What Manager Traits Matter? Productivity as a Worker and Common Characteristics with Workers

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Abstract

This paper studies what manager traits matter for firm and worker productivity in the context of Major League Baseball (MLB), by exploiting manager moves across teams. I find that managers have meaningful impacts on team wins, but good players do not necessarily make good managers. I also find improvements in player productivity if a manager and a player share a common characteristic, including same position, left-handedness, and same hometown. Effects are more pronounced for young players. There is little heterogeneity in treatment effects in terms of manager quality, indicating that good managers create value by improving all workers' productivity.

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1 Introduction

The existing literature clearly shows that managers create value for firms and matter substantially for firm productivity (Benson and Shaw, 2025). However, much of the existing literature focused

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on CEOs, and lower-level managers have not received much attention until recently. Emerging evidence indicates that lower-level managers matter for boosting subordinate performance (Minni, 2026), but there is much less conclusive evidence regarding what manager traits matter (Hoffman and Stanton, 2025). While it is difficult to precisely forecast the value-added of a managerial candidate in many contexts, it is possible to observe and select on particular managerial traits, which underscores the economic value of understanding what manager traits matter.

By running a lab experiment, Weidmann et al. (2026) find that managerial performance is strongly predicted by the economic decision-making skill, but not by demographic characteristics and personality traits. This finding is further verified using the administrative store data from a large retail firm. Moreover, the desire to lead is a negative characteristic. While this paper makes significant progress in answering this question, the economic decision-making skill is not always measurable. Therefore, many other manager traits are still worth investigation.

A natural characteristic to focus on is a manager's productivity as a rank-and-file worker. One might imagine that the best workers would naturally make the best managers, especially if ability within a firm is unidimensional. However, the existing literature find mixed evidence on this manager trait. Kim and Makadok (2022) find that National Basketball Association teams managed by head coaches who had previously been All-Star NBA players have better performance. Analyzing a large number of sales firms, Benson et al. (2019) find that more productive sales workers are much more likely to be promoted to managerial roles, but after becoming a manager, there is no positive relationship between former productivity as a worker and managerial productivity.

In addition to individual characteristics of managers, interactions between characteristics of managers and workers can also matter. Characteristics like race and gender have been examined extensively in other settings, such as teacher/professor-student and doctor-patient interactions. In the context of managers, the existing literature examined the importance of gender concordance (Cullen and Perez-Truglia, 2023; Fortin et al., 2022), value dissonance (Delfino and Espinosa, 2025) and hometown bias (Yonker, 2017; Ahmadi et al., 2022) between managers and workers. In professional sports specifically, interactions such as culture diversity (Peeters et al., 2021) and racial matching (Mills et al., 2025) has been well studied.

This paper analyzes how manager traits affect worker and firm performance, taking advantage of manager turnovers in Major League Baseball (MLB). Compiled from the Lahman Baseball

Database and Baseball Reference, the analytic data covers 153 MLB seasons from 1871 to 2023 including 20,731 players and 753 field managers, analogous to a typical matched employer-employee dataset. This novel setting is well suited to study the effects of manager traits for a number of reasons. First, team productivity, individual characteristics, worker labor supply and productivity, and worker/manager mobility are readily observable, which is rarely seen in other labor markets. Second, more than a half of MLB managers were MLB players and I can observe their performance as players. Third, manager turnovers are common in MLB. The analysis data set contains a total of 1,067 manager transitions involving 590 unique managers. Fourth, there is almost no potential (future) competition between managers and workers. In some cases, managers might be worried that some workers might be promoted due to good work performance and become a future competitor.

I first document the effects of a manager's productivity as a player. I show that MLB managers have a quantitatively meaningful impact on team wins. Then, I estimate managerial quality using a value-added model. Lastly, I investigate the correlation between managerial quality and a manager's productivity as a player. I find that good players do not necessarily make good managers. In other words, skills required to succeed at the player level are much different from those required to succeed as a manager. I also find that former-MLB-player managers tend to be better managers, but this positive correlation disappears in more recent years.

In the second part of the paper, I evaluate the effects of shared characteristics between managers and players. The common characteristics examined in this paper include whether a manager and a player were born in the same hometown, whether they play the same position, and whether they are both left-handed pitchers or batters. Coupling manager turnovers with a staggered difference-in-differences (DiD) estimator (Callaway and Sant'Anna, 2021), I am able to identify the causal effects of these common characteristics under the parallel trend assumption. Focusing on variation from manager moves rather than player moves allows me to rule out the effects of common characteristics on hiring. Prior research has demonstrated that managers are more likely to hire workers with shared characteristics (Giuliano and Ransom, 2013; Åslund et al., 2014; Hadlock and Pierce, 2021; Kerr and Kerr, 2021), which indicates potential selection and matching in player moves and can further impact player performance via a different mechanism.

I find that all these common characteristics have positive effects on player productivity. Specifically, the effect of same position is the largest and the effect of a shared hometown is the smallest.

I view these differences as reasonable, because mentoring might be more effective between same-position or both-lefties managers and players. I also find heterogeneity in treatment effects of common characteristics. Results show larger treatment effects for a player who was young when his new manager shared a common characteristic. These effects can be life-changing for young players, because early career shocks can have long-lasting effects on their labor market outcomes (Wachter, 2020; Arellano-Bover, 2022; Casas-Arce et al., 2024). Treatment effects are generally not heterogeneous by manager quality, which indicates that good managers create more team wins by improving all players' productivity, consistent with Altindag et al. (2025). Overall, these results suggest that same position and both lefties take effect via mentoring, while the effects of same hometown is more of in-group bias.

This paper make significant contributions to the personnel economics literature in multiple ways. First, it contributes to the literature on manager traits. Regarding a manager's productivity as a worker, where there is little hard evidence (Hoffman and Stanton, 2025), I find evidence at odds with Kim and Makadok (2022) but consistent with Benson et al. (2019), that there is no positive correlation between this manager trait and manager quality. With respect to common characteristics, this paper complements a prior study by investigating a different hierarchical level of interactions at workplace. While this closely related paper focuses on interactions (match quality) between top and lower-level managers in MLB (Peeters et al., 2020), my paper examines interactions between lower-level managers and workers.

It further contributes to the literature by discussing the mechanisms through which these common characteristics affect worker productivity. Specifically, this paper presents novel evidence of two skill-related common characteristics, same position and both lefties. I further provide suggestive evidence that mentoring drives the effects of these two common characteristics. Mentoring is an effective way to invest in human capital and improve worker productivity (Sandvik et al., 2025; Sariri, 2025), and the effects of mentoring depend on the characteristics between a mentor and mentee (Athey et al., 2000; Hilmer and Hilmer, 2007; Blau et al., 2010). This paper highlights the importance of mentoring in intergenerational transfer of skills at workplace.

Results also suggest that the other common characteristic, same hometown, is probably driven by in-group bias. This finding is consistent with the literature on hometown bias (Yonker, 2017; Ahmadi et al., 2022), which can be viewed as a special case of in-group bias. In-group bias refers

to a pattern of favoring members of one's in-group over out-group members, which has been well documented in referee/judge decisions (Price and Wolfers, 2010; Mocan and Osborne-Christenson, 2024) and teacher-student interactions (Hoffmann and Oreopoulos, 2009; Fairlie et al., 2014; Altindag et al., 2025; Fry and Glennon, 2025). In the context of workplace, Eren (2023) and (Jannati et al., 2025) both find evidence of evaluators overrating evalutees due to gender-related in-group bias in an educational setting and financial markets respectively. This paper complements these prior studies by showing that managers might not only overrate in-group workers, but also favor them through better exposure to opportunities.

Finally, this paper contributes to the literature on predicting and selecting baseball managers (Singell, 1993; Smart and Wolfe, 2003; Smart et al., 2008; Peeters et al., 2020; Crosby, 2021). The primary challenge in this literature is credibly measuring managerial quality. For instance, two related papers use different methods and arrive at different conclusions. Kahn (1993) adopts a market-based approach and measures managerial quality as the adjusted predicted salary. This paper concludes that managerial quality is positively correlated with team success and individual player performance. Demiralp et al. (2012) proxy managerial quality using residuals by regressing manager lifetime winning percentage on experience and other managerial characteristics, and conclude that managers have little influence on player performance. These sophisticated approaches highlight the measurement challenges, which makes my paper stand out. In this paper, I take advantage of manager moves across teams and measure managerial quality using a value-added model, with the identification assumption carefully checked. Then, my paper provides compelling evidence that manager quality is uncorrelated with his former productivity as a player.

2 Context and Data

2.1 Context

There are currently 30 MLB teams. Each team can have an active roster of 26 players and an expanded roster of 40 players in each season. Players on the expanded roster are under major-league contracts and players on the active roster can play MLB games. A team can be interpreted as a firm. Within each team, the general manager, field manager, and player are analogous to the CEO, a lower level manager, and workers respectively. It is easier to measure their individual

performance relative to common firms, especially the productivity of lower level managers and workers. Generally speaking, general managers are expected to maximize profits or team wins, while field managers are expected to maximize team wins and players are expected to maximize individual field performance.

