‘Overattention’ to First-Hand Experience in Hiring Decisions: Evidence from Professional Basketball

Michael Dalton[†] Peter Landry[‡]

April 9, 2020

Abstract

We provide evidence from a real-world, high-stakes, and empirically-advantageous labor market — the market for NBA basketball players — that employers’ hiring decisions rely too heavily on first-hand experiences with job candidates. Specifically, we find that employers are biased in favor of acquiring players with better-than-usual performances when the employer’s team was playing or preparing to play the player’s original team, with performance information receiving approximately 1.8 times more weight in hiring decisions if it is conveyed through such first-hand experiences. These effects are not predicted by leading behavioral learning theories used to explain similar effects observed in other domains. Instead, our findings point to overattention as a key mechanism through which first-hand experience biases can arise.

We are grateful for helpful comments from many who attended presentations of this work at the University of Toronto and at the Society of Labor Economists. We also benefit greatly from discussions with Attila Ambrus, Peter Arcidiacono, Joshua Gans, Daniel Goetz, Avi Goldfarb, Mitch Hoffman, Tanjim Hossain, Nico Lacetera, Matt Mitchell, Matt Osborne, Avni Shah, and especially Patrick Coate. Ian Weaver provided excellent research assistance. The views and opinions expressed in this paper are our own and do not reflect the views of the BLS or the Federal Government. The authors declare that they have no relevant interests (financial or otherwise) to disclose.

[†] Bureau of Labor Statistics, dalton.michael@bls.gov.

[‡] University of Toronto, Mississauga, and Rotman School of Management, peter.landry@rotman.utoronto.ca.

1 Introduction

Employers typically learn about job candidates from a variety of information sources. Some, such as a college transcript, a reference letter, or a performance evaluation from another job, are second-hand in nature. Others, such as an interview, an interaction at a networking event, or an opportunity to observe on-the-job behavior, may instead be described as first-hand experiences. A familiar hiring dilemma can arise when second-hand information and first-hand experiences point to different conclusions — one candidate may look better "on paper" while another seems better "in person." In this paper, we investigate whether employers optimally balance these information sources in real-world hiring decisions.

Past research suggests that hiring managers tend to be over-reliant on personal interviews (Highhouse, 2008; Dana et al., 2013) and under-reliant on the (inherently second-hand) input of "algorithmic" hiring aids (Kuncel et al., 2013; Hoffman et al., 2018). Such findings naturally raise the possibility that employers may systematically overweight first-hand experience relative to second-hand information in hiring decisions. However, these tendencies could instead reflect motives of hiring managers that go beyond hiring the best candidate for the job. For instance, personal interviews could allow hiring managers to tilt the scales in favor of job candidates with whom they would personally like to affiliate. Consistent with this idea, interviewers' evaluations are known to be swayed by a candidate's physical attractiveness (Ruffle and Shtudiner, 2015), relationship status (Rivera, 2017), and whether they enjoy similar leisure activities (Rivera, 2012). Moreover, a hiring manager could be motivated to over-ride the recommendations of algorithmic hiring aids considering deference to such technologies may send a signal that their own input is unimportant (Goldsmith, 2000; Highhouse, 2008).

It is also not clear whether an over-reliance on personal interviews and under-reliance on algorithmic hiring aids would generalize to other forms of first-hand experience and second-hand information. Even if we put aside any doubts regarding hiring managers' personal motives, these findings could merely reflect a bias in favor of tradition. After all, interviews have historically been (and continue to be) the most widely-used tool for evaluating job candidates (Buckley et al., 2000), while algorithmic hiring aids are a relatively recent innovation that (as with other new and unfamiliar technologies) decision-makers may naturally be reluctant to adopt (see Highhouse, 2008).

To investigate whether employers do in fact systematically and suboptimally overweight first-hand experience relative to second-hand information in real-world hiring decisions, the National Basketball Association (NBA) offers a near-ideal testing ground. NBA teams can learn about the quality of another team's player — a potential future employee — based on his current job performance, which is readily quantifiable and freely accessible through widely-published performance statistics (e.g., points per game). While these statistics efficiently convey the player's overall performance in all games played during a given period of time, some of these performances — such as those occurring in games against the team evaluating the player — are also experienced first-hand. To the analyst, this feature is especially helpful for isolating the impact of first-hand experiences on employers' hiring decisions.

As a first look at the effect of first-hand experience, consider the black data points in Figure 1. The horizontal axis indicates, by decile, the difference between a player's mean performance against a team and his mean performance against all teams in the year prior to the player joining a new team.[1] The vertical axis indicates the percentage of cases for which the player joins that particular team. As apparent from the upward-sloped trendline, teams are more likely to acquire players who played well against them in the past year.

As another source of first-hand experience, it is common practice for NBA teams (that is, certain personnel involved in player evaluation and acquisition decisions) to prepare for an upcoming game by observing their opponent's *previous* game.[2] The gray trendline in

[1] The performance measure we use, known as the 'efficiency' metric, is a widely-used (including by the NBA for official statkeeping purposes) and freely-available composite of the statistics reported in a standard NBA box score. Specifically, it is the sum of the good (points, rebounds, assists, steals, blocks) minus the bad (missed shots, turnovers) statistics. The ranges in players' mean performance deviations (according to this measure) for each decile in Figure 1 are provided in Appendix Table A1.

[2] Namely, NBA teams routinely send a 'scout' to an opponent's previous game, who then communicates opposing players' strengths and weaknesses to coaches and managers, and work with team-employed video specialists to create video clips of key plays for coaches to review and show their own players. Waiting until an opponent's immediately-preceding game before scouting them allows teams to acquire the most up-to-date information on an opponent's plays and play-calling signals. As one long-time NBA coach explains, "we prefer to scout an opponent as close to the date of our game as possible ... including in its last game prior to playing us" (Dunleavy and Eyen, 2009). This practice is reinforced by seating arrangements at NBA arenas, which generally hold seats specifically designated for scouts representing each of the two participating team's

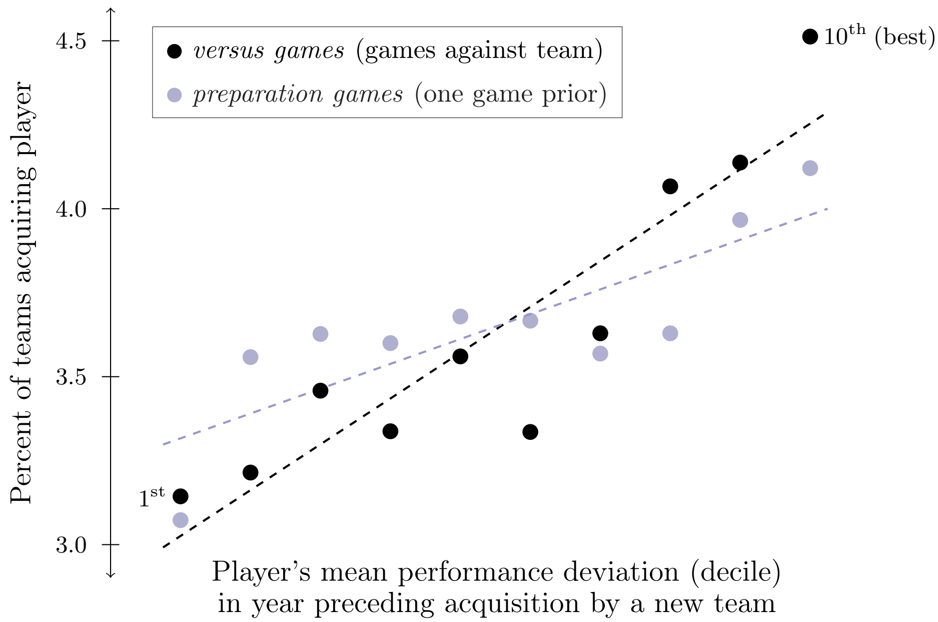

Figure 1. Teams are more likely to acquire players who played relatively well (compared to their mean performance) when the team was playing or preparing for the player's original team.

Figure 1 shows that teams are also more likely to acquire players who perform better in games immediately preceding games against their own team.

The results of our controlled multinomial regressions reaffirm these relationships, as we find that a team's likelihood of acquiring a player independently increases with the player's (relative) performance in "versus games" and in "preparation games" (i.e. games when the team is playing, and games when the team is preparing to play the player's original team, respectively). We argue that these relationships, which we refer to as the *versus* and *preparation effects*, reflect a bias of *overattention* to first-hand experience whereby teams are overly influenced by performances in versus and preparation games. That is, while players' performances in all games are presumably conveyed (and objectively weighted) through second-hand summary statistics, teams naturally allocate extra attention to games they experience first-hand — while evidently assigning meaning to the redundant performance signals provided in such games. As for the magnitude of this alleged bias, our estimates indicate that teams overweight performances in games they experience first-hand roughly

next opponents. According to one veteran scout: "Each arena gives you two seats ... I can't really do my job unless I'm the next opponent, because only the next opponent gets the seats" (Agness, 2016).

by a factor of 1.8, causing 2.3 percent of players to be matched to the "wrong" team.

Certainly the versus effect (as described thus far) lends itself to many alternate explanations. For example, a player may perform better against teams that lack — thus having greater existing demand for — players with his particular skillset. Or perhaps players perform better against worse teams, which are already more prone to acquire new players in order to improve their roster. While we use a variety of control variables to address these and several other alternate hypotheses, we can already see a problem with any hypothesis linking better performances to a higher pre-existing likelihood of being acquired by the opposing team: they do not account for the preparation effect. Indeed, with comprehensive game-by-game and aggregate performance statistics freely accessible through second-hand sources, it is particularly unclear why a team would logically place extra weight on a player's performance in a game that happens to precede a game against their own team.

The rest of the paper proceeds as follows. Section 2 provides relevant institutional background and describes the data. Section 3 describes our model of overattention. Section 4 presents our main empirical results. Section 5 addresses alternate explanations. Section 6 assesses the magnitude of the apparent first-hand experience bias. Section 7 concludes.

2 Background and Data

2.1 NBA Player Transitions

The NBA is a professional basketball league that currently employs roughly 500 players on 30 teams. NBA players tend to have short careers (approximately 99 percent retire before the age of 40) during which they change teams relatively often (roughly once every two years). There are two main ways NBA players can change teams. First, a team may sign a player to a new contract, provided that player is no longer under contract with another team. Second, teams may trade for another team's player (or players) on their existing contract(s), usually in exchange for a player (or players) on their own team.[3]

[3] Trades can also include cash, the rights to acquire a new player who has not previously played in the NBA, and, on occasion, coaches. Related to this, teams may also claim a player who is placed 'on waivers' by his original team, which gives other teams the opportunity to acquire the player on his existing contract — in effect, the opportunity to trade for the player in exchange for nothing. If a player is not claimed off waivers, he is then eligible to sign a new contract with another team.

All else equal, a team's decision to sign or trade for a new player suggests that the team values the player more than a team that did not acquire the player. In practice, however, both types of decisions are imperfect indicators (albeit for somewhat different reasons) of the team's valuation of the player as a prospective member of their team. To start, since a player must agree to sign a new contract, a team's decision to sign a new player presumably reflects the player's interest in joining that team as well as the team's interest in acquiring the player. While trades generally do not require the player's consent, they can still reflect other considerations beyond the team's perception of his value as a member of the team. For instance, there are many rules that restrict the types of trades teams can make, which can cause teams interested in trading one player for another to include other, non-targeted players in the deal simply as a means to bring the trade in compliance with league rules.[4]

There are two 'seasons' during which NBA teams may acquire new players: the 'offseason' (roughly June through October of each year) when no games are played, and the 'regular season' (November through mid-April of the following year) during which each team plays 82 games according to a predetermined schedule. In our sample (described shortly), a little more than half of new player acquisitions occur in the offseason (54.2%, including 56.5% for signed players and 52.7% for traded players) with the rest occurring in the regular season.

2.2 Team Personnel Involved In Player-Acquisition Decisions

Our understanding of team decision-making is invariably limited by a lack of available data as to which personnel actually watch relevant games, how information is aggregated and transmitted among relevant personnel, and which personnel actually have a say in teams' final decisions. From what we do know, some aspects of how teams learn about other teams' players are fairly standardized, yet there does not appear to be a single, generalizable template at higher levels of decision-making. For this reason, we will (throughout the text) generally treat teams as if they were individual decision-makers, abstracting from more refined descriptions of what goes on inside the black box of team decision-making.

[4] Appendix B discusses some other rules that restrict teams' player-acquisition decisions, with a particular focus on rule changes that occurred during our sample period (though empirical results presented in this appendix suggest these rule changes did not meaningfully affect teams' decision-making as it relates to their weighting of first-hand experience relative to second-hand information).

Table 1: Relevant Personnel in NBA Team Decision-Making

	Typically watch versus games?	Typically watch prep. games?	Final say in team decisions?
Coaches	Yes	Yes (on film)	Some*
Scouts	No	Yes	No
Owners	Some	No	Some
Other executives	Some	Some*	Some

"Other executives" may includes team presidents, managers, and directors. See Appendix C for a detailed discussion of the roles of each type of personnel in team decision-making and the evidence on which these classifications are based.

* Some coaches also serve as a team executive. In these cases, the individual would typically observe preparation games (in their capacity as coach) and may also have the final say in player-acquisition decisions (in their executive role). However, it is less likely team executives who do not coach would routinely observe preparation games, or that coaches without an executive role would have authority in player-acquisition decisions.

With these caveats in mind, Table 1 lists relevant team personnel and their potential roles in teams' hiring decisions. Here, it should also be noted that teams often employ several individuals in each of the four listed categories (see Appendix Table C1). Therefore, a 'Yes' or 'Some' designation in Table 1 may apply for at least one (but not necessarily all) individual(s) in that role. Furthermore, the designations only reflect an interpretation based on available information (discussed at length in Appendix C).

2.3 Our Data

Our analysis uses box score data (collected from basketballreference.com) for regular season NBA games between October 1983 and April 2016. For each game, the box score provides the date, the location, the number of fans in attendance, the two teams that competed, the players who played for each team, and various performance statistics for each of these players. Since players are identified by name, we can also identify cases in which a player changes teams from the box score data.[5]

[5] Our box score data is supplemented by other forms of data from the same source, including data on players' birth states and birth dates, data indicating each team's 'conference' and 'division' according to the NBA's (primarily geographical) categorizations, and data indicating whether a player who changes teams signed a new contract upon joining their new team or was traded on an existing contract. Also note, we can only identify a player as having been a member of a team if that player shows up in at least one box score for that team. Thus, if a player is traded from some team A to some team B, but never actually plays for team B because he is immediately traded from team B to some team C (who the player does end up playing for), this appears in our data (and is treated in our analysis) as a direct transition from team A to team C.

Table 2: Summary Statistics

	Mean	S.D.	Percentiles 10th	50th	90th	Obs.
Games in year before switch	62.66	23.72	22.0	75.0	82.0	4,567
Games per opponent	2.02	0.66	1.07	2.06	2.83	4,567
Mean performance per game	7.79	5.16	2.17	6.63	15.2	4,567
Std. Dev. performance (across games)	5.81	2.5	2.38	5.89	8.95	4,567

Our sample contains 4,567 cases in which a player changes teams. On average, players in the sample play in 62.66 games during the year preceding their first game on their new team, with 2.02 games played against each of the 22 to 29 other teams in the league at that time. Using the efficiency metric that adds up the good performance statistics and subtracts the bad performance statistics (see footnote 1), players' average per-game performance during the year preceding their transition is 7.79 while the average standard deviation (standard deviation across games, average across player-team switches) in performance is 5.81.[6]

3 Model

Our model is developed in three steps. In Section 3.1, we present a simple learning model that characterizes how a team balances second-hand information with first-hand experience in evaluating a player on another team. This learning model will serve as a building block for a multinomial model of teams' hiring decisions, which we present in Section 3.2. We then operationalize the model for empirical estimation in Section 3.3.

Before proceeding, we do note that our model is quite stylized. Most notably, the model treats teams as if they are individual decision-makers and does not distinguish between versus and preparation games. As mentioned in Section 2.2 (and discussed at greater length in Appendix C), realistically team decision-making involves input from scouts, coaches, owners, and other team executives. In addition, first-hand experiences in versus games and in preparation games may inform decision-making through somewhat different channels involving different personnel (see Table 1). With that said, we will make the distinction

[6] For comparison, the average per-game performance for all players in our data (regardless of whether they transition to a new team) is 7.82, while the average standard deviation is 6.33. As addressed in footnote 11, this suggests that players with moderate performance levels are disproportionately likely to change teams.

between versus and preparation games in some of our empirical specifications.

3.1 Team Learning

A team's assessment of the overall performance of a player on another team is presumed to be based on information concerning the player's performances, denoted by $\mathrm{P}(g)$, for each game $g \in \mathcal{G}$ that the player competes in during a given evaluation period. We assume that the team receives *second-hand information* concerning the player's performance in *all* games and that, for a subset of these games, the team receives an additional (i.e. redundant) signal of the player's performance through *first-hand experience*.[7]

Letting $\mathcal{G}^{\text{FH}} \subset \mathcal{G}$ denote the subset of games for which the team attains first-hand experience (implying its rank, $|\mathcal{G}^{\text{FH}}|$, denotes the number of such games), the mean first-hand performance signal is then $\overline{\mathrm{P}}^{\text{FH}} \equiv |\mathcal{G}^{\text{FH}}|^{-1} \sum_{g \in \mathcal{G}^{\text{FH}}} \mathrm{P}(g)$. Meanwhile, the mean second-hand performance signal is simply the mean performance in all games: $\overline{\mathrm{P}} \equiv |\mathcal{G}|^{-1} \sum_{g \in \mathcal{G}} \mathrm{P}(g)$.

While the second-hand performance signal $\overline{\mathrm{P}}$ can be understood as an objective measure of the player's overall performance, we allow for the possibility of a bias in the team's subjective assessment arising from 'overattention' to first-hand experience. The *overattention parameter* $\omega \geq 0$ captures the extent of the potential bias — or more specifically, the extent to which the team weights first-hand experience in relation to second-hand information. The team's subjective assessment is then given by

$$\widehat{\mathrm{P}} \equiv \frac{\overline{\mathrm{P}} + \omega \cdot \overline{\mathrm{P}}^{\text{FH}}}{1 + \omega}. \tag{1}$$

3.2 Player Acquisition

We now adapt the simple learning model developed thus far to a multinomial framework describing how a player is matched to one of multiple potential new teams. Here, $\mathcal{J}$ denotes the set of potential new teams and $j \in \mathcal{J}$ denotes a team in this set. In this multi-team setting, it will be useful to separate a team's assessment as given in (1) (except now indexed

[7] This characterization is compatible with two potential ways in which a team might attain second-hand information. First, the team may learn from the player's performance statistics on a game-by-game basis, as if reading the box scores, which are widely published in newspapers and online. Second, the team may learn from the player's cumulative performance statistics, which are also widely published.

by j) into a component that is common to all teams and a team-specific component as

$$\widehat{\mathrm{P}}_j \equiv \overline{\mathrm{P}} + \left(\frac{\omega}{1+\omega}\right) \cdot \overline{\mathrm{PD}}_j, \tag{2}$$

where $\overline{\mathrm{PD}}_j \equiv \overline{\mathrm{P}}^{\text{FH}}_j - \overline{\mathrm{P}}$ is the player's *mean performance deviation* in games for which team j attains first-hand experience. Team j's overall valuation of the player is presumed to depend (though not exclusively) on its assessment of the player's performance as follows:

$$v_j = f(\widehat{\mathrm{P}}_j) + \mu_j, \tag{3}$$

where μ_j simply encapsulates all other factors that affect team j's valuation of the player besides its assessment of the player's performance. Here it may be implicitly presumed that the team's valuation of the player is based on an underlying profit-maximization objective and that, all else equal, a team's profits increase with the performance of its players.[8]

Taking f to be linear ensures that the additive separability of $\overline{\mathrm{P}}$ and $\overline{\mathrm{PD}}_j$ in team j's assessment of performance $\widehat{\mathrm{P}}_j$ is maintained in team j's overall valuation, which is now:

$$v_j = \alpha + \beta\,\overline{\mathrm{P}} + \gamma\,\overline{\mathrm{PD}}_j + \mu_j. \tag{4}$$

We may note the overattention parameter can be expressed in terms of the coefficients β and γ as $\omega = \frac{\gamma}{\beta-\gamma}$, implying that teams are unbiased (i.e. $\omega = 0$) if $\gamma = 0$.

We assume that the player joins the team with the highest valuation. Abstracting from the possibility of a tie, team $j \in \mathcal{J}$ thus acquires the player if and only if $j = \arg\max_{k\in\mathcal{J}}\{v_k\}$. This matching rule captures the intuitive notion that a team with a higher valuation of a player is more likely to acquire that player. Alternatively, v_j can be interpreted as team j's bid, in which case the rule could be thought of as representing an auction in which the player is matched to the highest bidder.

