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The Effect of the Initial Distribution of Labor-Related Property Rights on the Allocative Efficiency of Labor Markets

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The Effect of the Initial Distribution of Labor-Related Property Rights on the Allocative Efficiency of Labor Markets

Abstract

We conduct an empirical study to examine the impact of the initial distribution of labor-related property rights on the allocative efficiency of labor markets for skilled workers in a highly competitive labor market (professional basketball). Specifically, we compare a regime where employers can trade workers to other employers without the worker's consent to one where workers have the right to negotiate freely with other employers and move without their employer's consent. Our results indicate that contrary to the predictions of the Coase Theorem, allocative efficiency decreases when workers have the initial right to negotiate freely and move to another employer.

Keywords: Coase theorem, labor market, allocative efficiency, productivity, sports as a lab

JEL Codes: E24, J01, J21, L83

1 Introduction

In his seminal paper, Coase (1960) claims that frictionless markets efficiently allocate scarce resources as long as property rights are fully specified, and specifically argues that the initial allocation of these property rights is irrelevant. The famous "Coase Theorem" has a less renowned predecessor—Rottenberg's (1956) "Invariance Principle"—that specifically addresses this point. Rottenberg argues that workers (i.e., professional baseball players) will always be efficiently allocated regardless of whether the workers' freedom to choose their employer (team) is restricted (e.g., by a reserve clause) or not (i.e., free agency).[1] In both cases, workers will end up with the employer for which they are most productive. The logic behind the Invariance Principle is straightforward: Without restrictions (free agency), the employer for which a worker is most productive can offer the highest salary, and the worker will end up signing a contract with this employer. If the employer can veto a transfer (i.e., the reserve clause), the employer must be induced to let the worker transfer. Since the employer for which the worker is most productive can always make an offer that exceeds the profits the current employer can realize by keeping the worker, under the reserve clause regime, workers will also transfer to where they are most productive.

While theoretically plausible, whether the Coase Theorem in general and the Invariance Principle in particular hold in the real world is still under scrutiny. In fact, there exists an extensive literature that seeks to test the predictions of the Coase Theorem either experimentally or empirically (Medema, 2020; Zerbe & Medema, 2000). The experimental tests of the Coase Theorem offer the possibility of imitating the conditions of complete property rights and zero (or low) transaction costs. Under such perfect "frictionless" conditions, the

[1] In North American professional sports, the reserve clause was part of a player contract and gave club owners an exclusive option to renew unilaterally the contracts of their players binding them to their clubs until release, retirement, or a trade. In the late 1970s, the reserve clause was replaced by free agency. A free agent is a player who is not under contract at any specific team and thus is eligible to sign with other employers.

experiments provide significant support for the predictions of the Coase Theorem (Hoffman & Spitzer, 1982, 1986). However, in more complex environments with non-negligible transaction costs and information asymmetries that resemble many real-world settings, the results from experiments are mixed (Cherry et al., 2005; Croson & Johnston, 2000; Harrison et al., 1987; Shogren, 1992).

While most of these tests of the Coase Theorem have been conducted with a particular focus on the theorem's efficiency proposition, there are some case studies and econometric analyses that test allocative outcomes under alternative legal regimes. For example, a stream of literature tested the farmer-rancher parable of Coase in the real world and found empirical support for the theorem's predictions that agents seem to cooperate to find efficient solutions (Ellickson, 1986; Fischel, 1995; Hanley & Summer, 1995; Vogel, 1987). Another popular approach to empirically test the theorem is to turn the world of divorce (Chiappori et al., 2015; Peters, 1986; Wolfers, 2006; Zelder, 1993), where scholars find admittedly weaker support for the Coase Theorem.

Understandably, professional team sports provide a fertile testing ground for the Invariance Principle (Fort et al., 2016). Specifically, sports economics scholars have empirically tested whether the Invariance Principle holds by examining the impact of player ownership on player mobility (Hylan et al., 1996; Krautmann & Oppenheimer, 1994; Lin & Chang, 2011; Surdam, 2006), competitive balance (Szymanski, 2007), distribution of talent (Crooker et al., 2019), winning probabilities (Surdam, 2006) and player salaries (Sanderson & Siegfried, 1997). Despite the vast array of studies, no consensus has arisen on whether the Invariance Principle holds. Nor is there a study that examines the influence of initial property rights allocations on player productivity.

Whether frictionless markets efficiently allocate scarce resources is thus still an open question. To answer it, one needs to be able to measure allocative efficiency, which, in turn, requires comparing the productivity of resources in alternative settings. This comparison is a demanding task. In most industries, it is practically impossible to isolate and objectively compare the productivity of single resources like surgeons, lawyers, or sales agents across alternative uses under ceteris paribus conditions because one cannot control for all external factors that codetermine the final output.

To overcome the obstacles of measuring the influence of the initial distribution of labor-related property rights on allocative efficiency, we focus on an industry in which workers' productivity is highly measurable and production takes place in a laboratory-like environment. Specifically, we compare the productivity of professional basketball players in the National Basketball Association (NBA) under two different property rights regimes. In the first regime, the employer has the right to send the worker to another employer (i.e., trade the player) without the worker's consent. In the second regime, the worker can freely negotiate with other employers and move to another employer (via free agency) without the consent of the original employer.