In most teams, the general manager (GM) has final say in terms of roster decisions (e.g. trades, free-agent signings) and coaching/front office personnel (e.g. hiring, firing, promotions, reassignments) (Major League Baseball, 2025a). There are also teams that employ a president of baseball operations (POBO), and the GM in such an organization is considered second in command.

The field manager (commonly referred to as the manager) is a subordinate of the GM, and is typically not involved in off-field personnel decisions or long-term club planning (Major League Baseball, 2025b). Instead, the field manager is the equivalent of a head coach in other professional sports. Assisted by a staff of assistant coaches whose responsibilities are specialized, the field manager is responsible for overseeing and making final decisions on all aspects of on-field team strategy, lineup selection, training and instruction. Managers are often former players, as said experience can help them relate to current players by virtue of shared experiences and familiarity with the inner workings of the clubhouse.

This paper focuses on the effects of managers on players for two reasons. First, compared to CEOs and other top managers, less attention is paid to the role of lower-level managers (Hoffman and Stanton, 2025). However, as lower-level managers have much more direct interaction with workers, it is important to understand whether and how they matter for augmenting their subordinates' performance. Second, as many managers are former players, there is rich information about their past performance and characteristics that can affect how they interact with players, allowing a comprehensive analysis.

2.2 Data

The data is compiled from two sources. The Lahman Baseball Database (`http://seanlahman.com/`) represents the primary data source, which is often used in personnel economics and human resource management research (e.g., Hakes and Sauer (2006); Wang and Cotton (2018, 2025)). It covers 153 MLB seasons from 1871 to 2023, involving 20,731 players and 753 managers. Importantly, one can clearly observe player and manager moves across teams in this dataset, which makes it

analogous to a matched employer-employee dataset that is frequently used by labor economists. In addition, it contains rich player information, such as position, hometown, handedness, games played and started, and etc. I further supplement it with Wins Above Replacement (WAR) data from Baseball Reference, which is a clean measure of individual productivity. In this sense, this dataset is well suited for labor economics research where identification comes from player/manager moves. Appendix A describes the details of how these datasets are merged together. Table 1 presents summary statistics of players.

Games played, games started, and WAR are player outcomes analyzed in this paper. These variables measure player labor supply and productivity respectively. Typically, pitchers have a lower labor supply than other positions, because pitching is the most physically demanding position in baseball. WAR measures how much better a player is than a player that would typically be available to replace that player, in terms of his contribution to team wins[1].

Adding up all of the WAR on a team (adding in replacement level (48 wins for a full season)), should yield results very close to the team's actual wins and losses. This measure of player productivity has several advantages compared to other in-field productivity measures. First, it has a consistent unit, the number of team wins, which is the outcome a team and a team manager wants to maximize theoretically. Second, it is independent of player position and thus can be compared across positions. Third, it is composite because it aggregates both defensive and offensive values of players and reduce multi-dimensional measures into a uni-dimensional measure of player productivity.

Among 753 MLB managers, 144 managers did not play in MLB and 362 became managers after retirement as an MLB player. The other 247 are player managers, who were both players and manager at the same time. 166 player managers did not serve as manager after retirement as a player. As the game became more complicated and the job of MLB managers became more demanding, player managers have rarely been observed since the 1960s. The last player manager was Pete Rose in 1984-1986. There are cases that a manager is fired during the season, and for simplicity this paper takes the last manager as the manager of the team for the entire season. There are 646 unique managers remaining in the analysis sample. At team level, there are a total of 1,067 manager transitions involving 590 unique managers. On average, there are 6.97 manager

[1]See `baseball-reference.com/about/war_explained.shtml` for detailed explanations

transitions each MLB season. From the perspective of players, there are 46,718 manager transition events in total, and 13,513 players experience at least one manager transition event. Table 2 presents summary statistics on managers in the sample.

3 Manager's Productivity as a Worker

3.1 Identification

I first estimate a value-added model similar to Fenizia (2022) and Weidmann et al. (2026), with team as the unit:

$$\text{Wins}_{jt} = \theta_{m(j,t)} + \alpha_j + \delta_t + X'_{jt}\gamma_{jt} + u_{jt}, \tag{1}$$

where $\theta_{m(j,t)}$ is interpreted as the portable component of managers' ability, interchangeably referred to as "manager quality". The team fixed effects α_j proxy for the time-invariant characteristics of the team (e.g., geographical location, fan base, team culture), and season fixed effects δ_t absorb league-wide changes across seasons, such as changes in the length of the season. X_{jt} is a vector of time-varying team characteristics, including player inputs measured by the sum of WAR of current players in the previous season and the proportion of games played by current players in the previous season, and organizational upheavals measured by the number of managers a team has within one season.

An alternative model is to use player as the unit and WAR as the outcome, which would be similar to the teacher-student case in Chetty et al. (2014). However, this model is problematic in my case because unlike students, players can move across teams. Such movements are very likely to be nonrandom, making the assignment of players to managers not clean. Therefore, my preferred model is team level and managerial quality is measured as the contribution of a manager to team wins.

OLS identifies $\theta_{m(j,t)}$ under the exogenous manager mobility assumption, that manager mobility is as-good-as random, conditional on team and time fixed effects as well as time-varying team characteristics. These orthogonality conditions permit managers to move to teams based on team productivity (α_j) or manager quality ($\theta_{m(j,t)}$). That is, better managers sorting into more successful teams would not violate the identifying assumptions. By the same token, if good man-

agers systematically moved to certain teams, this would not represent a threat to the identification strategy.

3.2 Regression Results and Variance Decomposition

Table 3 reports the amount of variation in team wins that can be attributed to managers, by comparing the adjusted R^2 estimated from different models with/without manager fixed effects. Column 1 and 3 show that the adjusted R^2 increases from 0.16 to 0.48 when manager fixed effects are added on top of time and team fixed effects. Similarly, Column 2 and 4 show that when I additionally control for time-varying team characteristics, adding manager fixed effects increases the adjusted R^2 from 0.57 to 0.73. More formally, an F-test strongly rejects the null hypothesis that all the manager fixed effects are zero (p-value = 0.000). Taken together, these results suggest that managers explain a quantitatively meaningful portion of the variation in wins across teams, consistent with Kahn (1993); Singell (1993); Peeters et al. (2020); Crosby (2021) but different from Smart and Wolfe (2003); Smart et al. (2008); Demiralp et al. (2012).

I test the exogenous mobility assumption to check the validity of these results, following Fenizia (2022). A major concern in this context is how teams and managers are matched. Manager mobility is endogenous if managers sort into teams on the basis of their comparative advantage. In this case, a more flexible saturated model with manager-by-team dummies should fit better than the additively-separable specification of equation 1. Therefore, I test this threat to identification by comparing column 4 and 5 of Table 3. The fit improves only marginally, moving from an adjusted R^2 of 0.73 to 0.79, suggesting that match components are not quantitatively relevant in this context.

Then, I perform a variance decomposition to assess the relative importance of time-invariant team characteristics and manager effects. If manager quality and team characteristics are important determinants of team success, then $\text{Var}(\alpha_j)$ and $\text{Var}(\theta_{m(j,t)})$ should explain a large share of the variation in team wins. Since time fixed effects are not of interest, I partial it out first to simplify the decomposition, by which I get Wins'_{jt}, α'_j, and $\theta'_{m(j,t)}$. Then, the variance of time-adjusted team wins can decompose into (Abowd et al., 1999):

$$\text{Var}(\text{Wins}'_{jt}) = \text{Var}(\theta'_{m(j,t)}) + \text{Var}(\alpha'_j) + \text{Var}(u_{it}) + 2\text{Cov}(\theta'_{m(j,t)}, \alpha'_j) \tag{2}$$

Table 4 reports results of variance-covariance decomposition estimated on the largest connected set. Results show that managers explain roughly 20% of the variance in team wins. Moreover, the covariance between manager and team effects is negative: more productive managers work at less productive teams. The negative assortative matching result is consistent with Fenizia (2022). Meanwhile, team fixed effects only account for a very small portion of the variance in team wins, about 4%. However, this is still reasonable because teams might experience many major changes over the long panel, such as relocations and changes in ownership.

3.3 Manager Quality and Manager Characteristics

To assess whether manager's productivity as a worker predicts manager quality, I estimate the following OLS regression:

$$\widehat{\theta_{m(j,t)}} = \beta_0 + Z_m'\beta_1 + u_m, \tag{3}$$

where $\widehat{\theta_{m(j,t)}}$ are fitted values extracted from estimating Equation 1 and Z_m is a vector of measures of manager m's productivity as a player.

The marginal distribution of $\widehat{\theta_{m(j,t)}}$ is a convolution of the distribution of $\theta_{m(j,t)}$ and normal noise, so there is classical measurement error in $\widehat{\theta_{m(j,t)}}$. However, classical measurement error on the left-hand side of a regression does not lead to bias. In contrast, empirical Bayes shrinkage of $\widehat{\theta_{m(j,t)}}$ introduces non-classical measurement error, which attenuates point estimates of β_1 toward zero (Walters, 2024), and is thus unnecessary in this case.