In practice, there are multiple (and often complex) processes by which NBA players are matched to a new team. As discussed in Section 2.1, some players switch teams by

[8] Higher-performing players may increase team profits by increasing the demand for tickets or team merchandise, or by helping a team qualify for — and thus sell additional tickets during — the playoffs (an annual tournament after the regular season that determines a league champion). Alternatively, a team's objective may be to maximize the proportion of games it wins or its likelihood of winning a championship, though higher-performing players would presumably serve these objectives as well.

signing a contract with that team while others are traded on an existing contract. Certainly it is not clear that an auction would provide a reasonable description of a player who is traded. Even for those who sign a new contract, the negotiation process is rather opaque, and affected by many rules constraining the contracts teams can offer players.[9,10] Lastly, it is possible that only a subset of a player's potential new teams actually consider the player for acquisition (conversely, a team may only consider a subset of all available players). While such considerations would complicate modeling the matching mechanism with greater specificity, they can be crudely understood as entering our model through μ_j.

3.3 Adaptations for Empirical Estimation

We now adapt our model for empirical estimation. To reflect the relevant unit of observation in our data, we will now use i to denote a particular player and t to denote the time at which a particular player changes teams. Thus, μ_{ijt} represents the component of team j's valuation of player i at time t that does not depend on the team's subjective assessment of the player's performance. Decomposing $\mu_{ijt} = m_{ijt} + \epsilon_{ijt}$ into an observed component, m_{ijt}, and an unobserved component, ϵ_{ijt}, team j's valuation of player i at time t is then

$$v_{ijt} = \alpha + \beta\,\overline{\mathrm{P}}_{it} + \gamma\,\overline{\mathrm{PD}}_{ijt} + m_{ijt} + \epsilon_{ijt}. \tag{5}$$

In our empirical estimations, m_{ijt} will include a variety of control variables (described in the next section). We assume ϵ_{ijt} is independently and identically distributed according to the type I extreme value distribution. This assumption allows us to estimate our model as a pure conditional logit regression where the probability that team $j \in \mathcal{J}_{it}$ acquires player i at time t can be expressed in closed-form as:

$$\Pr[j = \arg\max_{k \in \mathcal{J}_{it}} \{v_{ikt}\}] = \frac{e^{\gamma\,\overline{\mathrm{PD}}_{ijt} + m_{ijt}}}{\sum_{k \in \mathcal{J}_{it}} e^{\gamma\,\overline{\mathrm{PD}}_{ikt} + m_{ikt}}}. \tag{6}$$

Note, we were able to eliminate $\alpha + \beta\overline{\mathrm{P}}_{it}$ from this expression because it is common to all of player i's potential new teams.

[9] As examples of such constraints (which have changed over time), the NBA imposes both maximums and minimums on players' salaries, contract lengths, total team salaries, and the number of players per team.

[10] The potential role of player preferences will be addressed at length in Section 5.

4 Results

In this section, we report empirical estimates capturing the effect of first-hand experience on teams' player-acquisition decisions. Here and in subsequent discussions, a "player" will (unless otherwise noted) refer to any player who changes teams and a "team" will refer to any of the player's potential new teams (i.e. any team *except* for the player's pre-transition team).[11] The relevant evaluation period is taken to be the one-year period that precedes the player's transition to a new team.

With our pure conditional logit matching model, the coefficients on control variables that would be the same for all teams are not identified. This includes the coefficient β in equation (5) on the player's mean performance in all games $\overline{\mathrm{P}}_{it}$ (i.e. the second-hand performance signal). However, we do include several team-varying control variables: the team's winning percentage during the year; the mean performance of all players that played against the team; the player's mean utilization (in minutes per game) in versus games and in preparation games; the numbers of potential and actual versus games; the numbers of potential and actual preparation games; dummies indicating whether any actual versus games and any actual preparation games occurred; and dummies indicating whether the team is located in the player's birth state, whether the team is located in a state that borders the player's birth state, whether the team and the player are in the same conference, and whether the team and the player are in the same division.[12]

There was at least one versus game for 75% of player-team observations, while the same proportion had at least one preparation game. However, only 22% of players had at least one versus game and at least one preparation game for all of their potential new teams. Whenever there was no versus and/or preparation game, the associated mean performance and mean utilization variables were set to the player's mean in all games (implying the mean

[11] Recall, our main sample excludes players who do not change teams and players who leave their team, but remain unmatched thereafter. Since higher-performing players tend to change teams less often and lower-performing players are more likely to remain unmatched, players with moderate performance levels are overrepresented. With that said, unmatched players and players who do not change teams are considered in Section 6.1 and in Appendix D, respectively. In both cases, the estimates of interest are either statistically indistinct or greater than those reported in Table 3, which suggests that, if anything, the overrepresentation of moderate-performing players leads us to underestimate the magnitude of first-hand experience effects.

[12] In all of our analyses, standard errors are clustered around each subset of player observations.

performance deviation was set to zero), while the dummy variable(s) indicating whether or not at least one such game occurred allowed us to control for these undefined measures. Removing these observations from the sample did not meaningfully impact the magnitude or interpretation of our results (see Appendix Tables A2 and A3).

4.1 Main Results

The first two columns of Table 3 provide estimates (with and without control variables) of the overall effect of first-hand experience, as captured by the coefficient γ on the player's mean performance deviation in versus and preparation games (see equation 4), while the last two columns provide separate estimates for versus games and for preparation games. In all four specifications, the estimated coefficient(s) of interest are positive and statistically significant. This indicates that teams have a tendency to acquire players with better-than-usual performances in versus and preparation games, as we would expect if teams are 'overattentive' to first-hand experience (as modeled in Section 3).[13,14]

Table 3: Estimates of Conditional Logit Player-Team Matching Model

Mean performance deviation in versus and prep. games	0.033 (.004) [<.001]	0.028 (.005) [<.001]		
... in versus games only			0.023 (.004) [<.001]	0.017 (.004) [<.001]
... in preparation games only			0.012 (.003) [<.001]	0.013 (.004) [.001]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

Standard errors are in parentheses and p-values are in square brackets. See Appendix Table A4 for an expanded version of this table with estimates for all control variables (where applicable).

As detailed in Appendix F, if we use the total (instead of mean) performance deviation

[13] Furthermore, the effect does not appear to be confined to a small subset of teams in our sample. For instance, when separately estimated for each team, the coefficients on relative performance in versus and preparation games are positive for 28 out of the 30 teams (Appendix Table A6). While most of these team-level estimates are, on their own, not statistically significant — which is not surprising considering they are (on average) identified based on 1/30th of the observations in our sample — they are collectively distinct from zero ($p < .001$) and indistinct from each other ($p = .21$).

[14] As addressed in Appendix E, the effect also does not appear to be driven by teams responding to serious injuries to key player(s). This is noteworthy because injuries are largely unexpected, and may compel a team to acquire a player to help replace the injured player(s) with less planning and deliberation than usual.

across applicable games or the normalized mean performance deviation (so that its unit is the standard deviation of the player's mean performance deviations across teams) as our measure of relative performance, the estimated coefficient(s) of interest remain positive and statistically significant (see, in particular, Appendix Tables F1 and F2). The same is true when using a variety of alternatives to the efficiency metric as our underlying measure of performance (Appendix Tables F3 through F9).

To provide a sense of the overall impact of first-hand experience, suppose teams j and k initially have the same valuation of a given player and are thus equally likely to acquire that player. A one unit increase in the player's mean performance deviation in versus and preparation games with team j would then, all else equal, make team j 2.8% more likely than team k (in terms of the odds ratio) to acquire the player, based on the estimate reported in the second column of Table 3.[15] As seen in the fourth column of Table 3, this overall effect can be separated into a versus effect and a preparation effect of comparable strengths (while the magnitude of the versus effect is larger, the difference is not statistically significant).[16]

There were three control variables for which the estimates were highly significant in both controlled specifications: (i) the birth state dummy, which had the largest coefficient (.389 in the regression reported in the second column of Table 3, with $p < .001$); (ii) team winning percentage ($-.259$, $p < .016$); and (iii) mean performance of all players against the team (.106, $p < .001$). A more detailed look at how the sizes of these effects compare to the effect of first-hand experience is provided in Appendix G.

The results of two placebo tests are presented in Table 4. Besides re-estimating the impact of a player's performance in a team's versus and preparation games, here we also estimated the impact of performances in games that the team does not experience first-hand, yet are

[15] Note, this does not mean that team j's probability of acquiring the player increases by 2.8 percentage points. Instead, the ratio between the probability that team j acquires the player (p_j) and the probability that team k acquires the player (p_k) increases from 1 to 1.028. This can be seen from the conditional logit player-acquisition probabilities given in (6), as (suppressing the i and t subscripts) the marginal effect of an increase in a player's mean performance deviation on the odds ratio is given by $\partial(p_j/p_k)/\partial\overline{\mathrm{PD}}_j = \partial(e^{\gamma\overline{\mathrm{PD}}_j+m_j}/e^{\gamma\overline{\mathrm{PD}}_k+m_k})/\partial\overline{\mathrm{PD}}_j = \gamma\cdot e^{\gamma\overline{\mathrm{PD}}_j+m_j}/e^{\gamma\overline{\mathrm{PD}}_k+m_k} = \gamma\cdot p_j/p_k = \gamma \approx .028$, given $p_j = p_k$.

[16] Without controls, the difference — in which the versus game coefficient is roughly 90 percent larger than the preparation game coefficient — is statistically significant. The extent of this difference is on par with the difference as depicted in Figure 1, where the slope of the linear best fit is roughly 85 percent larger for versus games than for preparation games. We do note, however, that Figure 1 only includes observations for which there was at least one game of the corresponding type (unlike our present analysis).

temporally close to those that do. Specifically, our first placebo test includes an estimate for the game after the versus game, while the second test includes estimates for all games within a 2-game window of the versus game.[17,18] The estimated magnitude and statistical significance of the versus and preparation effects are nearly identical to those without placebo games, while the placebo game estimates are all statistically indistinct from zero.

Table 4: Placebo Test Results

Mean performance deviation in games that take place...		
...2 games before versus games		0.000 (.004) [.939]
...1 game before versus games (in prep. games)	0.013 (.004) [<.001]	0.013 (.004) [.001]
...in versus games	0.018 (.004) [<.001]	0.018 (.004) [<.001]
...1 game after versus games	-0.003 (.004) [.502]	-0.003 (.004) [.438]
...2 games after versus games		0.001 (.004) [.712]
Observations	126,404	126,404

Both regressions include control variables. Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

4.2 The Validity of the Preparation Effect

The placebo test results also help alleviate any potential concerns that the preparation effect — which will prove especially useful for addressing alternatives (discussed at length in Section 5) to our overattention hypothesis — may simply be a byproduct of the versus effect. Namely, if the preparation effect merely arises because preparation games are temporally adjacent

[17] For each type of placebo game, we also included controls for the numbers of actual and potential games played, a control for the player's mean utilization, and a dummy indicating whether any such games were played (analogous to the existing controls defined for versus and preparation games).

[18] While it may be natural to wonder if teams scout opponents two games prior to the versus game, in which case it would be dubious to classify such games as placebos, it is only standard practice for NBA teams to scout an opponent's last game before playing their own team. As discussed in footnote 2, for example, NBA arenas only reserve seats for scouts representing the participating teams' *next* opponents.

to versus games, we would naturally expect an analogous "next game effect." However, no such effect is observed.

The validity of the preparation effect is reinforced by the results of AR(1) regressions estimating serial correlations between a player's relative performance over consecutive games of interest. As shown in Table 5, the estimated autoregressive coefficient between a player's relative performance in his future team's preparation and versus games is positive and statistically significant (column 1). However, this estimate is on par with (and statistically indistinct from) the estimated autoregressive coefficients between each of these games and the adjacent placebo games (columns 2 and 3), as well as between any two consecutive games (column 4). This suggests that the positive relationship between a player's relative performance in preparation and versus games involving his future team is not responsible for the preparation effect.[19]

Table 5: AR(1) Model Estimates

Dependent variable: mean perf. deviation in...	games versus future team	1 game before vs. future team	1 game after vs. future team	any game
Mean perf. deviation 1 game before game in dependent variable	0.083	0.062	0.096	0.079
	(.014)	(.014)	(.014)	(.003)
	[<.001]	[<.001]	[<.001]	[<.001]
Observations	6,384	6,384	6,384	172,036

Standard errors are in parentheses and p-values are in square brackets.

Described at length in Appendix H, a final check on the validity of the preparation effect assesses whether a proxy for a team's *level* of preparation predicts the strength of the preparation effect. In this exercise, we used data indicating the 'odds on favorite' (i.e. the expected winner) for regular season games to identify cases of team overachievement and underachievement — that is, winning despite being expected to lose and losing despite being expected to win — in versus games as proxies for high and low levels of preparation (respectively). Here, the idea is that a team that is more attentive to the preceding prepa-

[19] As seen in Appendix Table A5, there also is no apparent relationship between a player's relative performance and relevant characteristics of the team that is preparing for the player's team (i.e. the player's next opponent), such as the team's winning percentage, the new-player acquisition rate, and being located in the player's birth state. Thus, it does not appear that players are simply more prone to better performances in games that precede games against teams with a greater predisposition to acquire them, nor do we see any indication that nonrandom scheduling plays a role.

ration game would be more prepared and hence more likely to overachieve (and less likely to underachieve) in the versus game. Presuming a player's performance in a preparation game is more salient when the (preparing) team is more attentive to that game, our notion of team overattention naturally suggests that the preparation effect (but not the versus effect) would be stronger when the team overachieves in the versus game and weaker when the team underachieves, which is exactly what we find.

4.3 A Systematic Bias?

According to the overattention hypothesis, the observed first-hand experience effects reflect a systematic and suboptimal bias in teams' player-acquisition decisions. To further test this idea, it helps to consider whether a team's prior first-hand experiences with a newly-acquired player relate to the realized value of that player to the team. If teams are *not* biased, we might expect a player's performance in games providing first-hand experience to his future team to be positively related to measures that reflect the player's value to the team — such as the player's overall performance on his new team, the team's winning percentage after acquiring the player, and the team's utilization of the player, and the length of the player's tenure (number of games played) as a member of that team. While all of these measures are positively and statistically related to the second-hand performance signal capturing the player's overall performance prior to the transition, they are not statistically related to the player's relative performance in games providing first-hand experience to his future new team (see Appendix Tables A7, A8, A9, and A10).[20] This suggests that first-hand experiences are not genuinely linked to the realized value of the player to the team.

The notion of a *systematic* bias in team-decision-making is also supported by additional results indicating that the observed effect of first-hand experience is not limited to specific situations within our domain. In particular, we find that first-hand experience has a positive and statistically significant effect on teams' player-acquisition decisions regardless of whether

[20] While a player's relative performance in games providing first-hand experience to his future new team is not statistically related to the number of games played by that player during his full tenure as a member of that team, it *is* statistically (and positively) related to the number of games played during his first year on the team (Appendix Table A10). This finding suggests that past first-hand experiences may continue to influence teams' valuations of newly-acquired players in the short-run, while teams may eventually learn to objectively value such players in the long-run. We will revisit this idea in Section 7.1.

a player is acquired through a trade or signed to a new contract (see Appendix Table A11); whether the acquisition happens in the regular season or in the offseason (Appendix Table A12); whether the player's overall performance level is relatively high or relatively low (Appendix Table A13); whether the team's first-hand experience is attained at 'home' or on the 'road' (Appendix Table A14); and whether the transaction occurs in the earlier years or in the later years covered by our sample (Appendix Table A15).[21]

5 Alternate Hypotheses

We now address alternate explanations for the observed first-hand experience effects.

5.1 Player Agency

Players may have some degree of control in determining their new team along with a tendency to perform better when being observed by a team they would like to join. Addressing the second part, a player may naturally be excited to perform against a preferred future team due to the presence of that team's current players (desired future co-workers) and fans. In addition, if players prefer to join teams in or near cities where friends and family reside, players' friends and family may also attend such games. This idea is partially addressed by the dummy indicating whether the team is located in the player's birth state. Its effect is positive and statistically significant, suggesting players are more likely (perhaps due to their agency in determining their next team) to be acquired by teams near their place of birth. Still, players' preferences may vary due to other idiosyncratic reasons not captured here.

While there may be doubts as to whether it would explain the preparation effect, there is arguably a more compelling reason to reject the player agency hypothesis.[22] Namely, players

[21] Of note, the test in Appendix Table A15 does not provide evidence of a meaningful time trend, as the estimated effect of first-hand experience in the first half of our same is the same as the estimated effect in the second half of our sample (based on a rough median split). Similarly, we find no evidence of a linear time trend (Appendix Table A16). With that said, in Appendix B we consider various regime tests to explore the possibility that the effect may have varied with changes in the rules that govern teams' player-acquisition decisions or as a result of the so-called "analytics movement." The results of these tests raise the possibility that the effect may have diminished in the final years of our sample, but are also compatible with the possibility that the effect did not change. See Appendix B for details.

[22] In preparation games, the preferred team would generally only be represented at the game by its (relatively inconspicuous) scout sitting in the stands. Certainly, however, a player's preference to join a particular team could naturally translate to better performance in preparation games if the player is strategic. We will consider this possibility in Section 5.2.

who are traded generally have little or no say over their new team — with the exception of some high-profile "stars."[23] Thus, according to the player agency hypothesis, we would expect the effect (if any) of first-hand experience among traded players to be negligible compared to the effect among players who sign with a new team. However, the estimated effect among traded players is still positive, statistically significant, and indistinct from the effect for players who sign with a new team. Furthermore, the effect among traded players who are not "stars" — i.e. those who do not earn "all-star" status at any point in their career, and for whom agency in trading decisions is particularly unlikely — is nearly identical to the effect among "stars" who are traded (see Appendix Tables A11 and A18).

5.2 Player Auditions

Players may treat games observed by a desired future team as "auditions." As in the player agency hypothesis, the player's preferences may create a correlation between his performances and his future match, except in this story the player strategically allocates effort to impress the team, thus increasing the likelihood that the team decides to acquire him.

If a player auditioned for his future team by exerting extra effort in preparation and versus games, we would naturally expect higher performances in consecutive preparation and versus games followed by lower performance in the next game. As a result, a player's performance in a versus game against his future team would tend to be closer to his performance in the preceding preparation game than in the next game. However, a player's performance against his future team is, on average, slightly closer to his next game performance than to his preparation game performance (mean absolute differences of 5.41 and 5.40, respectively).

The lack of a special relationship between a player's performances in consecutive preparation and versus games with his future team is reinforced by the AR(1) estimates in Table 5. Unlike what we would expect if players treated these games as auditions, the estimated autoregressive coefficient between a player's performance deviations in consecutive preparation and versus games with his future team is statistically indistinct from the coefficients between each of these games and the adjacent placebo games as well as the coefficient between any

[23] As discussed at length in Appendix I, players who are not considered "stars" often emphasize their lack of control over when, whether, and where they will be traded.

two consecutive games in the year preceding the transition.[24]

5.3 Team Showcasing

A team's interest in a player may be known to the player's current team. In response, the player's current team may use the interested team's versus and preparation games as opportunities to "showcase" the player by giving him more or better opportunities to perform in such games, with the intent of increasing the interested team's valuation and thus strengthening the current team's bargaining power in a potential trade. A team's pre-existing interest in a player — which would naturally correlate with a higher likelihood of eventually acquiring that player — may therefore also correlate with that player's performance in versus and preparation games as a result of efforts by the player's current team to increase the value they could attain in return for that player if traded to the interested team.

To address this "team showcasing" hypothesis, it may be useful to consider *how* a team could showcase a player. One likely possibility is through increased utilization (playing time), though this channel is controlled for by our controls on a player's mean utilization in versus and preparation games.[25] A team could also showcase a player by running more offensive plays for that player (thus providing more opportunities to score) without increased utilization. That said, if we exclude scoring-based statistics from our performance measure, we still see a positive and significant effect of first-hand experience.[26]

[24] The audition hypothesis also cannot simultaneously explain (a) the lack of a long-term impact of first-hand experience (i.e. for time-horizons longer than one year), as demonstrated in Appendix J, and (b) the observation that the estimated first-hand experience effect is as large for players who are traded as it is for players who sign with a new team (Appendix Table A11). That is, since NBA player contracts usually span multiple years, players who sign with a new team upon the expiration of a previous contract will have anticipated the possibility of switching teams at that time. However, traded players often have one or more years remaining on their existing contract, making them less likely to have anticipated switching teams. Thus, the observed first-hand experience effect among traded players would, according to the audition hypothesis, indicate that players are auditioning well before their current contracts end. Assuming players' preferences for teams are serially correlated, higher relative performances from 'auditions' occurring one to two years before switching teams would thus also predict a higher likelihood of being acquired by that team. However, this prediction is not supported by the data.

[25] The estimated coefficient on versus game utilization is positive and borderline significant (with $p = .084$ in the regression that separately estimates the versus and preparation effects and $p = .027$ in the regression that estimates the overall effect), offering some support for the idea that showcasing may contribute to the versus effect as estimated without controls. The estimated coefficient on preparation game utilization is negative and statistically insignificant (in both controlled regressions), which suggests that teams do not showcase players through increased playing time during an interested team's preparation games.