The NBA provides an ideal "laboratory" for measuring and comparing labor productivity (Arcidiacono et al., 2017).[2] Compared to surgeons, lawyers, or sales agents, professional basketball players usually "work" under standardized "laboratory" conditions. The size of the court, the height of the rim, the playing time, the weight and the diameter of the ball, etc., do not change from game to game. Moreover, the standardized rules enforced by the NBA for all

[2] Several other scholars have used sports as a laboratory in labor economics (Bollinger & Hotchkiss, 2003; Gould & Winter, 2009; Guryan et al., 2009; Hendricks et al., 2003).

games ensure that labor productivity can be objectively compared. Major "external" factors which might distort productivity are held constant.

In addition, several characteristics of the NBA limit the extent to which players behave opportunistically and strategically, e.g., by shirking (Berri & Krautmann, 2006). Players' productivity is constantly observed and measured by various relevant stakeholders, such as team managers, owners, and fans. At the same time, the NBA is the top basketball league in the world from a sporting and financial point of view. For example, player salaries are among the highest in all professional team sports (Statista, 2022), which translates into intense competition between and even within teams. Whether a player receives a contract strongly depends on his productivity and effort. Therefore, we argue that the "basketball laboratory" setting allows us to define comprehensive and largely objective labor productivity measures.

We empirically test the Invariance Principle and by extension the Coase Theorem by comparing the productivity of workers (players) before and after moving to a new employer (team) under two different initial distributions of property rights. If the Coase Theorem holds, we should not observe any significant differences in labor productivity between labor-related property rights regimes. On the other hand, if the Coase Theorem does not hold, we should observe labor productivity differences for workers who can freely negotiate with other employers and move to another without the consent of the original employer compared to those workers who are bound to their original employer.

Using 8,078 player-season observations from the NBA, we demonstrate that the Coase Theorem and the Invariance Principle do not empirically hold. Specifically, we observe that the initial distribution of labor-related property rights significantly affects the allocative efficiency of labor markets. Contrary to the predictions of the Coase Theorem, we find that workers (players) are allocated more efficiently if employers (teams) can send their workers to another employer without the workers' consent. Allocative efficiency decreases if the worker has the initial right to negotiate freely and move to another employer. These effects are driven by workers with above-median average productivity and seem to persist beyond the very short run.

The remainder of the paper is structured as follows: Section 2 describes the data and the empirical model. Section 3 presents the results of the empirical investigation, and Section 4 concludes the paper.

2 Data and Empirical Model

2.1 Data and Sample

To test whether the initial distribution of labor-related property rights influences the allocative efficiency of labor markets, we build a unique panel dataset consisting of 8,078 player-season observations from the NBA, spanning 17 regular seasons from 2003/04 to 2019/20 and containing a total of 1,766 players and 33 teams. We use data from several publicly accessible data sources: Rod Fort's Sports Business pages for the player and team productivity measures; ESPN and the official NBA website for player and team characteristics; and Spotrac, which is based on official NBA data sources, for information on players' contract situations.[3]

We undertake several data adjustments to isolate the impact of different property rights regimes on player productivity by excluding groups of observations for different factors. There is substantial overlap among these excluded observations when considering these four reasons. First, even though most of the contractual changes happen in the offseason, they can also appear during the regular season. Since we only have productivity data for one player-team

[3] Rod Fort's Sports Business page: https://sites.google.com/site/rodswebpages/codes (accessed May 26, 2021); ESPN: http://insider.espn.com/nba:hollinger/statistics (accessed October 10, 2021); Official NBA website: http://stats.nba.com/stats (accessed February 3, 2022); Spotrac: https://www.spotrac.com/nba/free-agents/ufa/ (accessed May 26, 2021).

combination per season, we exclude 1,791 observations where a player's contractual situation changed within a given season. Similarly, we exclude 1,571 player-season observations when a player experienced a contractual change (within and between seasons) other than moving to a new team without the consent of the existing team (free agent) or being traded to a new team without his explicit consent (non-free agent). Examples include layoffs, sending players to minor leagues, and unique contractual settings (e.g., for young players). These observations represent unique contexts that are not covered in our research question.

Second, to ensure an accurate comparison, we excluded 1,442 observations of players who played less than 21 games (out of a maximum of 82) in a given season and had an average court time of less than 3.3 minutes (out of a maximum of 48) per game. These observations represented the lowest 5% of the sample in terms of games played and average minutes played per game. This exclusion allows us to focus on members of each team with a sufficient amount of productivity data.[4]

Third, we exclude 1,679 observations of players with less than four years of experience in the NBA during the observation period. Again, the reason is that we want to focus on established players with sufficient skills whose employment prospects in the league are favorable.

Fourth, since we compare the productivity of players between seasons, we exclude 2,171 observations for players who are in their first season and players who missed entire seasons (e.g., due to an injury). Our final sample consists of 3,132 unique observations, corresponding to 686 different players. Our results are robust against variations in the mentioned exclusions (see section 0).