Table 5 reports regression results between manager quality and manager characteristics. In column 1 and 2, where the estimation sample are managers who were MLB players, there is no significant relationship between manager quality and measures of player productivity. These measures include games played, WAR, and whether one was elected to the Hall of Fame (HoF) as a player, and none of them are powerful predictors of manager quality, different from prior studies (Singell, 1993). Also, manager quality is uncorrelated with the handedness of a manager. Moreover, none of position indicators is significant, at odds with the anecdotal evidence that catchers are more likely to be good managers (Schumann, 1983). In column 3, where the estimation sample expands to non-MLB-player managers, the experience as an MLB player is positively correlated with manager quality.

Overall, these results suggest that former MLB players tend to become good managers, but manager quality is not correlated to a manager's productivity as a player, indicating heterogeneity between player skills and manager skills. This finding is different from Kim and Makadok (2022), which finds that head coaches who had previously been NBA All-Star players outperform other coaches in NBA.

3.4 Results of 1984-2016 Seasons

Two other concerns arise in the identification of manager effects. First, while the long panel dataset allows me to observe more manager transition events, the role of managers experienced significant changes along with the changes in this sport. Therefore, it might be of more interest to focus on recent years to provide practical implications. Second, payroll, which is the sum of a team's player salary in a season, is probably the best measure of player inputs. However, it is available in my dataset only for 1984-2016 seasons.

Taken together, a natural robustness check is to estimate Equation 1 with payroll as an additional covariate, using 1984-2016 data. In this 33-year panel dataset, I observe 347 manager transitions with 197 unique managers and 7,331 unique players involved. Results are reported in Table 6. These results are very similar to baseline results, indicating that our baseline specification is sophisticated enough to capture the effects of players.

Then, we regress new estimates of manager effects on manage characteristics. Results are reported in Table 7. Again, there is no positive relation between manager quality and manager's productivity as a player. Moreover, HoF-player managers show lower manager quality. A caveat is that only 7 managers are HoF players, so it is hard to rule out the possibility that these managers are outliers. Nevertheless, this finding is consistent with baseline results that good players do not necessarily make good managers.

I also find that former-MLB-player managers do not show higher manager quality on average, different from baseline results. This pattern likely reflects two reinforcing shifts in the structure of the manager role during the modern era. First, the rise of sabermetrics and front-office analytics beginning in the 1980s progressively transferred strategic decision-making—lineup construction, bullpen sequencing, defensive positioning—from field managers to general managers and analytics departments. By the 2000s, managers increasingly implemented pre-determined decisions rather

than exercising independent judgment informed by playing experience. This organizational change weakens the channel through which a background as an MLB player would generate managerial value, compressing the premium observed in the full historical sample. Second, the composition of the non-player comparison group shifted over this period. Before the modern era, managers without MLB playing careers were relatively rare and often brought non-standard credentials. In more recent years, more non-player managers had accumulated sport-specific human capital through bench-coaching, pitching-coaching, and other field staff roles, reducing the gap in baseball knowledge between the two groups and narrowing the estimated experience premium.

4 Common Characteristics between Manager and Worker

4.1 Identification

To identify the causal effect of interactions between managers and workers on worker performance, I take advantage of manager turnovers in MLB, and focus on three types of interactions as treatments, namely whether a player and his manager play the same position, whether they were born in the same place, and whether they are both left-handed pitchers or batters[2]. As a baseline specification, I estimate the following two-way fixed-effect (TWFE) model:

$$Y_{it} = \beta D_{it} + \alpha_i + \delta_t + e_{it}, \tag{4}$$

where Y_{it} represents labor market outcomes of player i in season t, including games played, games started and WAR. D_{it} is an indicator of treatment status, equal to one if player i and his manager share some common characteristic in season t and zero otherwise. The rest in the regression are player fixed effects α_i and season fixed effects δ_t. e_{it} represents unobservables that are not captured by the regression.

Variation in manager-player pairs comes from two sources, player moves and manager moves. My identification relies on the variation coming from manager turnovers, which is similar to the setting of Cullen and Perez-Truglia (2023) because the whole team, rather than a specific worker,

[2]I also considered same college as a treatment, that is, a player is treated if he and his manager went to the same college. Unfortunately, there are only 37 treated observations in the analysis sample and thus there is no statistical power to detect any significant treatment effects.

experiences the manager transition. An apparent advantage of this variation is that it is more exogenous and involves less selection into treatment than variation from player moves. Also, players experiencing team changes need to adjust to new teams, which might impede human capital growth. Therefore, I further limit the sample to player observations affiliated with their first MLB team, which excludes the variation from player moves.

Also, observations with player managers are excluded from the analysis sample. I make this sample selection because the effects of player managers contain both the effects as a manager and the effects as a teammate. Since I am not able to disentangle these effects, the effects of player managers could be biased.

Moreover, I focus on the scenario of a treatment turning on to avoid the issue of selection into treatment, which is the primary threat to my identification. In another word, I will compare players going from untreated to treated and players going from untreated to untreated with manager changes. I assume the treatment is unforgettable, which means a player is always treated once he is treated in any season. This is a plausible assumption in this setting, as Minni (2026) finds that good managers have a long-lasting impact on workers' trajectories that outlives their time overseeing the workers. There are other issues in identification depending on the treatment, and I will discuss these issues and how I address them when it comes to the specific treatment.

To interpret $\hat{\beta}$ as causal, the parallel trend assumption is needed, that players who go from untreated to treated with manager replacements would have experienced same changes in productivity measures as those who are always untreated in the absence of treatment. In order to test for parallel trends and study the dynamics of treatment effects, I estimate an event-study version of the TWFE model with indicators for event periods. Specifically, I estimate the following event study model:

$$Y_{it} = \beta_k \times \sum_k D_{k(it)} + \alpha_i + \delta_t + v_{it}. \tag{5}$$

Since manager turnovers take place at different times, this is a staggered adoption design and TWFE estimates cannot deliver consistent estimates of the causal parameters if treatment effects are heterogeneous across groups or time (Goodman-Bacon, 2021). Therefore, Callaway and Sant'Anna (2021)'s robust estimator (hereafter, CS) is adopted to produce baseline regression results.

4.1.1 Same Position

Player position is defined as a player's primary position, at which he played most games. In the dataset, there are 13 positions, including pitcher, catcher, first baseman, second baseman, third baseman, shortstop, left fielder, right fielder, center fielder, and outfielder, designated hitter, pinch hitter, and pinch runner. I adopt a coarser classification of player positions, including pitcher, hitter, catcher, infielder, and outfielder. A player is considered treated if his manager used to play the same position in MLB as him.

A manager might have different impacts on players in his team depending on the position he used to play. Since same-position managers have more position-specific knowledge, treated players might receive better mentoring relative to players at other positions, which then results in higher human capital gain.

As mentioned above, I will focus on the scenario of a treatment turning on. As a result, I limit the sample to players whose initial manager did not play the same position. Then, with manager turnovers, some of them go from untreated to treated while others stay untreated. For untreated, they either have managers who used to play a different position, or managers who did not play in MLB. This sample selection allows me to rule out selection into treatment or sorting, which would cause positive bias if former-pitcher managers are better at hiring pitchers or former-batter managers are better at hiring batters. It also rules out the possibility of the treatment turning off, which would cause bias in $\hat{\beta}$ if the treatment is unforgettable and has a long-lasting impact.

4.1.2 Both Lefties

For the treatment of both lefties, a left-handed pitcher is considered treated if his manager was also a left-handed pitcher, and a left-handed batter is considered treated if his manager was also a left-handed batter. Again, left-handed players might receive better mentoring from left-handed managers due to their better knowledge about pitching/batting using the left hand, which would lead to an increase in productivity. Also, a left-handed player might benefit from a left-handed manager's in-group bias. For example, left-handed managers can favor left-handed players when making roster decisions, which would lead to an increase in games played and started and potentially an increase in WAR through learning-by-doing.

For this analysis, the sample is limited to left-handed players who are untreated initially and are quasi-randomly assigned treatment with manager turnovers. This sample selection allows me to address several concerns that could impair causal identification. The first concern is again selection into treatment. Left-handed managers might prefer left-handed players and recruit more of them, or, in the other direction, left-handed players prefer to play for left-handed managers for some reason. I test whether such sorting exists in my data and present results in Table 8. It appears that left-handed managers do not manage more left-handed players than right-handed managers, which indicates that such selection is not a concern.

The second concern is spillover effects, especially when the outcome variable is games played or games started. Players with the same position are substitutes, so if a left-handed manager favors left-handed players, there would be a positive effect on left-handed players and a negative effect on right-handed players simultaneously. If this is true and right-handed players are also included in the sample, $\hat{\beta}$ would be positively biased again. Therefore, although selection into treatment does not seem to be a concern, I still exclude right-handed players from the sample.