[26] Here, "scoring-based statistics" refer to points, attempted and made field goals, and attempted and made free throws. If we also exclude assists (passes that lead to a made field goal by a teammate) from our

While the team showcasing hypothesis suggests that a first-hand experience effect may, in contrast to the player audition hypothesis, be driven by strategic actions of the player's current team as opposed to the player himself, it too would naturally suggest that a player's (on average, elevated) performance in a versus game against his future new team would tend to be closer to his (also elevated) performance in the preceding preparation game than in the game after the versus game when showcasing has ended. As noted earlier, however, a player's performance against his future team is, on average, slightly closer to his next game performance than to his previous game performance.[27]

The team showcasing hypothesis is also challenged by our finding that the effect of first-hand experience is as large for players who sign a contract with their new team as it is for players who are traded (Appendix Table A11). After all, a team gains nothing by showcasing a player to a team that eventually signs the player. With that said, a team may showcase a player to improve their bargaining position in a potential trade, and if a trade does not materialize, the prospective trade partner may still have an elevated interest in (and hence, likelihood of signing) the player during free agency. However, a team would presumably have little incentive to showcase a player *after* the annual "trade deadline" when trades are no longer possible. Nonetheless, whether estimated for signed players or for all players in our sample, the estimated effect of first-hand experience in games played after the trade deadline remains positive and statistically significant, and is statistically indistinct from the effect in games played before the trade deadline (see Appendix Table A17).

5.4 Fan Overattention

Perhaps it is not the team, but its fans who over-weight first-hand experiences with other teams' players. Teams may then be inclined to acquire players who perform relatively well in games that are experienced first-hand because these players are over-valued by their fans,

performance measure, the estimates of interest are still positive and statistically significant. See Appendix Tables F5 and F6.

[27] The possibility of a special relationship between a player's performances in consecutive versus and preparation games with his future team is also undermined by our previously-discussed findings that the estimated autoregressive coefficient between a player's performance deviation in consecutive preparation and versus games with his future new team is statistically indistinct from the coefficients between each of these games and the adjacent placebo games, and also between any two games in the pre-transition year (see Table 5).

which could then lead to higher profits from ticket and merchandise sales. However, fans do not typically watch their team's preparation games, which rarely involve their own team.[28] Thus, "fan overattention" does not provide an explanation for the preparation effect.

In addition, the fan overattention hypothesis suggests that a team's fans would (all else equal) have a greater interest in watching a newly-acquired player with better relative performance in past first-hand experiences with the team. As a result, the team's utilization of the player would naturally be higher — especially in home games — than it otherwise would; fan attendance at the team's home games may also be higher. However, higher relative performance in games providing first-hand experience to a player's future new team does not predict higher utilization on his new team, does not predict disproportionate utilization in home games for his new team, and does not predict higher home game attendance for his new team (see Appendix Tables A9, A19, and A20).

5.5 Private Learning

Perhaps second-hand performance statistics do not tell the full story, while first-hand experiences allow a team to learn about a player's unreported attributes, such as his ability to defend opposing players. A first-hand experience effect could then arise if a player's relative performance in versus and preparation games was positively correlated with the amount of private information conveyed in such games.[29] Such a correlation could naturally arise through the player's utilization, as higher utilization would allow a player to accumulate higher performance statistics while also giving teams more time to learn about unreported attributes. Our controls for the player's mean utilization in versus and preparation games help account for these potential effects. In addition, our controls for the numbers of versus and preparation games help account for the number of opportunities teams have to learn about players' unreported attributes.

Our results suggest that any effect of private learning stemming from a player's utilization

[28] A team only participates in its preparation game if they play the same opponent in two consecutive games. In our sample, teams play in less than 2 percent of their preparation games.

[29] Under standard assumptions, extra learning of this sort would increase the likelihood that a team's valuation of the player is extreme (in either direction) in comparison to other teams' valuations, thus increasing the likelihood of acquiring the player.

or from the number of versus and preparation games is negligible, as we fail to reject the hypothesis that these four coefficients are jointly equal to zero (p-value of .22 for the model in Table 3, column 2). Even if we accept the coefficients at face value, the estimates imply that the average effect of an extra game providing first-hand experience is just one-seventh the size of the effect of a one-unit increase in the player's mean performance deviation, thus undermining the notion that teams' evaluations are meaningfully dependent on extra information conveyed in versus and preparation games.[30]

5.6 Bad Teams

Worse teams tend to allow better performances by opposing players. They also acquire new players at a higher rate. These two factors could generate a positive correlation between a player's relative performance against a team and the team's likelihood of acquiring the player. However, our controls for the mean performance of all players against the team and for the team's winning percentage help account for such possibilities. Furthermore, this "bad teams" hypothesis cannot explain the preparation effect since a player's relative performance in a game would not be correlated with the quality of the player's next opponent.[31]

5.7 Void-Filling

A similar hypothesis is that teams that lack players with a particular skill would tend to allow better performances by players who possess that skill while also acquiring such players at a higher rate. For example, a slow team may allow fast players to perform especially well, while also having a higher demand for fast players. However, this void-filling hypothesis

[30] Even if the amount of private information conveyed through first-hand experience was not fully captured by utilization and the number of versus and preparation games, it would correlate strongly with these measures. Their lack of an apparent impact seems to rule out any secondary channel linking a player's relative performance and a team's private information that could explain the observed first-hand experience effects. The lack of a long-term effect of first-hand experience (see Appendix J) provides another reason to reject the private learning hypothesis, as it is implicitly predicated on the notion that a player's unreported attributes are serially correlated from year to year — otherwise, a team's private information concerning such attributes, as attained through first-hand experience in the preceding year, would be largely outdated and thus not indicative of the player's potential future value to the team. Thus, the private learning hypothesis would (incorrectly) imply substantial long-run persistence in the effect of first-hand experience.

[31] See Appendix Table A5, which shows that relevant characteristics (e.g. the team's winning percentage and new-player acquisition rate) of the player's current opponent — but not the next opponent — are related to the player's relative performance level.

also cannot explain the preparation effect because a player's relative performance does not plausibly depend on whether the next opponent lacks players of a similar skillset.

6 Quantifying the Magnitude of the Bias

This section considers two exercises that will help us get a better sense of the magnitude of the apparent first-hand experience bias. First, in Section 6.1, we estimate the overattention parameter ω and use it to quantify the extent to which teams are disproportionately influenced by first-hand experience. In Section 6.2, we estimate the proportion of players who are matched to the "wrong" team as a result of the bias. Also see Appendix G, where we estimate the dollar-value welfare loss associated with the bias, quantify the magnitude of the bias in terms of team wins, and compare the size of the first-hand experience effect to the sizes of the effects of other factors that affect team decision-making.

6.1 Measuring Overattention to First-Hand Experience

The overattention parameter, ω, introduced in equation (1), captures the degree to which teams' player-acquisition decisions are swayed by first-hand experience relative to second-hand information. As previously noted, we can express ω in terms of γ and β, which capture the respective effects of first-hand experience and of second-hand information, as $\omega = \frac{\gamma}{\beta-\gamma}$. However, while γ was estimated in Section 4.1, we were not able to identify β because all of a player's potential new teams observe the same second-hand performance signal describing his overall performance in all games.

Therefore, to estimate ω, we first need to adapt our empirical framework in a manner that allows us to estimate β. To do this, we add an outside option representing the possibility that a player who leaves his original team might not be matched to a new team, while expanding our sample to include roughly 2,500 players who we observe leaving a team but remaining unmatched thereafter. The value of the outside option is normalized to zero, which captures the idea that a player will only be matched to a new team if at least one team has a positive valuation of the player.[32] With the no-match option, we estimate $\beta = .39$ and $\gamma = .02$, with

[32] A positive valuation may more accurately be understood as a positive net valuation, as the gross value of the player to the team would need to exceed his salary as well as the potential lost option value from filling

$p < .001$ for both effects.[33] As expected, our estimate of β is large, which suggests that lower-performing players are more likely to remain unmatched.

The estimates of β and γ imply $\omega = \frac{\gamma}{\beta-\gamma} = .054$ (with $p < .001$ in a nonlinear Wald test if $\omega = 0$). This indicates that teams weight players' mean performance in games providing first-hand experience roughly 5.4 percent as heavily as they weight the second-hand signal conveying players' overall performance in all games. Recall, any $\omega > 0$ represents overattention in that players' performances in games providing first-hand experience are already embedded in the second-hand performance signal.

Next, we use ω to calculate the weighting of a player's performance in a game that is experienced first-hand relative to a game that is not.[34] Letting $v^+(g)$ denote the increase in a team's valuation of a player if the player's performance in game $g \in \mathcal{G}$ was one unit higher, we want to compute $\lambda \equiv \frac{v^+(g|g \notin \mathcal{G}^{\text{FH}})}{v^+(g|g \in \mathcal{G}^{\text{FH}})}$. Here, λ captures the effect of higher performance in a game that does *not* provide first-hand experience, expressed as a fraction of the effect in a game that does. Using equation (4), we can re-express λ as:

$$\lambda = \frac{|\mathcal{G}^{\text{FH}}|/|\mathcal{G}|}{\omega + |\mathcal{G}^{\text{FH}}|/|\mathcal{G}|}, \tag{7}$$

where $|\mathcal{G}^{\text{FH}}|/|\mathcal{G}|$ is the fraction of the player's games that the team experiences first-hand.

By calculating the distribution of λ across all applicable player-team observations, we find that the mean and the median are both $\lambda = .55$, with a standard deviation of .17.[35] This indicates that a player's performance in a game that is experienced first-hand is typically

the vacancy on the team's roster. Though we abstract from such complications, neither of these costs would be trivial in light of the NBA's rules mandating a minimum player salary and a maximum team roster size.

[33] Our estimate of γ is smaller but statistically indistinct from our estimate in Table 3. For a more detailed discussion of these results and the exact model used in the estimation, see Appendix K. There we also address issues of interpretation and limitations of our approach.

[34] The overattention parameter ω does not provide a direct measure of how a team's weighting of a player's performance in a single game depends on whether it was experienced first-hand. There are two reasons for this. First, part of the impact of a player's performance in a game the team experiences first-hand still arises through the second-hand signal conveying a player's overall performance in all games, yet this is not captured by ω. Second, ω represents the weight on the player's mean performance in games providing first-hand experience (i.e. $\overline{\text{P}}^{\text{FH}}$), relative to the weight on the player's mean performance in all games ($\overline{\text{P}}$). However, teams generally only attain first-hand experience in a small fraction of the player's games, which suggests the degree of overattention on a per-game basis would be substantially higher than ω.

[35] Since $|\mathcal{G}^{\text{FH}}|/|\mathcal{G}|$ varies across player-team observations in our sample, the value of λ implied by (7) would likewise vary across observations, thus precluding a single point estimate of λ. In addition, λ is not defined for the player-team observations in our sample for which there are no versus or preparation games.

weighted about $1/\lambda \approx 1.8$ as heavily as it would have been weighted if the game was not experienced first-hand.[36] It is also worth noting that $1 - \lambda$ offers a natural analog to the 'inattention parameter' estimated in other contexts (here, reflecting the degree to which attention to second-hand information is diminished in relation to first-hand experience).[37] Following this interpretation, our estimates would imply a mean inattention parameter of $1 - \lambda = .45$, which is within the range of estimates from other empirical settings.[38]

6.2 Player-Team Mismatch

To get a sense of the proportion of player-team matches affected by the bias, Table 6 presents the simulated outcomes of three different models: (A) our estimated model from Section 6.1; (B) an 'optimal' model that lacks a first-hand experience bias (i.e. with γ set to zero), but is otherwise the same as model (A); and (C) a pure random matching model, in which each of a player's possible outcomes are equally likely. In each simulation, all models used the same draws of the unobserved ϵ_{ijt} error terms. Thus, whenever a player's simulated match differs between models (A) and (B), the discrepancy can be attributed to the first-hand experience bias. As seen, our simulations indicate that the bias causes roughly 2.3 percent of players in our sample to be mismatched in this sense.

7 Additional Discussion

In this paper, we provided evidence from the NBA labor market that employers' hiring decisions are overly influenced by first-hand experiences with job candidates. In particular,

[36] By this measure, the extra bias from first-hand experience is roughly 80% as strong as the influence of objective performance information in teams' player-acquisition decisions. Though not directly comparable, this is on par with the relative magnitude of NBA teams' previously-reported bias in favor of early draft picks, as Staw and Hoang (1995) estimate that the effect of being selected in the first round of the NBA draft instead of the second round on a player's career length is roughly 70% (3.3 years versus 4.6 years) as large as the effect of a one-standard-deviation increase in a "scoring index."

[37] In the simplest formulation, if the true value of an object is $a + b$, b (but not a) is an attribute drawing less than full attention, and θ is the inattention parameter, the object's perceived value is then $a + (1 - \theta)b$ (see DellaVigna, 2009, for relevant background). With that said, since teams appear to place a nonzero weight on redundant performance information conveyed through first-hand experience, the discrepancy in the effective attention paid to games providing first-hand experience as compared to other games naturally reflects overattention to first-hand experiences as opposed to inattention to other information.

[38] Other estimates of the inattention parameter include .18 to .45 in Hossain and Morgan's (2006) study of shrouded shipping costs on eBay, .46 to .59 in DellaVigna and Pollet's (2007) study on investors' attention to earnings announcements occurring on Fridays, .75 in Chetty et al.'s (2009) study on consumers' attention to sales taxes, and .31 in Lacetera et al.'s (2012) analysis of a left-digit bias in evaluating used car mileage.

Table 6: Summary of Simulations

		% of players with different matches		
		(A)	(B)	(C)
Estimated model with first-hand experience bias	(A)	0	2.3	35.2
'Optimal' model without first-hand experience bias	(B)		0	34.4
Pure random matching model	(C)			0

Simulations from the estimated model (A) use the coefficients from the conditional logit estimation described in Section 6.1 (see Appendix Table K1). Simulations from the 'optimal' model (B) use the coefficients from (A), except the coefficient on a player's performance deviation in versus and preparation games is set to zero. The random matching model (C) simply uses $v_{ijt} = \epsilon_{ijt}$. Each simulation uses the same type-I extreme value draw of ϵ_{ijt} for models (A), (B), and (C). Results are based on the average of 10,000 simulations.

we found that teams are biased in favor of acquiring players with better-than-usual performances in games against their own team (the versus effect) and in games immediately preceding such games (the preparation effect). In closing, we discuss whether teams may learn from their mistakes due to overattention (Section 7.1), whether the observed first-hand experience bias might generalize to hiring decisions in other industries (Section 7.2), and the potential implications of our findings for understanding first-hand experience effects in other decision-making contexts (Section 7.3).

7.1 Do Teams Learn?

Several of the empirical tests considered in this paper offer clues as to whether or not teams may learn from their mistakes due to overattention. As a whole, the evidence is mixed, and the answer may depend on what exactly is meant by learning. Accordingly, we will consider three separate aspects of learning and address the implications of our analysis for each of these aspects.

First, we consider whether a team that acquires a player based on overattention eventually learns that they overvalued that particular player. If teams do *not* learn in this sense, then a team's valuation of a newly-acquired player would continue to be positively associated with the player's relative performance in past games that the team experienced first-hand. However, a team's past first-hand experience with a newly-acquired player does not predict the team's utilization of that player (in terms of playing time) nor does it predict the length of the player's tenure (number of games played) as a member of that team (see Appendix Tables A9 and A10). Thus, to the extent that utilization and tenure length reflect a team's valuation of a player *after* acquisition, it appears that teams do eventually learn that players acquired due to overattention are less valuable than they previously believed. That said, if the first-hand experience bias is *fully* overcome in teams' valuations of acquired players, it may be reasonable to expect a *negative* relationship between past first-hand experience and tenure length. In that case, the lack of an observed relationship may reflect partial learning in the sense that a team's biased impressions are not completely overcome.[39] Furthermore, our finding that past first-hand experience is statistically and positively associated with the number of games played by that player in his *first* year on that team (again see Appendix Table A10) suggests that learning of this sort may be a gradual process.

As a second aspect of learning to consider, we might wonder whether a team that acquires a player based on overattention learns to avoid making the same mistake in future player-acquisition decisions involving *other* players. Presumably, if teams did learn to avoid repeating such mistakes, the effect of first-hand experience would decline over time. However, the lack of an apparent time trend (see Appendix Tables A15 and A16) casts doubt on this possibility. Thus, to the extent that teams learn that they overvalued players acquired due to overattention (as suggested by our utilization and tenure results), their apparent propensity to repeat the mistake with other players suggests that teams may not realize *why* they overvalued these players in the first place.

Lastly, we might wonder whether teams may ever learn to avoid the first-hand experience bias in player-acquisition decisions. While we cannot draw strong conclusions from our analysis (and despite the lack of an apparent time trend in the effect of first-hand experience over our full sample), our regime test results in Appendix B raise the possibility that teams

[39] If we interpret the utilization result as evidence of complete learning and the tenure result as evidence of partial learning, the discrepancy could reflect heterogeneity in the extent of learning among team personnel. In particular, a player's utilization is generally at the discretion of the coach, and thus would primarily depend on the coach's valuation of the player, while tenure may, to a greater degree, reflect the valuations of other team personnel involved in the acquisition decision. In light of this, our findings could be interpreted as evidence of greater learning among coaches compared to other team personnel.

became less susceptible to the bias in the final years of our sample. If so, such learning may have been spurred by the "analytics movement" and its far-reaching influence on NBA team decision-making in recent years (as opposed to teams learning as a direct result of their own mistakes). While the analytics movement did not suddenly give teams a newfound ability to recognize and overcome a first-hand experience bias in player-acquisition decisions (as the bias was always evident from traditional box score data), it may have allowed a widespread philosophical shift towards more objective and data-driven decision-making among NBA teams, leading to a reduction in the influence of first-hand experience. Note, this idea is still largely untested and future work would be needed to assess with confidence whether or not the effect of first-hand experience truly diminished in the final years of our sample, as our regime test results are also compatible with the possibility that the effect did not change.[40]

7.2 Generalizability to Hiring Decisions in Other Industries

Next, we consider whether a first-hand experience bias might generalize to hiring decisions in other industries. While this remains an open empirical question, some superlative characteristics of our setting may actually strengthen our confidence that the bias may apply elsewhere. With easy access to comprehensive performance data (and video footage of every game), the NBA provides unusually favorable conditions for employers to recognize and overcome a potential bias. Furthermore, NBA team owners tend to be successful executives in other industries, suggesting relatively high managerial competence and thus relatively low susceptibility to a potential hiring bias.[41]

With that said, most other jobs do not entail 'contests' involving direct competition with workers from other firms. Since the versus effect applies when teams are actively participating in such contests, its generalizability may, on its own, seem limited. However, the preparation effect suggests that the bias can also sway an impartial observer of a worker's performance. Even so, both effects apply to a specific form of first-hand experience — on-the-job observation of a worker's performance — attained in a contest-like setting.

[40] For additional details and discussion, see Appendix B.

[41] As of 2015, the average estimated net worth among NBA team owners was \$3.3 billion (see http://www.businessinsider.com/sports-owners-net-worth-tenure-2015-10). With that said, owners do not necessarily have complete or direct influence in player-acquisition decisions (see Appendix C for details).

As an example of another contest-like setting where similar effects may arise, suppose, after observing an opposing attorney at trial, a legal client (whether an individual or a business) is considering hiring that attorney for another case. If the client's decision is excessively influenced by their prior observation of the attorney, this would be analogous to the versus effect in NBA teams' decision-making since, in both cases, first-hand experience is attained from observing the worker perform on behalf of the employer's opponent in the contest (whether a trial or a basketball game). A more natural analog to the preparation effect could then arise if, instead of being directly involved as a plaintiff or a defendant, the client previously observed the attorney while serving as a juror or as court stenographer.

Certainly, it may not be essential for a worker's performance to be observed during a *contest* for a first-hand experience bias to apply. For instance, consider a decision to re-hire an independent contractor — such as an interior decorator, accountant, electrician, math tutor, or personal bodyguard — who the employer previously hired for another job. The key question is then whether the employer's decision is excessively influenced by their first-hand observation of the contractor's past job performance. Of note, the contractor's previous job performance would have still directly affected the employer at that time, which is also true of the versus effect, though the preparation effect suggests that such 'stakes' are not necessary for a first-hand experience bias to exist.

To consider this idea in another non-contest setting, except without stakes in this sense, suppose an academic department is considering hiring a researcher from another institution. Incidentally, the chair of the hiring committee previously attended a conference seminar delivered by the researcher (who was not seeking a new job at the time). While the researcher's previous seminar performance may not have directly affected the chair's academic department at the time, it is conceivable that the performance could still be overweighted in the eventual hiring decision.

In all of the examples considered thus far, the employer's first-hand experience is attained from observing the worker work on behalf of their original employer. With that said, if the chair of the academic department overweights the researcher's conference seminar performance, then it is not much of a leap to think that such overweighting might also occur if the chair's first-hand experience instead came from a recruiting seminar during an official campus visit (even though the researcher would no longer be observed working on behalf of their current employer). If that were the case, it would indicate that the bias may extend to first-hand experiences besides on-the-job observation of a worker's performance — and could (returning to our example) perhaps even apply to the department chair's other first-hand experiences, such as a one-on-one meeting with the researcher during the campus visit or an initial screening interview with the department's full hiring committee.

