Winter (2004) demonstrates theoretically the optimality of offering differential incentive contracts in order to elicit worker effort that generates externalities on other workers. Kandel and Lazear (1992) examine the theory of team production within the firm and focus on how teams produce social pressure to solve the free-riding problem. The most related papers to ours are by Ichino and Maggi (2000) and Mas and Moretti (2006). Ichino and Maggi (2000) examine shirking behavior within a large banking firm, and show that a worker's shirking behavior significantly responds to the behavior of his coworkers when they move across branches within the same firm. Using data on workers from a large grocery store chain, Mas and Moretti (2006) examine how the productivity of a worker varies according to the productivity of other workers on the same shift, and provide additional evidence that behavior considerations such as peer pressure and social norms are significant. Some of our empirical specifications employ a similar identification strategy in the sense that we exploit differences in the composition of one's coworkers to explain variation in an individual's performance level over time and across workplaces. However, our paper differs by examining the theoretical and empirical differences in the nature of the interaction across workers depending on whether they are substitutes or complements with each other. In this manner, our paper contributes to the literature by providing a theoretical foundation and empirical evidence for both positive and negative interactions in the effort choices of workers in a real work environment.

II. The Model

In this section, we show how the effort choices of workers within the same firm interact with each other, and how this interaction depends on the technology of the team production function. To do this, we present a parsimonious principal-agent model where the optimal contract is derived under two different scenarios. In one scenario, players are complementary to one another, and in the second scenario, workers are considered substitutes. In order to characterize the two different types of technologies, we borrow the concept of strategic substitution and complementarity (see Milgrom & Shannon, 1994, and Topkis, 1998). Our model is similar to Holmstrom (1982) and Holmstrom and Milgrom (1991) in the sense that the outcome of effort is uncertain, but risk aversion plays no role in our model. That is, our model is based only on the issue of moral hazard.

A team consists of two agents $\{1,2\}$. Each agent is responsible for a task. A worker's task is successful with probability β if he exerts effort, but is successful with probability $\alpha < \beta$ if no effort is exerted. We assume that the cost of effort is c for both agents.[2] On each team, the tasks of the two workers jointly determine the success of a project according to a technology $p: \{0, 1, 2\} \rightarrow [0, 1]$, where $p(k)$ is the probability that the project succeeds given that exactly k agents have successfully completed their tasks (the assumption of symmetry is used only for the sake of simplicity). In order to allow workers to base their effort choices on the performance of other workers, we assume that player 1 performs his task first, and then player 2 chooses his effort after observing the outcome of the task performed by agent 1.

We derive the optimal contracts for two teams—each team representing a different type of technology. The production function for team 1 is characterized by complementarity or supermodularity between the agents, which is represented by $p(2) - p(1) > p(1) - p(0)$. In contrast, team 2 is characterized by substitution between workers, which is represented by $p(2) - p(1) < p(1) - p(0)$. This framework captures the basic intuition that, in the case where workers are complements in production, the success of one agent in completing his task contributes more to the prospects of the entire project succeeding if the other agent succeeded as well. In contrast, in the case where workers are substitutes, the marginal contribution of a successfully completed task by one worker is higher when the other worker fails in his task.

The principal is facing moral hazard. He cannot monitor the effort of his workers, nor is he informed about which tasks have ended successfully. Instead, he is informed only about whether the project as a whole is successful. Therefore, the principal offers contracts to agents that are contingent only on whether the overall project succeeded or not. Specifically, the principal offers a contract to each member of the team, represented by a vector of rewards $v = (v_1, v_2)$ with agent i receiving v_i if the project succeeds and 0 otherwise.

For a mechanism v, we have an extensive form game $G(v)$ between the two players. If the overall team project is successful, the project generates a benefit B for the principal. Given a mechanism v, let $q(v)$ be the probability of success in the unique[3] (subgame perfect) equilibrium of the game $G(v)$. The principal designs the incentive mechanism v optimally, so as to maximize his net revenue, represented as $v = \arg\max q(v)[B - \Sigma_j v_j]$.

We assume that the overall project is valuable enough so that the optimal mechanism awards each player with a

[1] See Angrist and Lang (2004); Guryan (2004); Hoxby (2000); Sacerdote (2001); Zimmermann (2003); Katz, Kling, and Liebman, (2001); Edin, Fredriksson, and Aslund (2003); Oreopoulos (2003); Jacob (2004); Weinberg, Reagan, and Yankow (2004); Gould, Lavy, and Paserman (2004a, 2004b).

[2] In reality, the cost of effort would be a function of a person's innate ability. Also, as we later discuss, the probability of the task succeeding conditional on effort would also be a function of personal characteristics. However, we maintain the assumption of a uniform cost for the sake of simplicity.

[3] We assume that indifference is resolved in favor of exerting effort.

positive reward if the project is successful. That is, B is sufficiently high ($B > B_*$) so that $v_j > 0$ for both players in the optimal mechanism. Note that this assumption implies that player 1 exerts effort. If this were not the case, then $v_1 > 0$ cannot be optimal since the principal would be better off paying zero to player 1. Depending on the value of B as well as the values of the other parameters in the game, the optimal mechanism must yield one of the following equilibria in the corresponding game:

1. Player 1 exerts effort and player 2 exerts effort if and only if the first task succeeded.
2. Player 1 exerts effort and player 2 exerts effort if and only if the first task failed.
3. Player 1 exerts effort and player 2 exerts effort regardless of the outcome of the first task.

If B is sufficiently high ($B > B^*$), then the project is so valuable that the principal will induce equilibrium 3 so that player 2 always finds it worthwhile to exert effort regardless of whether player 1 succeeded and regardless of whether the technology is one of substitution or complementarity. If, however, B is high enough to induce the principal to provide incentives to exert effort but not so high that this is always the case ($B_* < B < B^*$), the optimal strategy will depend on the technology of production. The following proposition states what happens whenever B is sufficiently high ($B > B_*$) so that the principal provides at least some incentives to exert effort.