4.1.3 Same Hometown

For the treatment of same hometown, an American player is considered treated if he was born in the same state as his manager, and an international player is considered treated if he was born in the same country as his manager. Thus, the control group are players whose new managers are still from other states/countries. This treatment also captures two types of interactions. First, a manager can simply favor a player from the same hometown due to the hometown bias (Yonker, 2017; Ahmadi et al., 2022), which can be viewed as a special case of in-group bias. Second, a manager and a player from the same hometown (especially same country) can have a better matching quality, which has been found to be true in the setting of coethnic advisor and PhD students (Ge et al., 2021; Fry and Glennon, 2025).

To identify the causal effect of this interaction on labor market outcomes, the analysis sample is limited to players who were drafted by a manager from different hometowns and then experienced manager changes. I make this sample selection due to similar concerns. Specifically, selection into treatment is again the major concern, especially when a manager drafts a player from the same hometown. The manager might have private information about the treated player due to networks

at his hometown, which would again lead to a positive selection bias.

4.2 Results

Table 9 presents treatment effects of common characteristics by estimating Equation 4 with different treatments respectively. Panel A shows the treatment effects of same position on WAR, which can be interpreted as that assigning a manager who used to play the same position to a player can increase the player's WAR by 1.03. In another word, a player can contribute 1.03 more wins to the team if his manager used to play the same position as him.

Panel B reports treatment effects of both lefties. Results show that assigning a left-handed manager who used to play the same position to a left-handed player have positive treatment effects on the number of games a player plays and starts, as well as WAR. Specifically, point estimates indicate that there is an increase of 22.64 games in games played, an increase of 25.01 games in games started, and an increase of 0.55 wins in WAR. To summarize, treated left-handed players play, start, and win more games relative to untreated left-handed players.

Panel C presents treatment effects of same hometown. Results show that assigning a manager from the same state or country to a player have positive treatment effects on the number of games a player plays and starts, as well as WAR. Specifically, point estimates indicate that there is an increase of 16.71 games in games played, an increase of 16.21 games in games started, and an increase of 0.43 wins in WAR. To summarize, a player whose manager is from the same hometown plays, starts, and wins more games relative to a player whose manager is from somewhere else.

Figure 1 to 7 plot CS event study estimates. There is no detectable pre-trend in any plot, lending credibility to the parallel trend assumption and the validity of the research design exploiting manager turnovers. Moreover, event study estimates show that generally the treatment effects gradually increase and remain stable over seasons, which is consistent with the economic intuition, that players gradually build managers' trust and accumulate human capital. It also supports the mechanism of mentoring rather than pure in-group bias. Manager moves are likely to happen again in the post-treatment period, but there are persistent positive effects on treated players, which would not be possible if it is driven by in-group bias alone.

Overall, the treatment effects of common characteristics range from 0.3-0.4 standard deviations for games played, 0.4-0.6 standard deviations for games started, and 0.3-0.7 standard deviations for

WAR respectively. Therefore, these treatment effects seem significant and reasonable economically. Notably, the effect size of same position on player productivity is much bigger than that of both lefties and same hometown (1.03 versus 0.55 and 0.43). I view this difference as reasonable, because the primary source of the effect of same hometown might be in-group bias through more game exposure and learning by doing, while the effect of same position stems primarily from mentoring. Mentoring is also the primary mechanism of both lefties, but the effect size is closer to same hometown, probably because batting is not the only task for position players. For example, if a manager used to be a left-handed shortstop, he can and probably provide limited mentoring on defense for his left-handed fielder.

4.3 Heterogeneity

4.3.1 Player Age

Treatment effects might be heterogeneous depending on the age of a player receiving the treatment. If a player receives the treatment at a younger age, when his learning curve is steeper and mentoring is more valuable, maybe he can benefit more from the treatment. Also, Altindag et al. (2025) finds that large age differences are necessary for the role model effect to be effective in the professor-student context. In the context of MLB, the average age difference between a manager and a player is 23.52 years, and 95% of manager-player pairs have an age difference of at least 10 years. Therefore, in most cases managers are old enough to be a role model for players, which allows me to focus on the heterogeneity associated with the age at which a player becomes treated.

In the sample for estimating the effects of same position, the average and median age of receiving the treatment are 26.58 and 26, respectively. I select the median as the cutoff, and divide the sample into a young and an old subsample. Then, I estimate the treatment effects of same position for two subsamples separately. Similarly, I explore the age heterogeneity in the treatment effects of both lefties and same hometown, where the age cutoffs are 26 and 27 respectively. Callaway and Sant'Anna (2021)'s robust estimator is used throughout this subsection.

Results of same position are presented in Table 10. There are positive treatment effects on both young and old players (when getting treated), but the effect size on WAR are much larger for young players (1.71 versus 0.22). These results indicate that player productivity could get much

larger improvements if a player was assigned a same-position manager at a younger age. Results of both lefties and same hometown are presented in Table 11 and 12. With respect to WAR, results are similar, that there are positive treatment effects on players who were young when receiving the treatment, but there is no significant effect on players who were older when becoming treated.

An important difference in Table 11 and 12 is that with similar precision in estimation, both lefties have insignificant effects on old players' games played and started, while same hometown have positive effects. On the contrary, the effects of both lefties on young players' labor supply are much larger. These differences suggest that the effects of same hometown might stem from in-group bias, while the effects of both-lefties are driven by mentoring. Moreover, the effect size on young players' WAR is much smaller for the treatment of same hometown compared to the other two treatments, which indicates that mentoring is an important driver in treatment effects of same position and both lefties.

To conclude, I find heterogeneous treatment effects of common characteristics associated with the age of receiving the treatment, which is somewhat consistent with Altindag et al. (2025). There is compelling evidence that common characteristics between managers and players have a stronger effect on the performance of younger players. Results also suggest that mentoring is driving the results of same position and both lefties, while the result of same hometown is more of in-group bias. These effects on young players can be life-changing, because early career shocks can have long-lasting effects on their labor market outcomes (Wachter, 2020; Arellano-Bover, 2022; Casas-Arce et al., 2024). Given that the career of a professional athlete is much shorter than a common worker, such effects might be more pronounced.

4.3.2 Manager Quality

There can also be heterogeneous treatment effects stemming from differences in manager quality. In the educational context, Altindag et al. (2025) find that while student outcomes are improved due to student-teacher racial congruence, high-quality professors can improve all the students, not just one race or age group. Similarly, although high-quality managers win more games by definition, it remains unknown whether high-quality managers achieve this by improving the productivity of *some* players, e.g., playing the same position, versus the productivity of *all* players regardless of position, handedness, or hometown.

To test this conjecture, I first classify managers into two groups according to their manager fixed effects estimates from Equation 1, which proxies manager quality. Among 646 managers whose quality estimates are available, the median manager quality is -1.4. These managers are categorized into two groups based on the median, and two subsamples are created accordingly. Thus, the first subsample contains treated player observations with low-quality managers and all untreated player observations, while the second subsample contains treated player observations with high-quality managers and all untreated player observations. Then, I repeat the baseline analysis using different subsamples respectively to examine whether there are heterogeneous treatment effects.

Table 13 reports heterogeneous treatment effects of same position. Compared to low-quality managers, high-quality managers have slightly larger positive impacts on the productivity of players playing the same position (p-value = 0.046). Table 14 and 15 present the results of both lefties and same hometown, by and large similar for high-quality managers and low-quality managers. Overall, these results show that common characteristics between managers and players do not have heterogeneous treatment effects associated with manager quality. It immediately follows that high-quality managers create more team wins because they can improve all players' productivity compared to low-quality managers, which is consistent with Altindag et al. (2025).

4.4 Robustness Checks

A battery of robustness checks are performed to assess the validity of baseline results. First, in the baseline analysis I exclude observations whose manager is a player manager. I make this sample selection because a player manager can affect the game via not only managing but also playing. A player manager will mechanically affect the labor supply of players playing the same position, and indirectly affect WAR of his teammates. In Table B.1, I show results without excluding player managers. These results are similar to baseline results.

Moreover, in the baseline analysis I assume the last manager a team has within a season to be the manager of the team for the entire season. As a robustness check, the first manager within a season is considered the manager of the team for the entire season and thus treatment indicators are created based on this new assignment of managers. Results are reported in Table B.2 and remain similar to baseline results.

Two more robustness checks are performed for the treatment of both lefties. First, in my dataset,

handedness can take three values, left, right, or both. In the baseline analysis of both lefties, a player and a manager have to be both left to be considered treated. To check the robustness of this variable definition, I assume both is equivalent to left and repeat the staggered DiD regression. Also, in the baseline analysis I compare treated left-handed players to untreated left-handed players by excluding all the right-handed players. To check the robustness to this sample selection, I estimate the same specification without excluding right-handed players. Results of these two robustness checks are reported in Table B.3, very similar to baseline results.

5 Conclusion

Using data compiled from the Sean Lahman Baseball Database and Baseball Reference, this paper investigates what manager traits matter in the context of MLB. The first type of manager traits is a manager's productivity as a player. First, I find that MLB managers have a considerable impact on team wins. Then, I estimate manager quality using a value-added model and regress it on measures of a manager's productivity as a player. Although former MLB players have better manager quality on average, a manager's productivity as a player is not correlated with manager quality.