[4] In our robustness checks, we expanded the sample size by including players with less court time and fewer games played, but our results remained consistent

2.2 Variables and Measures

This paper focuses on the impact of different property rights regimes (free agent vs. non-free agent) on player productivity. We identify this impact using dummy variables for players who move to a new team as free and non-free agents. We control for two additional categories: players who stay with their current team after becoming free agents and players who forego free agency by extending their contracts early. The reference group contains players who remain with their current team under an ongoing contract.

Table 1 gives a brief overview of the variables used in our models.

< Insert Table 1 about here >

The primary dependent variable is player productivity. To measure a player's productivity, we follow Berri (1999), Berri et al. (2011), Berri and Schmidt (2010) and Price et al. (2010), who employ *Wins Produced* (WP) as the productivity measure. WP connects a player's productivity to team outcome and is regarded as the best objective measure of a player's productivity in a given season (Berri & Bradbury, 2010). It is based on observable and measurable statistics of a player, his team, and the opposing teams. WP explains over 90% of the variation in team wins and is relatively stable from season to season (Berri & Schmidt, 2010).[5]

Independent variables for each player include their contractual status (free agent vs. non-free agent), minutes played in the previous season as a proxy for general (or average) productivity,[6]

[5] We prefer WP to a more standardized measure, such as *Wins Produced per 48 minutes* - WP48 (Berri & Schmidt, 2010). The latter reflects a minute-based version of WP. A high WP48, however, does not necessarily translate into a high WP. The reason is that we assume WP48 to be decreasing in playing time beyond a certain player-specific threshold because basketball is a highly exhausting activity. For example, players with relatively low WP may only be given a few minutes on the court by their coaches and thus still could exhibit a high WP48. In other words, a coach presumably is much more interested in a player's WP than in his WP48. Therefore, we argue that WP is the appropriate and relevant measure for player productivity in the present context.

[6] We cannot use lagged or aggregated values of the dependent variable as measure for players' general (or average) productivity, since in linear panel models this potentially leads to endogeneity issues (Cameron & Trivedi, 2005).

experience in the NBA, tenure with their team (or in case of moving to a new team with the last team), teammates' productivity, team turnover, and fixed effects for teams and seasons.

2.3 Empirical Model

To empirically examine our research question, we estimate linear panel regression models with WP as the dependent variable. We estimate the effect of a contractual change on productivity using two specifications: First, we investigate the overall impact of moving to a new team, whether as a free or non-free agent (equation 1). Second, we distinguish between free and non-free agents (equation 2). To take advantage of the panel structure of our data, we use a fixed effects linear panel regression model (FE), as shown in equations 1 and 2.[7] One significant advantage of this model is that it allows us to capture unobserved time-invariant individual-specific effects, thereby attenuating a potential omitted variable bias (Cameron & Trivedi, 2005).[8]

$$WP_{it} = X'_{i,t}\theta + \beta NEW\ TEAM_{i,t} + \alpha_i + T_t + \varepsilon_{i,t} \quad (1)$$

$$WP_{it} = X'_{i,t}\theta_1 + C'_{i,t}\theta_2 + \beta_1 FA_{i,t} + \beta_2 NON\ FA_{i,t} + \alpha_i + T_t + \varepsilon_{i,t} \quad (2)$$

$X'_{i,t}$ is a vector of the control variables described in Table 1 including team fixed effects. The vector $C'_{i,t}$ includes the dummy variables STAY FA and EXTENSION. α_i are player-specific intercepts (player fixed effects) that capture unobserved time-invariant player characteristics. T_i are time fixed effects. θ, θ_1, θ_2, β, β_1, and β_2 are the coefficients (vectors) to be estimated, while our main interest is on β, β_1, and β_2. $\varepsilon_{i,t}$ is the usual independently and identically

[7] We use Stata's xtreg package to estimate our models (StataCorp, 2017).

[8] We test the FE specification against a pooled OLS panel regression (POLS) and against a random effects specification (RE). We reject POLS using a standard F-Test for joint irrelevance of player fixed effects. Likewise, we reject the RE specification using a Hausman test. Therefore if the model is correctly specified, coefficient estimation using a FE specification will be consistent (Cameron & Trivedi, 2005).

distributed (iid) error component. We use robust standard errors to deal with heteroscedasticity and potentially clustered standard errors.[9]

3 Results

This section presents our results and is structured as follows: First, we summarize and provide information about our sample data. Second, we report the main results of our empirical investigation. Third, we offer supplementary analyses to better understand the underlying mechanisms that drive our results. Finally, we report different robustness analyses.

3.1 Summary Statistics

Table 2 presents the descriptive statistics for all main variables included in the analysis.

< Insert Table 2 about here >

The table shows that around 13% of all player-season observations consist of moving free agents (FA), while 8% move as non-free agents (NON-FA). In total, 21% of player-season observations moved to a new team (NEW TEAM). 7% of player-season observations are free agents who stayed with their current team (STAY FA), and 4% of players extended their contracts before becoming free agents (EXTENSION). The reference group, therefore, amounts to 68%. The average WP is 4.24. Players in the sample average 1,757 minutes on the court in the previous season (L_MINUTES) and have 5.8 years of experience in the NBA (EXP). They stay 2.6 seasons with a given team (TENURE). The average WP of all other players on a given player's team (AV TEAM WP) amounts to 2.6, which is substantially smaller than the average of the dependent variable. This difference is because the team average considers all players, while in our model, we only focus on those players who participate in

[9] Formal tests of heteroskedasticity reject the null hypothesis of homoskedasticity in the error terms. We therefore use robust standard errors and cluster them at the player level. However, results are robust against different ways of correcting standard errors (see section 0).

more than a quarter of the team's games and a minimum number of minutes per game. Finally, on average, 4.9 new players join a team at the beginning of each season (TURNOVER).

3.2 Main Estimates

In this subsection, we report our main estimates in Table 3. We proceed in two steps: First, we examine how player productivity (WP) changes after moving to a new team without controlling for the contractual status of a player (Model 1). Second, we rerun the regression but control for the contractual status of a player (Model 2).

< Insert Table 3 about here >

In the first specification, we observe a significant decline in player productivity after moving to a new team. Specifically, the variable NEW TEAM is highly significant with a coefficient of -0.398, which means that moving to a new team is associated with a reduction of WP by 0.398 relative to the reference group. This reduction in WP translates into a productivity drop of 9% compared to the mean of WP in the sample (4.24).

In the second specification, we distinguish between free and non-free agents and examine whether the initial distribution of property rights influences allocative productivity. Contrary to the predictions of the Coase Theorem, we find significant differences in player productivity between the different property rights regimes after moving to a new team. Specifically, we observe that WP drops significantly for free agents by 0.544, while for non-free agents, the estimated coefficient is not statistically significant. Compared to the mean of WP in our sample, free agent productivity declines by 13% after they move to a new team. In contrast, we find no statistically significant effect for free agents who stay with their current team nor for players who extend an ongoing contract.

We summarize our main results: moving to a new team generally leads to declining player productivity for free agents but not for non-free agents. In other words, players are allocated more efficiently if they are sent to another team (i.e., traded) without their consent. Allocative efficiency decreases if the player has the initial right to negotiate freely and move to another team as a free agent. In addition, these results suggest that the initial distribution of labor-related property rights significantly affects the allocative efficiency of labor markets; thus, the Coase Theorem and Invariance Principle do not hold.

3.3 Supplemental Analyses

In this subsection, we report additional results in three ways: First, we analyze a different dependent variable (minutes played) to shed light on potential mechanisms behind our main results. Second, we distinguish subsamples regarding players' current average career productivity as a proxy for skill level. Third, we analyze the persistence of the effects beyond one season.

Additional Productivity-Related Measure

We consider an additional productivity-related measure to understand better the impact of moving to a new team on player productivity. We use minutes played in a given season as a dependent variable (MINUTES). This variable reflects how coaches intensively use their players. We assume that coaches use players primarily based on their overall productivity (WP) to maximize team wins over the whole season. In other words, players with a high WP will play more minutes than players with a low WP. Inefficient allocation of playing time to players results in a competitive disadvantage against other teams, which is associated with negative sporting performance and, subsequently, has negative financial consequences. We, therefore, hypothesize that if a player experiences a drop in productivity (e.g., after moving to a new team), he will get less playing time.

The empirical model we estimate to test this hypothesis is very similar to the previous models for WP. Table 4 displays the results.

< Insert Table 4 about here >

The result resembles those for WP. Specifically, we find that new players are used for significantly fewer minutes. Free agents, whose minutes on the court drop by around 120 minutes per season, drive this result. Considering the sample average minutes per game of approximately 25, this decline in playtime corresponds to the equivalent of four to five games. This finding is in line with our main results. To efficiently allocate court time among players, coaches reduce the court time of players who experience a drop in productivity after moving to a new team.

Subsample Analysis

We now examine whether our results depend on a player's skill level and, thus, his productivity. More skilled and productive players may have different motives and opportunities for moving to a new team. A player's skill level may also determine how fast he can adjust to playing with new teammates. In Table 5, we estimate our models separately for the upper and the lower half of the distribution of players' current average career WP (until *t-1*).[10] Again, we examine how player productivity WP changes after moving to a new team without (Model 1) and with (Model 2) controlling for the contractual status of a player.

< Insert Table 5 about here >

Without controlling for the contractual status of a player, we observe that the players' productivity only declines for above-median players after moving to a new team. The

[10] The average values of the upper and lower half of the distribution of the current average career WP are 1.26 (below median) and 6.83 (above median). The effects remain similar if we use different variables to measure players past or general productivity.

productivity of below-median players is not affected by moving to a new team. Distinguishing between free and non-free agents reveals that only the productivity of above-median free agents decreases after a trade. In contrast, the productivity of below-median free agents is not affected. Moreover, compared to the main results, we observe a decline in productivity for (above-median) non-free agents. The effect, however, is much smaller than for free agents and statistically significant only at the 10% level. We do not observe any effects for the lower half of the distribution.

In sum, the upper half of the skill distribution drives the effects from our main estimates. In addition, the predictions of the Coase Theorem seem to hold for the lower half of the skill distribution because, for those players, the initial distribution of property rights does not influence allocative efficiency.

Persistence

A decline in productivity after moving to a new team could, for example, result from short-term difficulties that players experience when adjusting to a new team and environment. In this case, we would expect the adverse effects on productivity to diminish over time.

In Table 6, we examine the potential persistence of the productivity drop by re-estimating our WP model in season *t+1*, i.e., in the second season after the possible move to a new team.[11]

< Insert Table 6 about here >

Results in Table 6 indicate that the overall drop in productivity after moving to a new team also persists in season *t+1*. This result is valid for free agents (at the 10% level), while for non-free agents, there is no effect, which is consistent with our previous findings for season *t*. We

[11] We do not cover further seasons due to a substantial drop in the sample size since we then must focus on players who remain with their (new) team for three or more seasons. We already see that the coefficients in Table 6 are estimated less precisely than in our main models.

conclude that, even if short-term difficulties to adjust might play a role, the drop in productivity for free agents, who move to a new team, persists over time. Given that an NBA season consists of at least 82 games,[12] two seasons (*t* and *t*+1) is a highly relevant period for a team from a sporting and financial perspective.

3.4 Robustness Analysis

Potential Biases

Several endogeneity issues regarding the move to a new team could bias our results. First, teams usually trade players when they underperform. If the player improves after the trade, there is no way to tell whether the improvement is the result of the trade or a so-called regression to the mean (Nevill et al., 2004). Second, the prospect of free agency might affect productivity. Towards the end of their contract, players are incentivized to increase their effort to strengthen their negotiation position for a new contract. Consequently, an adverse effect of moving to a new team on productivity could result from reduced effort (i.e., "shirking") after a new contract has been signed (Berri & Krautmann, 2006). In the following, we try different approaches to attenuate the potential biases.

First, using simple paired samples t-tests, we compare players' WP in the last season before moving to a new team (*t-1*) with their WP in the previous season (*t-2*). We find no significant difference for either free or non-free agents.[13]

Second, we estimate our models (including dummy variables) for the season before a player moves to a team as a free agent or gets traded. As Table A1 in the appendix shows, there is no significant change in WP for players (both free agents and non-free agents) before they move

[12] After the regular season, 16 of currently 30 teams qualify for the post-season with a minimum of four and a (theoretical) maximum of 28 additional games each.
[13] P-values of these tests are 0.27 (free agents) and 0.44 (non-free agents).

to a new team. We therefore have no evidence that an upcoming change in the contractual status is related to superior (free agent) or inferior (non-free agent) productivity.

Third, we adopt a simplified version of a method developed by Freyaldenhoven et al. (2019) to correct pre-trends in panel event-study designs. We regress WP on the player and team-season fixed effects in the first step.[14] We use the fitted values $\widehat{\mathrm{WP}}_t$ of this regression to calculate the change of WP against the predicted values of the previous season, $\Delta WP = WP_t - \widehat{WP}_{t-1}$. In the second step, we apply our models with ΔWP as the dependent variable and a slightly adjusted set of control variables.[15] The idea behind this approach is that we use some average value for each player as a basis for the potential productivity change after moving to a new team. Thereby, temporary effects that lead to under- or overperformance are (at least partially) attenuated.[16] Results are shown in Table A2 in the appendix. The coefficients of the variables of interest are similar to the above results. The adverse effects on productivity in general (and for free agents in particular) become slightly stronger while the coefficient for non-free agents is closer to zero.

Finally, we try an instrumental variable (IV) approach for the variables NEW TEAM, FA, and NON-FA to cope with the potential endogeneity issues discussed above. The main challenge in an IV approach is to find solid and valid instruments for the potentially endogenous variables (Angrist & Pischke, 2009). Since we are unaware of IV approaches in related papers, we devise our own instruments. We try different specifications with different instruments that are all supposed to cause exogenous variation in the potentially endogenous variables. For FA, we use eligibility as FA, i.e., the end of an ongoing contract, as the main instrument, an approach used by Abadie (2003) for the participation of workers in 401(k) savings plans.

[14] We try different specifications. All of them lead to similar results.
[15] The variables AV TEAM WP and VAR TEAM WP (see Table 1) have been adjusted to reflect changes between two seasons. Results, however, are robust against alternative definitions of controls.
[16] The Pearson correlation coefficient between WP and $\widehat{WP}$ amounts to 0.8 in our sample.

We instrument NON-FA with variables indicating how many additional players a team traded in a given season, a player's 'movement history' (previous trades and previous moves in general), and whether a player is in his last contract year or not. Some of these variables are used by (Cymrot & Dunlevy, 1987) to model the decision of baseball players to move to a new team. For the variable NEW TEAM, we use a combination of the instruments for FA and NON-FA. Table A3 in the appendix contains the results of a two-stage least square approach (2SLS) with the instrumented variables NEW TEAM, FA, and NON-FA.[17] We observe similar results as before. The effect of NEW TEAM is slightly larger, while the impact of FA is smaller. Both remain statistically significant. The coefficient for NON-FA is now positive but still insignificant due to a lack of precision.[18]

In summary, the above robustness analyses support our main findings.

Additional Robustness Checks

We perform different additional robustness checks to validate our results. First, we further restrict the sample in the following ways: We only consider players who stay at least two seasons with their team before moving to a new team; we exclude players who play under a rookie contract, which usually lasts two to four years and contains specific restrictions on player movement; we do not include players in their last two years of their career, because, e.g., there could be different incentives ('end of career effects'); and we exclude peculiar events such as a new team joining the league[19] or the lockout season 2011/2012.[20] Our results do not

[17] We use Stata's xtivreg command (StataCorp, 2017) and the xtrivreg2 package (Schaffer, 2010).

[18] A word of caution: Even if standard post-IV tests (e.g., F-Test of the first stage, tests for under-identification, over-identification, and weak instruments) do not indicate apparent problems, the potential lack of convincing instruments could bias results. We, therefore, consider the IV results merely as complementary evidence.

[19] If a new team joins the NBA, it can pick players from existing teams under some conditions (the so-called expansion draft). Since these players cannot choose whether they want to join the new team or not, we treat these observations as non-free agents.

[20] In this season, only 80% of games were played because club owners and the players labor union (National Basketball Players Association NBPA) could not agree upon a new deal about, e.g., revenue sharing between owners and players. During the negotiations that lasted until December 2011, no sign-ups of new players nor trades were allowed.

change qualitatively by using more restricted samples. Second, we try larger sample sizes by considering players with less court presence and fewer games played and by relaxing the restriction that players must be at least four years in the NBA. In other words, we consider less essential and less established players. Again, the basic pattern of results does not change qualitatively. Third, our results do not depend on how we correct standard errors. Similarly, our results are robust against excluding season and/or team fixed effects as control variables, even though each of these effects matters. Fourth, we exclude the variable L_MINUTES as a proxy for players' skills. Therefore, we aim to avoid potential bias due to the use of lagged variables strongly correlated with the lagged dependent variable (see footnote 6). Estimating our models without L_MINUTES (and its square term) does not change the results qualitatively. In short, our results are robust against various sample definitions and model specifications.

4 Conclusions

This paper empirically tests the allocative efficiency of labor markets under different property rights regimes. Specifically, we compare workers' productivity before and after moving to a new employer under two different initial distributions of property rights, thereby testing the predictions of the Coase Theorem (Coase, 1960) and the Invariance Principle (Rottenberg, 1956). In the first regime, the employer has the right to send the worker to another employer without the worker's consent. In the second regime, the worker can freely negotiate with other employers and move to another employer without the consent of the original employer.

While individual worker productivity is not accessible in most industries, professional sports provide a rigorous laboratory setting for measuring and comparing personal productivity data (Fonti et al., 2022). Not only do professional athletes "work" under standardized conditions, but standardized rules for games also allow the objective comparison of individual productivity. Indeed, due to the unusual granularity of sports data, many scholars use sports data to test and develop theories and explore relevant phenomena outside the sports context (Arcidiacono et al., 2017; Bothner et al., 2007; Day et al., 2012; Gould & Winter, 2009; Guryan et al., 2009; Moliterno & Wiersema, 2007; Stuart & Moore, 2017). In this paper, we use the NBA as a "laboratory" for our analysis and build a unique panel dataset consisting of 8,078 player-season observations from the NBA, spanning 17 regular seasons from 2003/04 to 2019/20 and containing a total of 1,766 players and 33 teams.

Without controlling for the initial distribution of labor-related property rights, our results indicate that workers' productivity (players) decreases after moving to a new employer (team). By controlling for the prevailing property rights regime, we find that workers (players) are allocated less efficiently if the worker has the initial right to negotiate freely and move to another employer (team). Surprisingly, allocative efficiency increases compared to the previous property rights regime if employers (teams) can send their workers to another employer without the workers' consent. Moreover, we examine whether our results depend on a worker's skill level. More skilled and, therefore, more productive workers may have different motives and opportunities for moving to a new employer. Our results indicate that only the productivity of workers with above-median skill levels suffers from moving to a new employer. In contrast, the productivity of workers with below-median skill levels is not affected. In summary, our results provide robust evidence that the initial distribution of labor-related property rights significantly affects the allocative efficiency of labor markets. The results are driven by workers with above-median average productivity and persist beyond the short run.

Previous literature that empirically tested the predictions of the Coase Theorem, specifically allocative outcomes under alternative legal regimes, focused on the farmer-rancher example (Ellickson, 1986; Fischel, 1995; Hanley & Summer, 1995; Vogel, 1987) or the world of divorce

(Chiappori et al., 2015; Peters, 1986; Wolfers, 2006; Zelder, 1993). An essential contribution of our study is to provide an empirical test of the predictions of the Coase Theorem in a labor market setting. Contrary to previous literature that finds (weak) support for the predictions of the Coase Theorem, our study indicates that the predictions do not hold in general but only for a particular type of workers, namely workers of below-median average productivity.