Proposition 1. (i) *If the team's technology satisfies complementarity, then the optimal mechanism induces either equilibrium 1 or equilibrium 3.* (ii) *If the team's technology satisfies substitution, then the optimal mechanism induces either equilibrium 2 or equilibrium 3.*

Proposition 1 asserts that unless it is a dominant strategy for agent 2 to always exert effort ($B > B^*$), the optimal pattern of behavior in equilibrium will be consistent with our empirical results. If workers are complementary, a failure on the part of player 1 will trigger player 2 to shirk. In contrast, if workers are substitutes in production, a failure on the part of player 1 will trigger player 2 to exert effort.

The intuition for proposition 1 is straightforward. In general, the principal will find it cost effective to provide incentives for the agent to exert effort when the marginal return to the worker's effort is high. So, if workers are complementary to each other, player 2's effort will have a bigger impact on the overall success of the team if player 1 succeeded rather than failed. Therefore, in order for player 2 to exert effort, he will need to be compensated for the lower probability of team success in the case where player 1 failed versus the case where player 1 succeeded. If the project's value is sufficiently high ($B > B^*$), the principal will find it profitable to provide incentives to player 2 even if player 1 failed. But, if the project's value is lower than this threshold ($B_* < B < B^*$), the principal will find it too costly to provide incentives to player 2 to exert effort if player 1 failed. Although it might seem intuitive that the principal would create an incentive mechanism to counter the urge for player 2 to shirk when player 1 fails, the model shows that this is only the case when the value of the project is sufficiently high. In intermediate cases, it is optimal for the principal not to waste his money on providing incentives to player 2 when the chances are low that player 2's effort will result in the overall success of a project which is not sufficiently valuable.

In contrast, if workers are substitutes in production, player 2's effort is more effective if player 1 fails in his task. If player 1 succeeds, then player 2 knows that his effort is not as crucial for the team to be successful, and therefore, player 2 would need a higher payment to exert effort in the case where player 1 succeeds. If the project is worth a lot ($B > B^*$), then the principal will find it profitable to incur this cost in order to improve the chances of team success even when the success of player 1 has already rendered player 2's effort to be less crucial. But, in the intermediate case ($B_* < B < B^*$), the principal will find it optimal to pay enough to player 2 to exert effort only when player 1 fails, since this is the case where player 2's effort is more critical to the success of the team. Once again, we see that the principal will not always design the optimal contract to guard against shirking in all cases—if workers are substitutes, it is often the case that it is not profitable to guard against shirking by player 2 if player 1 has already done most of the work that is critical for team success.

Proof of proposition 1: We start by deriving the optimal mechanism for a team where workers are complementary with each other. Let us examine the behavior of player 2, who is paid v_2 if the overall project is successful. Consider player 2's decision node after task 1 succeeded. Player 2's expected payoff will be $[\beta p(2) + (1 - \beta)p(1)]v_2 - c$ if he exerts effort and $[\alpha p(2) + (1 - \alpha)p(1)]v_2$ if he shirks. The optimal reward for player 2 should make him indifferent among these two options. Hence $v_2 = \frac{c}{(\beta - \alpha)[p(2) - p(1)]}$, and player 2 will exert effort under this contract if player 1 succeeded in his task. Furthermore, because the two workers are complementary, player 2 will shirk if player 1 failed in his task. This follows from the fact that player 2's effort has a lower marginal effect when player 1 fails and from the fact that player 2 is indifferent between shirking and exerting effort when player 1 succeeded. Hence v_2 is a mechanism that induces equilibrium 1. Consider now a mechanism v_2' under which player 2 exerts effort when player 1 fails in his task. The incentive constraint for this mechanism must be $[\beta p(1) + (1 - \beta)p(0)]v_2' - c \geq [\alpha p(1) + (1 - \alpha)p(0)]v_2'$ and $v_2' \geq \frac{c}{(\beta - \alpha)[p(1) - p(0)]}$. Due to the complementarity condition $[p(2) - p(1)] > [p(1) - p(0)]$, it follows that $v_2' > v_2$. Hence, if the contract is v_2', it is a dominant strategy for player 2 to exert effort in the complementarity case. This proves that the optimal mechanism in the case where workers are complements induces either equilibrium 1 or equilibrium 3. We now examine the case where workers are substitutes in production. We have seen that a mechanism that induces player 2 to exert effort when player 1 fails in his task must pay $v_2' = \frac{c}{(\beta - \alpha)[p(1) - p(0)]}$. Consider an alternative mechanism that induces player 2 to exert effort when player 1 succeeds. In this type of mechanism, player 2 faces the following constraint $[\beta p(2) + (1 - \beta)p(1)]v_2 - c \geq [\alpha p(2) + (1 - \alpha)p(1)]v_2$ and hence $v_2 \geq \frac{c}{(\beta - \alpha)[p(2) - p(1)]}$. Since workers are substitutes in production, the condition must hold that $[p(1) - p(0)] > [p(2) - p(1)]$. Therefore, it must be the case that $v_2 > v_2'$, which means that under v_2 it is a dominant strategy for player 2 to exert effort. Q.E.D.

Proposition 1 shows that the optimal mechanism in our moral hazard model yields equilibria that are consistent with the empirical results presented in the rest of the paper. We have managed to do so by specifying only the rewards that player 2 receives. For the sake of completeness, we now present the entire optimal mechanism in proposition 2 by specifying the rewards of both players.

Proposition 2. *Assume the technology is one of complementarity (substitution) and that the optimal mechanism yields equilibrium 1 (equilibrium 2). (The value of the project is B where $B_* < B < B^*$.) Then the optimal contract is given by*

$$v_1 = \frac{c}{(\beta - \alpha)[\beta p(2) + (1 - \beta)p(1) - (\alpha p(1) + (1 - \alpha)p(0))]}$$

$$\textit{and } v_2 = \frac{c}{(\beta - \alpha)[p(2) - p(1)]},$$

$$\left(v_1' = \frac{c}{(\beta - \alpha)[\alpha p(2) + (1 - \alpha)p(1) - (\beta p(1) + (1 - \beta)p(0))]}, \right.$$

$$\left. v_2' = \frac{c}{(\beta - \alpha)[p(1) - p(0)]} \right).$$

Proof of proposition 2: Consider the strategy of player 2 specified in equilibrium 1. If agent 1 exerts effort, he will succeed with probability β, thus equilibrium 1 implies that player 2 exerts effort with probability β. Therefore, if player 1 exerts effort, player 2's expected payoff is $[\beta(\beta p(2) + (1 - \beta)p(1)) + (1 - \beta)(\alpha p(1) + (1 - \alpha)p(0))]v_1 - c$. If player 1 shirks, he succeeds in his task with probability α. Thus, equilibrium 1 implies that player 2 exerts effort with probability α, and therefore receives $[\alpha(\beta p(2) + (1 - \beta)p(1)) + (1 - \alpha)(\alpha p(1) + (1 - \alpha)p(0))]v_1$. By equating these two expressions, we get $v_1 = \frac{c}{(\beta - \alpha)[\beta p(2) + (1 - \beta)p(1) - (\alpha p(1) + (1 - \alpha)p(0))]}$. Consider now the strategy of player 2 specified in equilibrium 2. In this case, if player 1 exerts effort he will trigger player 2 to exert effort with probability α. If player 1 shirks instead, he will trigger player 2 to exert effort with probability β. The incentive constraint faced by player 1 is now given by

$$\begin{aligned}&[\beta(\alpha p(2) + (1 - \alpha)p(1)) + (1 - \beta)\\ &\quad \times (\beta p(1) + (1 - \beta)p(0))]v_1' - c\\ &= [\alpha(\alpha p(2) + (1 - \alpha)p(1)) + (1 - \alpha)\\ &\quad \times (\beta p(1) + (1 - \beta)p(0))]v_1',\end{aligned}$$

yielding v_1' as specified above.

Overall, the simple framework in this section shows that a "positive" interaction should exist between workers who are complements in production, while a "negative" interaction should exist between workers who are substitutes. Psychological factors such as peer pressure and shame play no role in creating this interaction of effort choices. Our purpose is not to claim that workers can never affect each other because of behavioral considerations. Rather, our purpose is to demonstrate that these interactions could result from fully rational (income maximizing) considerations without relying on behavioral responses. Indeed, the remainder of the paper presents evidence from professional baseball that these types of interactions between workers are significant, and appear to be based on a rational response to the technology.

III. The Data and Background

The data were obtained from the "Baseball Archive," which is copyrighted by Sean Lahman and is freely available on the Internet for research purposes. The data contain extensive personal and yearly performance information on players, coaches, and teams from 1871 through the 2003 season. The analysis focuses on the modern period from 1970 to 2003 because Major League Baseball underwent a major expansion and restructuring into divisions just prior to that period. However, a similar analysis using data from 1871 to 1969 reveals very similar results.

The game of baseball presents an ideal case where the performance of each player is easily measured in a uniform way, and in complete isolation from the performance of his teammates. This contrasts with other sports, such as basketball, where total performance is hard to quantify and where the actions of one player, which do not always show up in statistics, can complement or come at the expense of the performance of his teammates. In addition, baseball players are easily divided into two distinct types: pitchers and batters. The function of pitchers is to prevent the other team from scoring runs, while the function of batters is primarily to help score runs for the team. In this sense, the two types of players are perfect substitutes for one another in team production—since the goal is to score more runs than the

TABLE 1.—SUMMARY STATISTICS FOR BATTERS AND PITCHERS, 1970–2003

	Batters	
	Means	Std.
Batting average	0.256	0.039
On-base-percentage	0.323	0.047
Slugging percentage	0.384	0.086
Teammates' pitching ERA	4.009	0.579
Teammates' batting average	0.265	0.012
Tenure	6.09	4.353
Age	29.009	4.132
Division batting average	0.266	0.007
Ballpark hitting factor	100.22	4.817
Manager's lifetime winning percentage	0.501	0.042
Sample size	13,767	

	Pitchers			
	Starters		Relief	
	Mean	Std.	Mean	Std.
ERA	4.826	2.487	4.695	3.415
Opponents' batting average	0.285	0.097	0.279	0.108
Teammates' ERA	3.981	0.562	4.084	0.580
Teammates' batting average	0.260	0.012	0.261	0.012
Games pitched per season	30.15	11.091	43.628	20.283
Games started	19.279	11.741	0	0
Age	28.455	4.247	29.771	4.131
Tenure	5.402	4.338	5.545	4.249
Division batting average	0.265	0.007	0.267	0.007
Ballpark pitching factor	100.341	4.919	100.237	5.101
Manager's lifetime winning percentage	0.501	0.042	0.501	0.042
Sample size	6,691		3,990	

other team. However, there is complementarity among the batters since it typically takes a series of hits within the same inning to score a run for the team. That is, a typical "hit" is meaningless for the team by itself (unless it is a home run), and therefore, the marginal productivity of getting a hit increases with the batting performance of the players who batted right before you. Therefore, batters can be considered complements with each other while batters and pitchers can be considered substitutes for each other.

In addition, pitchers are typically divided into two types: "starters" and "relief" pitchers. Starting pitchers typically start the game and continue until they get tired or into trouble, and then relief pitchers are called in to finish the game. A relief pitcher can ruin a good performance by the starter with a bad performance, or he could "save" the game with a good performance. Since multiple starting pitchers are never used in the same game, starting pitchers can be considered substitutes and competitors with each other, while being complements with relief pitchers.

Table 1 presents summary statistics for the sample of players from the 1970 to 2003 seasons. The sample includes all batters who batted at least fifty times in a season and pitchers who pitched in at least ten games. The main performance measure for batters is the "batting average" (BA), which is defined as the number of hits divided by the number of opportunities to bat ("at-bats") in a season. According to table 1, batters obtain a hit in 26% of their chances. Another conventional measure of batting performance is the "on-base-percentage," which takes into consideration other ways a batter can get on base (walks, hit by pitch, and so forth).[4] The standard indicator of a pitcher's performance is called the ERA (earned run average). This measure takes the number of runs that a pitcher allows the opposing team to obtain, and scales it by the number of innings played, so that it represents the average number of runs that would have been scored off the pitcher in a full game.[5] As such, a higher ERA reflects poorer performance. The average ERA is 4.83 for starting pitchers and 4.70 for relief pitchers.[6] Another indicator of a pitcher's performance is the "opponent's batting average," which is defined as the number of hits allowed divided by the number of batters faced. Although there are only small differences in the average performance measures between starting and relief pitchers, the differences in their roles are highlighted by the average number of games pitched (44 for relief pitchers

[4] The exact definitions of the batting measures are as follows: batting average equals the number of hits divided by the number of at-bats. On-base-percentage is defined as (hits + walks + number of times hit by pitch) divided by (at-bats + walks + sacrifice flies + number of times hit by pitch). Slugging percentage is equal to (singles + 2 × doubles + 3 × triples + 4 × home runs)/(at-bats).

[5] The ERA is calculated by: (number of earned runs/innings pitched)×9.

[6] A pitcher was defined as a starting pitcher if he started at least one game in the season.

versus 30 for starters) and the average number of games started (19 for starters versus 0 for relief pitchers).

There is very little mobility between the two types of pitchers, and batters can also be categorized into three main categories: (i) "skilled positions" (second base, third base, and short-stop), which emphasize fielding skills at the expense of hitting prowess; (ii) "power positions" (first base, outfielders, and designated hitters), which primarily emphasize power hitting; and (iii) "catchers," which have distinct fielding skills and are typically power hitters. The specialization of batters into these three categories means that players in two different categories can be considered as complements in the production of team runs, and not as competitors or substitutes with each other. The next section examines whether a player's performance interacts with the actions of his teammates as indicated by the theory in section II.

IV. The Basic Regression Analysis

This section examines how the performance of individual players varies with the performance of his fellow workers. The basic regression equation is the following:

$$
\begin{aligned}
\mathit{performance}_{it} = \beta_0 \\
&+ \beta_1(\mathit{teammates' \ pitching \ ERA})_t \\
&+ \beta_2(\mathit{teammates' \ batting \ ave})_t + \mu_i \\
&+ \beta_3(\mathit{other \ controls})_t + \varepsilon_{it},
\end{aligned}
$$

where the performance of player i in year t depends on his teammates' pitching performance in year t, his teammates' batting performance in year t (not including the batting performance of pitchers), the ability of player i represented by μ_i, other observable control variables, and the unobserved random component, ε_{it}. The other control variables include the batting average in player i's division (excluding his own team) in year t, which controls for the quality of the pitching and batting in the team's division in the same year; the team manager's lifetime winning percentage, which is an indicator for the quality of the team's coaching; the ballpark hitting and pitching factors, which control for whether the team's ballpark is easy or difficult for batters in year t; the player's years of experience (number seasons played in the league); year effects; and dummy variables for each division. The unobserved ability of player i, μ_i, is controlled for by using a fixed-effects specification or by using a first-differences specification between consecutive years.

Teams naturally choose their rosters in an endogenous way. Given a team's budget constraint, team owners will maximize the team's success by picking players who will interact in an optimal way. This process will produce some teams that concentrate on acquiring a group of strong batters (since a group is necessary to produce runs) at the expense of acquiring good pitchers. In fact, there is a negative correlation between team batting and team pitching performance in a cross section of teams within a given year. However, this negative relationship will not produce spurious effects in the regression specification above because of the inclusion of player fixed effects and the use of other controls at the individual, team, year, and division level. In particular, after controlling for the fixed effect of player i and for a typical player's experience profile, identification comes from seeing whether variation within a given player from the typical player's experience profile can be explained by variation in his teammates' performance levels.

It is important to note that the pitching and batting variables for the teammates of player i do not include the performance of player i. Therefore, identification of the model does not suffer from the reflection problem pointed out by Manski (1993) that occurs when a variable is regressed on a transformation of itself. As stated above, identification of β_1 and β_2 comes from seeing whether variation within player i's performance levels across years is correlated with the performance levels of his teammates. Within a given player's career, variation in his performance over time cannot be aggregated to produce the mean of his teammates' performance levels. So, the basic regression specification does not suffer from this aspect of the reflection problem pointed out by Manski (1993), but as we discuss in the next section, problems could arise if there is a common shock to all team members in a given year.

The basic fixed-effect regressions for pitchers and batters are presented in table 2. Column 1 shows that after controlling for all the other variables, a batter's performance decreases when the pitchers on his team are pitching well. (A lower ERA indicates stronger pitching performance.) In contrast, column 2 shows that a given batter has better than average years when the other batters on the team are doing well. The specification in column 3 includes the performance measures of both the batters and pitchers as explanatory variables, and the results are essentially unchanged. Thus, the results are robust to estimating the effect of pitchers and batters separately (columns [1] and [2]) or when they are estimated together in column 3. Therefore, the results are not a product of a high correlation between the two variables.

Columns 5–7 present the basic results for pitchers, and show that a pitcher performs better when his fellow pitchers are doing better, but there is no significant effect of the team's batting performance on a pitcher's performance—a finding that repeats itself throughout the paper. Again, the effect of the player's fellow pitchers on his own performance is robust to the inclusion or exclusion of the team's batting performance. Regarding the other control variables, they all have the expected signs and are generally significant for the batting and pitching regressions, although it is worth noting that the results are robust to excluding them.

TABLE 2.—BASIC OLS FIXED-EFFECTS RESULTS FOR BATTERS AND PITCHERS, 1970–2003

	Batters				Pitchers			
	Batting Average				Pitching ERA			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Teammates' pitching ERA	0.318		0.282	0.303	0.521		0.523	0.529
	(0.082)		(0.083)	(0.107)	(0.080)		(0.081)	(0.117)
Teammates' batting average		0.221	0.214	0.164		0.719	−0.915	−1.064
		(0.031)	(0.031)	(0.045)		(3.133)	(3.136)	(5.006)
Annual division batting average	0.291	0.312	0.245	0.217	21.987	34.488	22.207	19.121
	(0.065)	(0.062)	(0.065)	(0.090)	(6.179)	(5.945)	(6.224)	(9.658)
Manager's lifetime winning pct.	0.052	0.025	0.034	0.044	−1.214	−2.556	−1.139	−4.698
	(0.009)	(0.009)	(0.009)	(0.014)	(0.834)	(0.848)	(0.874)	(1.536)
Ballpark hitting factor/1,000	0.509	0.482	0.418	0.436				
	(0.078)	(0.077)	(0.079)	(0.101)				
Ballpark pitching factor					0.025	0.039	0.025	0.013
					(0.007)	(0.007)	(0.007)	(0.011)
Log salary				0.002				0.064
				(0.001)				(0.063)
Division dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenure dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,767	13,767	13,767	6,486	10,681	10,681	10,681	4,985

Standard errors are in parentheses. The coefficient and standard error for teammates' pitching ERA have been multiplied by 100 for the batting results, but not for the pitching regressions. Tenure and year dummies are included for every five-year interval.

One possible explanation for the pitching results is that a coach is more likely to let a pitcher stay in the game longer, or use him in more games, if the other pitchers on the team are weaker. That is, the coach will let the pitcher struggle longer in the game when there are weaker replacements on the bench, thus inducing a positive correlation between a pitcher's ERA and those of his fellow pitchers. We can control for this by including the number of innings and games played by the pitcher in the regression. After adding these variables into the specification, the coefficient on his teammate's ERA goes from 0.523 (t-statistic of 6.51) to 0.625 (t-statistic 8.01). Therefore, the interaction between pitchers appears even stronger after controlling for how long the pitcher is left in the game. However, including these variables is problematic since a player's performance and playing time are clearly determined simultaneously. For this reason, we choose not to include these variables in the core specification, but it is worth noting that the results are robust to including the amount of playing time into the regressions for both pitchers and batters.

An additional complication could arise if the terms of the contract are endogenous (see Kendall, 2003). For example, if a team owner guards against "psychological" shirking behavior by creating more incentives to exert effort when a batter is playing with less talented batters (or batters that like to shirk), we should see batters play better when they are playing with less talented batters, which is the opposite of what we see in table 2. Therefore, if contracts are structured to prevent "group shirking," the response of a batter to his fellow batters would be biased toward 0 in table 2, but the response of a batter to his fellow pitchers would be biased away from 0. In addition, if this scenario were true, the response of a pitcher to his fellow pitchers would also be biased toward 0 in table 2. Therefore, the overall significance of the results suggests that contracts are written in a way that is consistent with the theory in section II, so that a "positive interaction" exists between workers who are complements while a "negative interaction" exists between workers who are substitutes. However, to see whether the results are robust to controlling for the various incentives built into the contract, table 2 includes the individual's salary as a control variable in column 4 for batters and column 8 for pitchers. Salary data are available only for the years 1985–2003, so the sample is smaller than the other columns, but the results are similar to what is found without including salary in the regressions. Therefore, the results appear to be robust to the various confounding factors built into each player's contract.

Although many of the coefficients in table 2 are significant statistically, the implied magnitudes are not very large. If we take a 2-standard-deviation change in the batting average of a player's teammates (0.024 from table 1), the predicted change in a player's batting average based on the results in the third column of table 2 is 0.005, which is only 13% of the standard deviation in a player's batting average (0.039 in table 1). The predicted change in a batter's batting average due to a 2-standard-deviation change in the team's pitching ERA would be 0.0032 (the coefficient 0.282 in table 2 multiplied by 100, multiplied by two times 0.579, the standard deviation of team ERA in table 1). The predicted change in a pitcher's ERA due to a 2-standard-deviation change in his teammates' ERA is 0.588. This predicted increase represents a little more than 23% of the standard deviation of a starting pitcher's ERA. Although these predicted changes are not very large, adding five points (the predicted change of 0.005 for a batter from the other batters

TABLE 3.—BASIC OLS FIRST-DIFFERENCES RESULTS FOR BATTERS AND PITCHERS, 1970–2003

	Batters				Pitchers			
	Δ Batting Average				Δ Pitching ERA			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Teammates' pitching ERA	0.423		0.388	0.477	0.356		0.353	0.250
	(0.087)		(0.088)	(0.112)	(0.080)		(0.081)	(0.114)
Δ Teammates' batting average		0.178	0.166	0.151		2.903	1.445	2.154
		(0.033)	(0.033)	(0.045)		(3.196)	(3.210)	(4.986)
Δ Annual division batting average	0.297	0.355	0.252	0.250	23.767	32.253	23.329	16.580
	(0.065)	(0.062)	(0.066)	(0.90)	(5.995)	(5.726)	(6.074)	(9.061)
Δ Manager's lifetime winning pct.	0.033	0.011	0.020	0.009	−1.311	−2.186	−1.428	−5.109
	(0.010)	(0.010)	(0.010)	(0.016)	(0.944)	(0.964)	(0.979)	(1.711)
Δ Ballpark hitting factor/1,000	0.290	0.299	0.219	0.150				
	(0.097)	(0.097)	(0.098)	(0.121)				
Δ Ballpark pitching factor					0.016	0.025	0.016	0.004
					(0.009)	(0.009)	(0.009)	(0.013)
Δ log salary				−0.009				0.585
				(0.001)				(0.107)
Δ division dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenure dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,863	10,863	10,863	5,321	8,026	8,026	8,026	3,957

Standard errors are in parentheses. The symbol Δ indicates the first difference between consecutive years in the variable for the same person. The coefficient and standard error for Δ Teammates' pitching ERA have been multiplied by 100 for the batting results, but not for the pitching regressions. Tenure and year dummies are included for every five-year interval.

on the team) or increasing a pitcher's ERA by 0.562 would probably not be considered entirely trivial to a fan or a player.

Table 3 performs a similar analysis to table 2 but controls for unobserved heterogeneity by using a first-difference specification between consecutive years rather than using fixed effects. The results are very similar in the sense that a batter is shown to be affected by the batting and pitching performance of his teammates, while a pitcher is affected only by his fellow pitchers. The magnitudes of the coefficients are a little different from the estimates in table 2, with some smaller and some bigger in size, but inferences regarding significance are very similar.

Overall, the results are consistent with the theory that players should be positively affected by the performance of their fellow workers when they are complements in production (like batters between themselves), but negatively affected by the performance of their fellow workers when they are substitutes in production (like batters and pitchers). Although the finding that a pitcher is positively affected by other pitchers is consistent with the theory, the theory cannot explain why a pitcher does not seem to react in either direction to the team's batting performance. However, the fact that there is a differential reaction to both types of players from both types of players is strong evidence against a behavioral explanation for the results. A typical behavioral response would be for any worker to work harder when his coworkers are working harder, regardless of whether they are complements or substitutes in team production. This prediction is clearly rejected in the analysis. So, the differential responses according to the role of each type of player can be viewed as evidence for the idea that the technology of production significantly influences the interaction of effort choices across workers.

V. Alternative Explanations and Robustness Checks

A. *Different Performance Measures*

We now examine whether the results in tables 2 and 3 are robust to using different measures of a player's performance. As a basis for comparison, the first column in the upper panels of tables 4 and 5 replicate the batting regression results already seen in tables 2 and 3 for the fixed-effects and first-differences specifications, respectively. The first column in the bottom panels of tables 4 and 5 use the on-base-percentage of a player instead of a player's batting average as the dependent variable. This measure differs from the batting average by considering the ability of a player to get on base by a "walk" or getting hit by a pitch rather than only by hitting. Tables 4 and 5 show that the results using on-base-percentage as the dependent variable are virtually identical to those using a player's batting average as the performance measure. Although not shown, the results are also similar if we use a player's home-run output as the dependent variable. Tables 6 and 7 present results for pitchers using a different measure of a pitcher's performance: the opposing team's batting average while he was pitching. As a basis for comparison, the first columns in tables 6 and 7 replicate the results for pitchers seen in tables 2 and 3 using a pitcher's ERA as his performance measure. The first column in the bottom panel uses the opposing team's batting average as the dependent variable, and the results are very similar to those obtained using the ERA. Therefore, the results for pitchers and batters appear to be robust to using alternative, conventional measures of a player's performance.

TABLE 4.—ROBUSTNESS CHECKS FOR BATTERS USING FIXED EFFECTS, 1970–2003

	OLS					2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Batting Average						
Teammates' pitching ERA/100	0.283 (0.083)	0.191 (0.141)	0.282 (0.083)	0.321 (0.083)	0.410 (0.197)	0.293 (0.083)
Teammates' bitting average	0.214 (0.031)	0.142 (0.055)				0.150 (0.061)
Teammates' in other positions batting average			0.180 (0.026)			
Teammates' lifetime BA				0.140 (0.057)		
Teammates' lifetime BA before season					0.209 (0.093)	
On-Base-Percentage						
Teammates' pitching ERA/100	0.384 (0.089)	0.229 (0.151)	0.384 (0.089)	0.422 (0.089)	0.595 (0.210)	0.388 (0.090)
Teammates' batting average	0.219 (0.034)	0.175 (0.058)				0.194 (0.066)
Teammates' in other positions batting average			0.175 (0.028)			
Teammates' lifetime BA				0.180 (0.061)		
Teammates' lifetime BA before season					0.225 (0.100)	
Sample restriction	None	One ballpark, manager per player	None	None	First season on new team	None

Standard errors are in parentheses. The coefficients in each column of each of the two panels come from the same regression with the dependent variable defined by the panel. Each regression included the annual division batting average, manager's lifetime winning percentage, ballpark hitting factor, year dummies, tenure dummies, division dummies, and individual fixed effects. The 2SLS regression uses teammates' lifetime batting average as the instrument for teammates' batting average. The sample size in column 5 is 3,968.

B. *Switching Teams*

One can think of players switching teams as potentially an ideal natural experiment that produces variation in one's coworkers. Ichino and Maggi (2000) use this type of variation to test whether changing locations affects the shirking behavior of workers. The second column in table 5 replicates the first-differences analysis, but uses only the sample of years where the player changed teams across consecutive years. The results are virtually identical to those using the full sample in column 1. The results are also very similar using the restricted sample of pitchers who changed teams in column 2 of table 7. Therefore, the results show that

TABLE 5.—ROBUSTNESS CHECKS FOR BATTERS USING FIRST DIFFERENCES, 1970–2003

	OLS					2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Batting Average						
Δ Teammates' pitching ERA/100	0.388 (0.088)	0.650 (0.157)	0.205 (0.135)	0.340 (0.088)	0.424 (0.088)	0.401 (0.089)
Δ Teammates' batting average	0.166 (0.032)	0.176 (0.060)	0.104 (0.049)			
Δ Teammates' in other positions batting average				0.098 (0.027)		
Δ Teammates' lifetime batting average					0.094 (0.072)	0.108 (0.083)
Δ On-Base-Pct.						
Δ Teammates' pitching ERA/100	0.426 (0.093)	0.768 (0.167)	0.247 (0.142)	0.436 (0.093)	0.465 (0.093)	0.451 (0.094)
Δ Teammates' batting average	0.179 (0.035)	0.159 (0.064)	0.126 (0.053)			
Δ Teammates' in other positions batting average				0.115 (0.029)		
Δ Teammates' lifetime batting average					0.055 (0.076)	0.063 (0.088)
Sample restriction	None	Team-changers	Same ballpark, manager	None	None	None

Standard errors are in parentheses. The coefficients in each column of each of the two panels come from the same regression with the dependent variable defined by the panel. Each regression includes the Δ annual division batting average, Δ manager's lifetime winning percentage, Δ ballpark hitting factor, year dummies, tenure dummies, and the Δ division dummies. The 2SLS regression uses Δ teammates' lifetime batting average as the instrument for Δ teammates batting average.

TABLE 6.—ROBUSTNESS CHECKS FOR PITCHERS USING FIXED EFFECTS, 1970–2003

	OLS						2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pitcher's ERA							
Teammates' pitching ERA	0.523	0.576					0.680
	(0.081)	(0.114)					(0.206)
Teammates' ERA of relief pitchers			1.262				
			(0.083)				
Teammates' ERA of starting pitchers				0.643			
				(0.157)			
Teammates' lifetime ERA					0.532		
					(0.161)		
Teammates' lifetime ERA before season						0.261	
						(0.336)	
Teammates' batting average	−0.915	6.487	−3.499	−3.486	1.149	14.381	−1.403
	(3.136)	(4.599)	(3.374)	(6.601)	(3.134)	(8.457)	(3.192)
Pitcher's Opponent's BA							
Teammates' pitching ERA	0.014	0.016					0.025
	(0.003)	(0.004)					(0.009)
Teammates' ERA of relief pitchers			0.034				
			(0.004)				
Teammates' ERA of starting pitchers				0.017			
				(0.006)			
Teammates' lifetime ERA					0.020		
					(0.007)		
Teammates' lifetime ERA before season						0.000	
						(0.014)	
Teammates' batting average	−0.204	0.091	−0.194	−0.508	−0.153	0.330	−0.237
	(0.126)	(0.179)	(0.150)	(0.248)	(0.126)	(0.338)	(0.128)
Sample restriction	None	One ballpark, manager per player	Starting pitchers	Relief pitchers	None	First season on new team	None

Standard errors are in parentheses. The coefficients in each column of each of the two panels come from the same regression with the dependent variable defined by the panel. Each regression includes the annual division batting average, manager's lifetime winning percentage, ballpark pitching factor, year dummies, tenure dummies, division dummies, and individual fixed effects. The 2SLS regression uses teammates' lifetime pitching ERA as the instrument for teammates' pitching ERA. The data for opponent's batting average are missing for 1998–2002.

players bat better when they move to teams with better batters and worse pitchers, while pitchers play better if they move to a place with better pitchers.

C. *Not Switching Teams*

One explanation for the previous set of results is that there might be an unobserved reason why certain teams have good batting but bad pitching, and when a player moves to that team his performance changes accordingly. For example, it could be that the player is affected by the coaching change, or that certain ballparks favor batters over pitchers, or the team could play in a new division where teams are very strong in pitching or batting, or the team's city may be in a part of the country where the weather favors pitching or batting. The basic regressions in tables 2 and 3, as well as the previous regressions using only the sample of team-changers, control for many of these scenarios by including measures of managerial quality, indices for whether the ballpark favors batting or pitching, the batting average of the division in the same year, and division dummy variables. However, these measures may be imperfect. So, to completely control for this scenario, we restrict the fixed-effect analysis only to the seasons in which the player played for the same team, manager, and ballpark (the combination of which the player stayed with the longest). For batters, the fixed-effect analysis is presented in column 2 of table 4, while the first difference analysis using a sample of players who do not change teams in consecutive years is shown in column 3 of table 5. For pitchers, the respective regressions are in column 2 of table 6 and column 3 in table 7. Overall, the results are very similar to those using the whole sample and the sample of players who changed teams. However, the magnitudes of the coefficients tend to be weaker for the players who stay on the same team over consecutive years versus those that change teams. This tendency is most likely due to the fact that most of the variation in one's coworkers comes from players changing teams. The fact that there is enough variation in one's coworkers even within the same team in consecutive seasons to explain variation in an individual's performance supports the interpretation that the main results are not due to endogenous moving.

D. *Complements or Competition between Players?*

The positive effect of fellow batters on the individual performance of a batter, or fellow pitchers on the pitching performance of an individual pitcher, could theoretically be due to the complementarity between players or the competition between players for increased playing time. We now attempt to isolate these two stories by dividing batters and

TABLE 7.—ROBUSTNESS CHECKS FOR PITCHERS USING FIRST DIFFERENCES, 1970–2003

	OLS						2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ ERA							
Δ Teammates' pitching ERA	0.353	0.483	0.226				0.552
	(0.081)	(0.190)	(0.096)				(0.249)
Δ Teammates' ERA of relief pitchers				1.068			
				(0.086)			
Δ Teammates' ERA starting pitchers					0.600		
					(0.148)		
Δ Teammates' lifetime pitching ERA						0.410	
						(0.185)	
Δ Teammates' batting average	1.445	3.996	−0.272	3.693	−9.834	3.250	0.623
	(3.210)	(7.346)	(3.872)	(3.488)	(6.263)	(3.199)	(3.355)
Δ Opponent's Batting Average							
Δ Teammates' pitching ERA	0.008	0.016	0.002				0.010
	(0.003)	(0.009)	(0.004)				(0.012)
Δ Teammates' ERA of relief pitchers				0.027			
				(0.004)			
Δ Teammates' ERA starting pitchers					0.016		
					(0.006)		
Δ Teammates' lifetime pitching ERA						0.007	
						(0.009)	
Δ Teammates' batting average	−0.101	−0.129	−0.067	0.127	−0.720	−0.065	−0.108
	(0.137)	(0.325)	(0.164)	(0.159)	(0.254)	(0.136)	(0.144)
Sample restriction	None	Team-changers	Same ballpark, manager	Starting pitchers	Relief pitchers	None	None

Standard errors are in parentheses. The coefficients in each column of each of the two panels come from the same regression with the dependent variable defined by the panel. Each regression includes the Δ annual division batting average, Δ manager's lifetime winning percentage, Δ ballpark hitting factor, year dummies, tenure dummies, and the Δ division dummies. The 2SLS regression uses Δ teammates' lifetime pitching ERA as the instrument for Δ teammates' pitching ERA. The data for opponent's batting average are missing for 1998–2002.

pitchers into types of positions that are clearly not substitutable (in other words, no competition between players of each type).

As discussed above, pitchers can be divided into "starters" or "nonstarters," while batters can be categorized as either a "skilled position," a "power position," or a "catcher." There is very little mobility between these types of positions, so it is reasonable to assume that there is very little competition between players across these types of positions. Therefore, the analysis is now performed using the average performance of player *i*'s teammates, but using only those teammates that play a different position than player *i*. For batters, the fixed-effect results are presented in column 3 of table 4 while the first-difference results are in column 4 of table 5. The results are virtually identical to those using the average performance of all the player's fellow teammates, including those that play the same position. For pitchers, the regressions are run separately for starting pitchers and nonstarting pitchers, where the explanatory variable is the performance measure of the other group. The fixed-effect results for pitchers are in columns 3 and 4 of table 6 while the first-difference results are in columns 4 and 5 of table 7. Interestingly, the pitching performance of nonstarters has a bigger effect on starting pitchers than vice versa. However, for starters and nonstarters, the coefficients are significant and generally larger in magnitude than the coefficients in previous specifications that lumped all pitchers together. This general pattern points to larger "cross" effects across types of pitchers than within types of pitchers. This pattern bolsters the technology-based interpretation of the results since each pitcher is clearly more complementary with a pitcher of the other type than pitchers within the same type.

E. *IV Results*

The results so far are robust to looking at the whole sample, a restricted sample of players changing teams, a restricted sample of players staying with the same team, and to using the performance of teammates playing in noncompeting positions. In addition, our use of fixed effects controls for the overall, unobserved ability of each player. However, as Manski (1993) points out, there could be an unobserved factor responsible for the high or low performance of all players on the team in a given year, and therefore, this produces a correlation between the performances of players without there actually being a causal effect. If this explanation were true, it seems inconsistent with our results which show that the sign of the effect depends on the degree of substitutability and complementarity between players—since it is unlikely that an unobserved factor is inducing all batters to do better in the same year that the team's pitchers are having bad years.

However, to further examine this possibility, we repeat the analysis using the lifetime performance of player *i*'s teammates instead of their current performance in year *t*. Using lifetime performance allows us to wash out all the idiosyncratic shocks to a specific team in a given year, since the lifetime performance of any given player does not change from year to year. As a result, variation in the lifetime performance of player *i*'s teammates stems only from changes in the composition of his coworkers. This identification strategy is similar to the one employed by Ichino and Maggi (2000) and used above when we restricted the sample to players who move teams. However, the use of the lifetime performance of one's teammates allows us to exploit changes in the composition of one's teammates within the same team as well as across teams when players move.

The results for batters are presented in column 4 of table 4 and column 5 of table 5, and are virtually identical to column 1, which uses the batting average of one's teammates in the current year. The first-difference results in table 5 are a little weaker in significance, but the magnitude is not much lower than the coefficient estimates in other specifications. For pitchers, the fixed-effect results are in column 5 of table 6 while the first-difference results appear in column 6 of table 7. The pitching results using the lifetime achievements of one's teammates are very similar to those using their current achievements. Furthermore, the last column of tables 5, 6, 7, and 8 repeats the analysis for batters and pitchers, but uses the lifetime performance as an instrumental variable for the current performance of one's coworkers in a 2SLS regression. Again, the results are very similar to using the lifetime achievements directly as a regressor and to using the current performance of one's teammates.

Finally, in order to net out the effect of player *i*'s effect on the lifetime performance of his teammates, column 5 in table 4 repeats the analysis for batters but restricts the sample to only those years when player *i* is playing on a new team, and then explains his current performance with the lifetime achievements of his new teammates up to the year prior to him joining the team. Column 6 of table 6 runs a similar regression for pitchers. Using this specification, the results for batters in table 4 are unchanged from previous regressions, but the results for pitchers in table 6 are not significant. This last result is probably due to the fact that prior lifetime results are more likely to be noisy for pitchers than batters (the coefficient of variation for a pitcher's ERA is twelve times larger than the coefficient of variation of a batter's batting average: 1.94 for pitchers versus 0.152 for batters in table 1). The overall results in this section point once again to the idea that batters are positively affected by other batters but negatively affected by their fellow pitchers, while pitchers seem to be positively affected by other pitchers, but not by their fellow batters.

VI. Conclusion

This paper analyzes how a player's performance is dependent on the performance of his coworkers. Using data on professional baseball players, the results show that a batter's performance increases with those of his fellow batters, but decreases with the quality of his team's pitching. The results also indicate that a pitcher's performance increases with the pitching performance of the other pitchers on the team, but is unaffected by the batting performance of the team.

The differential reaction to both types of players from both types of players suggests that the results are not likely to be explained by behavioral explanations such as peer pressure, guilt, and social norms. These types of explanations would predict that any type of workers will work harder when his coworkers are working harder, regardless of the function of his job in relation to the function of his coworkers. Therefore, the differential responses according to the role of each type of player can be viewed as evidence in favor of the idea that workers adjust their effort in a rational way that is dependent on the technology of team production. This interpretation is strengthened by the many robustness checks with different samples and specifications, as well as instrumenting the performance of one's coworkers with their lifetime performance. All of the variation in this instrument comes from changes in the composition of one's coworkers, and therefore, is unaffected by transitory shocks that may affect the performance of all players on a team in a given year.

Although the empirical analysis is performed using data on baseball players, the results are likely to apply to many work environments where there is an element of team production. Whenever workers have to work in teams, there are bound to be complementarities and substitutability between different kinds of workers, and therefore, the framework analyzed in this paper is likely to be relevant. It is important to note that a key assumption driving the theoretical results is that an individual's wage is a function of the aggregate team performance. If this assumption did not hold, there would be no "technological" basis for a worker to alter his performance according to the output of his fellow workers, since his wage would be purely a function of his own individual performance. However, there are two main reasons to expect that an individual's wage is affected by the performance of the whole team, even in cases where there are effective ways to evaluate individual performances (see also Alchian & Demsetz, 1972). First, a higher team performance may generate higher profits, and thus, increase the value of the marginal product of labor. Second, a higher team performance can serve as a signal for aspects of a player's ability that are hard to observe or quantify, and thus, are not reflected in typical performance measures. For example, individuals on a winning team project may be considered industrious workers who are able to work well together with fellow workers. Therefore, the "technological" interaction of effort choices highlighted in this paper is likely to be relevant for many work environments where cooperation among workers is important.

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