The second type of manager traits is the common characteristic between a manager and a player. Specifically, coupling manager turnovers with a staggered DiD estimator, I investigate three common characteristics, including same position, both lefties, and same hometown. All these common characteristics have positive effects on player productivity. The effect of same position is the largest, and the effect of same hometown is the smallest. Results further suggest that same position and both lefties take effect via mentoring, while the effects of same hometown might stem from in-group bias. I also find that these effects are more pronounced for players who were young when the manager turnover event occurred. I find little heterogeneity in treatment effects associated with manager quality, which indicates that good managers can improve all players' productivity.

My findings have implications for firms selecting managers based on managerial traits. First, good workers do not necessarily make good managers. Second, while it is true that same position and both lefties are specific to the context of professional sports, they can be abstracted as characteristics of a manager's expertise. Thus, my findings can provide insights into the general

labor market, that a manager can boost worker productivity if they share common characteristics in their skill sets. These managers have rich knowledge about these skills, and thus can provide better mentoring. Therefore, selecting managers who share common characteristics of skill sets with workers might be a good practice, especially when these are important skills.

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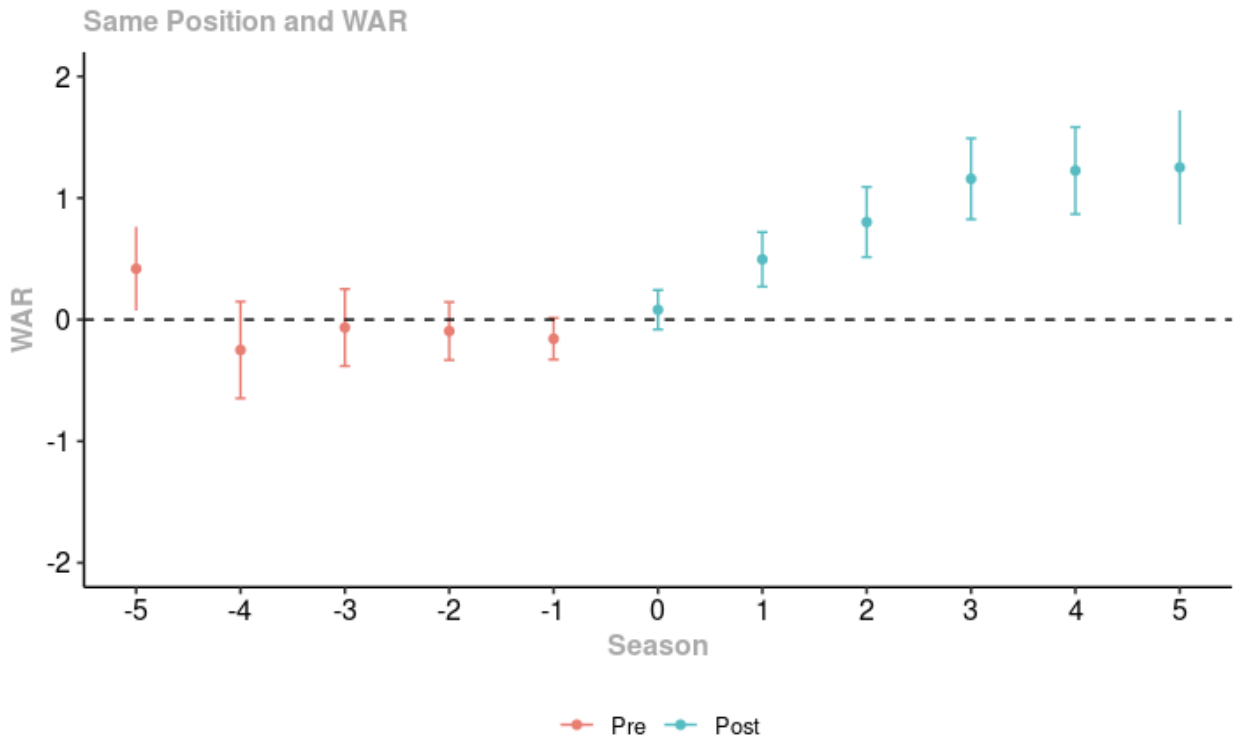


Figure 1: Dynamic Treatment Effects of Same Position on WAR

Notes: This figure plots event study estimates of same position by estimating Equation 5, including point estimates and 95% confidence intervals. Red indicates pre-treatment periods, and blue indicates post-treatment periods. I use Callaway and Sant'Anna (2021)'s estimator.

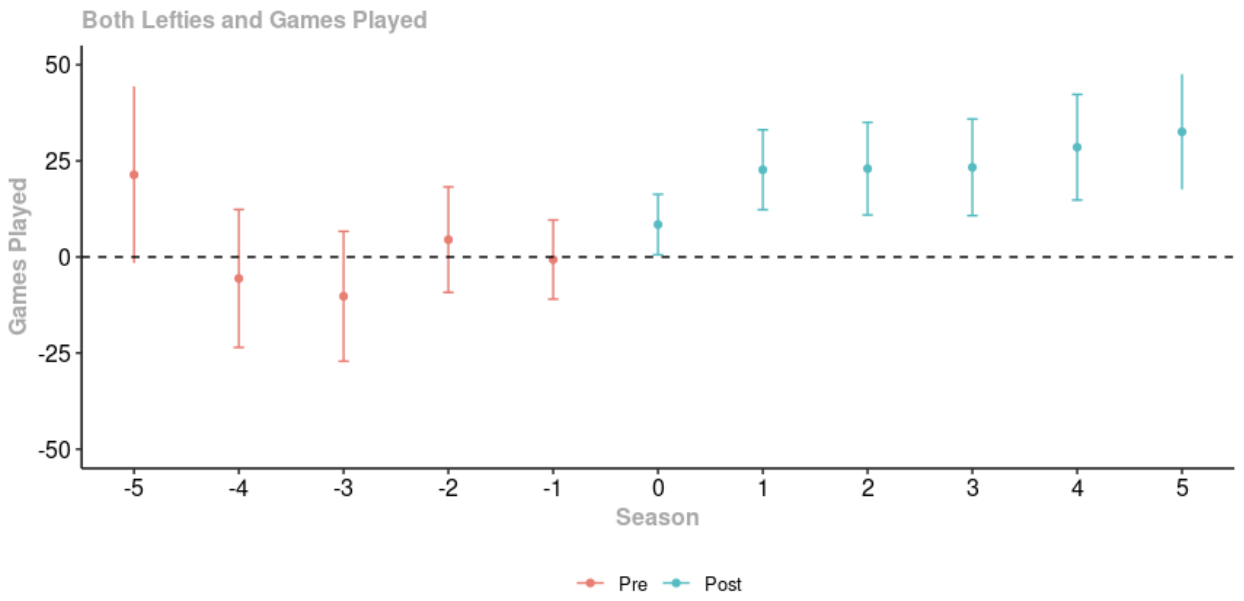


Figure 2: Dynamic Treatment Effects of Both Lefties on Games Played

Notes: This figure plots event study estimates of both lefties on games played by estimating Equation 5, including point estimates and 95% confidence intervals. Red indicates pre-treatment periods, and blue indicates post-treatment periods. I use Callaway and Sant'Anna (2021)'s estimator.

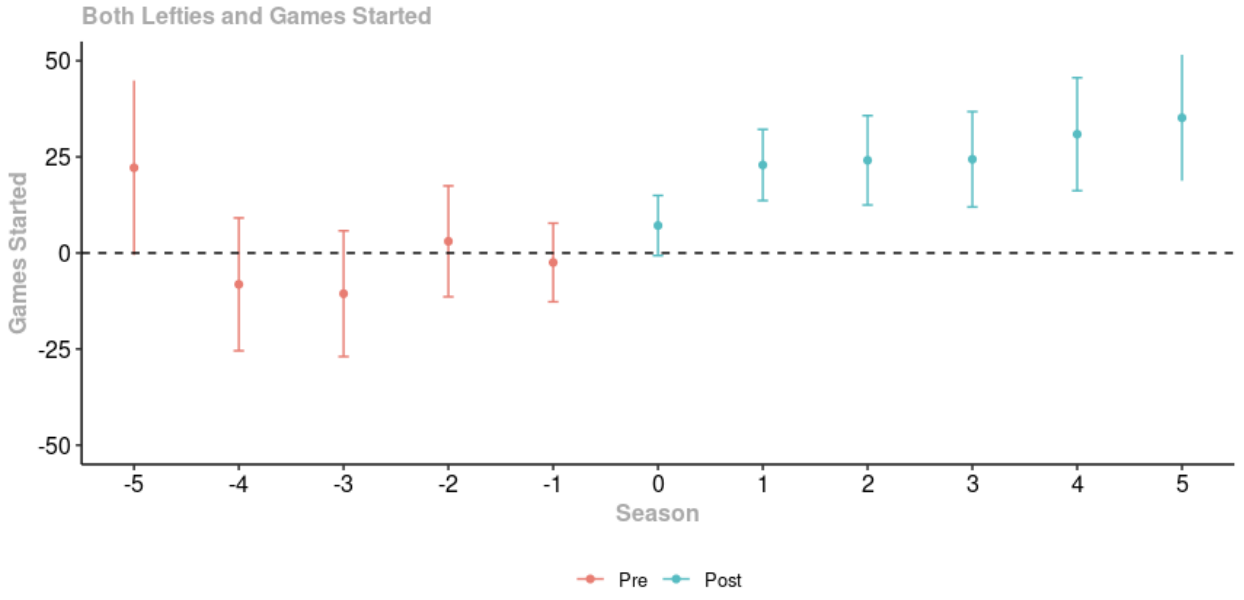


Figure 3: Dynamic Treatment Effects of Both Lefties on Games Started

Notes: This figure plots event study estimates of both lefties on games started by estimating Equation 5, including point estimates and 95% confidence intervals. Red indicates pre-treatment periods, and blue indicates post-treatment periods. I use Callaway and Sant'Anna (2021)'s estimator.