Indeed, this notion that the bias could apply to other first-hand experiences (besides on-the-job observation of a worker's performance) fits with previously-discussed findings that employers tend to be too reliant on personal interviews (Highhouse, 2008; Dana et al., 2013). As noted earlier, however, this tendency could also reflect the personal motives of hiring managers. For instance, past research indicates that interview evaluations are often swayed by a candidate's physical attractiveness (Ruffle and Shtudiner, 2015), relationship status (Rivera, 2017), and whether they enjoy similar leisure activities (Rivera, 2012), which suggests that hiring managers may sometimes inflate their interview evaluations for candidates with whom they would personally like to affiliate.[42] In this light, the present work indicates that hiring decisions can be excessively influenced by first-hand experiences besides personal interviews, and without such discrepancies in the motivations of the hiring manager and the firm. In doing so, our findings lend support to the idea that a more pervasive and systematic decision-making bias may underlie (at least in part) previous findings regarding the overuse of personal interviews in hiring decisions.

A recent study by Leung (2017) finds that employers of online freelancers often over-extrapolate from past experiences (especially bad experiences) with employees of a given nationality when evaluating a current job candidate of the same nationality. Such findings raise the possibility that employers' hiring decisions may even be susceptible to a first-hand experience bias based on experiences with other individuals (besides the job candidate) with

[42] As discussed in the introduction, a related line of research suggests that hiring decisions also tend to under-rely on algorithmic hiring aids (Kuncel et al., 2013; Hoffman et al., 2018). As with the apparent over-reliance on personal interviews, the under-reliance on algorithmic hiring aids may also be explained by hiring managers' personal motivations, as a hiring manager could perceive an algorithmic hiring aid as a threat to their autonomy, and thus resist its input.

shared demographic traits. In this way, the bias may even contribute to ethnic and racial discrimination in hiring decisions (Bertrand and Mullainathan, 2004; Carlsson and Rooth, 2007), though such a link is only speculative at this point.

7.3 Relevance to Other First-Hand Experience Effects

While we have focused on employers' hiring decisions, the present work may also relate to evidence of first-hand experience effects in other decision-making contexts. For instance, investors tend to over-invest in assets (such as IPO subscriptions, 401k accounts, and stocks) that have previously brought high returns (Kaustia and Knupfer, 2008; Choi et al., 2009; Chiang et al., 2011; and Strahilevitz et al., 2011). First-hand experience may also distort consumers' expectations of future macroeconomic conditions, as those who have lived through better market conditions tend to be more bullish on future conditions, and those who have lived through higher-inflation periods tend to expect higher future inflation (Malmendier and Nagel, 2011, 2016).[43]

We note, however, that our findings are not explained by leading theories used to explain first-hand experience effects in these other domains. The consensus explanation for investors' over-reliance on past experience is reinforcement learning, which holds that good payoffs from a past action can bias future choices towards repeating that action.[44] In our setting, the relevant action (acquiring a particular player) is not generally repeated and payoffs from past actions are not the relevant source of first-hand experience. The broader premise that these effects are payoff-driven is also challenged by the preparation effect, as it suggests such effects can arise even when experiences are merely observational. In turn, Malmendier and Nagel's (2011, 2016) age-based learning hypothesis attributes differential weighting of past macroeconomic conditions to differences in age (and lifespan), and thus does not address a

[43] As other examples, Haselhuhn et al. (2012) find that a (non-informative) late-return fee on a video rental increases future compliance; Giuliano and Spilimbergo (2014) find that living through unfavorable macroeconomic conditions can affect beliefs regarding the degree to which luck determines success. Similarly, laboratory studies show that players in repeated games can be excessively swayed by prior experience (e.g. Erev and Roth, 1998; Camerer and Ho, 1999; Simonsohn et al., 2007), while psychology research shows that individuals, in choice situations ranging from gambling to medical diagnoses, tend to over-rely on personal experience in relation to relevant summary statistics (e.g. Weber et al., 1993; Hertwig et al., 2004).

[44] For theoretical background and laboratory evidence in multi-player games (in the non-sports sense), see Roth and Erev (1995), Erev and Roth (1998), as well as the generalizations by Camerer and Ho (1999) and Ho et al. (2007) permitting reinforcement of unchosen actions based on their hypothetical payoffs.

team's differential weighting of games occurring in the same timeframe.[45]

As discussed, our findings are instead compatible with a notion of overattention to first-hand experience, as modeled in Section 3. That is, while players' performances in all games are presumably conveyed (and objectively weighted) through second-hand summary statistics, teams can allocate extra attention to games they experience first-hand — while evidently assigning meaning to the redundant performance signals provided in such games. To the extent that an investor's returns from a past investment or the macroeconomic conditions during a consumer's lifetime are attentionally salient (despite the availability of more complete second-hand information sources), overattention may help explain first-hand experience effects observed in these domains too, though future work is needed to test such possibilities.

[45] Unlike reinforcement learning, age-based learning is compatible with a merely observational first-hand experience bias, though the necessity of (potential) payoffs in driving such effects had remained an open question. As Malmendier and Nagel (2016) write, "it would be useful to further analyze the exact transmission channel of experience effects ... how do they depend on the prices of items personally consumed versus the CPI?" That said, the necessity of *realized* payoffs is inconsistent with Koudijs and Voth's (2016) finding of a first-hand experience effect among lenders exposed to a potential financial loss that was ultimately avoided.

References

[1] Agness, Scott, "Advance Scout Jimmy Powell," podcast interview (2016) http://vigilantsports.com/ep-34-advance-scout-jimmy-powell/ (accessed 4/18/2018).

[2] Barnes, Christopher, Reb, Jochen, and Ang, Dionysius, "More Than Just the Mean: Moving To a Dynamic View of Performance-Based Compensation," *Journal of Applied Psychology*, 97 (2012), 711–718.

[3] Barnes, Christopher, and Morgeson, Frederick, "Typical Performance, Maximal Performance, and Performance Variability: Expanding Our Understanding of How Organizations Value Performance," *Human Performance*, 20 (2007), 259–274.

[4] Bertrand, Marianne, and Mullainathan, Sendhil, "Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination," *American Economic Review*, 94 (2004), 991–1013.

[5] Buckley, Ronald, Norris, Amy, and Wiese, Danielle, "A Brief History of The Selection Interview: May The Next 100 Years Be More Fruitful," *Journal of Management History*, 6 (2000), 113–126.

[6] Camerer, Colin, and Ho, Teck-Hua, "Experience-Weighted Attraction Learning in Normal-Form Games," *Econometrica*, 67 (1999), 827–874.

[7] Carlsson, Magnus, and Rooth, Dan-Olof, "Evidence of Ethnic Discrimination in the Swedish Labor Market Using Experimental Data," *Labour Economics*, 14 (2007), 716–729.

[8] Chetty, Raj, Looney, Adam, and Kroft, Kory, "Salience and Taxation: Theory and Evidence," *American Economic Review*, 99 (2009), 1145–1177.

[9] Chiang, Yao-Min, Hirshleifer, David, Qian, Yiming, and Sherman, Ann, "Do Investors Learn From Experience? Evidence From Frequent IPO Investors," *Review of Financial Studies*, 24 (2011), 1560–1589.

[10] Choi, James, Laibson, David, Madrian, Brigitte, and Metrick, Andrew, "Reinforcement Learning and Savings Behavior," *Journal of Finance*, 64 (2009), 2515–2534.

[11] Coon, Larry, "NBA Salary Cap FAQ" (2017), online article, http://www.cbafaq.com/salarycap.htm (accessed January 2, 2020).

[12] Dana, Jason, Dawes, Robyn, and Peterson, Nathanial, "Belief in the Unstructured Interview: The Persistence of an Illusion," *Judgment and Decision Making*, 8 (2013), 512.

[13] DellaVigna, Stefano "Psychology and Economics: Evidence from the Field," *Journal of Economic Literature*, 47 (2009), 315–372.

[14] DellaVigna, Stefano, and Pollet, Joshua, "Investor inattention and Friday earnings announcements," *Journal of Finance*, 64 (2009), 709–749.

[15] Dunleavy, Mike, and Eyen, Jim, "Game Preparation" in *NBA Coaches Playbook: Techniques, Tactics, and Teaching Points*, Ed. Giorgio Gandolfi, Champaign, IL: Human Kinetics (2009), 287–298.

[16] Erev, Ido, and Roth, Alvin, "Predicting How People Play Games: Reinforcement Learning in Experimental Games with Unique, Mixed Strategy Equilibria," *American Economic Review*, 88 (1998), 848–881.

[17] Goldsberry, Kirk, "The NBA is Obsessed with 3s, So Let's Finally Fix the Thing" (2019), online article, https://www.espn.com/nba/story/_/id/26633540/the-nba-obsessed-3s-let-fix-thing (accessed January 4, 2020).

[18] Goldsmith, Daena, "Soliciting Advice: The Role of Sequential Placement in Mitigating Face Threat," *Communications Monographs*, 67 (2000), 1–19.

[19] Giuliano, Paola, and Spilimbergo, Antonio, "Growing Up in a Recession," *Review of Economic Studies*, 81 (2014), 787–817.

[20] Haselhuhn, Michael, Pope, Devin, Schweitzer, Maurice, and Fishman, Peter, "The Impact of Personal Experience on Behavior: Evidence From Video-Rental Fines," *Management Science*, 58 (2012), 52–61.

[21] Hertwig, Ralph, Barron, Greg, Weber, Elke, and Erev, Ido, "Decisions from Experience and the Effect of Rare Events in Risky Choice," *Psychological Science*, 15 (2004), 534–539.

[22] Highhouse, Scott, "Stubborn Reliance on Intuition and Subjectivity in Employee Selection," *Industrial and Organizational Psychology*, 1 (2008), 333–342.

[23] Ho, Teck-Hua, Camerer, Colin, and Chong, Juin-Kuan, "Self-Tuning Experience Weighted Attraction Learning in Games," *Journal of Economic Theory*, 133 (2007), 177–198.

[24] Hoffman, Mitchell, Kahn, Lisa, and Li, Danielle, "Discretion in Hiring," *Quarterly Journal of Economics*, forthcoming (2018).

[25] Hofler, Richard, and Payne, James, "Efficiency in the National Basketball Association: A Stochastic Frontier Approach with Panel Data," *Managerial and Decision Economics*, 27 (2006), 279–285.

[26] Hossain, Tanjim, and Morgan, John, "... Plus Shipping and Handling: Revenue (Non) Equivalence in Field Experiments on eBay," *BE Journal of Economic Analysis & Policy*, 5 (2006), 1–30.

[27] Kaustia, Markku, and Knüpfer, Samuli, "Do Investors Overweight Personal Experience? Evidence From IPO Subscriptions," *Journal of Finance*, 63 (2008), 2679–2702.

[28] Koudijs, Peter, and Voth, Hans-Joachim, "Leverage and Beliefs: Personal Experience and Risk-Taking in Margin Lending," *American Economic Review*, 106 (2016), 3367–3400.

[29] Kuncel, Nathan, Klieger, David, Connelly, Brian, and Ones, Deniz, "Mechanical Versus Clinical Data Combination in Selection and Admissions Decisions: A Meta-Analysis," *Journal of Applied Psychology*, 98 (2013), 1060-1072.

[30] Lacetera, Nicola, Pope, Devin, and Sydnor, Justin, "Heuristic Thinking and Limited Attention in the Car Market," *American Economic Review*, 102 (2012), 2206–2236.

[31] Leung, Ming, "Learning to Hire? Hiring as a Dynamic Experiential Learning Process in an Online Market for Contract Labor," *Management Science*, forthcoming (2017).

[32] Malinowski, Erik, "Hoops 2.0: Inside the NBA's Data-Driven Revolution" (2011), online article, https://www.wired.com/2011/04/nba-data-revolution/ (accessed 1/20/2020).

[33] Malmendier, Ulrike, and Nagel, Stefan, "Depression Babies: Do Macroeconomic Experiences Affect Risk Taking?," *Quarterly Journal of Economics*, 126 (2011), 373–416.

[34] Malmendier, Ulrike, and Nagel, Stefan, "Learning From Inflation Experiences," *The Quarterly Journal of Economics*, 131 (2016), 53–87.

[35] Price, Joseph, Soebbing, Brian, Berri, David, and Humphreys, Brad, "Tournament Incentives, League Policy, and NBA Team Performance Revisited," *Journal of Sports Economics*, 11 (2010), 117–135.

[36] Price, Joseph, and Wolfers, Justin, "Racial Discrimination Among NBA Referees," *Quarterly Journal of Economics*, 125 (2010), 1859–1857.

[37] Rivera, Lauren, "Hiring as Cultural Matching: The Case of Elite Professional Service Firms," *American Sociological Review*, 77 (2012), 999–1022.

[38] Rivera, Lauren, "When Two Bodies are (Not) a Problem: Gender and Relationship Status Discrimination in Academic Hiring," *American Sociological Review*, 82 (2017), 1111–1138.

[39] Ross, Terrance, "Welcome to Smarter Basketball: The Rise of Big Data is Pitting the Old School Against the New School as the NBA Undergoes its Analytics Revolution" (2015), online article, https://www.theatlantic.com/entertainment/archive/2015/06/nba-data-analytics/396776/ (accessed January 13, 2020).

[40] Roth, Alvin, and Erev, Ido, "Learning in Extensive-Form Games: Experimental Data and Simple Dynamic Models in the Intermediate Term," *Games and Economic Behavior*, 8 (1995), 164–212.

[41] Ruffle, Bradley, and Shtudiner, Ze'ev, "Are Good-Looking People More Employable?," *Management Science*, 61 (2014), 1760–1776.

[42] Simonsohn, Uri, Karlsson, Niklas, Loewenstein, George, and Ariely, Dan, "The Tree of Experience in the Forest of Information: Overweighing Experienced Relative to Observed Information," *Games and Economic Behavior*, 62 (2008), 263–286.

[43] Staw, Barry, and Hoang, Ha, "Sunk Costs in the NBA: Why Draft Order Affects Playing Time and Survival in Professional Basketball," *Administrative Science Quarterly*, 40 (1995), 474–494.

[44] Stiroh, Kevin, "Playing for Keeps: Pay and Performance in the NBA," *Economic Inquiry*, 45 (2007), 145–161.

[45] Strahilevitz, Michal Ann, Odean, Terrance, and Barber, Brad, "Once Burned, Twice Shy: How Naive Learning, Counterfactuals, and Regret Affect the Repurchase of Stocks Previously Sold," *Journal of Marketing Research*, 48 (2011), S102–S120.

[46] Weber, Elke, Bockenholt, Ulf, Hilton, Denis, and Wallace, Brian, "Determinants of Diagnostic Hypothesis Generation: Effects of Information, Base Rates, and Experience," *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 19 (1993), 1151–1164.

[47] Wong, Alex "A High-Scoring Revolution Has the Rockets Soaring" (2017), online article, https://www.theatlantic.com/entertainment/archive/2017/11/a-high-scoring-revolution-has-the-rockets-soaring/547103/ (accessed 1/21/2020).

Online Appendix to Dalton & Landry (2020) "Overattention..."

A Additional Tables

Table A1: Relative Performance Percentiles

Mean perf. deviation in	Min	10^{th}	20^{th}	30^{th}	40^{th}	50^{th}	60^{th}	70^{th}	80^{th}	90^{th}	Max
	Percentile										
... versus games	-28.77	-5.56	-3.7	-2.44	-1.38	-0.37	0.70	1.96	3.52	5.97	33.17
... prep. games	-25.95	-5.49	-3.69	-2.45	-1.41	-0.38	0.70	1.95	3.52	5.96	33.84

Consistent with the construction of Figure 1, the sample used to calculate the percentiles included all player-team observations for which there was at least one game of the corresponding type.

Table A2: Excluding Missing Observations — with Controls

Mean performance deviation in versus and prep. games	0.029 (.005) [<.001]		0.032 (.006) [<.001]	
... in versus games only		0.018 (.004) [<.001]		0.019 (.004) (.004)
... in preparation games only		0.014 (.004) [.001]		0.013 (.004) [.002]
Obs. excluded from sample	No versus games & no prep. games	No versus games & no prep. games	No versus games or no prep. games	No versus games or no prep. games
Controls?	Yes	Yes	Yes	Yes
Observations	113,705	113,705	80,021	80,021

Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

Table A3: Excluding Missing Observations — without Controls

Mean performance deviation in versus and prep. games	0.033 (.004) [<.001]		0.035 (.005) [<.001]	
... in versus games only		0.023 (.004) [<.001]		0.023 (.004) (.004)
... in preparation games only		0.012 (.003) [<.001]		0.012 (.003) [.001]
Obs. excluded from sample	No versus games & no prep. games	No versus games & no prep. games	No versus games or no prep. games	No versus games or no prep. games
Controls?	No	No	No	No
Observations	113,705	113,705	80,021	80,021

Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

Table A4: Estimates of Conditional Logit Matching Model (Expanded)

Mean perf. deviation in vs. & prep. games	0.033 (.004) [<.001]	0.028 (.005) [<.001]		
... in versus games only			0.023 (.004) [<.001]	0.017 (.004) [<.001]
... in preparation games only			0.012 (.003) [<.001]	0.013 (.004) [<.001]
Team win %		-0.259 (.107) [.016]		-0.258 (.107) [.016]
Mean performance of all who played team		0.106 (.030) [<.001]		0.104 (.030) [<.001]
Mean utilization in versus games		0.008 (.003) [.027]		0.006 (.004) [.084]
Mean utilization in prep. games		-0.004 (.003) [.257]		-0.003 (.004) [.430]
# versus games		0.073 (.096) [.448]		0.065 (.096) [.500]
# prep. games		-0.068 (.096) [.480]		-0.062 (.096) [.520]
# potential vs. games		-0.007 (.038) [.860]		0.001 (.039) [.985]
# potential prep. games		0.015 (.037) [.689]		0.010 (.038) [.785]
= 0 versus games		0.053 (.078) [.496]		0.043 (.078) [.580]
= 0 prep. games		-0.072 (.076) [.340]		-0.065 (.075) [.386]
Birth state		0.389 (.064) [<.001]		0.390 (.064) [<.001]
Border state		0.063 (.056) [.260]		0.063 (.056) [.258]
Same conference		-0.017 (.043) [.686]		-0.017 (.043) [.689]
Same division		0.048 (.043) [.272]		0.048 (.043) [.271]
Observations	126,404	126,404	126,404	126,404

Standard errors are in parentheses and p-values are in square brackets. These estimates are the same as the baseline estimates of the conditional logit player-team matching model in Table 3, except here the effects of all coefficients are listed.

Table A5: How Relative Performance Relates to Traits of Current and Next Opponents

	Mean	Player's mean performance deviation decile									
		1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}	6^{th}	7^{th}	8^{th}	9^{th}	10^{th}
Current opponent...											
win %	*50.0*	+1.7	+1.2	+0.9	+0.5	+0.2	+0.1	−0.4	−0.9	−1.3	−1.9
new players (within 1 year)	*6.2*	−0.1	−0.1	−0.1	0.0	0.0	0.0	0.0	+0.1	+0.1	+0.1
% in birth state	*4.5*	−0.5	+0.1	0.0	+0.2	+0.2	−0.1	+0.1	−0.1	0.0	+0.1
% in border state	*7.6*	0.0	+0.1	+0.1	−0.4	−0.3	+0.2	+0.1	+0.6	0.0	−0.3
Next opponent...											
win %	*50.0*	−0.3	0.0	−0.1	−0.2	−0.4	+0.2	+0.1	+0.3	+0.1	+0.3
new players (within 1 year)	*6.2*	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
% in birth state	*4.4*	−0.1	−0.1	−0.2	0.0	−0.1	+0.5	+0.3	+0.1	+0.3	−0.6
% in border state	*7.6*	−0.2	−0.3	+0.1	+0.1	+0.5	−0.3	−0.2	+0.3	+0.3	−0.2

Deciles were calculated based on the distributions of mean performance deviations for versus (top 5 rows) and preparation (bottom 5 rows) games using all player-team observations with at least one game of that type. The entries for each decile are expressed relative to the mean value on the left.

Table A6: Team-By-Team Estimates

Team	Versus	Preparation	Overall
Atlanta	−1.52	2.32	0.34
Boston	1.14	1.83	1.21
Brooklyn	0.42	1.34	0.68
Charlotte	1.18	−0.31	0.66
Chicago	−0.19	1.17	0.65
Cleveland	−0.03	0.81	0.14
Dallas	1.35	0.38	0.99
Denver	5.02*	0.35	3.21*
Detroit	−0.78	1.65	0.14
Golden State	2.92*	3.03*	2.93*
Houston	−0.52	1.20	0.18
Indiana	−0.75	1.20	0.56
LA Clippers	1.30	1.21	1.11
LA Lakers	−0.04	2.56	0.98
Memphis	1.92	0.78	1.73
Miami	−0.35	1.40	0.43
Milwaukee	2.82*	1.35	2.26*
Minnesota	0.01	0.60	0.06
New Orleans	0.08	1.74	0.69
New York	0.48	−0.44	0.14
Oklahoma City	−0.44	−0.97	0.16
Orlando	3.07*	−2.04	1.34
Philadelphia	2.47*	1.09	1.65*
Phoenix	1.81	0.83	1.54*
Portland	1.02	0.67	0.97
Sacramento	1.15	−1.82	−0.03
San Antonio	0.74	−0.25	−0.11
Toronto	1.95	1.80	1.62
Utah	−0.22	3.11	1.30
Washington	0.42	1.33	0.79

Estimates are expressed in proportion to the corresponding estimate (with all teams) in Table 3. The underlying regressions (one that estimates the overall first-hand experience effect, and one that separately estimates the versus and preparation effects) include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and shown in Appendix Table A4). Standard errors and p-values are omitted for cleanliness, though estimates with $p < .05$ are denoted with an asterisk. As noted in footnote 13, most of the team-level estimates are, on their own, not statistically significant (which is unsurprising considering they are, on average, identified based on 1/30th of the observations in our sample), though the estimates of the overall effect are collectively distinct from zero ($p < .001$) and indistinct from each other ($p = .21$).

Table A7: Does 'bias' predict performance on new team?

	Dep. variable in OLS regression: mean performance on new team			
	...in first year		...entire tenure	
Mean performance deviation in future team's versus and preparation games	-0.003 (.016) [.867]		0.008 (.016) [.610]	
... versus games only		0.005 (.012) [.709]		0.012 (.012) [.327]
... prep. games only		-0.005 (.012) [.699]		0.000 (.012) [.970]
Mean performance in all games before joining new team	0.690 (.024) [<.001]	0.690 (.024) [<.001]	0.650 (.024) [<.001]	0.650 (.024) [<.001]
Mean performance of all other players on new team (total)	-0.134 (.006) [<.001]	-0.134 (.006) [<.001]	-0.139 (.006) [<.001]	-0.139 (.006) [<.001]
Observations	4,567	4,567	4,567	4,567

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A8: Does 'bias' predict team wins?

	Dep. variable in OLS regression: new team's winning percentage			
	...in first year		...entire tenure	
Mean performance deviation in future team's versus and preparation games	-0.006 (.004) [.123]		-0.003 (.003) [.264]	
... versus games only		-0.004 (.003) [.173]		-0.001 (.002) [.595]
... prep. games only		-0.001 (.003) [.807]		-0.001 (.002) [.525]
Mean performance in all games before joining new team	0.015 (.007) [.037]	0.015 (.007) [.036]	0.014 (.009) [.092]	0.015 (.009) [.091]
Mean performance of all other players on new team (total)	0.030 (.002) [<.001]	0.030 (.002) [<.001]	0.028 (.001) [<.001]	0.028 (.001) [<.001]
Observations	4,567	4,567	4,567	4,567

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A9: Does 'bias' predict utilization on new team?

	Dep. variable in OLS regression: mean utilization on new team			
	...in first year		...entire tenure	
Mean performance deviation in future team's versus and preparation games	0.004 (.029) [.879]		0.009 (.028) [.753]	
... versus games only		0.015 (.022) [.486]		0.021 (.021) [.324]
... prep. games only		-0.007 (.021) [.760]		-0.007 (.021) [.740]
Mean performance in all games before joining new team	0.610 (.039) [<.001]	0.611 (.039) [<.001]	0.556 (.038) [<.001]	0.556 (.038) [<.001]
Mean performance of all other players on new team (total)	-0.297 (.013) [<.001]	-0.297 (.013) [<.001]	-0.305 (.013) [<.001]	-0.305 (.013) [<.001]
Observations	4,567	4,567	4,567	4,567

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A10: Does 'bias' predict tenure on new team?

	Dependent variable in OLS regression: number of games played on new team					
	...in 1st year		...after 1st year		...entire tenure	
Mean performance deviation in future team's versus and preparation games	0.293 (.118) [.013]		-0.114 (.781) [.884]		0.524 (.456) [.251]	
... versus games only		0.169 (.087) [.053]		-0.008 (.644) [.990]		0.164 (.360) [.650]
... prep. games only		0.106 (.091) [.245]		-0.374 (.589) [.526]		0.184 (.362) [.612]
Mean performance in all games before joining new team	0.852 (.174) [<.001]	0.850 (.174) [<.001]	3.870 (1.194) [.001]	3.860 (1.195) [.001]	6.607 (.812) [<.001]	6.599 (.812) [<.001]
Mean performance of all other players on new team (total)	-0.362 (.048) [<.001]	-0.362 (.048) [<.001]	-1.471 (.416) [<.001]	-1.466 (.417) [<.001]	-1.026 (.144) [<.001]	-1.026 (.144) [<.001]
Observations	4,567	4,567	1,477	1,477	4,263	4,263

The estimates reported in the two right-most columns exclude 304 observations in which a player was still playing for their new team at the end of our sample (thus preventing us from observing the number of games that the player played in their full tenure on their new team). The estimates reported in the middle two columns exclude these observations as well as an additional 2,786 observations in which we observe a player's tenure on their new team ending less than one year after they were acquired by the team. All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A11: Estimates of Matching Model — Signs and Trade

Mean performance deviation in versus and prep. games	0.033 (.007) [<.001]		0.023 (.008) [.005]	
... in versus games only		0.025 (.006) [<.001]		0.009 (.006) [.130]
... in preparation games only		0.012 (.006) [.033]		0.014 (.006) [.020]
How acquired?	Signed	Signed	Traded	Traded
Observations	73,613	73,613	44,392	44,392

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A12: Regular Season vs. Offseason Transitions

Mean performance deviation in versus and prep. games	0.035 (.007) [<.001]		0.022 (.008) [.005]	
... in versus games only		0.022 (.005) [<.001]		0.013 (.006) [.039]
... in preparation games only		0.015 (.005) [.005]		0.011 (.006) [.065]
When acquired?	Offeason	Offseason	Reg. Season	Reg. Season
Observations	68,560	68,560	57,844	57,844

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A13: Estimates for High- and Low-Performance Players

Mean performance deviation in versus and prep. games	0.025 (.010) [.009]	0.031 (.006) [<.001]		
... in versus games only			0.016 (.008) [.038]	0.021 (.005) [<.001]
... in preparation games only			0.011 (.008) [.154]	0.013 (.005) [.007]
Players included in the sample	< median overall perf.	≥ median overall perf.	< median overall perf.	≥ median overall perf.
Observations	63,194	63,210	63,194	63,210

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A14: Home/Road Estimates

Mean performance deviation in...	
... versus games played at home (for the team evaluating the player)	0.011 (.004) [.002]
... versus games played on road	0.016 (.004) [<.001]
... all preparation games	0.012 (.004) [.002]
Observations	126,404

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A15: Time Trend Estimates, Pre- and Post-2005

Mean performance deviation in versus and prep. games	0.028 (.007) [<.001]	0.028 (.007) [<.001]		
...in versus games only			0.014 (.006) [.012]	0.020 (.006) [.001]
...in preparation games only			0.013 (.006) [.018]	0.012 (.006) [.026]
Years Included	< 2005	≥ 2005	< 2005	≥ 2005
Observations	64,982	61,422	64,982	61,422

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A16: Linear Time Trend Estimates

Mean perf. deviation in vs. & prep. games	0.032 (.004) [<.001]	
Mean perf. deviation in vs. & prep. games $\times$ transaction date	-0.000 (.000) [.542]	
...in vs. games only		0.023 (.004) [<.001]
...in vs. games only $\times$ transaction date		0.000 (.000) [.801]
...in prep. games only		0.012 (.003) [<.001]
...in prep. games only $\times$ transaction date		-0.000 (.000) [.827]
Observations	126,404	126,404

Transaction dates are expressed in days and demeaned relative to the mean transaction date across all observations. All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A17: Pre- and Post-Trade Deadline Estimates

Mean performance deviation in versus and prep. games played *before* trade deadline	0.038 (.009) [<.001]	0.034 (.007) [<.001]		
... in versus games before trade deadline			0.033 (.007) [<.001]	0.022 (.005) [<.001]
... in preparation games before trade deadline			0.010 (.007) [.152]	0.014 (.005) [.007]
Mean performance deviation in versus and prep. games played *after* trade deadline	0.024 (.011) [.025]	0.025 (.008) [.002]		
... in versus games after trade deadline			0.015 (.009) [.100]	0.014 (.007) [.037]
... in preparation games after trade deadline			0.011 (.008) [.181]	0.015 (.006) [.015]
Signed or Traded?	Signed	Both	Signed	Both
Observations	73,613	126,404	73,613	126,404

Roughly two-thirds of regular season games occur before the trade deadline. All regressions include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table A18: Estimates for 'Star' and 'Non-Star' Players

Mean performance deviation in versus and preparation games			
... for 'star' players (i.e. past, present, or future all-star)	0.024 (.014) [.086]	0.053 (.018) [.003]	0.038 (.011) [<.001]
... for all other players	0.023 (.009) [.015]	0.029 (.008) [<.001]	0.026 (.006) [<.001]
How acquired?	Traded	Signed	All
Observations	44,392	73,613	126,404

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets. The 'star' designation applied to 13.4% of player-team transitions in our sample.

Table A19: Does 'bias' predict disproportionate home utilization

	Dep. variable in OLS regression: mean home utilization minus mean road utilization on new team			
	...in first year		...entire tenure	
Mean performance deviation in future team's versus and preparation games	0.020 (.015) [.169]		0.022 (.014) [.116]	
... versus games only		0.011 (.011) [.318]		0.009 (.010) [.383]
... prep. games only		0.003 (.011) [.748]		0.006 (.010) [.528]
Mean performance in all games before joining new team	0.035 (.018) [.056]	0.034 (.018) [.060]	0.032 (.017) [.059]	0.031 (.017) [.064]
Mean performance of all other players on new team (total)	0.006 (.007) [.419]	0.006 (.007) [.425]	0.006 (.007) [.423]	0.006 (.007) [.426]
Observations	4,385	4,385	4,388	4,388

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets. The number of observations in these regressions is smaller than the number (4,567) of player-team transitions in our full sample because some players did not play in at least one home game and at least one road game on their new team. The number of observations in the third and fourth columns is slightly larger than the number of observations in the first and second columns because there were three cases in which a player did not play in at least one home game and at least one road game during their first year on their new team, but did play in at least one home game and at least one road game during their entire tenure on their new team.

Table A20: Does 'bias' predict team attendance at home games?

	Dep. variable in OLS regression: mean home game attendance for player's new team (1,000s)			
	...in first year		...entire tenure	
Mean performance deviation in future team's versus and preparation games	-0.008 (.012) [.527]		-0.010 (.012) [.406]	
...versus games only		-0.006 (.009) [.457]		-0.005 (.009) [.540]
...prep. games only		-0.001 (.010) [.945]		-0.005 (.009) [.615]
Mean performance in all games before joining new team	0.040 (.015) [.009]	0.040 (.015) [.009]	0.055 (.015) [<.001]	0.055 (.015) [<.001]
Mean performance of all other players on new team (total)	0.045 (.005) [<.001]	0.045 (.005) [<.001]	0.046 (.005) [<.001]	0.046 (.005) [<.001]
Observations	4,497	4,497	4,497	4,497

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets. The number of observations in these regressions is smaller than the number (4,567) of player-team transitions in our full sample because some players' tenures on their new teams did not include any home games.

B Regime Tests

This appendix considers various changes occurring during our sample period that may have affected NBA teams' player-acquisition decisions. Of particular interest is whether (and if so, how) the effect of first-hand experience was impacted by these changes — and more broadly, whether the effect may have changed over time. As we will see, however, we will not be able to draw strong conclusions about such possibilities due to a lack of statistical power in our relevant empirical tests.

B.1 Rule Changes

We first consider changes in rules that may have affected NBA teams' player-acquisition decisions. In particular, NBA player-team relations are governed by a legal contract known as a *Collective Bargaining Agreement* (CBA). When a CBA expires, it is replaced by a new CBA, and each new CBA may entail new rules or changes to existing rules that affect team decision-making. The first column of Table B1 lists nine key rule changes instituted by new CBAs during our sample period.[46] These rule changes naturally motivate a partitioning of our sample into five regulatory regimes, as defined in the headers of the five remaining columns in Table B1.

It is evident that we will not be able to test the effect of each individual rule change listed in Table B1. For instance, some rule changes were enacted simultaneously with others, preventing us from separating their effects, while there is also substantial overlap between rule changes that were not enacted simultaneously. That said, by separately estimating the effects for each of the five regulatory regimes, we can test the broader notion that the effect of first-hand experience may have been impacted by these rule changes.

Table B2 reports estimates of the effect of first-hand experience in each of the five regulatory regimes defined in Table B1. For all five regimes, the estimates of interest remained positive, with varying levels of statistical significance. The lack of statistical significance in some cases is unsurprising due to the loss of statistical power from partitioning our sample

[46] While each CBA is very detailed and contains many provisions, the rule changes listed in Table B1 correspond to the major "milestones" identified in a summary of past CBAs by Coon (2017), but exclude rule changes that would not have directly impacted transactions of players between teams (e.g. rules that only affect rookies who are new to the league).

Table B1: Key Rule Changes and Implied Regulatory Regimes, 1983-2015

	Regulatory Regime				
Rule (CBA Year)	1st 1983-1987	2nd 1988-1998	3rd 1999-2004	4th 2005-2010	5th 2011-2015
Salary Cap (1983) *sets maximum total salary of all players on a given team*	X	X	*X*	*X*	*X*
Unrestricted Free Agency (1988) *players may sign with a new team with no option for prior team to match*		X	X	X	X
Maximum Salaries (1999) *imposes a maximum annual salary for a given player*			X	X	X
Escrow Tax (1999) *players forfeit 10% of their salary in years with relatively low league revenues*			X	X	X
Luxury Tax (1999) *teams may now exceed the salary cap, but subjected to a financial penalty*			X	X	*X*
Mid-Level Exception (1999) *allows teams to sign one new player without counting against salary cap*			X	X	X
Misc. Contract Limits, 1/2 (2005) *reduces the maximum annual raise and maximum length of player contracts*				X	*X*
Misc. Contract Limits, 2/2 (2011) *further reduces max. raise, contract length; players' share of revenues also reduced*					X
Luxury Tax Increase (2011) *increases financial penalty for teams that exceed the salary cap*					X

Here, 'X' denotes that the rule was in effect during that regime, while an italicized '*X*' indicates that the rule (even if largely intact) was modified by subsequent listed rule changes. The time spans for each regime are expressed in terms of the start years of the first and last seasons (e.g. the 5th regime includes the 2011/2012 season through the 2015/2016 season).

into five regimes. This is especially true for the estimates of the versus and preparation effects, which represent an additional separation of the overall effect into two components.[47]

[47] For example, the preparation effect in the 3rd regime was no longer significant even though the estimated was slightly larger than the estimated coefficient in Table 3.

Table B2: Regulatory Regime Estimates

Mean perf. deviation in vs. & prep. games during...	1st regime (1983 to 1987)	0.035 (.016) [.028]	
	2nd regime (1988 to 1998)	0.021 (.009) [.018]	
	3rd regime (1999 to 2004)	0.039 (.010) [<.001]	
	4th regime (2005 to 2010)	0.042 (.010) [<.001]	
	5th regime (2011 to 2015)	0.009 (.010) [.346]	
...in versus games only	1st regime (1983 to 1988)		0.002 (.012) [.874]
	2nd regime (1988 to 1998)		0.015 (.007) [.028]
	3rd regime (1999 to 2004)		0.025 (.008) [.001]
	4th regime (2005 to 2010)		0.032 (.008) [<.001]
	5th regime (2011 to 2015)		0.002 (.008) [.838]
...in prep. games only	1st regime (1983 to 1987)		0.031 (.013) [.014]
	2nd regime (1988 to 1998)		0.004 (.007) [.522]
	3rd regime (1999 to 2004)		0.014 (.008) [.072]
	4th regime (2005 to 2010)		0.017 (.008) [.030]
	5th regime (2011 to 2015)		0.011 (.008) [.152]
Observations		126,404	126,404

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Focusing on the estimates of the overall effect, we do not find strong evidence that the effect was impacted by rule changes during our sample period, as we fail to reject the hypothesis that the estimates in all five regulatory regimes are equal (though this test was quite close, with $p = .08$). That said, while these five estimates were collectively distinct from zero at the 0.1% level, the estimate for the 5th and final regime — which covered the 2011/12 to 2015/16 NBA seasons — was not statistically distinct from zero, and notably smaller (roughly one-third the size of the overall effect in Table 3) than the estimates for the other regimes. While this may just be a statistical aberration, if the effect of first-hand experience truly did diminish during the 5th regime, it could in principle reflect unique regulatory factors at play during this period. That said, it is not obvious how or why this might be the case considering the two key rule changes (listed in Table B1) from the 5th regime appear quite similar to rule changes in earlier regimes.[48]

B.2 The Analytics Movement

If the smaller estimated effect of first-hand experience in the 5th regulatory regime was *not* merely a statistical aberration, it could have also been caused by non-regulatory changes during this period. Notably, in recent years the NBA has experienced a so-called *analytics movement* whereby teams have become substantially more reliant on "analytics" — referring to statistical, data-driven analyses, often based on previously-unavailable types of data — as inputs in decision-making (e.g. Malinowski, 2011; Ross, 2015; Wong, 2017).[49] Even though teams had easy access to comprehensive performance data (and thus the ability to overcome a potential first-hand experience bias) before the analytics movement, the movement may have been a catalyst for teams to embrace more analytical approaches to decision-making. In turn, this shift in "philosophy" may have made teams less susceptible to the bias.

[48] For instance, the new restrictions on player contracts in the 2011 CBA were, by and large, qualitatively quite similar to the restrictions imposed by the 2005 CBA at the start of the 4th regime. Moreover, the increased luxury tax effectively made the salary cap "harder" than in the 3rd and 4th regimes (which featured a lower luxury tax), but it was still "softer" than in the 1st and 2nd regimes (before the luxury tax existed).

[49] While player-acquisition decisions have certainly been affected, the analytics movement is known to have impacted several other aspects of team decision-making. For instance, every NBA team now uses video tracking technology to monitor players' on-court activity, which then informs coaching strategy (e.g. helping a coach learn and encourage players to shoot from "sweet spots" where they make a higher percentage of their attempts). As another example, teams have increasingly encouraged players to attempt more 3-point shots in games (and practice) based on statistical evidence indicating that 3-point shots were historically under-utilized. As Goldsberg (2019) notes, NBA players collectively made more 3 pointers during the 2018-2019 NBA season (27,955) than during the *entire* 1980s (23,871).

To explore such possibilities, we now consider tests that separately estimate the effects of first-hand experience in a "pre-analytics" regime and in an "analytics" regime. One challenge, however, is that it is open to interpretation when the analytics movement truly took effect. Ross (2015) suggests the 2013/14 NBA season as a natural turning point, as this was the first season that video tracking systems (see footnote 49) were used in all NBA arenas. The 2010/11 season could also reasonably be considered as a natural cutoff, as video tracking systems were first adopted by a handful of teams during this season (Malinowski, 2011). Yet another possibility is the 2007/08 season, as this was the first season that Daryl Morey — arguably the pioneer of the NBA's analytics movement — severed as general manager of the Houston Rockets (Wong, 2017).

Table B3 reports estimates of the analytics regime tests, based on each of the three proposed cutoffs between the pre-analytics and analytics regimes mentioned above. In all three cases, the point estimate capturing the overall effect of first-hand experience during the analytics regime is, while still positive, smaller than the estimate during the pre-analytics regime. Furthermore, when using the latest cutoff (i.e. with the analytics regime starting in the 2013/14 season), the analytics regime estimate is no longer statistically significant. These results are compatible with the possibility that the effect of first-hand experience on teams' player-acquisition decisions diminished during — and perhaps as a result of — the modern analytics movement. That said, we cannot draw strong conclusions, as our estimates are also compatible with the possibility that the effect did not change. In particular, for all three cutoffs considered, the estimated effect of first-hand experience during the analytics regime was *not* statistically distinct from the estimated effect during the pre-analytics regime (with p values of .12, .12, and .11 in tests of equality based on the estimates with the earliest, middle, and latest regime cutoffs, respectively).

Table B3: Analytics Regime Estimates

Mean perf. deviation in vs. & prep. games, pre-analytics regime	0.034 (.006) [<.001]		0.032 (.006) [<.001]		0.031 (.006) [<.001]	
...in versus games, pre-analytics reg.		0.019 (.005) [<.001]		0.020 (.005) [<.001]		0.020 (.004) [<.001]
...in prep. games, pre-analytics reg.		0.015 (.005) [.002]		0.013 (.005) [.003]		0.013 (.004) [.003]
Mean perf. deviation in vs. & prep. games, analytics regime	0.019 (.007) [.009]		0.017 (.009) [.062]		0.010 (.013) [.435]	
...in versus games, analytics regime		0.014 (.006) [.021]		0.009 (.007) [.219]		0.001 (.010) [.896]
...in prep. games, analytics regime		0.010 (.006) [.105]		0.011 (.007) [.113]		0.013 (.010) [.173]
Assumed start of analytics regime	2007/08 season		2010/11 season		2013/14 season	
Relevant milestone	D. Morey hired as Rockets GM		early adopters of video tracking		all teams use video tracking	
Observations	126,404	126,404	126,404	126,404	126,404	126,404

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

C Team Decision-Making: What Do We Know?

Throughout this paper, we have considered teams' player-acquisition decisions without much reference to how NBA teams actually make these decisions or to the individual personnel involved. In this appendix, we take a closer look at teams' decision-making processes and discuss relevant implications for this study.

C.1 Who Is Involved?

To begin, Table C1 highlights six relevant categories of NBA team personnel — owners, presidents, directors, managers, coaches, and scouts — based on the job titles of currently-employed personnel for each NBA team (as of April 20, 2018) collected from realgm.com. The italicized examples of job titles within each category represent actual positions for which, to the best of our knowledge, the employee may play a key role in the team's player-acquisition decisions. Such roles may involve collecting information about other teams' players, conveying that information to other personnel, or making final decisions. As seen, NBA teams generally employ *many* individuals — averaging about 47 per team — across the six categories. We can also see substantial variation across teams in the numbers of each type of personnel. For example, the Milwaukee Bucks did not list any individuals with "owner" in their job title, while the the Philadelphia 76ers listed twelve.[50]

Table C1: NBA Team Personnel

Position *relevant example(s)*	Mean	S.D.	Min	Max
Owners *owner, co-owner, managing owner*	2.10	2.82	0	12
Presidents *president, president of basketball operations*	1.47	0.86	0	3
Directors *...of player personnel, of pro evaluation, of scouting, of basketball operations*	16.33	6.26	6	32
Managers *general manager, manager of basketball operations*	13.20	6.43	3	30
Coaches *head coach, assistant coach, video coach*	8.67	3.00	1	12
Scouts *scout, advance scout, pro scout*	4.87	3.20	1	12
TOTAL	*47.23*	*11.59*	*30*	*77*

Summary of currently-employed NBA team personnel, as of April 20, 2018, based on job title data collected from realgm.com. Personnel with "vice presidents," "assistant directors," or "assistant managers" as (or in) their job titles were excluded.

While many team employees have titles suggestive of potential roles in player-acquisition decisions, it is difficult (due to a lack of data) to pinpoint exactly who, to what extent, and how each such individual is involved in player-acquisition decisions. With that said, there are some cases in which the relevant personnel are better known than others. However, these cases reveal substantial variation from one team to the next, indicating there is no consistent template for team decision-making.

With the above caveats in mind, next we describe what we know about NBA team decision-making. Our focus will be on clarifying, to the extent possible, how teams' first-

[50] The Philadelphia 76ers list one "managing owner," ten "co-owners," and one "co-managing owner."

hand experiences may come to bias their player-acquisition decisions. In doing so, we will first consider how teams attain first-hand experience with other teams' players, followed by a discussion of how teams arrive at a final decision, and then we address how impressions of players formed through first-hand experiences may be aggregated and transmitted among relevant team personnel between the time of the experience and the final decision. After this, we will discuss relevant implications for our research.

C.2 Teams' First-Hand Experiences: Who Watches the Games?

Throughout, we have considered a team's first-hand experiences with a player on another team as arising from two sources: 'versus' games against that player's team and the preceding 'preparation' games when the team is preparing for the player's team. To go deeper, it may help to be more precise as to what actually constitutes first-hand experience for an individual observer. Surely, observing a player's performance in person qualifies. To paint a more comprehensive picture of how teams may attain first-hand experience, we will also consider observing a player's performance on film or television as a form of first-hand experience.

First-Hand Experience in Versus Games

Of the personnel categories listed in Table C1, a player's performance in a versus game would (at the very least) generally be experienced first-hand by the team's coaches — including the head coach as well as the assistant coaches — who attend and are highly involved in coaching such games. It is also quite possible that a substantial portion of these games (particularly those played at the team's 'home' arena) would also be attended by other personnel, such as managers, directors, presidents, and owners. With that said, teams are not necessarily alike in this regard. For example, Dallas Mavericks' owner Mark Cuban is known to regularly attend his team's games (including games played in opponents' arenas), while Brooklyn Nets' owner Mikhail Prokhorov is known to rarely attend his team's games (Mazzeo and Gutierrez, 2014).

First-Hand Experience in Preparation Games

As mentioned in the introduction (see footnote 2), a preparation game will generally be attended by a team scout. While it is unlikely that other personnel would regularly attend as well, team coaches routinely watch and analyze video footage of these games in order to prepare for the versus game. This video footage is typically prepared by team-employed video specialists in collaboration with the scout who attended the game to highlight key plays from the previous game. As one scout explains, "the coaches watch a lot of video" (Agness, 2016), while one former NBA coach writes (Dunleavy and Eyen, 2009): "The video coordinator and his staff are crucial to our overall game-plan preparation. Countless hours are involved in recording games, logging the film, and establishing breakdowns. At the end of the day, the video personnel are as versed on the opponent as the scouts or coaches are."

In addition, a scout prepares a 'scouting report' of each preparation game for coaches to review along with the video footage before the versus game. As Dunleavy and Erev (2009) describe, the content of these reports can vary depending on the head coach: "A report is compiled for each game scouted. The depth and breadth of a written scouting report is based solely on the philosophy of the head coach. Some coaches feel the need to include every detail of the opponent in the report; others desire only the basic tendencies."

C.3 The Final Decision: Who's In Charge?

We now consider how NBA teams arrive at a final decision to acquire (or not acquire) a new player, focusing in particular on which personnel may have a say (and to what extent) in this decision. With the exception of scouts, all of the personnel categories listed in Table C1 can, in at least some cases, have a significant degree of authority in a team's player-acquisition decisions. However, considering the high numbers of personnel employed by teams across these categories, the opinions of the large majority of these individuals would presumably carry little (if any) weight in such decisions.

Providing some insight as to who is in charge, many NBA teams publicly identify a named employee as being responsible for player-acquisition decisions. Perhaps the most common job title of such individuals is "General Manager." However, some teams employ more than one individual with this title, while in many other cases the named employee has a different title, such as "President of Basketball Operations." Teams also use many close variations of these titles, such as "Manager of Basketball Operations," "Director of Basketball Operations," or simply "President," for which is is unclear what the position actually entails.

Furthermore, many teams employ multiple individuals with different titles, each of which are indicative of having authority in player-acquisition decisions. As one example, in 2017 the Los Angeles Lakes hired a President of Basketball Operations to oversee all player-acquisition decisions, while specifying that he would work closely with the General Manager (as well as the head coach) — where, in this case, the General Manager would report to the President of Basketball Operations — leaving it rather ambiguous who has the final say in player-acquisition decisions (Martin, 2017).[51]

To complicate matters further, relevant personnel often serve in multiple roles simultaneously. For instance, some head coaches are also officially in charge of player-acquisition decisions, typically assuming a second title such as General Manager or President of Basketball Operations, though some head coaches lacking a second title are nonetheless reported to have "massive sway" in decision-making (Powell, 2017). There are even cases in which a 'superstar' player is widely-believed to be a de facto authority in team decision-making, but there are conflicting reports as to whether (or to what extent) this is true (Keeley, 2018).

Yet another issue with identifying the relevant decision-maker(s) is that those officially in charge of player-acquisition decisions may, in practice, not be able to exercise their authority. In one rare case of a team granting the public an inside look at their decision-making processes, the Sacramento Kings allowed the sports channel ESPN to broadcast their deliberations during the NBA draft — an event where NBA teams take turns selecting new players who have not yet played in the NBA (such as those who have recently finished playing college basketball). When it was time to decide among two players, Nik Stauskas and Elfrid Payton, Sacramento Kings' owner Vivek Ranadive began the final deliberation by enthusiastically announcing his preference for Stauskas. With Ranadive leading the discussion, other team personnel — including the purported authority in such matters, then-General Manager Pete D'Alessandro — took turns expressing their agreement with their owner's preference for Stauskas. This episode was widely-viewed as an example of meddling by the owner, in effect usurping authority of team decision-making from the General Manager who, according to

[51] As quoted in this article, Lakers' owner Jeanie Buss stated: "Effective immediately, [new President of Basketball Operations] Earvin Johnson will be in charge of all basketball operations and will report directly to me. Our search for a new General Manager to work with Earvin and Coach Luke Walton is well underway and we hope to announce a new General Manager in short order. Together, Earvin, Luke and our new General Manager will establish the foundation for the next generation of Los Angeles Lakers greatness."

later reports, actually preferred Payton but was reluctant to contradict his boss' preference for Stauskas (Feldman, 2015).

While a few owners are publicly-known to be the final authority in their team's player-acquisition decisions (see, for example, McGee's 2012 description of Michael Jordan's tenure as owner of the Charlotte Bobcats), for the most part owners present themselves as taking a hands-off approach. With that said, the case of the Sacramento Kings drafting Nik Stauskas raises open questions regarding the extent to which owners privately take a more hands-on approach in player-acquisition decisions.

C.4 Information Aggregation and Transmission: Who Talks to Whom?

In light of our preceding discussions, it is clear that there is a lot we don't know about how exactly teams attain first-hand experience and make player-acquisition decisions. From what we do know, it appears that there can be (as in cases where the head coach is in charge of player-acquisition decisions) significant overlap between the personnel who observe other teams' players in versus and preparation games and those who actually decide whether or not to acquire these players, though the extent of overlap appears to vary from team to team and even year to year for a given team. It also appears that many personnel would generally be involved in a team's learning and decision-making processes.

Considering the effects of first-hand experience on team decision-making may often reflect observations by multiple individuals, and these individuals may not have a direct say in final player-acquisition decisions, it would be helpful to know how impressions of players arising from individual experiences are aggregated and transmitted among relevant team personnel. In principle, any employee with a say in the final decision of whether to acquire a particular player could consult any employee who observed the player perform in versus and/or preparation games. Unfortunately, however, there is little we can say beyond speculation due to a lack of data regarding communication within NBA teams.[52]

[52] For instance, in addition to advising coaching staff, team scouts and video specialists are anecdotally known to be consulted by personnel, such as a General Manager, who are presumably not involved in team preparation (Smith, 2016). In these cases, we may naturally speculate that scouts and video specialists are consulted in order to learn more about players on other teams who are being considered for acquisition, but another potential reason is to learn more about their own players who are being considered for a potential new contract or a trade to another team.

C.5 Implications for Our Research

Our present examination of NBA teams' player-acquisition decisions reveals a few relevant implications for our research. First, it appears that team decision-making is, to a large degree, a collaborative process — while it may be difficult to pinpoint exactly who does what, the influence of first-hand experience on a team's player-acquisition decisions does not appear to arise exclusively from the experiences and decisions of any single employee. This collaborative aspect of the first-hand experience effects reported in this paper represents a unique contribution to the literature, as the empirical settings considered in other studies documenting first-hand experience effects focus on individual decision-makers drawing on their own personal experience.

However, other relevant implications for our research are less encouraging, as the lack of data on teams' learning and decision-making processes mainly serve to illustrate the obstacles to understanding the effect of teams' first-hand experiences at a more micro-level. In particular, our present examination of NBA teams' player-acquisition decisions reveals the difficulty in identifying for a given team (let alone for all observations in our sample): (i) which team employee(s) actually observed the player's performance in each versus game and each preparation game, (ii) which employee(s) were responsible for the final decision to acquire (or not acquire) the player, and (iii) how and when the latter employee(s) may have learned from the former employee(s).

Without these data constraints we could, hypothetically, have explored deeper questions relating to the effects of teams' first-hand experiences. For example, is the effect of first-hand experience stronger (and if so, by how much) if player-acquisition decisions are made by employees who personally observe versus and preparation games — as opposed to learning about players' performance in versus and preparation games from other employees? As another example, how does the aggregation of first-hand experience across employees affect the strength of the effect? (For instance, as a simple illustration, if one employee observes an average performance by a given player and another employee observes a better-than-average performance, how much weight does the better-than-average performance carry in their composite evaluation?)

The data constraints highlighted above also exacerbate the challenges of meaningfully exploring the impact of employee turnover on first-hand experience effects. For example, we would have liked to have been able to test whether, in the event that a relevant team employee was recently employed by a different team, the team's player-acquisition decisions are affected by that employee's past first-hand experiences while employed by his or her former team. We would have also liked to test whether first-hand experience effects disappear when the employee(s) responsible for a team's player-acquisition decisions is/are replaced.[53]

With that said, even if the aforementioned data constraints were overcome, it is still unlikely that we would have been able to learn much from these tests. In fact, before we gained a sufficient appreciation of the these constraints, we naively attempted both of the tests mentioned above using data on teams' owners, general managers, and coaches during our sample period. There were several data quality problems (including ambiguity regarding the precise timing of relevant personnel changes and whether or not certain player transitions happened before or after a change in personnel), but suffice to say every test we performed had the same result: a treatment effect — either referring to the estimated effect of first-hand experience for teams with recently-replaced personnel or the estimated effect of first-hand experience from a recently-hired employee's previous team on current decision-making — that, due to a lack of statistical power, was both statistically indistinct from the main estimated effect of first-hand experience and statistically indistinct from zero.

References Cited in Appendix C

Agness, Scott, "Advance Scout Jimmy Powell" (2016), podcast interview, vigilantsports.com/ep-34-advance-scout-jimmy-powell/ (accessed April 18, 2018).

Dunleavy, Mike, and Eyen, Jim, "Player Development" in *NBA Coaches Playbook: Techniques, Tactics, and Teaching Points*, Ed. Giorgio Gandolfi, Champaign, IL: Human Kinetics (2009), 287–298.

Feldman, Dan "Rumor: Kings Management Wanted to Draft Elfrid Payton, Owner Vivek Ranadive Overruled for Nik Stauskas" (June 1, 2015), online article, nba.nbcsports.com/2015/

[53] To illustrate the problems with such a test, suppose a particular team employee is fired. We would then expect the effect of first-hand experience to disappear if all of a team's first-hand experiences occurring prior to the termination were experienced by that employee. However, this is highly unlikely considering any single employee is one of many with a potential role in decision-making. Along similar lines, the employee's replacement may have been promoted from within, as is often the case when a team fires a general manager or head coach, and thus could very well have already been exposed to the same experiences. Furthermore, even an external replacement could effectively 'catch up' on the team's past first-hand experiences by watching — or consulting retained employees who have watched — past versus and preparation games.

06/01/rumor-kings-management-wanted-to-draft-elfrid-payton-owner-vivek-ranadiv-overruled-for-nik-stauskas/ (accessed May 26, 2018).

Keeley, Sean, "Lebron James, Cleveland's Real GM, Approves of the Cavaliers' Trades," (February 9, 2018), online article, thecomeback.com/nba/lebron-james-cleveland-real-gm-approves-trades (accessed May 26, 2018).

Martin, Jill, "Magic Johnson Named Lakers President of Basketball Operations" (March 31, 2017), online article, www.cnn.com/2017/02/21/sport/magic-johnson-lakers-president (accessed May 26, 2018).

Mazzeo, Mike, and Gutierrez, Bryan, "Mark Cuban: Shot at Prokhorov?" (Jan 24, 2014), online article, www.espn.com/dallas/nba/story/_/id/10347460/dallas-mavericks-owner-says-absentee-owners-bad-game (accessed May 14, 2018).

McGee, Ryan, "So Unlike Mike" (September 4, 2012), online article, www.espn.com/nba/story/_/id/8336863/michael-jordan-ultimate-nba-failure-owner-charlotte-bobcats-espn-magazine (accessed May 27, 2018).

Powell, Shaun, "Balancing Dual Roles of Coach, GM a Tough Act to do in NBA" (January 12, 2017), online article, www.nba.com/article/2017/01/12/coaching-running-team-tough-balancing-act (accessed May 26, 2018).

Smith, Keith, "So You Want to be an NBA Scout..." (August 1, 2016), online article, basketball.realgm.com/article/243008/So-You-Want-To-Be-An-NBA-Scout (accessed May 28, 2018).

D Results with Non-Switchers

In this appendix, we re-estimate the effect of teams' first-hand experiences using an alternate sample specification that includes players who do not change teams, but excludes players who change teams during the regular season. The exclusion of players who change teams during the regular season allows us to treat the offseason as a discrete period, occurring once per year, in which every player may or may not change teams. As a result, the number of potential times at which a player may change teams — and hence, the number of observations when including players who do not switch teams — decreases by two orders of magnitude.[54]

As seen in Table D1, the estimated coefficients of interest in our controlled regressions with the expanded sample remain positive and statistically significant. This suggests that our main results in Table 3 are not reliant on the exclusion of players who do not switch teams from our main sample. Note, for comparison, in our main sample (which included both regular season and offseason transactions, but excluded players who did not switch teams) the estimated coefficients of our controlled regressions (as reported in Table 3) were 0.028 for the overall effect, 0.017 for the versus effect, and 0.013 for the preparation effect.

[54] There are far fewer observations when we exclude regular season observations because a player may (in principle) begin playing for a new team on any day of the regular season — yet in the vast majority of days remains on the same team from the day before.

Table D1: 'Offseason' Sample Estimates

Mean performance deviation in versus & preparation games	0.030 (.006) [<.001]		0.054 (.006) [<.001]	
... in versus games only		0.019 (.005) [<.001]		0.030 (.005) [<.001]
... in preparation games only		0.013 (.005) [.005]		0.026 (.004) [<.001]
Include Non-Switchers?	No	No	Yes	Yes
Observations	77,635	77,635	257,485	257,485

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

E Recent Serious Injuries to Key Players

Generally speaking, rushed decisions may be more susceptible to bias. In our context, this may apply to a team that unexpectedly experiences a serious injury to a key player, since the team may then be compelled to acquire a new player on relatively short notice to help fill the role of the injured player. Furthermore, acquiring a new player for this purpose may be a particularly urgent priority for a team that is in contention for the playoffs, which generate substantial additional revenues for qualifying teams. With these ideas in mind, this appendix explores the possibility that the effect of first-hand experience bias on teams' player-acquisition decisions may be stronger among such teams.

Since injuries are not observed in our data, we construct a proxy measure that classifies a team as being likely to have experienced a recent, serious injury to a key player (RSIKP) if and only if at least one player (a) played for that team in his last game of the season; (b) played his last game of the season within the last 1 to d days; and (c) played at least m minutes per game up to his last game of the season. Here, d determines how long a (presumed) injury remains "recent," while m determines the minimum utilization to qualify as a "key player." While it is debatable what constitutes a *recent* injury or a *key* player, we consider $d \in \{10, 30\}$ and $m \in \{10, 20\}$ to accommodate both strong and weak notions.[55]

[55] Since teams cannot acquire new players at the very end of the season (and recalling observations are

Table E1: Recent Serious Injuries to Key Players (RSIKP)

Mean perf. deviation in vs. & prep. games, without RSIKP	0.028 (.005) [<.001]	0.030 (.005) [<.001]	0.029 (.005) [<.001]	0.032 (.005) [<.001]
Mean perf. deviation in vs. & prep. games, with RSIKP	0.032 (.026) [.231]	-0.008 (.019) [.696]	0.007 (.023) [.771]	-0.010 (.016) [.534]
Observations	126,404	126,404	126,404	126,404
...in vs. games only, without RSIKP	0.017 (.004) [<.001]	0.018 (.004) [<.001]	0.018 (.004) [<.001]	0.018 (.004) [<.001]
...in vs. games only, with RSIKP	0.017 (.027) [.540]	0.006 (.018) [.752]	0.005 (.021) [.821]	0.009 (.014) [.529]
...in prep. games only, without RSIKP	0.013 (.004) [.002]	0.014 (.004) [.001]	0.013 (.004) [.001]	0.015 (.004) [<.001]
...in prep. games only, with RSIKP	0.015 (.023) [.505]	-0.012 (.016) [.473]	0.005 (.017) [.756]	-0.014 (.012) [.253]
Observations	126,404	126,404	126,404	126,404
Recency cutoff (d)	10 days	10 days	30 days	30 days
Minutes cutoff (m)	10 mpg	20 mpg	10 mpg	20 mpg

Each column includes estimates from two regressions: one that combines and one that separates versus and preparation games. All regressions include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

Table E1 reports the estimated effects of first-hand experience for teams with and without the RSIKP classification. In all four specifications, the RSIKP estimates are *not* statistically significant. However, in three of the four specifications, the estimates are also statistically indistinct from the corresponding non-RSIKP estimates — which *are* positive and statistically significant in every case.[56] The fact that the RSIKP estimates are both indistinct from zero and (in most cases) indistinct from the non-RSIKP estimates likely reflects our limited statistical power when estimating the effect of first-hand experience among RSIKP teams — notably, the share of observations in this category was quite low, ranging from 2.5 percent with $d = 10$ and $m = 20$ to 9.8 percent with $d = 30$ and $m = 10$ — and indicates

defined at the time of a transaction), the requirement that the presumed injured player has already played his last game of the season ensures that the player misses a substantial number games, and thus qualifies as a "serious" injury. While a player may also stop playing if he is cut from a team or demoted to a "development league" affiliate, many of these players are likely excluded by our minimum minutes-per-game requirement.

[56] In the specification with $d = 10$ and $m = 20$, the RSIKP estimate *is* statistically distinct, but is in fact *smaller* than the non-RSIKP estimate.

that we cannot draw strong conclusions from the results in Table E1 as to whether the effect of first-hand experience was stronger among RSIKP teams. At the very least, however, the results do suggest that the effect was not primarily driven by teams' rushed responses to unexpected injuries since the non-RISKP estimates were, in all cases, positive, highly significant, and nearly identical to the estimates in Table 3.

As noted earlier, teams that are in contention for the playoffs may feel a greater sense of urgency to acquire a new player in response to an injury of an existing player, to avoid falling behind other teams in playoff contention. With this in mind, Table E2 presents results from tests in which the effects of first-hand experience are estimated separately for RSIKP teams in the bottom, middle, and top winning percentage tertiles of our sample.[57] Presumably, this roughly separates lower-tier teams that are not in contention for the playoffs, middle-tier teams that are near the threshold of qualifying for the playoffs, and higher-tier teams that may be in contention to win a NBA championship.

While our continued lack of statistical power prevents us from drawing strong conclusions from this analysis, we do find intriguing differences in our RSIKP estimates across the different tiers. Namely, while the estimated (overall) effect of first-hand experience is never positive and significant for the bottom- and middle-tier RSIKP teams, it is positive and significant for the top-tier RSIKP teams in three of the four specifications we considered. This raises the possibility that the best teams may be disproportionately susceptible to biased decision-making in response to an injury. However, based on our results, we still cannot be too confident that the effect for top-tier RSIKP teams is meaningfully different from the effect for non-RSIKP teams.

[57] Here, we used winning percentages at the end of the season in which the transaction occurred. The cutoffs between tertiles were .415 and .561.

Table E2: RSIKP Estimates by Contention Status

Mean perf. deviation in vs. & prep. games, without RSIKP	0.028 (.005) [<.001]	0.030 (.005) [<.001]	0.029 (.005) [<.001]	0.032 (.005) [<.001]
...in vs. & prep. games bottom win % tertile, with RSIKP	-0.020 (.034) [.570]	-0.009 (.027) [.737]	-0.043 (.036) [.226]	-0.060 (.026) [.021]
...in vs. & prep. games middle win % tertile, with RSIKP	0.074 (.060) [.218]	-0.031 (.038) [.414]	0.016 (.048) [.737]	-0.011 (.032) [.737]
...in vs. & prep. games top win % tertile, with RSIKP	0.083 (.039) [.034]	0.025 (.036) [.482]	0.066 (.031) [.032]	0.047 (.022) [.032]
Observations	126,404	126,404	126,404	126,404
...in versus games only, without RSIKP	0.017 (.004) [<.001]	0.018 (.004) [<.001]	0.018 (.004) [<.001]	0.018 (.004) [<.001]
...in versus games only, bottom win % tertile, with RSIKP	-0.014 (.034) [.680]	-0.014 (.026) [.593]	-0.039 (.031) [.200]	-0.035 (.022) [.120]
...in versus games only, middle win % tertile, with RSIKP	0.032 (.051) [.525]	-0.008 (.028) [.770]	0.007 (.030) [.817]	0.020 (.026) [.432]
...in versus games only, top win % tertile, with RSIKP	0.051 (.048) [.282]	0.047 (.032) [.139]	0.054 (.034) [.106]	0.044 (.021) [.038]
...in prep. games only, without RSIKP	0.013 (.004) [.002]	0.014 (.004) [.001]	0.013 (.004) [.001]	0.015 (.004) [<.001]
...in prep. games only, bottom win % tertile, with RSIKP	-0.011 (.036) [.763]	-0.001 (.025) [.954]	-0.012 (.027) [.664]	-0.037 (.020) [.069]
...in prep. games only, middle win % tertile, with RSIKP	0.052 (.041) [.197]	-0.008 (.025) [.757]	0.018 (.032) [.576]	-0.015 (.018) [.429]
...in prep. games only, top win % tertile, with RSIKP	0.026 (.038) [.498]	-0.033 (.036) [.362]	0.018 (.032) [.564]	0.012 (.022) [.589]
Observations	126,404	126,404	126,404	126,404
Recency cutoff (d)	10 days	10 days	30 days	30 days
Minutes cutoff (m)	10 mpg	20 mpg	10 mpg	20 mpg

Each column includes estimates from two regressions: one that combines and one that separates versus and preparation games. All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

F Alternate Specifications of Relative Performance

This appendix considers several tests of whether the first-hand experience effects reported in Table 3 are robust to alternate ways of defining a player's relative performance in games providing first-hand experience to a particular team.

F.1 Alternatives to *Mean* Performance Deviation

To begin, recall that our main specification used the *mean* performance deviation to express a player's relative performance in versus and preparation games (as determined by the efficiency metric described in footnote 1). Alternatively, Table F1 shows that the coefficients of interest are still positive and statistically significant if we instead use a player's *total* performance deviation across applicable games. This is also true if we use the *normalized* mean performance deviation, so that its unit is the standard deviation of the player's mean performance deviation across all teams, as shown in Table F2.

Table F1: Estimates Using *Total* Performance Deviation

Sum of performance deviations in versus and preparation games	0.009 (.001) [<.001]	0.007 (.001) [<.001]		
... in versus games only			0.012 (.002) [<.001]	0.009 (.002) [<.001]
... in preparation games only			0.006 (.002) [.001]	0.006 (.002) [.002]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

Table F2: Estimates Using *Normalized* Mean Performance Deviation

Normalized mean perf. deviation in versus and preparation games	0.019 (.003) [<.001]	0.015 (.003) [<.001]		
... in versus games only			0.015 (.002) [<.001]	0.011 (.003) [<.001]
... in preparation games only			0.008 (.003) [.002]	0.008 (.003) [.004]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

F.2 Alternatives to the Efficiency Metric

In our remaining tests, we return to our original use of a player's mean performance deviation to express relative performance. As our underlying measure of performance, however, we consider alternatives to the efficiency metric, mostly drawn from past empirical studies that consider aspects of NBA player performance. Exact expressions for each of the alternative performance measures we consider (and also for the efficiency metric itself) are provided at the end of this appendix.

The first alternative measure we consider is a *weighted composite measure*, hereafter WCM, that assigns weights to various box score statistics based on the composite weighting scheme used by Stiroh (2007) to quantify NBA player performance. There are many differences between WCM and the efficiency metric, several of which reflect ways in which WCM improves upon known weaknesses of the efficiency metric. As one example, efficiency applies equal penalties for turnovers and missed field goals while WCM applies a larger penalty for turnovers, as is arguably appropriate since turnovers guarantee a lost possession for the player's team while missed field goals do not (e.g. a teammate may rebound the miss). As another example, efficiency equally rewards offensive and defensive rebounds, while WCM assigns a higher weight for offensive rebounds — which makes sense from the standpoint that it is less likely that a player's teammate would have secured an offensive (as opposed to a defensive) rebound if the rebound were not secured by the player himself. As seen in Table F3, the estimated effects of first-hand experience remain positive and statistically significant with WCM as our underlying measure of performance.

Next, we consider *points* as our measure of performance. Unlike most of the other alternative performance measures considered in this appendix, points is clearly simpler than efficiency and likely a worse measure of overall performance. That said, points was previously used as a measure of NBA player performance by Barnes et al. (2012), who argue that teams "focus on points scored as the primary criterion on which NBA players are evaluated," as points scored accounts for over half of the variance in players' compensation. Here, it might be puzzling if a first-hand experience effect was not observed with points as our measure of performance, as this would suggest that the previously-observed first-hand experience effects

Table F3: Estimates with the Weighted Composite Metric

Mean WCM deviation in vs. and prep. games	0.041 (.006) [<.001]	0.036 (.007) [<.001]		
... in versus games only			0.029 (.005) [<.001]	0.023 (.005) [<.001]
... in preparation games only			0.014 (.004) [.001]	0.015 (.005) [.004]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

See equation (9) for the definition of WCM (based on Stiroh, 2007). Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

were driven entirely by other, less salient box score statistics. However, as seen in Table F4, the estimates of interest remain positive and statistically significant.

Table F4: Estimates with Points

Mean points deviation in vs. and prep. games	0.035 (.005) [<.001]	0.029 (.007) [<.001]		
... in versus games only			0.027 (.004) [<.001]	0.021 (.005) [<.001]
... in preparation games only			0.011 (.004) [.010]	0.011 (.005) [.026]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

The fact that we observe first-hand experience effects with points as our performance measure naturally raises the question of whether other aspects of performance play a role. While initially motivated as a test of an alternate "team showcasing" hypothesis in Section 5.3, the next two sets of results are also helpful for addressing this question. In particular, Table F5 shows that the estimates are still positive and statistically significant when using a "non-scoring" version of the efficiency metric that is based only on a player's assists, rebounds, blocks, steals, and turnovers. If assists, which directly lead to points, are also excluded from this measure, the point estimates are virtually identical (while the estimates of the individual versus and preparation effects are no longer significant at the 5% level, the overall effect of first-hand experience remains significant at the 1% level). See Table F6.

Table F5: Estimates with Non-Scoring Efficiency, Including Assists

Mean perf. deviation in vs. and prep. games	0.046 (.008) [<.001]	0.033 (.010) [.001]		
…in versus games only			0.030 (.007) [<.001]	0.016 (.008) [.038]
…in preparation games only			0.016 (.006) [.010]	0.016 (.007) [.027]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

See (13) for the definition of the performance metric used in these regressions. Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

Table F6: Estimates with Non-Scoring Efficiency, Excluding Assists

Mean perf. deviation in vs. and prep. games	0.049 (.011) [<.001]	0.033 (.012) [.005]		
…in versus games only			0.031 (.008) [<.001]	0.015 (.009) [.097]
…in preparation games only			0.018 (.008) [.027]	0.016 (.009) [.067]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

See (14) for the definition of the performance metric used in these regressions. Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

The next two measures we consider are motivated by estimates of the effects of various box score statistics on other measures of a player's total value (in contrast to WCM and points, which were simply taken as proxies for a player's value in the original studies from which they were drawn). The first of these two measures, referred to as *implied win value* (IWV), is formed using the estimated effects of various box score statistics on team wins in Hofler and Payne's (2006) regression.[58] Unlike the other alternative performance measures considered in this appendix, IWV is quadratic and thus provides a check on the other, linear measures.

[58] Specifically, IWV uses the estimated weights in the first column of Hofler and Payne's (2006) Table 1.

That said, IWV also has some odd properties. Most notably, it actually *penalizes* offensive rebounds and assists (by contrast, none of the other performance measures considered in this appendix penalize "good" statistics such as these). In any event, Appendix Table F7 reports estimates with IWV as our measure of performance. While the estimated preparation effect is no longer significant at the 5% level (with $p \approx .08$), all estimates of interest are still positive, with the estimates of the versus effect and of the overall effect of first-hand experience still significant at the 0.1% level.

Table F7: Estimates with Implied Win Value

Mean IWV deviation in vs. and prep. games	0.004 (.001) [<.001]	0.003 (.001) [.002]		
...in versus games only			0.003 (.001) [<.001]	0.002 (.001) [.006]
...in preparation games only			0.001 (.001) [.029]	0.001 (.001) [.079]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

See equation (10) for the definition of IWV (based on Hofler and Payne, 2006). Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

The next measure, referred to as *implied contract value* (ICV), uses the estimated effects of various box score statistics on a player's contract value as estimated by Stiroh (2007).[59] While ICV is, in several ways, simpler than WCM — ICV ignores all box score statistics except for points, rebounds, assists, and blocks — Stiroh (2007) finds ICV to be a better predictor of contract value. In any event, the coefficients of interest remain positive and statistically significant with ICV, as seen in Table F8.

Our final measure of performance, referred to as *linearized player efficiency rating* (L-PER) is based on the well-known PER metric used to capture a player's overall performance over an entire NBA season. In particular, L-PER provides an approximation of PER that uses linear weights assigned to various box score statistics (as described in Fein's, 2009, article on calculating PER "without all the mess"), and unlike PER it is well-defined for

[59] In particular, ICV uses the estimated weights of a player's past performance statistics on contract value reported in the second column of Table 2 in Stiroh (2007).

Table F8: Estimates with Implied Contract Value

Mean ICV deviation in vs. and prep. games	0.247 (.038) [<.001]	0.206 (.050) [<.001]		
... in versus games only			0.182 (.033) [<.001]	0.130 (.042) [.002]
... in preparation games only			0.074 (.031) [.015]	0.086 (.039) [.028]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

See (11) for the definition of ICV (based on Stiroh, 2007). Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

individual games, and thus readily usable as a measure of (relative) performance in a team's versus and preparation games. As with WCM, L-PER penalizes turnovers more than missed field goals and rewards offensive rebounds more than defensive rebounds, and thus alleviates the problematic equal weighting of such measures by the efficiency metric (as previously discussed). In addition, L-PER alleviates the inherent over-penalization of missed free throws relative to missed field goals by the efficiency metric, whereby a missed free throw and a missed field goal receive equal penalties even though one missed field goal (roughly speaking) has the same effect as two missed free throws. As seen in Table F9, the estimates of interest with L-PER are positive and statistically significant.

Table F9: Estimates with Linearized Player Efficiency Rating

Mean L-PER deviation in vs. and prep. games	0.045 (.006) [<.001]	0.039 (.007) [<.001]		
... in versus games only			0.031 (.005) [<.001]	0.024 (.005) [<.001]
... in preparation games only			0.017 (.005) [<.001]	0.017 (.005) [.001]
Controls?	No	Yes	No	Yes
Observations	126,404	126,404	126,404	126,404

See equation (12) for the definition of L-PER. Standard errors are in parentheses and p-values are in square brackets. Baseline estimates of the conditional logit player-team matching model are provided in Table 3.

F.3 Definitions of Performance Measures

Here, we provide precise definitions of the alternate performance measures considered in this appendix. Starting with the simplest metric considered, points (PTS) can be expressed in terms of 2-point field goals (2FG), 3-point field goals (3FG), and free throws (FT) as

$$\text{PTS} = 2 \cdot \text{2FG} + 3 \cdot \text{3FG} + \text{FT}. \tag{8}$$

Next, the weighted composite measure is given by:

$$\begin{aligned} \text{WCM} = {} & 1.4 \cdot \text{FG} + 1.4 \cdot \text{BLK} + \text{FT} + \text{AST} + \text{STL} + .85 \cdot \text{ORB} \\ & + .5 \cdot \text{DRB} - .8 \cdot \text{TO} - .318 \cdot \text{PF} - .6 \cdot (\text{FGA} - \text{FG}), \end{aligned} \tag{9}$$

where FG = 2FG + 3FG is total made field goals, BLK denotes blocked shots, AST denotes assists, STL denotes steals, ORB denotes offensive rebounds, DRB denotes defensive rebounds, TO denotes turnovers, PF denotes personal fouls, FGA denotes field goals attempted, and FTA denotes free throws attempted (implying FGA − FG is the number of missed field goals and FTA − FT is the number of missed free throws). Note, WCM as defined here (and in Stiroh, 2007) treats 3-point field goals the same as 2-point field goals, even though 3-point field goals are worth three points instead of two.

Next, implied win value (IWV) can be expressed as

$$\begin{aligned} \text{IWV} = {} & 1.67 \cdot \text{FG} + .121 \cdot \text{FT} - .11 \cdot \text{ORB} + 1.46 \cdot \text{DRB} + .01 \cdot \text{AST} \\ & + .191 \cdot \text{STL} - .89 \cdot \text{TO} + .0042 \cdot \text{BLK} + 1.21 \cdot \text{FG}^2 + .639 \cdot \text{FT}^2 \\ & - .94 \cdot \text{ORB}^2 + .35 \cdot \text{DRB}^2 - .51 \cdot \text{AST}^2 + .31 \cdot \text{STL}^2 + .32 \cdot \text{TO}^2, \end{aligned} \tag{10}$$

As seen here (and noted earlier), IWV actually penalizes assists and offensive rebounds.[60]

Letting REB = ORB + DRB denote total rebounds, implied contract value is given by

$$\text{ICV} = .047 \cdot \text{PTS} + .158 \cdot \text{REB} + .210 \cdot \text{AST} + .186 \cdot \text{BLK}. \tag{11}$$

[60] While the coefficient on AST is positive, the larger (in magnitude) negative coefficient on AST^2 implies that an increase from 0 to 1 assist (and any increases thereafter) will cause IWV to decrease.

In turn, linearized player efficiency rating is given by:

$$\begin{aligned}\text{L-PER} = 1.591 \cdot \text{2FG} + 2.549 \cdot \text{3FG} + .868 \cdot \text{FT} + .998 \cdot \text{STL} + .726 \cdot \text{BLK} \\ + .642 \cdot \text{AST} + .726 \cdot \text{ORB} + .272 \cdot \text{DRB} - .318 \cdot \text{PF} \\ - .372 \cdot (\text{FTA} - \text{FT}) - .726 \cdot (\text{FGA} - \text{FG}) - .998 \cdot \text{TO}.\end{aligned} \tag{12}$$

To facilitate comparisons, the efficiency metric (as described in footnote 1) is given by

$$\text{EFF} = \text{PTS} + \text{REB} + \text{AST} + \text{BLK} + \text{STL} - \text{TO} - (\text{FGA} - \text{FG}) - (\text{FTA} - \text{FT}).$$

Note, another inherent weakness of the efficiency metric (besides those discussed earlier): personal fouls are not penalized (unlike WCM and L-PER). Lastly, the (non-scoring) versions of the efficiency metric considered in Tables F5 and F6 are (respectively):

$$\text{EFF}^{*} = \text{REB} + \text{AST} + \text{BLK} + \text{STL} - \text{TO}, \tag{13}$$

$$\text{EFF}^{**} = \text{REB} + \text{BLK} + \text{STL} - \text{TO}. \tag{14}$$

G The Magnitude of the Bias: Additional Exercises

Building on our analysis in Section 6, this appendix considers additional exercises to help us gain a better sense of the magnitude of the first-hand experience bias. Specifically, we first estimate the dollar-value welfare loss caused by the first-hand experience bias (Appendix G.1), then consider various ways of relating the bias to team wins (Appendix G.2), and lastly compare the effect to the (estimated) effects of other factors that affect team decision-making (Appendix G.3).

G.1 The Dollar-Value Welfare Loss

To quantify the dollar-value welfare loss caused by the first-hand experience bias, we draw on our simulation results from Section 6.2, which indicated that the bias causes 2.3% of players to be mismatched in the sense that the team a player was matched to by our model differed from the team the player was matched to by an "optimal" model with the coefficient γ on a player's mean performance deviation in games providing first-hand experience set to zero (see Table 6). In principle, if the optimal model predicts a player will be matched to team b (using "team" loosely to include the no-match outcome) and the estimated model predicts the player will be matched to team $a \neq b$, then the difference between team b's and team a's unbiased valuations can be interpreted as the welfare loss from that player being mismatched to team a instead of team b.

To help us re-express such welfare losses in dollar units, we divided the total revenues of all NBA teams by the number of active players for each year in our sample, giving us a rough upper bound on the mean value generated by all players during that year. Since NBA rules restrict the total salary of all players to be roughly 50% of total revenues from the previous year, we can (assuming players are not paid more than they are worth) also get a rough lower bound on players' mean value in a given year by dividing 50% of the previous year's revenues by the number of players. By equating these bounds (averaged across all years in our sample) to the mean unbiased valuation of the player-team matches predicted by the estimated model (averaged across all players in our sample except for those who remain unmatched in the simulations), we get a crude conversion factor that allows us to re-express the welfare loss from a mismatched player in terms of a range of dollar values. Using this procedure, we estimate an average welfare loss between \$305,861 and \$610,189 (in 2015 dollars) per mismatched player.[61] While these figures may seem alarmingly large, they represent just 8.3 percent of the mean value per player during our sample period.

G.2 Relating the Bias to Team Wins

By drawing on a past empirical estimate of the monetary value of a win in the NBA, the estimated dollar-value welfare loss associated with the first-hand experience bias can be readily translated to its implied cost in terms of team wins. In particular, Price et al. (2010) estimate that the value of a win in the 2007/08 NBA season was \$197,304, which implies that our estimated dollar-value welfare loss corresponds to a decrease of 2.12 to 4.24 wins per

[61] These dollar-value estimates should only be interpreted as crude approximations. One issue is that we calculated our conversion factor by equating the mean value of players in our sample to the mean value of all NBA players. In practice, more valuable players likely change teams less frequently, in which case the mean value of players in our sample would be less than the mean value of all players, implying our estimated welfare impacts would be biased upwards. With that said, these estimates may also be biased downward since we are equating a team's valuation when acquiring a player to the dollar value generated by a player in a single year. Since NBA players often remain with the same team for multiple years, a player's true value would often exceed his value in a single year.

mismatched player in our sample.[62] Note, however, since these bounds are calculated based on indirect translations of our dollar-value welfare estimates, they too should be interpreted as crude approximations (with limitations noted in footnote 61). In any event, this range can also be expressed as corresponding to a 0.91 to 1.83 percentage-point decrease in a team's winning percentage in a given year.[63] Of course, however, the aggregate effect of the bias on teams' winning percentages must be zero (since the average winning percentage across all teams must be 50 percent). Thus, on average, the decrease in a team's winning percentage attributable to their own bias is offset by an increase in their winning percentage attributable to their opponents' biases.

Next, we consider other, more direct methods of relating a team's first-hand experience bias to wins. For this purpose, we let $B_{jt} = \sum_{i \in \mathcal{A}_{jt}} \overline{\text{PD}}_{ijt}$ denote team j's total bias in season t, where $\mathcal{A}_{jt}$ is the set of players acquired by team j in season t and $\overline{\text{PD}}_{ijt}$ is player i's mean performance deviation in games providing first-hand experience to team j in the year before the acquisition. Team j is then considered "more biased" than team k in season t if $B_{jt} > B_{kt}$ and "less biased" if $B_{jt} < B_{kt}$.