We have built our study on the best productivity measure available. WP is the most highly recognized and accepted productivity measure of NBA players (Berri, 1999; Berri et al., 2011; Berri & Schmidt, 2010; Price et al., 2010). Nevertheless, it is not perfect. For example, this measure does only take on-court performance into account. Off-court performance, such as the social charisma of a player, which also highly affects a team's financial success, is not considered. A second limitation is the complexity of the contractual framework in the NBA and the lack of more detailed information on player contracts. Especially, contract duration or the number of teams interested in a free agent would have added exciting insights to our analysis. A third limitation concerns the inability to include trades and moves of players that occurred within a season. A fourth limitation concerns the possibility that the under-/ overperformance of a player may depend on expectations, a counterfactual we cannot observe and measure.

Finally, shirking after signing a new contract could also bias our results. Berri and Krautmann (2006) test this hypothesis using data from the NBA with a similar dependent variable as in the present paper. However, they find no evidence for shirking and argue that opportunistic behavior in professional basketball is prevented because team managers, owners, teammates, and fans constantly observe (and measure) players' productivity. In addition, contracts include vital incentive clauses that link compensation to individual and team productivity. In addition, a shirker might develop a bad reputation, lowering the likelihood of getting a lucrative contract in the future (and thereby losing related sources of income, such as advertising contracts), and thus, a shirking player takes a considerable risk regarding career income.

There are several promising avenues for future research on this topic. For example, a more detailed analysis of the mechanisms driving our results could be conducted using more granular data and/or combining a quantitative analysis with a qualitative study. It would also be interesting to include an examination of the monetary effects (such as salaries and revenues) of moving to a new employer. Another possibility is to investigate whether there is a difference in performance after moving to a new team between players who received "exorbitant" contracts (relative to perceived market value) and players with "normal" contracts. Additionally, it would be valuable to determine whether the negative performance effect is also observed for free agents who move to a new team with the goal of winning the NBA championship. Many veteran NBA players reportedly prioritize winning titles over earning higher salaries.

In summary, this research contributes to our knowledge of the link between the initial distribution of labor-related property rights and the efficiency of labor markets and indicates that the predictions of the Coase Theorem and the Invariance Principle do not hold in the context of a highly competitive labor market such as professional basketball. Further research is necessary to fully comprehend the mechanisms driving our findings and to evaluate their potential generalizability to other settings.

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Table 1 Variable overview.

Variable	Description	Reason for inclusion
Dependent variable		
WP	Wins Produced	Measures player productivity per season
MINUTES	Total minutes in a given season	Measures the time a player is on the court during a season
∆WP	Change of Wins Produced from the previous season's predicted WP	Used in robustness analysis
Independent variables		
L_MINUTES	Total minutes on court in previous season	Proxy for a player's recent productivity and importance
L_MINUTES2	Square of L_MINUTES	Allows for (potential) non-linearities in the effect L_MINUTES
EXP	Experience in the NBA in years	Captures a player's professional experience
EXP2	Square of EXP	Allows for (potential) non-linearities in the effect of experience
TENURE	A player's tenure at the end of the previous season	Captures the stability of a player's existing (or previous) contractual status
AV TEAM WP	Average Wins Produced of all other players in a team	Captures productivity of all other players in a team
VAR TEAM WP	Variance of Wins Produced of all other players in a team	Captures dispersion of productivity of all other players in a team
TURNOVER	Number of (additional) new players in a team	Captures the team turnover
NEW TEAM	Dummy variable indicating players who move to a new team	Captures the change in productivity for players who move to a new team
FA	Dummy variable indicating players who move to a new team as free agents	Allows to capture the change in productivity for this group of players
NON-FA	Dummy variable indicating players who get traded to a new team	Allows to capture the change in productivity for this group of players
STAY FA	Dummy variable indicating players who sign with the existing team as free agents	Allows to capture the change in productivity for this group of players
EXTENSION	Dummy variable indicating players who extend an ongoing contract with the existing team	Allows to capture the change in productivity for this group of players
BEFORE NEW TEAM	Dummy variable indicating the season before a player moves to a new team	Allows to capture (potential) change in productivity before moving to a new team (used in robustness analysis)
BEFORE FA	Dummy variable indicating the season before a player moves to a new team as free agent	Allows to capture (potential) change in productivity before moving to a new team (used in robustness analysis)
BEFORE TRADE	Dummy variable indicating the season before a player is traded to a new team	Allows to capture (potential) change in productivity before moving to a new team (used in robustness analysis)

Table 2 Summary statistics (n=3,312).

Variables	Mean	SD	Min	Max
Dependent variables				
WP	4.24	4.1	-6.19	23.3
MINUTES	1,748.31	748.62	72	3,388
ΔWP (n=2,804)	0.263	2.55	-10.31	10.26
Independent variables				
L_MINUTES	1,756.96	749.27	72	3,388
L_MINUTES2	3,648,118	2,623,543	5,184	11,478,544
EXP	5.75	3.23	1	17
EXP2	43.46	47.37	1	289
TENURE	2.56	2.01	1	15
AV TEAM WP	2.61	0.88	0.19	5.07
VAR TEAM WP	13.5	7.92	1.5	45.52
TURNOVER	4.9	2.27	0	16
NEW TEAM	0.21	0.41	0	1
FA	0.13	0.34	0	1
NON-FA	0.08	0.27	0	1
STAY FA	0.07	0.25	0	1
EXTENSION	0.04	0.19	0	1
BEFORE NEW TEAM	0.21	0.41	0	1
BEFORE FA	0.13	0.34	0	1
BEFORE TRADE	0.08	0.27	0	1

Table 3 Results of fixed effects panel regression models.

	WP	
	(1)	**(2)**
L_MINUTES	-0.0004	-0.0004
	(0.0004)	(0.0004)
L_MINUTES2	0.000	0.000
	(0.000)	(0.000)
EXP	0.788***	0.8***
	(0.251)	(0.252)
EXP2	-0.038***	-0.038***
	(0.006)	(0.006)
TENURE	-0.021	-0.026
	(0.046)	(0.046)
AV TEAM WP	0.686***	0.689***
	(0.115)	(0.116)
VAR TEAM WP	0.036**	0.036**
	(0.015)	(0.015)
TURNOVER	0.058**	0.057**
	(0.027)	(0.027)
NEW TEAM	-0.398***	
	(0.136)	
FA		-0.544***
		(0.16)
NON-FA		-0.228
		(0.198)
STAY FA		-0.146
		(0.188)
EXTENSION		-0.014
		(0.27)
_CONS	3.807***	3.801***
	0.845	(0.843)
TEAM DUMMIES	Y	Y
SEASON DUMMIES	Y	Y
N of Observations	*3,132*	*3,132*
Adj. R2 (within)	*0.21*	*0.21*

Note: The unit of observation is a player. Clustered robust standard errors are in parentheses. Coefficients are estimated by a fixed effects linear panel regression model. The dependent variable is WP.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 Extract of results of fixed effects panel regression models with MINUTES as the dependent variable.

	MINUTES	
	(1)	**(2)**
NEW TEAM	-89.08***	
	(28.7)	
FA		-118.08***
		(34.95)
NON-FA		-29.01
		(42.12)
N of Observations	*3,132*	*3,132*
Adj. R2 (within)	*0.24*	*0.24*

Note: The unit of observation is a player. Clustered robust standard errors are in parentheses. Coefficients are estimated by a fixed effects linear panel regression model. The dependent variable is MINUTES.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 Extract of results of fixed effects panel regression models for subsamples regarding players' current average career productivity.

	WP			
	(1)		**(2)**	
	Below median	**Above median**	**Below median**	**Above median**
NEW TEAM	-0.024	-0.832***		
	(0.154)	(0.226)		
FA			-0.265	-0.963***
			(0.183)	(0.283)
NON-FA			0.249	-0.623*
			(0.231)	(0.324)
N of Observations	*1,566*	*1,566*	*1,566*	*1,566*
Adj. R2 (within)	*0.12*	*0.31*	*0.13*	*0.31*

Note: The unit of observation is a player. Clustered robust standard errors are in parentheses. Coefficients are estimated by a fixed effects linear panel regression model. The dependent variable is WP.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 Extract of results of fixed effects panel regression models for WP two seasons after a potential move to a new team.

	WP_{t+1}	
	(1)	**(2)**
NEW TEAM	-0.415**	
	(0.202)	
FA		-0.404*
		(0.235)
NON-FA		-0.465
		(0.297)
N of Observations	*2,122*	*2,122*
Adj. R2 (within)	*0.2*	*0.2*

Note: The unit of observation is a player. Clustered robust standard errors are in parentheses. Coefficients are estimated by a fixed effects linear panel regression model. The dependent variable is WP in *t+1*.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A1 Extract of results of fixed effects panel regression models capturing the potential change in productivity before moving to a new team.

	FE 1	**FE 2**
BEFORE NEW TEAM	0.047	
	(0.136)	
BEFORE FA		0.226
		(0.157)
BEFORE NON-FA		-0.023
		(0.198)
N of Observations	*3,132*	*3,132*
Adj. R2 (within)	*0.21*	*0.21*

The dependent variable is WP. Cluster robust standard errors in parenthesis.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2 Extract of results of fixed effects panel regression models with ΔWP against the predicted lagged value of WP ($\widehat{WP}$) as dependent variable.

	FE 1	**FE 2**
NEW TEAM	-0.389***	
	(0.148)	
FA		-0.53***
		(0.175)
NON-FA		-0.219
		(0.213)
N of Observations	*2,804*	*2,804*
Adj. R2 (within)	*0.14*	*0.14*

The dependent variable is ΔWP. Cluster robust standard errors in parenthesis.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3 Extract of results of IV estimates (2SLS).

	IV FE 1 (2SLS)	**IV FE 2 (2SLS)**
NEW TEAM	-0.58**	
	(0.233)	
FA		-0.473**
		(0.185)
NON-FA		0.242
		(0.902)
N of Observations	*3,132*	*3,132*
Adj. R2 (within)	*0.22*	*0.22*

The dependent variable is WP. Instruments used in first stage regressions are described in the text.
Cluster robust standard errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.