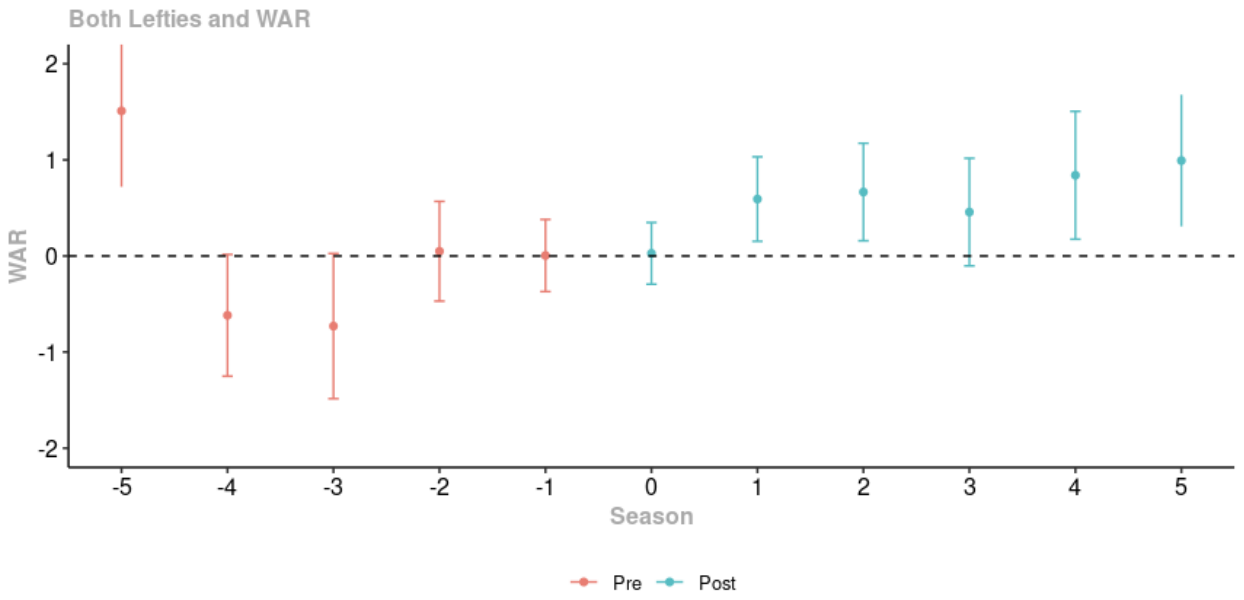


Figure 4: Dynamic Treatment Effects of Both Lefties on WAR

Notes: This figure plots event study estimates of both lefties on WAR by estimating Equation 5, including point estimates and 95% confidence intervals. Red indicates pre-treatment periods, and blue indicates post-treatment periods. I use Callaway and Sant'Anna (2021)'s estimator.

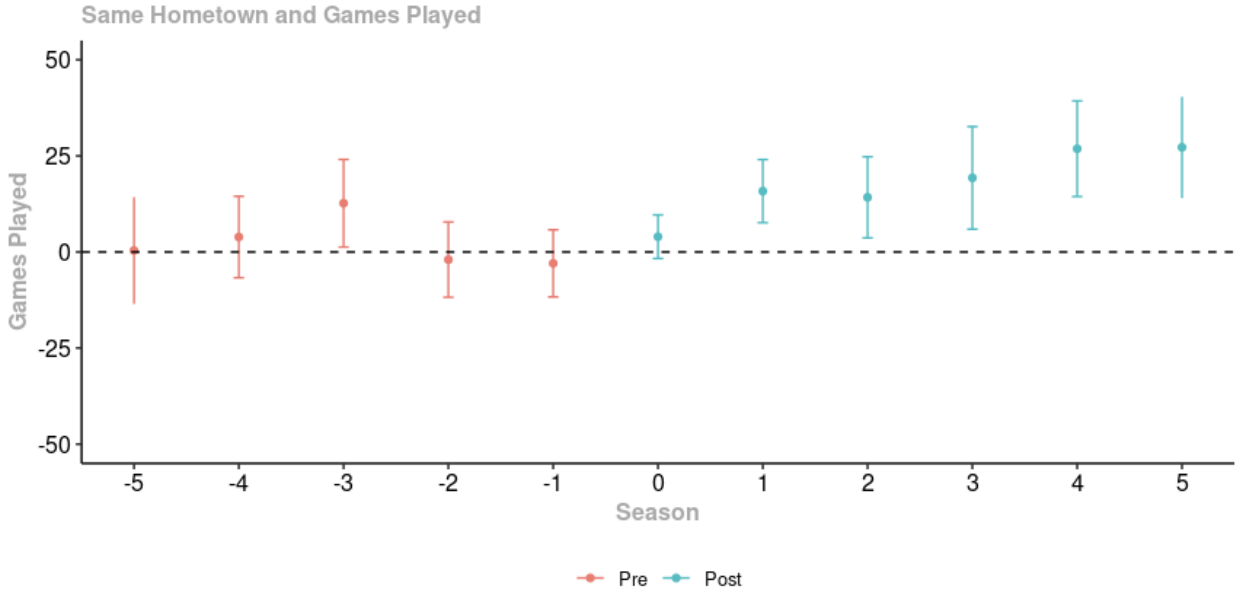


Figure 5: Dynamic Treatment Effects of Same Hometown on Games Played

Notes: This figure plots event study estimates of same hometown on games played by estimating Equation 5, including point estimates and 95% confidence intervals. Red indicates pre-treatment periods, and blue indicates post-treatment periods. I use Callaway and Sant'Anna (2021)'s estimator.

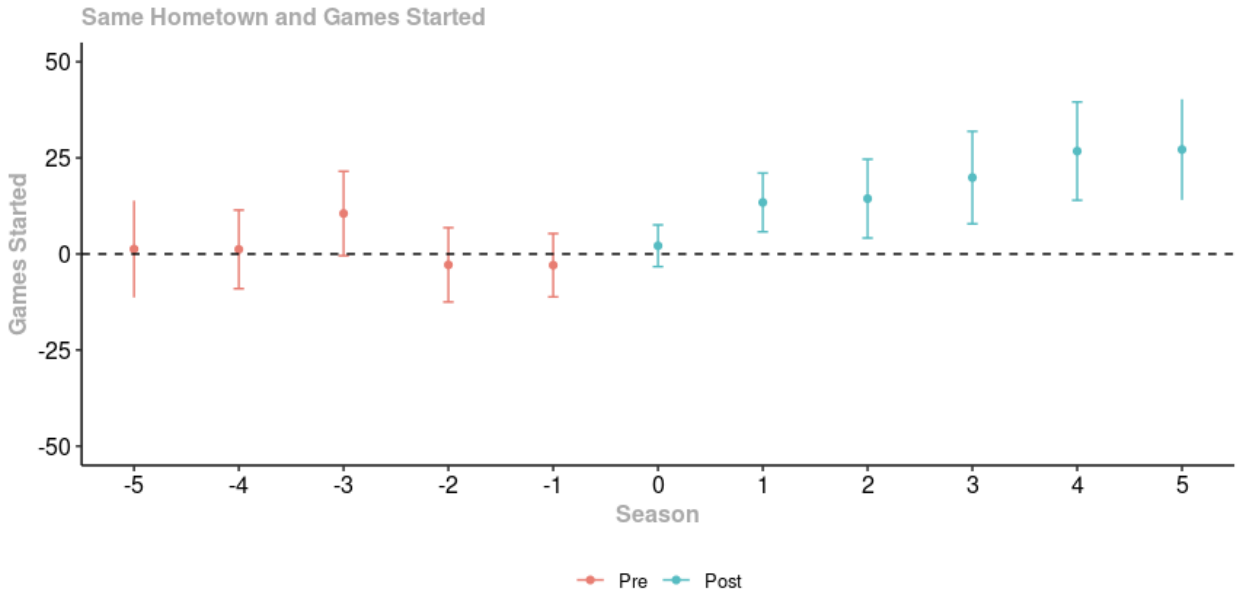


Figure 6: Dynamic Treatment Effects of Same Hometown on Games Started

Notes: This figure plots event study estimates of same hometown on games started by estimating Equation 5, including point estimates and 95% confidence intervals. Red indicates pre-treatment periods, and blue indicates post-treatment periods. I use Callaway and Sant'Anna (2021)'s estimator.