Table G1 reports the winning percentage of the more biased team in a given game, for all applicable games during our sample period. As seen, if teams' biases are calculated based on acquired players' past performances in versus and preparation games, the winning percentage of the more biased team is 1.13 percentage points below the league average of 50 percent. As a point of reference, this difference is on par with Price and Wolfers' (2010) analogous estimates relating team wins to the racial composition of NBA refereeing crews, whereby replacing a black referee with a white referee is found to be associated with a one percentage point decrease in the likelihood of winning by the team with a higher utilization of black players in a given game.

Also of note, the reported relationship between the first-hand experience bias and wins

[62] Specifically, these estimates were calculated as $\frac{D}{C \cdot V_{07/08} \cdot R_{07/08}}$, where $D \in \{\$305{,}861; \$610{,}189\}$ is the dollar-value welfare loss per mismatched player, $V_{07/08} = \$197,304$ is the estimated value of a win in the 2007/08 NBA season from Price et al. (2010), $C = 1.101$ is an inflation-adjustment factor that converts 2008 dollars (the unit of $V_{07/08}$) to 2015 dollars (the unit of D), and $R_{07/08} = .663$ is the ratio of the average annual league revenues in the years covered by our sample to league revenues in the 2007/08 season.

[63] These estimates are translated from the 2.12 to 4.24 reduction in wins per mismatched player based on a 2.3 percent mismatch rate, as estimated in Section 6.2, a 15-player roster, and an average of 80.4 games per season over our full sample period.

Table G1: Records of More and Less Biased Teams

If teams' total biases calculated using past	More biased team's win %	Relative to average	Obs.
...vs. & prep. games	48.87	-1.13	37,139
...versus games only	47.96	-2.04	37,139
...prep. games only	49.85	-0.15	37,139

The number of observations is the number of games in our sample. See the text for details on how teams' total biases were calculated.

in Table G1 is an order of magnitude *smaller* if teams' biases are calculated based only on past preparation games. The weakening of this relationship makes sense in light of our finding that the preparation effect is stronger for a team that performs relatively well in its subsequent versus game (see Appendix H). That is, greater attentiveness to a preparation game presumably amplifies the influence of first-hand experience in that game while also improving the team's preparation for (and thus, likelihood of winning) its next game. In turn, the positive association between the preparation effect and a team's performance in its subsequent versus game may offset much of the presumed negative effect of the bias on winning arising through suboptimal player-acquisition decisions.

Next, Table G2 presents estimates relating a team's total first-hand experience bias in a particular season to its winning percentage in that same season, as estimated in regressions with and without team and season fixed effects. In all specifications, we (again) find a negative relationship between the bias and wins, except in cases where teams' biases are calculated based only on past preparation games with recently-acquired players. As before, the absence of an observed relationship in these cases is not surprising in light of the aforementioned positive association between the preparation effect and the team's performance in the subsequent versus game.

Table G2: Relating the Bias to Team Wins

	Dependent variable in OLS regression: team's win %					
Team bias from past versus & prep. games	-0.187 (.064) [.004]	-0.128 (.064) [.047]	-0.136 (.068) [.046]			
...from past versus games only				-0.184 (.048) [<.001]	-0.130 (.048) [.007]	-0.138 (.050) [.006]
...from past prep. games only				-0.000 (.052) [.998]	0.007 (.050) [.895]	0.008 (.053) [.884]
Team Fixed Effects?	No	Yes	Yes	No	Yes	Yes
Season Fixed Effects?	No	No	Yes	No	No	Yes
Observations	877	877	877	877	877	877

Here, a team's win percentage and its (total) bias in past versus and/or preparation games are implicitly expressed for a single season. See the text for details on how teams' biases were calculated. Standard errors are in parentheses and p-values are in square brackets.

Focusing on the remaining estimates in which the bias is calculated based on past versus games (with or without preparation games), we see these estimates are relatively unaffected by season fixed effects, but noticeably smaller in magnitude (roughly 2/3 the size) with team fixed effects included in the regression. This suggests that the relationship between a team's bias and its winning percentage in a particular season stems in part from a tendency for teams with lower winning percentages over our full sample period to exhibit a larger first-hand experience bias. In light of this, the team fixed effects may be a useful control if the larger biases among teams with persistently lower winning percentages are due to random variation (i.e. if the lower winning percentages among such teams are attributable to factors unrelated to the bias, such as being in a location that players find less appealing, having an owner who is less willing to spend on players, or even bad luck with injuries or draft position). That said, to the extent that the persistently lower winning percentages among these teams stem from a persistently elevated bias, the estimates without team fixed effects may be more indicative of the underlying relationship. In any event, the range of estimates imply that the bias is, on average, associated with a 0.36 to 0.60 percentage-point reduction in a team's winning percentage.[64] While smaller, these estimates are on the same order of

[64] The lower bound of this range is calculated as the product of the absolute value of the estimate in column 2 of Table G2 (.128), the average, per-player mean performance deviation in past versus and preparation games with the player's future new team (.54), and the average number of players acquired by a given team

magnitude as the previous estimates based on different methodologies.

G.3 Comparison to the Estimated Effects of Relevant Control Variables

As noted in Section 4.1, our main estimations revealed three control variables with highly-significant effects on teams' player-acquisition decisions. The left-most column of Table G3 reproduces the estimated coefficients for each of these variables, along with our estimate of the overall effect of first-hand experience in versus and preparation games, based on our estimation results presented in the second column of Table 3. As seen, the coefficient capturing the effect of first-hand experience is the smallest in magnitude, while the coefficient capturing the effect of being located in a player's birth state is the largest. As discussed in Section 5, the observation that players are more likely to be acquired by teams that are located in their birth state could reflect a preference among teams for local players or a preference among players for joining teams located near their hometown. The other two effects may capture a tendency for worse teams (which tend to win fewer games and allow better performances by opposing players) to acquire new players at a higher rate in order to improve their roster.

Table G3: Comparison of Effect Sizes

	estimated coefficient	std. dev. of indep. var.	coefficient × std. dev.
Mean perf. deviation in vs. & prep. games	.028	3.22	.090
Team win %	-.259	0.15	-.039
Mean performance of all who played team	.106	0.83	.088
Team in birth state	.389	0.20	.079

One limitation of comparing the estimated coefficients listed in Table G3 is that the relevant independent variables are not necessarily expressed on a comparable scale.[65] Related

in a given season during our sample period (5.21). Similarly, the upper bound is the product of the absolute value of the top estimate in the fourth column of Table G2 (.184), the average, per-player mean performance deviation in past versus games with the player's future new team (.63), and the average number of players acquired by a given team in a given season during our sample period (5.21).

[65] For instance, if we expressed the control for team winning percentage on a scale from 0 to 100 instead of 0 to 1, the estimated coefficient would be $-.00259$ instead of $-.259$, even though its actual impact on teams' player-acquisition decisions would be unchanged.

to this, the overall impact of each effect on teams' player-acquisition decisions will also depend on the extent of sample variability for the associated independent variable. To help account for these differences, the standard deviation of each independent variable is listed in the second column of Table G3, while the product of these standard deviations and the corresponding estimated coefficients are listed in the third column. These final values can then be interpreted as the estimated effect of a standard deviation increase in the associated independent variable.[66] As seen, by this measure the size of the observed first-hand experience effect is on the same order of magnitude as the other three effects.

H Team Preparation and Achievement

Our interpretation of the preparation effect as a first-hand experience bias is predicated on the idea that teams pay extra attention to players' performances in games that immediately precede games against their own team. In practice, the level of a team's preparation for an opponent — and hence, the level of attention to that opponent's previous game — could vary from game to game. While we cannot directly observe a team's level of preparation for a given opponent, it would naturally correlate with the team's performance against that opponent. That is, a more prepared team is (all else equal) more likely to perform well than a less prepared team.

Building on this idea, we now examine whether the strength of the preparation effect relates to the team's performance relative to expectations in the subsequent versus game. To do this, we use data from www.covers.com indicating the odds-on 'favorite' — that is, the team that was expected to win (as implied by gambling markets) — in every regular season game since November 1990.[67] We then define 'underachievement' to mean that the team lost despite being the favorite and 'overachievement' to mean that the team won when its opponent was the favorite.

The results of our regressions using odds-on favorite data are reported in Table H1. Of particular interest, the second column provides separate estimates of the versus and

[66] In other words, the values in the third column represent the coefficients that we would have estimated if all of the independent variables were scaled such that their standard deviations were all equal to one.

[67] Since we could not obtain odds-on favorite data for earlier years, observations based on games played prior to November 1990 were excluded from the sample.

Table H1: Estimates with Odds Data

Mean perf. deviation in versus games given team		
... overachieves in versus game		0.015 (.006) [.016]
... underachieves in versus game		0.016 (.006) [.010]
... all remaining observations	0.019 (.004) [<.001]	0.013 (.004) [.001]
Mean perf. deviation in prep. games given team		
... overachieves in versus game		0.017 (.006) [.004]
... underachieves in versus game		0.004 (.006) [.532]
... all remaining observations	0.010 (.004) [.017]	0.009 (.004) [.021]
Observations	91,156	91,156

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

preparation effects in cases when the team overachieved or underachieved in the versus game, along with all remaining observations, in which case the team won the versus game if it was the favorite and lost if the player's team was the favorite.

As seen by comparing the coefficients in the top three rows, the strength of the versus effect does not meaningfully vary with team achievement in the versus game (a test of whether the three coefficients are equal yields a p-value of .95). However, we do see a relationship with team achievement in our estimates of the preparation effect, as it is significantly larger when the team overachieves in the versus game and significantly smaller (and no longer statistically indistinct from zero) when the team underachieves.

The strengthening of the preparation effect when the team overachieves in the versus game — our proxy for high preparation — and its weakening when the team underachieves — our proxy for low preparation — is consistent with the notion that the preparation effect is indeed driven by a team's attention to a player's performance when preparing for the player's team. Considering the degree to which a team pays attention to an opponent *during* the versus game would presumably not depend on the team's level of preparation for that game, the lack of an analogous relationship between team achievement and the strength of the versus effect is also consistent with the overattention hypothesis.

I Players' Perspectives on Getting Traded

In Section 5.1, we mentioned that NBA players often emphasize their lack of control over when, whether, and where they will be traded. As one example, Patrick Patterson describes learning the hard way that players "are often the last to find out about their own trade" after being "blindsided" when his first team, the Houston Rockets, traded him to the Sacramento Kings. His lack of agency in such matters was perhaps even more conspicuous the second time he was traded: "I'll be honest. I wasn't happy to be going to Toronto ... I remember as the pilot told us that we were about to descend, I pulled up the window shade and all I saw was white. Just white everywhere. I'd been traded to the North Pole" (Patterson, 2015).

Below, we list additional examples of players' perspectives on being traded to another team. While these examples lend credence to the notion that players generally do not have a say in the terms of their trades, there are several high-profile exceptions in which a "star" player has succeeded in demanding a trade to another team from a small set that he had designated as acceptable destinations (see Johnson, 2017).

- *NBA player Ante Zizic recalling his first time being traded (Bulpett, 2018):*

  "You never know when you're going to be traded. But I was preparing for a year mentally to be in Boston, then a couple of days before I was going to Boston, they traded me. It was really was a shock, like, what? When? How?"

- *Former NBA player Glen Davis — as quoted in an article describing how NBA players' "lives can change in an instant" with "little to no control over where they'll play and live" — discussing the prospect of being traded (Robbins, 2014):*

  "It doesn't really scare me. As a player, do you want to know where you're going to go if you get traded? Yeah, you want to know before everybody else knows. But, at the end of the day, this is still a business."

- *NBA player Anthony Tolliver — as quoted in an article suggesting that fans can't relate until they've been "transferred halfway across the country, though, with no notice or any say in the matter, and get planted in a cubicle with all new co-workers in an unfamiliar city" — explaining how teammates discuss the topic of trades (Aschburner, 2017):*

  "Everybody understands how it feels. It's not something we joke about. At least, we won't joke about you being traded. We might joke about ourselves being traded. 'If I'm here next week.' That type of thing. But for the most part, it's not something you joke about. You could be, 'Ha-ha, you're getting traded next week...' And then you get traded. No one knows. Most of the time it just happens."

- *Former NBA player Austin Daye explaining why he did not buy a house while playing for the Detroit Pistons (Burt, 2011):*

  "I don't feel a need to buy a place. In Detroit, there are great houses out there that I just lease. I don't feel there is a need for me to buy a house in Detroit, because you never know when you're going to be traded."

- *NBA player Evan Turner — as quoted in an article describing how NBA players tend to be traded "without much notice to the surprise of the players themselves" — addressing rumors that he might be traded (Blancarte, 2018):*

  "I don't really pay attention to it anymore. You don't really have any control over it. Like, when it actually happens to me, it's worth paying attention to. Rumors are rumors, anyone can make them up."

- *NBA player Taj Gibson addressing rumors that he might be traded from his team at the time, the Chicago Bulls (Friedell, 2018):*

  "Of course [a trade] would hurt because it seems like everybody's family. But at the end of the day it's a business, I totally understand that. Whatever happens is going to happen, it's in God's hands, in the GM's hands. All I can do is be a player and represent whatever team I'm wearing their jersey."

- *NBA player Marcin Gortat describing how it feels to be traded (MacKenzie, 2014):*

  "You're really lost. You don't know anything about the future, you don't know anything about how it's going to be for the next few days, how it's going to be for the rest of your life, how your career is going to look, how you'll adjust to a new team."

- *Former NBA player Quentin Richardson on getting traded four times during the same offseason (Gregory, 2009):*

  "The most difficult part is not knowing where you're going to end up, as far as living, getting settled in and situated. Other than that, it's not that big of a deal. I didn't have to do a whole lot of traveling. It wasn't like I had to go to every city and take a physical. I haven't really gone anywhere. Each time I was traded, I tried to figure out where I fit in with each team. I envisioned myself playing for that team, for however short a time that was. It wasn't a lot of moving. It was just a lot of not knowing."

References Cited in Appendix I

Aschburner, Steve, "Trade Deadline Period Can be Great Entertainment for Fans but Nerve-racking for NBA Players" (February 22, 2017), online article, www.nba.com/article/2017/02/22/trade-deadline-can-be-roller-coaster-ride-nba-players (accessed May 24, 2018).

Blancarte, James, "NBA Daily: Evan Turner on Trade Rumors And The West Playoff Race" (February 5, 2018), online article, www.basketballinsiders.com/nba-daily-evan-turner-on-trade-rumors-and-the-west-playoff-race (accessed May 24, 2018).

Bulpett, Steve, "Ante Zizic Adapts to Life in Cleveland After Blockbuster Deal Away From Celtics" (May 21, 2018), *Boston Herald*.

Burt, Tim, "NBA's Austin Daye returns for Irvine basketball camp" (August 4, 2011), *Orange County Register*.

Friedell, Nick, "Taj Gibson Wants to Stay with Bulls " (July 23, 2014), online article, www.espn.com/chicago/nba/story/_/id/11253489/taj-gibson-chicago-bulls-want-traded-kevin-love-minnesota-timberwolves (accessed May 24, 2018).

Gregory, Sean, "Quentin Richardson, The Human Trading Chip" (August 24, 2009), *Time*.

Johnson, Luke, "Top 15 NBA Superstars Who Forced a Trade" (February 26, 2017), online article, www.thesportster.com/basketball/top-15-nba-superstars-who-forced-a-trade/ (accessed January 4, 2019).

MacKenzie, Holly, "How NBA players Deal with the Mundane Reality of Getting Traded " (February 20, 2014), online article, www.sbnation.com/nba/2014/2/20/5426902/nba-trade-deadline-2014-mundane-rumors (accessed May 24, 2018).

Patterson, Patrick "You've Been Traded" (February 19, 2015), online article, www.theplayerstribune.com/en-us/articles/nba-trade-deadline-patrick-patterson (accessed 5/24/2018).

Robbins, Josh, "As NBA Trade Deadline Nears, Questions Loom on Magic Players' Futures" (February 11, 2014), *Orlando Sentinel*.

J First-Hand Experience in Previous Years

Table J1 reports the results of regressions in which performance deviations from earlier years were included as regressors. The effects of first-hand experience (including the overall, versus, and preparation effects) in the year preceding a player's transition to a new team remain positive and statistically significant. However, the effects do not show much persistence, as the longer-term estimates in Table J1 are, collectively, statistically indistinct from zero (p-values of .60 and .29 in columns 1 and 2, respectively) and statistically distinct from the one-year-prior estimates (p-value $< .001$ in both columns).[68]

[68] Even if we accept at face value the positive (yet statistically insignificant) estimate of the overall effect of first-hand experience 1 to 2 years before the player's transition, it would imply a rapidly decaying effect, as it is roughly 20% as large as the estimate of the effect within one year of the transition.

Table J1: Estimates of Longer-Run Effects

Mean performance deviation in versus & preparation games, 0-1 years before switching teams	0.038 (.007) [<.001]	
...1-2 years before switch	0.005 (.007) [.425]	
...2-3 years before switch	0.008 (.007) [.214]	
...3-4 years before switch	0.001 (.007) [.214]	
Mean performance deviation in versus games, 0-1 years before switching teams		0.025 (.005) [<.001]
...1-2 years before switch		0.002 (.005) [.674]
...2-3 years before switch		0.011 (.005) [.021]
...3-4 years before switch		-0.003 (.005) [.540]
Mean performance deviation in prep. games, 0-1 years before switching teams		0.016 (.005) [.004]
...1-2 years before switch		0.002 (.005) [.637]
...2-3 years before switch		0.000 (.005) [.940]
...3-4 years before switch		0.003 (.005) [.520]
Observations	62,604	62,604

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

K Estimating the Effect of Second-Hand Information

This appendix presents the results from a conditional logit model with a no-match option, while using an expanded sample with players who leave a team but remain unmatched thereafter. As discussed in Section 6.1, the no-match option allows us to estimate the effect of a player's mean performance in all games — i.e. the second-hand performance signal — as well as the effects of the following previously-unidentified control variables (which did not vary within the set of a player's potential new teams): the player's age, the number of games played in the pre-transition year, the length (in years) of the player's career at the time of the transition (for an unmatched player, career length is calculated up until his last game on his original team), the winning percentage of the player's pre-transition team, the player's utilization (in minutes per game) in all games, dummy variables for the number of teams the player has previously played for, and year fixed effects.

Table K1: Results with Unmatched Players

Mean perf. deviation in versus and prep. games	0.020 (.005) [<.001]	
...in versus games only		0.013 (.004) [.002]
...in prep. games only		0.008 (.004) [.045]
Mean performance in all games	0.390 (.026) [<.001]	0.390 (.026) [<.001]
Observations	186,980	186,980

All regressions also include the control variables that are used in our controlled baseline estimations of the conditional logit player-team matching model in Table 3 (and also shown in Appendix Table A4). Standard errors are in parentheses and p-values are in square brackets.

The estimates in Table K1 capturing the effect of first-hand experience are smaller in magnitude but statistically indistinct from those presented in Table 3. The estimated coefficient on the second-hand performance signal is very large, positive, and statistically significant, as expected — higher-performing players are less likely to remain unmatched.

It is worth highlighting some potential limitations of the approach we use to identify the effect of second-hand information. To start, since the careers of the roughly 2,500 players that we add to our sample have effectively come to an end, these players could simply be understood as having retired as opposed to our characterization as having failed to find a new match. Considering workers often retire at a time of their own choosing, it is natural to question whether these players are, as the adapted model assumes, truly not valued by any of their potential new teams. In this particular industry, however, our assumption seems more reasonable. NBA player salaries are highly lucrative — for example, the current league-mandated minimum annual player salary is roughly 1 million dollars — which suggests voluntary retirement would carry a high opportunity cost. Furthermore, after an early period of improvement, players' overall performance tends to decline with age. Accordingly, players' careers tend to end at an early age (in fact, the oldest player in NBA history was only 45), which suggests players do not continue their careers as long as they like.

Another limitation of this exercise is that we do not observe players who transition to a team in a less prestigious league. Our estimations essentially treat these players as not being valued by NBA teams, when it could be the case that they are just not valued by NBA teams as much as they are valued by a non-NBA team. A related issue is that some players' careers end prematurely due to major injuries. Since career-ending injuries can affect players for whom, based on our observable variables, retirement would be extremely unlikely, these players are also not well-accounted for.

Lastly, the estimates reported in Table K1 use a sample that still includes players who are traded to a new team. However, a player who is traded to a new team may have continued to play for their original team if their original team was unable to find another team willing to acquire the player. To the extent that this is the case, it would not make sense to include a no-match option as a potential outcome.[69]

[69] We nonetheless retained traded players in our sample so that we could use the estimates to simulate the total welfare costs of the first-hand experience bias (since traded players contribute substantially to the overall effects, it would not make sense to omit them). To address concerns that including traded players could bias the results, we repeated the estimations reported in Table K1 using a subsample that excludes traded players and our estimates of interest were virtually unchanged (the estimated first-hand experience effect decreased from .020 to .019 while the estimate of the second-hand performance signal remained .390).