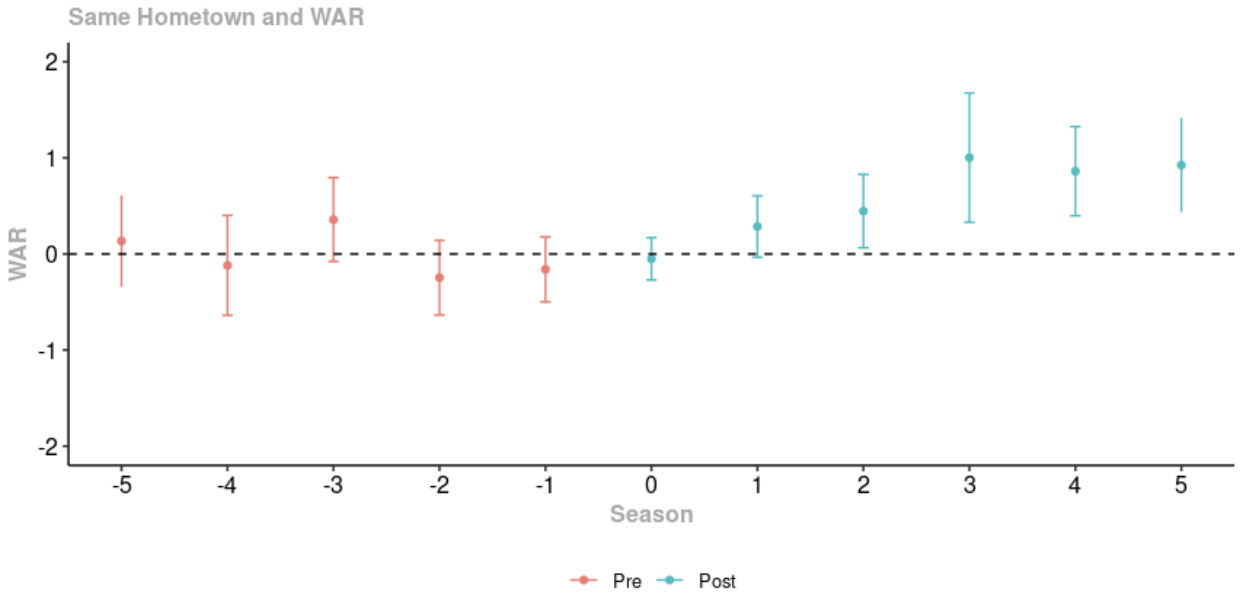


Figure 7: Dynamic Treatment Effects of Same Hometown on WAR

Notes: This figure plots event study estimates of same hometown on WAR by estimating Equation 5, including point estimates and 95% confidence intervals. Red indicates pre-treatment periods, and blue indicates post-treatment periods. I use Callaway and Sant'Anna (2021)'s estimator.

Table 1: Summary Statistics - Players

	Birth Year	Experience	Games Played	Games Started	WAR	N
Panel A: Grouped by Position						
Catcher	1938.78	5.74	37.67	28.83	0.216	1,461
Designated hitter	1945.63	4.69	37.43	23.69	0.218	1,544
Infielder	1939.77	5.81	56.23	45.67	0.436	3,586
Outfielder	1942.96	5.77	56.69	43.02	0.492	2,832
Pitcher	1951.00	4.83	18.34	5.47	0.145	8,926
Panel B: Grouped by Handedness						
Left-Handed	1940.30	5.27	36.85	23.87	0.318	6,363
Right-Handed	1939.43	5.17	34.28	22.84	0.245	13,634
Panel C: Grouped by Hometown						
International	1968.53	5.39	38.29	24.57	0.331	2,577
U.S.	1932.77	5.03	33.73	22.85	0.250	18,153

Notes: This table summarizes player characteristics by different classifications. Games played, games started, and WAR are season-level averages.

Table 2: Summary Statistics - Managers

	Birth Year	Experience	Games	Win%	Teams	N
Panel A: Grouped by Playing Position						
Non-MLB player	1909.89	3.70	465.72	0.459	1.57	122
Catcher	1915.58	5.71	787.06	0.459	1.77	109
Designated hitter	1951.26	4.00	526.37	0.460	1.58	19
Infielder	1910.73	4.80	641.26	0.462	1.67	251
Outfielder	1896.87	5.12	686.09	0.459	1.84	101
Pitcher	1903.91	3.95	524.05	0.508	1.43	44
Panel B: Grouped by Handedness						
Left-Handed	1914.99	5.30	739.49	0.472	1.62	131
Right-Handed	1915.09	4.92	655.86	0.472	1.74	445
Unknown	1867.45	2.30	245.49	0.393	1.34	70
Panel C: Grouped by Hometown						
International	1908.11	3.85	470.94	0.458	1.55	47
U.S.	1910.12	4.78	640.70	0.464	1.68	599

Notes: This table summarizes manager characteristics by different classifications.

Table 3: Analysis of Variance of Team Wins

	Wins (1)	Wins (2)	Wins (3)	Wins (4)	Wins (5)
N	3,045	3,045	3,045	3,045	3,045
R^2	0.24	0.61	0.63	0.81	0.87
Adj. R^2	0.16	0.57	0.48	0.73	0.79
Controls	No	Yes	No	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Team FE	Yes	Yes	Yes	Yes	Yes
manager FE	No	No	Yes	Yes	No
Manager-by-team FE	No	No	No	No	Yes
P-value			0.000	0.000	

Note: This table shows how much of the variance in team wins is explained by Model 1. N represents the number of team-season observations.The p-value at the bottom of the table tests the null hypothesis that manager effects are jointly zero.

Table 4: Variance-Covariance Decomposition

	(1) Component	(2) Share
Var(Wins)	157.02	100%
Var(Manager)	31.46	20.04%
Var(Team)	6.63	4.22%
Cov(Manager, Team)	-23.40	-14.90%
N	2,508	

Note: This table presents results of variance-covariance decomposition of team wins in the largest connected set. The model includes dummies for manager, team, and year fixed effects, as well as a vector of time-varying team characteristics.

Table 5: Manager Quality and Manager Characteristics as a Player

	Players	Players	Players v.s. Non-Players
Was an MLB player			6.010***
			(2.225)
Games played	0.000	0.001	
	(0.002)	(0.002)	
WAR	0.031	0.025	
	(0.066)	(0.065)	
HoF Player	0.433	-0.321	
	(3.944)	(4.073)	
Left	2.761	2.552	
	(2.190)	(2.222)	
Pitcher	-5.216		
	(4.548)		
Hitter	-10.837*		
	(6.318)		
Infielder	-3.819		
	(3.104)		
Outfielder	-1.585		
	(3.513)		
Catcher		3.975	
		(3.011)	
Observations	397	397	515

Note: This table presents OLS regression results by estimating Equation 3. All columns control for birth year and an indicator for being born in the United States. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table 6: Analysis of Variance of Team Wins, 1984-2016

	Wins (1)	Wins (2)	Wins (3)	
N	914	914	914	914
R^2	0.67	0.67	0.85	0.89
Adj. R^2	0.64	0.64	0.78	0.82
Payroll	No	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Team FE	Yes	Yes	Yes	Yes
manager FE	No	No	Yes	No
Manager-by-team FE	No	No	No	Yes
P-value			0.000	

Note: This table shows how much of the variance in team wins is explained by Model 1 using 1984-2016 data. N represents the number of team-season observations.The p-value at the bottom of the table tests the null hypothesis that manager effects are jointly zero.

Table 7: Manager Quality and Manager Characteristics as a Player, 1984-2016

	Players	Players	Players v.s. Non-Players
Was an MLB player			2.106
			(3.605)
Games played	0.001	0.001	
	(0.004)	(0.003)	
WAR	0.202	0.220*	
	(0.133)	(0.127)	
HoF Player	-22.311**	-22.856***	
	(9.069)	(8.685)	
Left	-0.267	-0.199	
	(3.717)	(3.630)	
Pitcher	-3.953		
	(6.365)		
Hitter	-6.921		
	(6.214)		
Infielder	-6.530*		
	(3.927)		
Outfielder	-4.192		
	(5.371)		
Catcher		6.101	
		(3.696)	
Obs	165	165	208

Note: This table presents OLS regression results by estimating Equation 3 using 1984-2016 data. All columns control for birth year and an indicator for being born in the United States. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table 8: Balance Table of Handedness

	Left-handed manager	Right-handed manager	P-value
Share of left-handed players	0.32	0.33	0.33
Share of left-handed observations	0.32	0.33	0.38
Observations	99	363	

Notes: This is a balance table testing whether there is any observable sorting of left-handed players to left-handed managers.

Table 9: Treatment Effects of Common Characteristics

	Games Played (1)	Games Started (2)	WAR (3)
Panel A: Same Position			
Same Position			1.03***
			(0.08)
Observations			39,013
Panel B: Both Lefties			
Both Lefties	22.64***	25.01***	0.55**
	(3.55)	(3.54)	(0.13)
Observations	13,528	13,528	13,224
Panel C: Same Hometown			
Same Hometown	16.71***	16.21***	0.43***
	(2.79)	(2.84)	(0.10)
Observations	47,642	45,424	45,654
Player FE		Yes	
Season FE		Yes	
Outcome Mean (SD)	53.24	38.52	0.66
	(47.35)	(47.83)	(1.54)

Notes: This table presents results of common characteristics by estimating Equation 4 with different treatments respectively. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table 10: Age Heterogeneity–Same Position

	Young (1)	Old (2)
Same Position	1.71***	0.22**
	(0.10)	(0.11)
Player FE	Yes	Yes
Season FE	Yes	Yes
Obs	34,798	36,174

Note: This table presents heterogeneous treatment effects of same position, where the estimation sample of column 1 are players whose age of receiving the treatment is below the median, and the sample of column 2 are players above the median treatment age. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table 11: Age Heterogeneity–Both Lefties

	Games Played (1)	Games Started (2)	WAR (3)
Panel A: Young Players			
Both Lefties	44.01***	44.71***	1.35***
	(5.75)	(5.45)	(0.19)
Observations	12,102	12,102	11,811
Panel B: Old Players			
Both Lefties	2.99	2.98	-0.23
	(4.04)	(3.89)	(0.22)
Observations	12,760	12,760	12,464
Player FE	Yes	Yes	Yes
Season FE	Yes	Yes	Yes

Notes: This table presents heterogeneous treatment effects of both lefties, where the estimation sample in panel A are players whose age of receiving the treatment is below the median, and the sample in panel B are players above the median treatment age. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table 12: Age Heterogeneity–Same Hometown

	Games Played (1)	Games Started (2)	WAR (3)
Panel A: Young Players			
Same Hometown	23.86***	22.84***	0.74***
	(3.95)	(3.83)	(0.13)
Observations	46,155	43,978	44,192
Panel B: Old Players			
Same Hometown	7.13*	7.13**	-0.13
	(3.80)	(3.57)	(0.17)
Observations	46,538	46,538	44,566
Player FE	Yes	Yes	Yes
Season FE	Yes	Yes	Yes

Notes: This table presents heterogeneous treatment effects of same hometown, where the estimation sample in panel A are players whose age of receiving the treatment is below the median, and the sample in panel B are players above the median treatment age. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table 13: Manager Quality Heterogeneity–Same Position

	Low (1)	High (2)
Same Position	0.92***	1.07***
	(0.08)	(0.08)
Player FE	Yes	Yes
Season FE	Yes	Yes
Observations	37,329	38,140

Note: This table presents heterogeneous treatment effects of same position, where the estimation sample of column 1 are player observations whose manager's quality is below the median, and the sample of column 2 are player observations whose manager's quality is above the median. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table 14: Manager Quality Heterogeneity–Both Lefties

	Games Played (1)	Games Started (2)	WAR (3)
Panel A: Low-quality Managers			
Both Lefties	25.75***	28.79***	0.61***
	(3.67)	(3.69)	(0.16)
Observations	13,018	13,018	12,719
Panel B: High-quality Managers			
Both Lefties	23.20***	25.73***	0.66***
	(3.55)	(3.82)	(0.16)
Observations	13,197	13,197	12,894
Player FE	Yes	Yes	Yes
Season FE	Yes	Yes	Yes

Notes: This table presents heterogeneous treatment effects of both lefties, where the estimation sample in panel A are player observations whose manager's quality is below the median, and the sample in panel B are player observations whose manager's quality is above the median. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table 15: Manager Quality Heterogeneity–Same Hometown

	Games Played (1)	Games Started (2)	WAR (3)
Panel A: Low-quality Managers			
Same Hometown	19.87***	20.19***	0.34**
	(4.23)	(4.59)	(0.17)
Observations	47,219	44,961	45,206
Panel B: High-quality Managers			
Same Hometown	18.02***	17.64***	0.40***
	(3.55)	(3.39)	(0.13)
Observations	47,803	45,582	45,803
Player FE	Yes	Yes	Yes
Season FE	Yes	Yes	Yes

Notes: This table presents heterogeneous treatment effects of same hometown, where the estimation sample in panel A are player observations whose manager's quality is below the median, and the sample in panel B are player observations whose manager's quality is above the median. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Appendix

A Data

In this paper, I combine data from two sources: (1) Wins Above Replacement (WAR) data from Baseball Reference; (2) other data on MLB from the Sean Lahman Baseball Database. The Sean Lahman Baseball Database provides player identifiers that Baseball Reference uses, making merging rather accurate and efficient. I combine these sources and prepare the dataset in three main steps: (1) build the manager cross-sectional data, (2) build the manager panel data, (3) build the player panel data.

A.1 Building the Manager Cross-Sectional Data

WAR data from Baseball Reference and 4 datasets from the Lahman Database, including People, Appearances, Managers, and HallofFame, are compiled to build the manager cross-sectional data. The People dataset describes an individual's demographics and time-invariant characterstics, such as which hand to throw and bat. The Appearances dataset allows me to identify whether a manager was once an MLB player, and provides measures of managers' skills as a player, such as games played and games won as a player. The Managers dataset is manager-team-season level panel data, providing information on team performance.

All of these measures are aggregated into the individual level, summarizing one's player career and manager career. Then these datasets are merged together into the manager cross-sectional data. This dataset is used to estimate the relationship between manager quality and manager characteristics.

A.2 Building the Manager Panel Data

The manager panel data is built through compiling People, Managers, and Appearances datasets. The observation in this panel data is team/manager-season level. This dataset is used to estimate the AKM model and obtain manager fixed effects. The identification of the AKM model relies on worker moves, and thus a key task in building this dataset is to track manager turnovers accurately, which imposes two challenges.

The first arises in dealing with mid-season manager turnovers. I cannot identify the exact time when a mid-season manager turnover took place. Therefore, the last manager in each season is considered the manager of that team throughout the entire season for simplicity. In this way, for each season, team and manager can be matched one-to-one.

The second challenge arises in dealing with team changes. I have a long panel of 153 seasons, and there were many team changes. However, to keep track of managers and players, a team that experienced name changes, relocations, and changes in ownership, is still considered as the same team.

A.3 Building the Player Panel Data

The player panel data is built through compiling WAR data, People, Managers and Appearances datasets, as well as the manager cross-sectional data. This dataset is used to estimate the effects of common characteristics between managers and players on player performance. Same challenges arise as in building the manager panel data, and I adopt the same approach to address these challenges.

B Robustness Checks

Table B.1: Treatment Effects of Common Characteristics–Player Managers Included

	Games Played	Games Started	WAR
	(1)	(2)	(3)
Panel A: Same Position			
Same Position			1.04***
			(0.07)
Observations			38,963
Panel B: Both Lefties			
Both Lefties	22.57***	24.15***	0.50***
	(3.44)	(3.42)	(0.14)
Observations	14,129	14,129	13,765
Panel C: Same Hometown			
Same Hometown	15.36***	13.91***	0.33***
	(2.61)	(2.75)	(0.09)
Observations	52,274	49,167	49,553
Player FE		Yes	
Season FE		Yes	

Notes: This table presents results of common characteristics by estimating Equation 4 with the sample including player managers. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table B.2: Treatment Effects of Common Characteristics–Alternative Assignment of Managers

	Games Played (1)	Games Started (2)	WAR (3)
Panel A: Same Position			
Same Position			1.08***
			(0.08)
Observations			39,103
Panel B: Both Lefties			
Both Lefties	22.97***	26.04***	0.80**
	(3.85)	(3.91)	(0.15)
Observations	13,546	13,546	13,267
Panel C: Same Hometown			
Same Hometown	17.20***	17.46***	0.39***
	(3.01)	(3.32)	(0.12)
Observations	47,917	45,468	45,901
Player FE		Yes	
Season FE		Yes	

Notes: This table presents results of common characteristics by estimating Equation 4 with different treatments respectively. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.

Table B.3: Treatment Effects of Both Lefties–Alternative Definition and Alternative Sample

	Games Played (1)	Games Started (2)	WAR (3)
Panel A: Alternative Definition			
Both Lefties	18.26***	19.40***	0.42**
	(4.21)	(4.48)	(0.17)
Observations	11,945	11,945	11,664
Panel B: Alternative Sample			
Both Lefties	24.41***	26.95***	0.57***
	(3.37)	(3.74)	(0.14)
Observations	46,220	44,692	44,657
Player FE		Yes	
Season FE		Yes	

Notes: This table presents results of both lefties by estimating Equation 4 with an alternative definition of left-handedness and an alternative sample. Robust standard errors are in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels.