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DO COACHES IN THE NATIONAL BASKETBALL ASSOCIATION ACTUALLY DISPLAY RACIAL BIAS? A REPLICATION AND EXTENSION

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DO COACHES IN THE NATIONAL BASKETBALL ASSOCIATION ACTUALLY DISPLAY RACIAL BIAS? A REPLICATION AND EXTENSION

ABSTRACT

We replicate and extend empirical evidence that has been interpreted as an indication of coaches' racial bias in the National Basketball Association (NBA) by Schroffel and Magee (2012) and Zhang (2017; 2019). After replicating the published results, we extend them by modeling coaches' decisions of "resting the starters," a common tactical decision based on players' quality and not on their race, thus exploring whether this tactic may provide an alternative explanation for associations that might thus far have been taken to indicate racial bias. Our results show that, after empirically accounting for "resting the starters," the magnitude of associations that have been taken to indicate coaches' racial bias becomes small and not statistically distinguishable from zero. We discuss the implications of these findings on literatures related to racial integration in organizations, and on the sociology of sports and race.

Keywords: Racial bias, coaches, discrimination, racial integration, National Basketball Association, NBA, basketball.

INTRODUCTION

Unfortunately, racial bias and discrimination continue to plague many sectors of society, and professional sports present no exception (Carrington, 2013; Lavelle, 2015). Past accounts have provided evidence for the existence of racial differences in players' salaries (Gius & Johnson, 1998; Hamilton, 1997), minutes played (Johnson & Marple, 1973), viewership (Kanazawa & Funk, 2001), referee bias (Price & Wolfers, 2010), and biased perceptions of white players' athleticism (Azzarito & Harrison, 2008; Stone, Perry, & Darley, 1997). A recent set of studies investigates another manifestation of racial bias: same-race preference among NBA coaches (Schroffel & Magee, 2012; Zhang, 2017; 2019; we refer to these three studies collectively as "SMZ" from now on). This recent finding about NBA coaches' same-race preference stands somewhat in contrast to a previous study by McCormick and Tollison (2001), which uses an older subsample of NBA data, also focusing on coaches. The new findings are surprising and worrying, not only because racial discrimination runs against the NBA's espoused values of social responsibility, diversity, and respect (NBA, 2018), but also because they have major implications for organizational theory. This is because they suggest that intergroup contact, or the amount of interaction and familiarity that individuals have with other racial groups, may not be quite as effective at reducing racial bias as was previously thought (Allport, 1954; Zhang 2017).

The core finding brought forward by SMZ is that NBA coaches are more likely to deploy on court – *ceteris paribus* – players of their race. SMZ's inferences indicate that coaches give, on average, between 30 to 60 seconds of extra playing time per game to players of their own race (whether the player is black or white). Even though such a difference in playing time might appear small, when added up over a player's career, it can result in substantial disparities in opportunities and salaries across races (estimates suggest a 4 percent salary increase for every additional minute played on average per game; Hamilton, 1997). Zhang (2017) suggests that while repeated interaction with a specific player may reduce a coach's bias towards him, repeated interactions with players of a different race generally do not help to reduce bias towards that race. This, in turn, casts doubt over whether the well-known intergroup "contact hypothesis" works in all organizational settings (Allport, 1954). Given the highly relevant theoretical, societal, and policy implications of studies that investigate racial bias, researchers should follow a careful approach in the analysis and presentation of their evidence, providing as much clarity as possible about mechanisms that could potentially generate any observed differences across races. In this vein, Zhang attributes the observed same-race associations to NBA coaches' "personal preferences" (2017, p.619), or to "taste-based" discrimination (Zhang, 2019).

The objective of our study is to explore whether there might be an alternative explanation of SMZ's baseline results, i.e. the authors' attribution of their reported same-race associations to coaches' racial preferences. We do so by replicating and extending SMZ's analyses, proposing and accounting for a different generating mechanism that might explain SMZ's observed associations. Our alternative explanation is motivated by the abductive conjecture that SMZ's same-race associations are spurious. Instead of being caused by and therefore being attributable to coaches' racial preferences, we suggest that they are due to coaches' strategic decision to "rest the starters," a common coaching tactic used in the NBA to deploy lower-performing and otherwise less-used players during "garbage time" (Wikipedia, 2017a) – or the period toward the end of timed competitions when the outcome of a game very likely has been decided. Building on this contention, we propose and show that systematic statistical differences that have been *observed* with respect to the performance of players and coaches across racial groups can generate correlational results that could be interpreted as racial bias, but in fact do not present valid or robust evidence for such a claim. To replicate and extend SMZ's results, we use 847,467 player-game observations from the 1981/82 to the 2016/17 NBA seasons, based on the same data source used by those previous studies. Our findings are consistent with our conjecture: once we model coaches' rationale for deploying players and account for "resting the starters," our analysis no longer reveals evidence to suggest that coaches display taste-based racial preferences that discriminate against different-race players in terms of playing time. We structure the rest of our study around our motivations, methods, and the theoretical and practical meanings (Miller & Bamberger, 2016).

MOTIVATION

Racial Bias and Discrimination in Professional Sports

Despite the societal importance of the topic and the wide availability of data to investigate the matter, the large number of studies on the existence and causes of racial bias in professional sports have provided mixed results. Past studies can be categorized along three dimensions; the source of discrimination (e.g., fans, employers or referees), the target of discrimination (e.g., players or coaches) and the outcome studied (e.g., salaries, team representation, or playing time). The majority of research has focused on evaluating the existence of a wage gap among players in different professional sports, often providing results that are at odds with implicit or explicit expectations. In general, *ceteris paribus*, studies on racial pay gaps do not find statistically significant effects in baseball and American football (Kahn, 2000). However, Medcalfe and Smith (2018) reported a wage differential in Major League Soccer in which players born in the U.S. earn less than comparable foreign-born players, while Chaplin (2012) did not find evidence for race-based differences in professional boxing. Kusterman (2013) finds a wage gap in Major League Baseball, in favor of Latinos.

Naturally, salaries are not the only relevant outcome studied by researchers, as discrimination can take the form of under-representation on teams (e.g., French Canadians in English Canadian National Hockey League teams; Longley 2000, 2003); outcomes related to specific positions (e.g., quarterback salaries for black players in the National Football League; Berri & Simmons, 2009); or playing time (e.g., no discrimination found for this outcome using data from the European Champions League; Posso, Fry, Gangemi, & Tawadros, 2016). In these cases, results on the presence of race-based discrimination have also generally been mixed.

Discrimination scholars have, in addition, studied the source of discrimination in professional sports, often attempting to disentangle the influence of employers, employees and fans. Recent studies have debated, with no clear conclusion, whether discrimination in voting patterns in the baseball Hall of Fame (Findlay & Reid, 1997; Jewell, Brown, & Miles, 2002) or in the attendance of English football matches (Preston & Szymanski, 2000) has been caused by fans or employers. Other studies have provided some evidence for racial bias from referees, who may give players of their same race or ethnicity favorable strike calls in Major League Baseball (Parson, Sulaeman, & Yates, 2011; however, other researchers, e.g., Tainsky, Mills, & Winfree, 2015, have suggests that these results may be dependent on model specifications and time periods), or who may issue fewer foul calls to players of their same nationality in the European Champions League (Pope & Pope, 2015; see also Gallo, Grund, & Reade, 2013).

The existence of employers' discrimination against coaches has also been explored. Studies have found that it is more difficult for non-white coaches to become managers and that they are more likely to be fired, both in baseball (Volz, 2013) and in American football (Madden, 2004; Mixon & Treviño, 2004; Kopkin, 2014). Yet, these results have also been called into question (Malone & Couch, 2008), as might be expected given the importance and implications of such findings.

Racial Bias in the National Basketball Association (NBA)

As is the case with other professional sports, the existence of racial bias in basketball (in particular in the NBA) has been investigated thoroughly. Kahn (2009) conducted a review of over 30 studies that analyzed racial discrimination in the NBA. In Kahn's synthesis, while there was a lot of evidence to support salary, hiring, and retention discrimination against black players in the 1980s, these effects became much weaker in the 1990s and 2000s, with a similar parallel drop in fans' discrimination.[1]

Even though studies on the wage gap, hiring, and retention do not seem to provide strong and consistent support for racial discrimination in the NBA after the 1990s, a subset of discrimination literature suggests that certain demographic factors might correlate with players' time on the court ("playing time"), but as observed associations and not inherent links. For instance, Berri, Deutscher, and Galletti (2015) use the Oaxaca-Blinder decomposition technique to show that NBA athletes born in the U.S. play 1.39 minutes more than non-U.S.-born players. The focus of our own study is on the demographic characteristic of race; specifically, we study whether the race of the head coach may interact with players' race to influence playing time. Researchers have previously proposed that NBA head coaches might be biased in favor of giving more playing time to players of their own race. To our knowledge, the first study that suggested this was Schroffel and Magee (2012), who used NBA game-level data from 1996/97 to 2003/04 to show that coaches deploy on-court players of their own race for an additional 45 to 55 seconds

[1] A shorter, but updated review on more recent work on racial discrimination in basketball can be found in Harris and Berri (2016), which itself focuses on own-race bias among coaches in terms of players' playing time in the WNBA (Women's National Basketball Association), not finding evidence of such a bias in the authors' summary of their findings, with some suggestive/speculative evidence that may indicate the presence of fan discrimination.

on average per game. This finding was recently confirmed in two articles by Zhang, who used a much larger dataset at the year level (1955/56 – 2009/10; Zhang, 2017) and then performed a game-level analysis between 1990/91 and 2014/15 (Zhang, 2019). These findings are surprising and also worrying in light of what seems to be a general trend; that post-1980s "obvious forms of discrimination in the NBA appear to have been minimized" (Schroffel & Magee, 2012: 148). Yet they are not consistent with other empirical investigations on coaches' own-race bias that did not find such an effect on playing time, using a year-level dataset spanning from 1980 to 1988 (McCormick & Tollison, 2001) or in the WNBA during the 2010 - 2014 seasons (Harris & Berri, 2016). Because these important, but also inconsistent, results appeared to us worthy of further investigation, we engaged in a replication and extension of SMZ's analysis to verify whether the associations they take to indicate racial bias would still hold after accounting for a plausible alternative explanation – that same race correlations could be generated spuriously by the coaching tactic of "resting the starters."

Coaches' Decisions of "Resting the Starters" during "Garbage Time"

> *"Garbage time was great – it allowed me to take my scoring average from 2.5 to 2.8"*
> *(Scott Brooks; Taylor 1999)*

To motivate our alternative explanation, we first contextualize and anecdotally review the criteria that drive coaches' player-deployment decisions in the NBA. In basketball, out of an available roster of up to 15 players, only five players per team are active on court. As is the case in other competitive, professional sporting events, the main aim of NBA coaches is to win. Consequently, in games that are contested throughout (i.e., where the outcome of the game remains uncertain until the very end, compared to games where one team has a clear lead over the other), coaches are incentivized to deploy their best players as much as possible and keep their lesser-quality, i.e. "reserve," players on the bench. This tactic allows them to use the best players available to them throughout the game to maximize their chances of winning. Consistent with this reasoning, Zhang proposes that the level of competition may rein in coaches' racial preferences, positing that coaches might feel more comfortable with (or feel less constrained about) displaying their racial bias during non-contested games. Zhang provides evidence (2017: Table 6) to show that "the percentage of closely contested games each season significantly reduces a coach's same-race bias," under the assumption that "competitive pressure should discourage coaches from indulging in their personal preferences and push them to use the best players available, thus reducing preference-induced bias" (Zhang, 2017: 619). In other words, according to Zhang, coaches' racial bias is mostly confined to and manifests most clearly in non-contested games, because coaches end up being freer to use non-rational, non-competitive criteria (e.g., racial bias) to choose which players to deploy.

Indeed, anecdotal evidence suggests that coaches' logic for deploying players changes dramatically when the game score at any given point in time is so lopsided that the game effectively becomes non-contestable, i.e., during "garbage time" (Taylor, 1999; Narciso, 2018). "Garbage time" refers to the final minutes of playing time in games for which the outcome has likely already been decided. Sometimes these games are called "blowout" games (Wikipedia, 2017a). For the losing team, the difference in the partial score is often large enough to make it very unlikely for them to turn the game around:[2]

> "No matter how a player interprets it, there comes a point when the game is likely out of reach. Sometimes, garbage time is only the final 40 seconds. Other times,

[2] Albeit rather rare, "miracle" recoveries do occur – for instance, the Utah Jazz came back from a 36-point deficit against the Denver Nuggets in 1998 (http://bleacherreport.com/articles/735341-10-greatest-comebacks-in-nba-history). The rarity of and the surprise generated by such recoveries support the overall expectation that in most such cases the outcome of the game has been decided.

> the blowout could start as early as the second quarter, depending on the size of the lead and the talent discrepancy between the teams.” (Narciso, 2018)

Since the outcome of blowout games is likely clear, during garbage time coaches prefer to substitute their best players with players who usually play less (Narciso, 2018). This tactic is commonly called “resting the starters”:

> “Starters are sometimes rested during a game during garbage time when the outcome is mostly certain. While usually garbage time takes place toward the end of the fourth quarter of a game, in games where there is such a vast difference in talent and the winning team very quickly gains a large lead, the starters will be removed from the game early -- sometimes well before the end of the first half -- and the second- and lower-string players will play the remainder of the contest. As such, the starters play long enough only to gain a significant lead, and giving the reserves extended playing time.” (Wikipedia, 2017b)

This tactic helps the starters avoid risking injury and keeps them in better physical condition for upcoming games. Even though the outcome of the game is probably already decided, garbage-time play can still be competitive and “tough” as reserves try to take advantage of any opportunity to demonstrate their skills and value, both to their current team and to other teams who might be interested in recruiting them at a later point in time:

> "Everyone comes to see the starters, of course. But the 13th, 14th guy on the roster, he's going to get in once every 10 games, so that is basically his playoff moment," [Gilbert] Arenas said. "I hated when someone said, 'Run the clock out, or don't shoot this.' But wait a minute, you guys played your game. This is my game now. When I get in, it's not minus-30, it's 0-0. I'm trying to show my coach and my teammates that, 'Hey, I can do the job, too.'" (Narciso, 2018)

Garbage time can also generate serious injuries that can keep players on the sidelines for long periods of time, which is another reason why coaches choose to rest, or not risk, their starters during this period.[3] Coaches may also deploy reserve players in garbage time to give these players some exposure (which could be useful for trading purposes) and to provide them with on-court and competitive playing time (Wikipedia, 2017a). Consequently, these players can gain “real” experience with the team’s tactics and be ready, should their help be needed in future games:

> “Fratello, who last coached the Memphis Grizzlies during the 2006/07 season, saw a number of players inch their way up the depth chart based on impressive performances during limited minutes. ‘If I put you in, show me what you can do so I can put you in the rotation,’ he said. ‘If you are No. 11, show me you can be No. 10 if we get an injury.’ ‘Rewarding them is giving them an opportunity,’ he added.” (Narciso, 2018)

This brief anecdotal review underscores how players’ quality[4] is a primary consideration for coaches’ player-deployment decisions during garbage time, which could confound a taste-based (i.e., racial preference related[5]) explanation of player deployment. If systematic differences are observed in the distribution of performance between white and black players, and between the amounts of “garbage time” managed by white and black coaches, an analysis that does not

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[3] For instance, Hassan Whiteside, of the Miami Heat, cut his hand dunking the ball in 2017 during “garbage time,” requiring 13 stitches. To give an example from a different professional sport, in 2016, the Pittsburgh Steelers head coach Mike Tomlin came under fire after playing star quarterback Ben Roethlisberger during garbage time, which resulted in a knee injury to “Big Ben.”

[4] We refer to players’ “quality” in the human capital tradition (Becker, 1964), i.e., by assuming the existence of relatively stable differences in skills/knowledge/productivity across players. Coaches consider players’ quality as a factor in their deployment decisions insofar as quality is positively correlated with players’ expected performance (output). Players’ recent past performance is a signal that coaches use to update their beliefs about such quality.

[5] Taste-based explanations for the presence of discrimination assume that the discrimination may be generated by preference – i.e. by the presence of some sort of animus or disutility derived from interacting with members of a particular demographic group (Becker, 1957). As one alternative explanation, statistical discrimination (Phelps, 1972; Arrow, 1973), suggests that discrimination can still exist as an outcome of rational decision makers, so that decision makers use observable traits – such as race or gender – to make assumptions about the expected productivity outcome of the individuals under consideration (usually workers) in conditions of uncertainty, where quality is not perfectly observable (Baert & De Pauw, 2014). We refer readers to reviews (e.g. Dymski, 2006; Guryan & Charles, 2013) for a consideration of how the debate on taste-based versus statistical discrimination has evolved over the last two decades.

account for these differences could lead to the emergence of statistically significant but "spurious" same-race associations between coaches and the playing time of players. These observed associations would be "spurious" in the sense that they would not in fact be generated by coaches' taste-based racial discrimination, but rather be explained by the observed systematic differences in the distributions of players' and coaches' performances. We explore this possibility in greater detail below.

Coaches' Racial Bias: Considering Systematic Statistical Differences across Races

The evidence that SMZ present in support of the presence of racial bias could be consistent with a statistical (rather than a taste-based) explanation if the following two conditions turn out to be true. First, if the performance indicators for white and black players show systematic differences, we would expect the racial group that has the lower observed performance to be deployed more often during garbage time. Zhang's own evidence is consistent with this first condition: based on (unreported) statistical analysis, he notes that "black players are better" (2017: 608), and also reports data showing that black players receive more playing time (overall) than white players (2017; Table 1). The second condition concerns observed performance differences between teams coached by white coaches and black coaches. If teams managed by white coaches happen to perform better than teams managed by black coaches, we would expect white coaches to manage more garbage time than black coaches.[6]

[6] To underscore an important and hopefully obvious point, for this condition to hold it is not assumed or inferred that white coaches are more skilled – or better – than black coaches, but rather that the teams they coach end up performing better (for any reason) than teams managed by black coaches. This, for instance, could be consistent with the "glass cliff" hypothesis, which predicts that minorities are more likely to be selected for managerial positions in poorly performing firms (e.g., Cook & Glass, 2014). Exploring these underlying mechanisms in our context is beyond the scope of our study, given our own focus on the playing time of players, but we encourage further research on this matter. A similar set of considerations apply for the observed performance differences across races for players.

If these two conditions hold, they could yield associations that appear to provide evidence for an imputed "same-race bias," and yet instead have an underlying explanation based on systematic statistical differences between white and black players and coaches. Indeed, if white coaches handle more garbage time, and if white players are less skilled and thus more likely to play more garbage time than black players (as a direct implication of the "resting the starters" tactic), we would observe that white players play more minutes under white coaches.[7] Everything else being equal, this correlation would generate an association that could mistakenly be attributed to racial bias, when instead this effect would be spurious, i.e., actually generated by the underlying systematic differences between players and coaches across races. Our conjecture can thus be summarized as follows: we expect that, once the effect of players' quality on their deployment during garbage time is accounted for, there will be no evidence left to indicate the existence of coaches' racial bias on players' playing time.

METHODS

Data Sources

We used the same data source as Zhang (2017; 2019), downloading NBA data from www.basketball-reference.com. We constructed two databases to replicate and extend SMZ's results. First, we followed Zhang (2017) and built a dataset that included variables at the year (or season) level. Zhang's 2017 dataset spanned from the 1955/56 season to the 1999/2000 season, yielding 163 head coaches, 2,360 players, and 11,618 player-year observations. After replicating

[7] Symmetrically, black coaches would also empty out their benches and play weaker players during this period ("resting the starters"). In their case, too, however, these players would be more likely to be white players. Therefore, even though we might observe a white coach-white player association (as we note in the main text), we do not expect to observe a black coach-black player association. In this sense, the second condition may not be necessary for our conjecture to hold. Indeed, in our analyses, we investigate this disaggregation and find results that are consistent with this speculation. In addition, we also see (in unreported analysis available from the authors) that black coaches are in fact more likely to play white players under conditions of more garbage time. In a model specification that approximates Models 13 and 14 in Table 5, we see that "Black coach- white player x Absolute difference in final score (cent.)" has a positive and significant ($p < 0.001$) coefficient.

his sampling choices,[8] we arrived at a comparable sample of 169 head coaches, 2,517 players, and 12,029 player-year observations. However, to engineer the highest statistical power (Miller & Bamberger, 2016), we also extended our dataset to 2016/17, which then included 243 head coaches, 3,725 players, and 19,837 player-year observations. We coded players' and coaches' race using Zhang's procedure (2017), and calculated Zhang's variables using his reported operationalizations to follow – to the best of our understanding[9] – the methods of the original study (as proposed by Miller & Bamberger, 2016).

Second, we followed Schroffel and Magee's (2012) and Zhang's (2019) approach and collected NBA data at the game level from www.basketball-reference.com to test more directly the implications of "resting the starters." Using game-level data is advantageous for three reasons: first, we can model coaching decisions based on focal game characteristics, for instance the game's final score (which we will use as a proxy for garbage time); second, we can control for players' performance in previous games of the same season, thus constructing a measure that more closely approximates their expected performance in a given game, regardless of their race; and third, we can leverage within season variation due to coach changes (as we do in robustness tests, where we report results from difference-in-differences estimates). Since game data containing minutes played per game for each player is available from the 1981/82 season onward, our game-level sample includes all player-game statistics from the 1981/82 season to the 2016/17 season, yielding 42,964 games and 847,467 game-player observations. Consistent with Zhang (2017), we excluded player-game data for players who joined a team during a given

[8] Using the head coach with most games managed for teams that changed coaches during a focal season, and discarding players after they changed teams during a season; please see Zhang (2017) for details.

[9] We made informed guesses for the very few cases where Zhang's operationalization was not completely clear to us (e.g., seniority rank, fouls per 48 minutes, and defensive player). We refer the reader to Zhang (2017) for a clear and detailed description of the operationalization of the overlapping measures.

season, as this is different from joining a team in between seasons; we also excluded player-game observations in which players did not play in a given game for non-coaching related decisions, i.e. injuries, personal reasons, etc.

Empirical Strategy

We followed two steps to investigate our conjecture. First, we replicated Zhang's (2017) results at the year-level. We did this to create a common statistical starting ground with SMZ's dataset. Second, we tested the implications of our conjecture leveraging game level data, to see whether any residual taste-based racial bias remains once we control for coaches' "resting the starters" tactic.

RESULTS

Step One: Replication of Zhang's 2017 Findings at the Year (Season) Level.

Table 1 presents the summary statistics and correlation table of our year-level dataset. The descriptive statistics are similar to Zhang's year-level summary statistics (Table 2 in Zhang, 2017) and correlation table (online appendix in Zhang, 2017). In Appendix A (Tables A1 and A2) we present a stricter comparison of the two samples by restricting our sample to the same years, 1955/56 – 1999/2000, used by Zhang (2017).

--- INSERT TABLES 1 AND 2 ABOUT HERE ---

Table 2 reports a replication of Zhang's player fixed-effects analysis (Zhang, 2017, Table 3), where our Models 1 through 4 mirror Zhang's models (2017: 613). Our results align with and confirm Zhang's results: at the year level, playing for a *Same-Race Coach* boosts players' average minutes ("playing time") by about 30 seconds ($\beta = 0.56$; $p < 0.05$; Model 1). Model 2 replicates Zhang's finding that coaches seemingly show less racial bias as they develop a relationship with the focal player ($\beta = -0.53$; $p < 0.05$). Models 3 and 4 break down the effect for black and white players, displaying a marginally significant effect of racial bias in favor of white players (by white coaches) with coefficients that are again comparable to Zhang's results (β = 0.89; $p < 0.10$ for white players playing for a white coach; β = 0.45; $p > 0.10$ for black players playing for a black coach). Model 5 then replicates Zhang's baseline model using data that extends up until 2017, again confirming Zhang's baseline result (β = 0.47; $p < 0.01$).

Step Two: Game Level Analysis

In the second step of our empirical strategy, we move our analysis to the game level (descriptive statistics can be found in Table 3), where we start by testing SMZ's same-race coach effect at the game level (Table 4).

--- INSERT TABLES 3 AND 4 ABOUT HERE ---

Analysis of baseline effect. The first model of Table 4 (Model 6) reports the estimates of the *Same-Race Coach* effect at the game level. Our modeling followed two criteria: we maximized statistical power by using all the available NBA seasons (1981/82 to 2016/17), and we used the same set of controls as in the year-level analyses (Zhang, 2017).

Model 6 is largely consistent with SMZ's findings (2012; 2019)[10]; playing for a same-race coach seems to boost players' playing time by about 20 seconds per game (β = 0.33; $p <$ 0.05).

Model 7 then provides evidence that the presence of the same-race effect is contingent upon the amount of garbage time in a game. We created *Absolute Difference in Final Score* as a proxy for the amount of garbage time in a specific game, and interacted it with *Same-Race*

[10] SMZ's game level analyses (2012; 2019) use smaller samples than ours (1990-2014 and 1996-2004) and different sets of control variables. Robustness analyses (available from the authors) confirm that the small differences in coefficient sizes between SMZ's and our reported results are attributable to either SMZ's sampling choices (i.e., restricting the sample to specific years, which reduces overall statistical power and leaves results more susceptible to influential outliers and censoring bias) or SMZ's choices of different control variables (for instance, Zhang, 2019 does not control for time trends using fixed effects, which is different from the approach in Zhang, 2017).

Coach, which yields a positive and statistically significant interaction coefficient ($\beta = 0.02$; $p < 0.01$; Model 7). These results establish that greater same-race effects are observed during less competitive games, which is evidence that could be consistent with Zhang's competition-based mechanism (2017). Next, we move on to test our conjecture.

Analysis of the two postulated conditions. First, we descriptively look into the two conditions (previously outlined in our Motivation section) that could yield an alternative explanation for the statistical associations attributed to racial bias and discrimination in SMZ (2012; 2017; 2019). The first condition is that white players have lower performance than black players. We proxy for players' quality by using a measure of individual past performance: the Player Efficiency Rating (PER), an established measure that is considered to be the best "comprehensive" index to capture individual performance in basketball (Kubatko, Oliver, Pelton, & Rosenbaum, 2007), and that has been used in past management and organizations research to measure human capital (Fonti & Maoret, 2016) and player performance (Ertug & Castellucci, 2013; Ertug & Castellucci, 2015). PER is an index composed of both successful (e.g., rebounds, assists, blocks, steals, and various types of scoring) and unsuccessful (e.g., missed shots, turnovers and fouls) player statistics. Importantly, PER is also standardized by the minutes played by the focal player, the pace of the team,[11] and the overall pace of the league (a higher game pace can inflate statistics). These standardizations make it more meaningful to compare the performance of players across time. We followed Zhang (2019) and operationalized PER as *Relative PER*, which we calculated as a *z*-score based on the average PER of the focal player's team and its standard deviation. Since players of the same team compete with each other for playing time, it is reasonable to consider the focal player's quality as relative to the teammates he

[11] The "pace" of a team measures how fast a team plays. A faster pace yields more plays, more shots, and thus higher statistics for the players of those teams, as compared to players in teams whose pace is slower.

competes with for playing time. We then adopted a second measure of players' quality: *Starting the Game.* As previously mentioned, in basketball only 5 players can be on the court at once, and strong anecdotal evidence indicates that players compete to become one of the 5 "starters" (i.e., the best players) composing the primary unit of the team.[12] Since the starters of a given team usually face the starters of the opposing team, we use *Starting the Game* as another proxy for players' quality.

We find that white players have lower performance, on average, than black players. A simple *t*-test performed on our year-level data (1955/56 – 2016/17) shows that white players' PER is 0.67 points lower than black players' ($p < 0.001$) and that white players start, on average, 9 fewer games than black players ($p < 0.001$). Multivariate regression analysis including random intercepts for players, fixed effects for coaches, teams, and years, and controlling for players' positions, age, and team tenure, reveals white players' PER is 1.55 points lower than black players' ($p < 0.001$), or 0.20 standard deviations in *Relative PER* ($p < 0.001$), and that white players start, on average, 5 fewer games per season ($p < 0.001$) and play 2.37 minutes less than black players ($p < 0.001$). These findings provide evidence that gives early support to our first condition (results available from the authors).

Our second condition is that the performance of teams managed by white coaches is higher than that managed by black coaches. We provide an initial test of this condition at the year level by comparing *Coach's Past Win–Loss Record,* as originally calculated by Zhang (2017), between these two race groups. A simple *t*-test shows that white coaches have a win-loss record that is 2 percent higher than black coaches' ($p < 0.001$). We also observed a difference in

[12] Competition to be a starter is particularly strong in the NBA, as it affects players' reputation, status, and future earnings (see, for instance, recent statements by Carmelo Anthony and Isiah Thomas on publicly rejecting a bench role; Boone, 2018; NBA media reports, 2018).

the same direction when moving our analysis to the game level, where we used a logistic regression to predict the likelihood of winning a game based on coaches' race, including a random intercept for coaches, and fixed effects for teams, years, and game number, whilst also controlling for Zhang's *Coach's Past Win–Loss Record* and coach NBA experience as a head coach and assistant coach. We limited our analysis to home games to avoid counting games twice. The results show that white coaches are more likely to win a game than black coaches ($p < 0.01$), and that they are also more likely to win a game by 10 points or more[13] ($p < 0.01$), indicating that they are – on average – experiencing "garbage time" conditions more often than black coaches (analyses available from the authors).

Testing our conjecture. We now investigate how these two observed performance differences between white and black players/coaches, which are consistent with our two conditions and the rationale behind our conjecture, might also correlate with Zhang's same-race effect. In other words, we use multivariate analysis to test whether an association that could be attributed to a same-race bias remains significant after we factor in coaches' "resting the starters" tactic. These results are presented in Table 4, Models 8-11.

Model 8 tests for the presence of the same-race effect during garbage time after controlling for *Starting the Game* as an additional control for players' quality. *Starting the Game* is a highly significant and relevant predictor of minutes played ($\beta = 11.75$; $p < 0.001$; t-statistic = 112.56), and it greatly increases the explanatory power of the model (the percentage of explained variance by the model increases by 15 percent, as based on Model 7). As is to be expected, starting players tend to play more minutes, *ceteris paribus*. We also see that the interaction between *Same-Race Coach* and *Absolute Difference in Final Score* remains significant ($\beta = 0.02$;

[13] We used this cut-off to be consistent with the operationalization in Zhang (2017) of "competitive games", i.e., a game that ends with fewer than 10 points in absolute final score difference

$p < 0.01$), with its coefficient almost unchanged, even when controlling for *Starting the Game*. All this amounts to more evidence that racial bias tends to manifest more during less competitive games. (Note, in Model 8, however, that the main effect of *Same-Race Coach* becomes non-significant ($\beta = 0.14$; $p = 0.25$), and the magnitude of its coefficient drops by 58 percent in comparison to Model 7.)

We then investigate our conjecture in Model 9, where we include interactions between the measures of players' quality (*Starting the Game* and *Points, Rebounds and Assists per 48 minutes*) and *Absolute Difference in Final Score*. All interactions are negative and significant ($p < 0.05$), indicating that better players play fewer minutes in games that are less competitive, i.e., games that have more garbage time. In addition, we see that the previously significant interaction between *Same-Race Coach* and *Absolute Difference in Final Score* becomes very small, with the coefficient approximating zero ($\beta = 0.00$, $p = 0.83$). Model 9 thus provides evidence for our prediction that after interacting our indicators of players' quality with the amount of garbage time in a given game, the effect of coaches' same-race bias disappears, even during garbage time. In Model 10, we replace players' raw statistics (*Points, Rebounds and Assists per 48 minutes)* with a more parsimonious, and stronger, measure of players' performance, namely *Relative PER*[14]. Model 10 confirms our results, while also providing an improved measure of model fit (an additional 1.5 percent of explained variance in comparison to Model 8). Most notably, the estimated coefficients for *Same-Race Coach* and its interaction with our garbage-time proxy become essentially zero, i.e.: (a) statistically not distinguishable from zero ($p = 0.63$) and (b) the coefficients' magnitudes indicate effect sizes that are essentially zero ($\beta = 0.05$).

[14] Zhang (2019) reports increased model fit when using Relative PER in lieu of several raw statistics to control for players' quality (see Appendix 2; 2019). We find the same in our analysis, i.e. that this one measure explains more variance in the outcome as compared to the multiple other simpler indicators.

Model 11 adds team and coach fixed effects to Model 10, again with results that confirm our inference and prediction for the effect of *Same-Race Coach* ($\beta = 0.08$; $p = 0.63$). Since our conjecture implies a null effect, the sheer absence of statistical significance is not, as such, enough to provide support for it. Therefore, in Figure 1a we plot the 95 percent confidence intervals of the *Same-Race Coach* effect at various levels of *Absolute Difference in Final Score*, to evaluate the potential existence of an effect within 5 percent margin of error.

--- INSERT FIGURE 1a ABOUT HERE ---

Figure 1a shows that the point estimate of the marginal effect of having a same-race coach is never above 0.15 (an actual effect of 9 seconds) even in the most skewed part of our sample (for observations that correspond to three standard deviations above the mean, which correspond to about 1 percent of all of the observations in our sample). It also shows that in all of the cases the 95 percent confidence intervals cover or include 0 (indicating that we cannot reject, at the $p < 0.05$ level, the null hypothesis that there is in fact no same-race effect). Notwithstanding the non-statistical significance of the same-race coach effect, just for illustration, the confidence interval stays within a coefficient of 0.3, or about 18 seconds, for over 86 percent of our sample. We interpret these results as confirmed evidence of a lack of a meaningful effect.

There is an important caveat to mention with respect to our results. It could be suggested that *Starting the Game* itself might be influenced by coaches' racial bias. If this were the case, the inclusion of *Starting the Game* as an additional control might absorb and mediate coaches' racial bias on minutes played, which might in turn explain why this latter relationship becomes non-significant. To investigate this possibility, we estimated a simple linear probability model that controlled for players' quality (*Relative PER*) and player fixed effects only. The results reveal a very small and statistically non-significant *Same-Race Coach* coefficient (β = 0.01; p = 0.25), despite a relatively high model fit (R-squared = 0.39), which suggests that coaches' decisions to deploy starters is not influenced by racial bias. This outcome, and our conclusion, remains the same when we also include in this model year-fixed effects and the standard set of Zhang's (2017) control variables.

Differential effects by race. In Table 5, Models 12 to 15, we replicate Zhang's findings (2017) by disaggregating the *Same-Race Coach* effect by white and black players (and, correspondingly, white and black coaches).

--- INSERT TABLE 5 ABOUT HERE ---

If the two conditions underlying our conjecture hold, i.e., if white players, on average, have lower PER and white coaches manage, on average, more garbage time, we would expect the *Same-Race Coach* effect to be present in garbage time mostly for white players. (We are referring to the observed same-race associations before we account for "resting the starters".) Indeed, that is what Model 13 shows: the interaction between *Absolute Difference in Final Score* and *White-White* (the same-race indicator for white players with white coaches) is significant (β = 0.04; p < 0.001), but the same interaction with *Black-Black* is not significant (β = 0.00; p = 0.77). These results provide further evidence for our explanation: that any observed coach bias is not fundamentally based on race preference *per se*, but instead results from a combination of coaches' playing weaker players during garbage time, lower observed performance for white players, and higher observed performance for teams coached by white coaches. This observed association disappears (β = 0.01; p = 0.28) once we include interactions between our garbage-time measure and indicators of players' quality (Models 14 and 15), providing support for our conjecture: the effect of coaches' racial bias disappears once the quality of the player and his previous utilization in the season (and the subsequent implications on any garbage time in the focal game) are taken into account.

Robustness Tests

We ran several tests to confirm the robustness of our results.

Alternative set of controls. We first tested whether our results were sensitive to specific control variables. We checked whether the *Same-Race Coach* effect depended on whether we used the set of control variables in Zhang (2017) or in Zhang (2019). Use of the control variables in the latter study displayed a slightly better model fit (with 1.4 percent of additional explained variance), but the two specifications fundamentally yield the same patterns. Additional estimations, available upon request, confirm that the *Same-Race Coach* effect approaches zero once we control for "resting the starters," even if we use Zhang's (2019) alternative set of controls, a restricted sample (only from 1990 onwards), and clustering standard errors on players and coaches (Zhang, 2019) rather than just on players (Zhang, 2017).

Influential outliers. We investigated the implications of influential outliers in our data, specifically regarding their impact of certain decades. In particular, Schroffel and Magee (2012) suggest that their reported *Same-Race Coach* effect appeared to be stronger during the late nineties (a finding also confirmed by Zhang in the appendixes of his 2017 piece).

--- INSERT TABLE 6 ABOUT HERE ---

Table 6 investigates these temporal patterns, and confirms the previous findings, showing that the *Same-Race Coach* effect is statistically significant only during the nineties ($\beta = 0.93$; $p < 0.05$; Model 17), and remains small and not statistically significant in the other decades ($p > 0.10$; Models 16, 18-19). It is outside the scope of this study to isolate why this pattern comes about. However, we can think of two statistical explanations for these results. First, the nineties could just so happen to include some influential outliers, which may be driving the abnormal results for that decade. Second, restricting the timeframe could introduce bias due to left or right censoring. This could be problematic because the variance in the focal variable *Same-Race Coach* mostly occurs across years (when players change teams and when most changes of coach happen) rather than within years (where such changes are less frequent). Therefore, by changing the timeframe studied, one would be (arbitrarily, even if completely unintentionally) altering the subsamples in which the information to estimate the same-race effect may change, in non-random, non-generalizable ways.[15]

To test the sensitivity of these results to different time windows and outliers, we disaggregated the 1990-1999 period into various time windows that include some years from this decade. Models 20 and 21 report the estimations on the subsamples 1986/87-1994/95 and 1995/96-2003/04, respectively, showing that while neither of these yield a statistically significant result for the *Same-Race Coach* indicator, including the latter part of the nineties seems to increase the magnitude of the coefficient. The results also reveal that the effect is rather sensitive to these choices; in particular, the inclusion of the 1997/98-1999/2000 period tends to have a big effect on the estimated coefficients, potentially pointing to the presence of influential outliers in this period (or some other information not captured by the estimation and variables for this three-year period only). Indeed, removing the 1997-1999 period from the otherwise full sample (Model 22) yields a small, non-significant coefficient for same-race (β = 0.17; p = 0.30). It is important to note that removing data for three years from a sample of 36 years leads to a non-

[15] An example might help to clarify this issue. Assume, for instance, that a black player plays for a white coach in 1989, and then for a black coach in 1990, and then never experiences a change in coach race ever again. Given the within-player estimation (which is what player-fixed-effects estimations produce, as Zhang also emphasizes in of his studies), this variance will be included in the estimation only if the sample includes both 1989 and 1990; however, different time-windows would potentially exclude some observations in favor of others, thereby yielding some time periods that just happen to include observations that yield evidence for the association.

significant coefficient (based on 33 years of data, with close to a million observations), and a coefficient which is also less than a third in magnitude when compared to what is reported in Zhang (2017; 2019).

Split sample analyses. In an additional analysis, Zhang (2019; Table 4, Models 4-7) suggests that the *Same-Race Coach* effect remains even in closely contested games (defined by Zhang as games with either under 5 or 10 points difference in the final score). This goes somewhat against our conjecture, and also contents Zhang's own previous finding that indicated that the *Same-Race Coach* effect was moderated by whether games were closely contested or not (see Zhang, 2017; Table 6, Model 3). We thus explore this issue in Table 7.

--- INSERT TABLE 7 ABOUT HERE ---

The first model we present in Table 7 (Model 23) is a strict replication of Zhang's Model 6 in Table 4 (2019). We meticulously follow his design choices in terms of sampling, control variables and the inclusion of fixed effects. The results of Model 23 closely mirror Zhang's results; the beta of the *Same-Race Coach* indicator is 0.46 (vs. 0.55 in Zhang) and the standard error is 0.17 (vs. 0.18). Thus, this initial evidence seems to confirm that racial bias could still be prevalent in closely contested games (i.e. matches with a final score difference that is smaller than 5- or 10-points), a finding that would not be consistent with our conjecture.

Considering the sensitivity of the analysis to different time samples, as well as the likely presence of influential observations (e.g., the three-year period), our approach to estimation is driven by two criteria: maximizing statistical power and correcting for possible time trends (or differences in the estimates of coefficients that are year-specific). We thus maximized statistical power and limited censoring bias by using all the available data (1981-2016) in Model 24. These results reveal, as a starting observation, that in the full sample the coefficient size is reduced by

37 percent, with statistical significance now at $p = 0.10$ ($\beta = 0.29$; se = 0.17). Given the sensitivity of the sample to time effects (see previous robustness tests), in Model 25 we also introduce year fixed effects (following Zhang, 2017); this reduces the coefficient of the same race effect again by 14 percent from the prior model ($\beta = 0.25$; se = 0.18), which now also ends up being not significant ($p = 0.15$). Thus, estimating the same-race effect in the whole sample, while accounting for time trends (or differences that are attributable to specific years), reduces the coefficient of *Same-Race Coach* by 46 percent ($\beta = 0.25$ from 0.46).

We follow the same procedure in Model 26, replacing the three player-performance variables with the single measure of *Relative PER*, which has proven to be a stronger measure of players' quality. Our results reveal an increase in model fit and a further drop in the *Same-Race Coach* coefficient, with its magnitude reduced yet again by 36 percent ($\beta = 0.16$; se = 0.17). Thus, in comparison to what is reported in Zhang (2019), a more parsimonious model that provides a better fit, and is based on a larger sample, reduces the coefficient of *Same-Race Coach* by 65 percent, and renders the coefficient not statistically significant ($p = 0.34$).

Finally, following our theoretical rationale, we include *Starting the Game* as an additional control in Model 27. The resulting estimations markedly improve the model fit (with the R-squared going from 0.50 to 0.63) and reduce the coefficient for racial bias ($\beta = 0.03$; se = 0.12; $p = 0.78$). The standard error of this coefficient is reduced by 33 percent compared to Zhang's original specification, indicating a more precise estimation of the now essentially null effect. These patterns appear to be very similar when considering games with a final score difference of fewer than 10 points, as shown in Model 28 ($\beta = 0.08$; se = 0.11; $p = 0.48$). Taken together, our results suggest that there is no evidence of coaches' racial bias in player deployment in closely contested games, as is consistent with the implications of our main results more generally.

Missing data. Our game-level dataset embodies a missing-data problem that could potentially be consequential for our analysis. Before the 2013/14 season, our data source (www.basketball-reference.com) does not include "did not play" (DNP) records, nor player-game records of players who were on the team roster for a given game (i.e., in principle available to be deployed on court by the coach)[16] but did not end up receiving any playing time. This means that our analyses up until the 2013/14 season include only those player-game observations where the players spent at least 1 second on court. By comparing team sizes before and after the 2013/14 season, we estimated the amount of missing data to be substantial (approximately 160k-170k observations). This missing data may bias our coefficients, as suggested by the negative and statistically significant coefficient of *Absolute Difference in Final Score* in all of our main analyses (e.g., Table 4, Model 7). By construction, this coefficient should be zero, as the average number of minutes played (across all players in a given team for a given match) is fixed, and independent of the *Absolute Difference in Final Score* (the total playable minutes in a regular, non-overtime, game for each team is 48*5 = 240; the average playing time for each player is then 240 divided by the team size, which is uncorrelated to the final score). However, there is a negative coefficient for this variable in Model 7 (β = -0.09; p < 0.001), which is likely to be generated by reasons tied to missing data. Indeed, the lower the *Absolute Difference in Final Score*, the more competitive the game is, with coaches more likely to choose to play their best players for longer periods of time. Therefore the number of players who might not play at all is higher (i.e. the number of players who might play in less competitive games, when the outcome of the game is clearer). This leads to more DNPs (missing data), which also yields smaller

[16] Additionally, please note that players might be on the team, but not be placed on the roster for a given game. The problem we refer to is the case of a player being on the roster for a given game, but ends up not receiving any playing time at all in that game (did not play/DNP).

observed team sizes and, consequently, a higher estimated average minutes per player. By influencing our dependent variable, this bias might impact the estimation of the effect of *Same-Race Coach* for those players who rarely see any playing time.[17]

We approached this problem in two ways. First, we found additional data on "did not play" records on NBA.com, but unfortunately this data was available from the 1996/97 season onward only. We downloaded, cleaned and matched this extra data with our main dataset, resulting in 80,834 additional DNP records. After dropping 17,806 records of players that did not play for reasons unrelated to coaching decisions, we re-ran all of our models only on this new subsample (n = 586,386), now with complete data from 1996/97 to 2016/17. While this subsample does not suffer from missing data problems, its shorter time span could be a cause of concern due to censoring and the presence of influential outliers. We present our results in Table 8.

--- INSERT TABLE 8 ABOUT HERE ---

The results in Table 8 are reassuring. The main coefficient of *Absolute Difference in Final Score* in Model 30 is reduced by 75 percent, compared to the previous main analyses featured in Model 7, while the main effect for *Same-Race Coach* remains fundamentally the same ($\beta = 0.34$; $p < 0.10$). Thus we are reassured that there is a lack of significant bias due to missing data.

However, while *Absolute Difference in Final Score* should theoretically not correlate with minutes played, it still remains negative and statistically significant ($\beta = -0.02$; $p < 0.001$)

[17] Whereas Schroffel and Magee (2012) remark upon this matter of missing data and collected additional data for their 1996-2004 sample to avoid this issue, Zhang's (2019) game-level analyses do not explicitly comment upon the potential consequences of this selection bias for his reported findings. Comparisons of sample size and descriptive statistics between same-year sub-samples of our data with Zhang (2019) indicate that his analyses could also be similarly affected by this potential issue, since we share the same data source.

until we control for the presence of overtime in the focal match (Model 31). Indeed, the likelihood of a given game to go to overtime is mechanically correlated with the difference in this game's final score, as more contested games are more likely to have gone to overtime. When controlling for this additional factor (Model 31), the coefficient of *Absolute Difference in Final Score* approaches the expected value of zero (β = -0.01; p = 0.28). The rest of the results present patterns that are very similar to the ones we report for the full sample (cf. Table 4). Specifically, Models 29, 30, and 31 report a statistically significant effect for the *Same-Race Coach* indicator ($p < 0.08$) when "resting the starters" is not accounted for. Again, this effect becomes smaller and is not statistically significant (β = 0.08; $p > 0.50$; Models 32-33) or even negative (β = -0.30; $p < 0.10$; Model 34) after we incorporate the reasoning in our conjecture, once again confirming our results. The only major difference in this additional test is that we do not find a significant interaction between *Same-Race Coach* and our garbage-time indicator in Models 30 and 31 (which could be due to various statistical reasons). Figure 1b reports confidence intervals for the same-race coach marginal effect, estimated on the full sample (1981/82 – 2016/17) now including the additional DNP records (n = 905,463). These additional results yield point estimates that are even closer to zero, at all levels of absolute difference in final score, i.e. garbage time.

--- INSERT FIGURE 1b ABOUT HERE ---

As an additional method to handle the missing data, we imputed missing data for the years for which we did not have DNP records (from 1981/82 to 1995/96), using data on players who played at least one minute for the focal team in the current season, and data on players' mobility across teams, to identify whether any player was still under contract with the focal team for each game. The results from the complete data subset (Table 8) are very similar to those from the analyses based on the original dataset with missing observations (Table 4).[18]

In conclusion, our investigation of the potential missing data bias suggests that our results (and therefore by implication also the results of Zhang, 2019) are robust to these issues.

MEANING

We reassessed SMZ's findings (Schroffel & Magee, 2012; Zhang, 2017; 2019) that directed scholarly attention to the presence of coaches' racial bias in the NBA. After replicating SMZ's results to establish a common starting point with their published findings, we showed that associations that might be indicative of coaches' racial bias are statistically detectable only during uncontested games, or games that feature more "garbage time." We then expanded our modeling to account for "resting the starters," a common coaching tactic whereby lower performing players are deployed when games become uncontested. We find that once the deployment decisions attributable to "resting the starters" are taken into consideration, any evidence that could indicate coaches' same-race bias, or racial discrimination, becomes statistically indistinguishable from zero.

We can interpret our findings in two ways. The first – and for us, the simpler – explanation is that, after accounting for the coaches' "resting the starters" tactic, decisions about player deployment seem to be unrelated to whether a coach and a player are of the same race or not. This interpretation would qualify SMZ's findings as attributable to systematic statistical (observed) differences across players and coaches of different races, and not necessarily to taste-based racial discrimination. A second explanation could be that our models include more precise player information than what is available to NBA coaches. In other words, it could be that our

[18] Regression results and explanations of the imputing procedure are available from the authors.

statistical modeling better accounts for player quality than the information that coaches have, and therefore coaches might rely on factors such as race as proxies for players' expected performance when deciding who to deploy. This explanation would also be consistent with statistical (but not taste-based) discrimination, an interpretation that is also suggested by Schroffel and Magee (2012) and with Zhang's discussion of learning effects (2017).

Theoretical Implications for Management Theories of Discrimination in Organizations

Regardless of the interpretation one might favor, the findings we present indicate that any residual evidence of taste-based racial bias is not statistically distinguishable from zero once we account for "resting the starters." In other words, our analysis in this study provides no evidence to support the notion that SMZ's findings are generated by coaches' racial preferences. This has two important implications for the theoretical inferences that Zhang (2017; 2019) derived from his findings.

First, since our analysis rules out preference-based discrimination in coaches' deployment decisions, we cannot support Zhang's claim that repeated interaction with a specific individual of a different race might subdue racial discrimination more than general interactions with multiple individuals of a different race. In other words, since we do not find any evidence for taste-based racial discrimination, we cannot confirm Zhang's assertion (2017) that the "contact hypothesis" (Allport, 1954) is not valid in highly interdependent and diverse settings, such as the NBA (Reskin, 2000; Kalev, 2009; Chakravarti, Menon, & Winship, 2014). One could, in fact, speculate the opposite: the lack of racial bias among NBA coaches, with respect to the outcome of playing time, could be due to extensive interactions with other race groups. NBA head coaches would have had many years of experience in basketball, whether as a player or a coach (or indeed spectator), and so the lack of racial bias could be due to their history of extensive interactions.

The second, related, implication is that in contrast to SMZ's considerations, our findings seem to be consistent with studies that find that racial bias is minimized in highly interdependent, integrated settings (Reskin, 2000; Kalev, 2009; Chakravarti et al., 2014). The reduction of bias in these settings speaks directly to the sociological debate between a "critical" view of race and sport that sees sports as a site of exploitation and inequality, and a functionalist-evolutionary paradigm that sees sport as a space to integrate communities (Carrington, 2013). In support of the latter view, our results do not yield any evidence for taste-based racial discrimination in coaches' allocation of playing opportunities among professional basketball players.

Implications of Our Findings for Future Studies

The NBA prides itself on being a global promoter of social justice and diversity, and tends to be more vocal on social and political problems than most other professional leagues (Engels, 2017; Garcia, 2018). Our findings indicate that the NBA might truly be "walking the walk," in that their workplace is one in which players are not discriminated on the basis of elements of their demographic characteristics (such as race). This, in turn, suggests that highly interactive social systems (such as professional sports) can serve as loci for cross-racial interactions, a fact which might help reduce discrimination in society. Since this suggestion is fundamentally speculative, we urge future studies to investigate, both qualitatively and quantitatively, whether and how the NBA has been able to limit racial bias in decisions, through coaches or otherwise. For instance, coaches might be aware that their decisions will be scrutinized by millions of fans, perhaps making them either more aware of their biases or less prone to reveal them in public, especially for biases that are more salient, such as a racial bias with respect to player use/deployment. Future studies could analyze if and why this is the case, perhaps comparing coaches' private and public decisions, and/or comparing the NBA with other sports leagues to better incorporate the historical and cultural factors of the league into their investigation. Further, we note that perhaps the mere availability of detailed and real-time metrics in sports, and the clear and quick feedback this data yields might reduce or constrain bias. Since such data is available not only to coaches and players but also to other stakeholders (such as fans, owners, and the media), data transparency can generate normative pressure for coaches to adjust their behaviors, by making clear the implications of bias or by enabling stakeholders to question whether bias might be present. For instance, Pope, Price, and Wolfers (2018) have found that awareness about racial bias can indeed help in reducing such bias. In settings where such metrics are not very readily available, or settings in which coaches' decisions are not as visible to the public (or stakeholders), it may be possible, worryingly, that different manifestations of racial bias might emerge, persist, and even grow undetected.

One could also investigate whether there might be biases with respect to other demographic or ascribed dimensions. Previous research has looked into how homophily (the tendency to establish and maintain ties with similar others, with implications for the more favorable treatment of such similar others) might be based on dimensions other than race, such as age, gender, and nationality (e.g., Ertug, Gargiulo, Galunic, & Zou, 2018). Investigations on the presence, sources and outcomes of bias in these other dimensions would also help identify and, hopefully ultimately eliminate, such biases. For instance, gender has been frequently studied in research on organizations and management (see Jones et al., 2017 for a meta-analytic review of discrimination in the workplace with respect to racism, sexism and ageism) but less so in sports. This is due to the rarity of mixed-gender teams and also because few women coach men's teams (even though the opposite scenario occurs relatively often). Initial evidence by Harris and Berri (2016) seems not to find evidence for coaching racial bias in the Women's National Basketball Association (WNBA). However, further work could compare and contrast men's and women's leagues in the same study, to better understand what kind of mechanisms and dynamics might apply to different demographic groups.

In addition, ongoing investigations into the sources of biases, for instance on stereotypes or meta-stereotypes,[19] will be useful in furthering our knowledge of the root causes of discrimination and observed relationships that might be taken as discrimination. More such work would also shed light on the degree to which the observed preference to interact with others who are similar to oneself is based on actual differences between groups, on perceptions of such differences (whether such differences exist – are observed – or not) coming from stereotypes, or on meta-stereotypes.

Future research could also look into whether evidence for the presence and types of biases might vary systematically depending on the environment. For example, keeping to the setting of basketball, would the occurrence and outcomes of biases be different in college teams compared to NBA teams? College teams and NBA teams have different team compositions, audience sizes, and number of coaches, and the performance-relevant correlations observed with race may vary between college basketball and the NBA. Future research could also consider whether racial biases differ between teams composed of different demographic dimensions. For example, as has been proposed by one of our reviewers: would racial bias in men's and women's

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[19] Meta-stereotypes refer to perceptions of what a focal actor thinks *others* think of members of the focal actor's own group. Though this is arguably less relevant for the particular issue we investigate here, it has been studied fruitfully in other settings, for example with respect to age (Finkelstein, King, & Voyles, 2015).

NBA manifest differently, or would the intersection of different demographic dimensions lead to different manifestations of biases?

Finally, it is also possible that under conditions of strict meritocracy or greater competition, coaches who exhibit racial bias are selected out by the environment (whether at a pre-NBA stage, at the pre-head coach stage, or earlier during their head-coaching career) and are not likely to be present as a substantial part of a sample such as ours. The selecting out of coaches who display racial bias may be driven by organizations' desire to eliminate bias as a goal itself, or because of the belief, or finding, that bias is performance reducing (see Ertug et al., 2018 for a discussion of when homophily, as a related phenomenon, might be detrimental to performance). There is margin for investigating this latter possibility further, by examining instances in which a greater manifestation of racial bias leads to a reduction of performance, regardless of whether the stakeholders identify such a bias in the coach or not (meaning if the coach has a bias, and this goes undetected, any subsequent performance implications of such bias would still materialize and have their own consequences). It could then be looked at whether lower-performing coaches would be less likely to receive continued employment or promotion to head coach positions.

Implications for Replicability and Using Sports Data

Our investigation also highlights the advantages of using sports data in social science. In our opinion, the biggest advantage is the transparency and replicability of findings. Even though the use of sports data is sometimes criticized based on (contestable) assumptions about narrow generalizability (Wolfe et al., 2005), an open platform of shared data across researchers is highly desirable, as it facilitates exchange among scholars and extensions of each other's work. To use our endeavor as an example, we engaged with SMZ's work out of interest in the setting, and because of our respect for their contribution. We commend the authors for the clarity of their writing and the transparency of their analysis; it enabled us to replicate and then extend and qualify their conclusions. Given that organizational researchers frequently, and lamentably, work on private datasets, we believe that our analyses are a good example of the value of open exchange in terms of facilitating scientific progress, especially on important topics such as racial bias.

Another implication of this replication and extension concerns the file drawer problem (otherwise known as publication bias). This is the tendency for positive findings (i.e., significant), rather than non-findings (i.e., null effects/hypothesis not being rejected), to be published more frequently. The reason is that such positive findings create opportunities for further engagement with the evidence offered (an idea discussed in general scientific work, e.g., Easterbrook et al., 1991, in psychology, e.g., Simmons, Nelson, & Simonsohn, 2011; and also noted as pertinent in management, e.g., Harrison et al., 2017). Even when significant previous findings are not replicated, sharing such work still paves the way for the extension or refinement of the original finding or ideas, in effect yielding a larger and more rigorous body of knowledge about the topic in question. Moreover, findings on matters that have broader societal or public policy implications, such as racial bias, may be more likely to be published in academic journals and also referred to in various media outlets. This is not surprising since treating individuals or groups differently based on their race or gender is a matter of great importance, and significant research results raise the possibility of intervening and reducing these problematic ways in which people are treated. Replication and extension, of the kind we engage in, and which the *Academy of Management Discoveries* makes possible in the field of management, are valuable to balance the well-documented, and understandable, publication tendencies.

The precise measurement of performance and the availability of micro-data also make professional sports a valuable setting to disentangle taste-based preferences from statistical discrimination (Guryan & Charles, 2013). Our study utilized this data to highlight how associations that can be taken to imply race-based preferences could instead be produced by observed differences in player attributes — such as performance — and that these observed differences could vary by race groups. Similarly, coaches' tactics might also come into play: anecdotal evidence suggests that different playing tactics might correlate with coaches' race (i.e., a faster or slower game; outside driven vs. inside-out); this could also lead coaches to deploy players of difference races, which could then be correlated with systematic differences in players' characteristics. For instance, despite having, on average, a lower PER, we see that white players in the NBA have a higher rate of assists per minute; higher three point shot percentages; and are taller and heavier than black players, making them more likely to play pivot positions. Future studies that look at race dynamics in sport should thus try to control for other possible differences that might correlate with race, such as coaches' tactical preferences and players' playing style and characteristics, to avoid attributing differences in outcomes to race when in fact they might well have different inherent explanations.

Conclusion

In 2014, Donald Sterling – the former owner of the Los Angeles Clippers – was forced to sell his franchise after making racist remarks, illustrating that taste-based racial bias continues to afflict professional sports. We believe in the importance of documenting any instance of racial bias and investigating possible underlying generating mechanisms so that researchers and practitioners can formulate and implement effective ways to eliminate it. We hope that our analysis will contribute to this goal. In work that investigates the differential treatment of racial groups, it is important to consider whether observed associations might be indicative of racial biases or whether they are generated by observable differences in other relevant attributes, such as individual talent. It is only after such relevant factors have been accounted for that one can fairly attribute observed same-race correlations to racial bias.

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TABLE 1. Descriptive statistics and correlation table (year-level data; n = 19,837)

	Mean	S.D.	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1 Minutes per game	20.87	10.55	0	48.53																		
2 Same-race coach	40.80%	0.491	0	1	-0.06																	
3 Years of collaboration (log)	0.49	0.60	0	2.94	0.32	-0.01																
4 Coach experience (log)	1.53	0.95	0	3.43	-0.01	-0.08	0.19															
5 Other-race players coached (log)	3.17	1.66	0	5.98	-0.02	-0.21	0.18	0.90														
6 Points per 48 minutes	18.51	6.82	0	144	0.45	-0.05	0.16	-0.01	-0.02													
7 Rebounds per 48 minutes	8.86	4.55	0	96	0.01	0.02	0.00	-0.05	-0.06	0.04												
8 Assists per 48 minutes	4.15	2.94	0	48	0.24	-0.03	0.10	-0.01	0.01	0.11	-0.43											
9 Team's past win–loss record	0.50	0.12	0.19	0.83	0.00	-0.03	0.23	0.14	0.12	0.04	0.01	0.02										
10 Team diversity	0.69	0.23	0	1	0.02	0.03	0.01	-0.06	-0.03	0.09	0.06	0.06	0.10									
11 Percentage same-race teammates	0.65	0.23	0.04	1	0.08	-0.27	-0.01	0.03	0.01	0.02	-0.10	0.04	-0.05	-0.49								
12 Seniority on the team	17.22	4.34	3	24	0.45	0.02	0.62	-0.02	-0.02	0.20	0.03	0.12	0.02	0.05	-0.02							
13 Years in the league	5.32	4.03	1	24	0.19	-0.01	0.30	0.09	0.08	-0.03	-0.05	0.06	0.15	-0.10	0.08	0.45						
14 Fouls per 48 minutes	5.08	1.75	0	48.00	-0.37	0.06	-0.11	-0.03	-0.03	-0.21	0.28	-0.29	-0.02	0.05	-0.12	-0.13	-0.15					
15 Defensive-oriented player	49.30%	0.5	0	1	-0.21	0.03	-0.05	0.00	0.00	-0.29	0.62	-0.59	-0.01	-0.01	-0.08	-0.08	-0.04	0.36				
16 Coach's past win–loss record	0.53	0.07	0.19	0.79	-0.01	-0.07	0.21	0.29	0.26	0.01	0.00	0.00	0.57	0.01	0.00	0.00	0.12	-0.03	0.00			
17 Player is also the coach	0.20%	0.044	0	1	0.01	0.05	0.00	-0.05	-0.05	0.00	0.00	0.04	0.00	0.02	-0.01	0.05	0.06	-0.01	-0.02	-0.01		
18 Coach is also the GM	10.20%	0.303	0	1	0.00	0.01	0.04	0.09	0.05	0.01	0.02	-0.01	-0.01	0.01	0.00	0.00	0.00	0.03	0.00	0.05	-0.01	
19 Western Conference	50.30%	0.5	0	1	0.00	-0.01	0.01	0.05	0.06	0.04	0.01	0.02	0.06	0.11	-0.06	0.01	-0.03	-0.01	-0.01	0.04	0.01	-0.01

TABLE 2. Replication of Zhang's year-level analysis (2017)

	Model 1 1955-2000	Model 2 1955-2000	Model 3 1955-2000	Model 4 1955-2000	Model 5 1955-2016
Same-race coach	.561*	.769*			.473**
	(0.252)	(0.357)			(0.150)
Same-race coach x Years of collaboration (log)		- .534*			
		(0.246)			
Same-race coach x Coach experience (log)		.041			
		(0.191)			
Black coach-Black player			.448	.701	
			(0.286)	(0.464)	
White coach-White player			.893#	1 .099#	
			(0.532)	(0.593)	
Black coach-Black player x Years of collaboration (log)				- .786#	
				(0.403)	
White coach-White player x Years of collaboration (log)				- .423	
				(0.277)	
Black coach-Black player x Coach experience (log)				.101	
				(0.297)	
White coach-White player x Coach experience (log)				.008	
				(0.213)	
Points per 48 minutes	.334***	.334***	.333***	.334***	.363***
	(0.042)	(0.042)	(0.042)	(0.042)	(0.032)
Rebounds per 48 minutes	- .069	- .069	- .069	- .069	- .028
	(0.047)	(0.047)	(0.047)	(0.047)	(0.041)
Assists per 48 minutes	.414***	.411***	.414***	.412***	.419***
	(0.078)	(0.078)	(0.078)	(0.078)	(0.059)
Team's past win–loss record	-3 .580***	-3 .591***	-3 .576***	-3 .588***	-2 .617***
	(0.929)	(0.929)	(0.928)	(0.928)	(0.671)
Team diversity	.381	.377	.355	.365	- .010
	(0.488)	(0.489)	(0.492)	(0.493)	(0.367)
Percentage same-race teammates	- .994	-1 .098	-1 .003	-1 .086	- .796
	(0.846)	(0.850)	(0.847)	(0.852)	(0.708)
Seniority on the team	.511***	.511***	.510***	.511***	.487***
	(0.030)	(0.030)	(0.030)	(0.030)	(0.022)
Years in the league	.386**	.389**	.385**	.389**	.088
	(0.137)	(0.137)	(0.137)	(0.137)	(0.125)
Fouls per 48 minutes	- .522***	- .520***	- .523***	- .522***	- .585***
	(0.062)	(0.062)	(0.062)	(0.062)	(0.054)
Defensive-oriented player	- .417	- .422	- .415	- .417	- .569*
	(0.342)	(0.342)	(0.342)	(0.342)	(0.262)
Coach's past win–loss record	-1 .170	-1 .222	-1 .214	-1 .294	-1 .882#
	(1.327)	(1.325)	(1.325)	(1.322)	(1.014)
Player is also the coach	-4 .475***	-4 .542***	-4 .451***	-4 .479***	-4 .336**
	(1.271)	(1.283)	(1.272)	(1.276)	(1.331)
Coach is also the GM	.017	.001	.014	- .009	.211
	(0.264)	(0.264)	(0.264)	(0.265)	(0.214)
Western Conference	.005	.006	.002	.005	- .187
	(0.199)	(0.199)	(0.199)	(0.199)	(0.154)
Years of collaboration (log)	.521**	.715***	.526**	.717***	.344**
	(0.165)	(0.188)	(0.165)	(0.189)	(0.125)
Coach experience (log)	- .479***	- .488***	- .484***	- .491***	- .306***
	(0.093)	(0.114)	(0.094)	(-0.115)	(0.067)
Year and player fixed effects	Yes	Yes	Yes	Yes	Yes
Constant	40.42***	40.46***	40.33***	40.41***	39.05***
Number of observations	12,029	12,029	12,029	12,029	19,837
R-squared (overall)	0.238	0.239	0.238	0.239	0.256
Robust standard errors clustered on players	Yes	Yes	Yes	Yes	Yes

[a] Robust standard errors are in parentheses.

[#] $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; two-tailed tests.

TABLE 3. Descriptive statistics and correlation table (game-level data; n = 847,429)

	Mean	S.D.	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1 Minutes per game	22.85	12.45	0	69.00																			
2 Same-race coach	37.70%	0.49	0	1	-0.05																		
3 Absolute difference in final score	11.26	8.09	1	68.00	-0.09	0.00																	
4 Starting the game	0.48	0.50	0	1.00	0.68	-0.04	-0.04																
5 Relative PER	0.36	0.95	-4.65	4.33	0.49	-0.04	-0.04	0.42															
6 Points per 48 minutes	17.96	8.03	0	1170	0.33	-0.07	-0.01	0.26	0.53														
7 Rebounds per 48 minutes	8.30	4.57	0	960	0.01	0.03	0.00	0.07	0.16	0.08													
8 Assists per 48 minutes	4.05	3.15	0	96	0.21	-0.04	-0.01	0.15	0.21	0.18	-0.34												
9 Team's past win–loss record	0.51	0.13	0.19	0.83	0.00	-0.05	0.01	0.00	-0.01	0.03	0.00	0.04											
10 Team diversity	0.61	0.28	0	1	-0.04	0.05	0.05	-0.02	-0.02	0.02	0.01	0.03	0.09										
11 Percentage same-race teammates	0.69	0.25	0.00	1	0.11	-0.30	-0.03	0.07	0.07	0.05	-0.07	0.04	-0.05	-0.56									
12 Seniority on the team	16.98	4.88	0	27	0.31	-0.01	-0.03	0.28	0.28	0.18	0.04	0.11	0.06	0.00	0.01								
13 Years in the league	5.99	4.08	1	24	0.12	-0.01	-0.02	0.10	0.05	0.00	-0.01	0.05	0.21	-0.06	0.06	0.30							
14 Fouls per 48 minutes	5.28	8.31	0	2880	-0.11	0.02	0.01	-0.09	-0.09	0.02	0.10	-0.08	0.00	0.00	-0.03	-0.06	-0.05						
15 Coach's past win–loss record	53.00%	0.08	0.191	1	0.00	-0.09	0.01	0.00	-0.01	0.02	0.00	0.02	0.59	0.02	-0.01	0.04	0.16	0.00					
16 Defensive-oriented player	0.48	0.50	0.00	1.00	-0.19	0.05	0.01	-0.09	-0.13	-0.23	0.58	-0.55	-0.01	-0.01	-0.09	-0.06	-0.04	0.12	0.00				
17 Years of collaboration (log)	56.40%	0.63	0	3	0.24	-0.02	-0.01	0.22	0.23	0.16	0.03	0.12	0.27	0.04	-0.03	0.64	0.24	-0.04	0.23	-0.04			
18 Coach experience (log)	165.50%	0.94	0	3	0.01	-0.06	0.01	0.01	0.00	0.02	0.00	0.01	0.15	0.04	-0.02	0.04	0.06	0.00	0.27	-0.01	0.16		
19 Coach is also the GM	8.80%	0.28	0	1	0.00	0.00	0.00	0.00	0.00	0.01	-0.01	0.00	-0.05	0.02	-0.01	-0.02	-0.01	0.02	-0.02	0.00	-0.01	0.04	
20 Western Conference	50.00%	0.50	0	1	-0.01	-0.04	0.02	0.00	0.00	0.04	0.01	0.03	0.10	0.12	-0.07	0.02	0.00	0.00	0.07	0.00	0.05	0.12	0.03

TABLE 4. Analysis of same-race coach effect at the game level

	Model 6 **1981-2016**	**Model 7** **1981-2016**	**Model 8** **1981-2016**	**Model 9** **1981-2016**	**Model 10** **1981-2016**	**Model 11** **1981-2016**
Same-race coach	.332*	.329*	.138	.135	.052	.078
	(0.161)	(0.161)	(0.108)	(0.108)	(0.109)	(0.142)
Absolute difference in final score (centered)		- .092***	- .084***	.140***	.037***	.037***
		(0.005)	(0.005)	(0.008)	(0.003)	(0.003)
Same-race coach x Absolute difference in final score (cent.)		.017**	.018**	.000	.004	.005
		(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
Starting the game			11 .750***	11 .770***	11 .680***	11 .550***
			(0.109)	(0.108)	(0.101)	(0.099)
Starting the game x Absolute difference in final score (cent.)				- .217***	- .204***	- .204***
				(0.004)	(0.004)	(0.004)
Points per 48 mins x Absolute difference in final score (cent.)				- .005***		
				(0.001)		
Rebounds per 48 mins x Absolute difference in final score (cen				- .002*		
				(0.001)		
Assists per 48 mins x Absolute difference in final score (cent.)				- .005***		
				(0.001)		
Relative PER					2 .432***	2 .418***
					(0.067)	(0.066)
Relative PER x Absolute difference in final score (cent.)					- .056***	- .056***
					(0.002)	(0.002)
Points per 48 minutes (in previous games during season)	.275***	.274***	.184***	.198***		
	(0.049)	(0.049)	(0.034)	(0.030)		
Rebounds per 48 minutes (in previous games during season)	.201***	.198***	.149***	.148***		
	(0.044)	(0.043)	(0.030)	(0.027)		
Assists per 48 minutes (in previous games during season)	.405***	.402***	.167***	.168***		
	(0.058)	(0.058)	(0.035)	(0.033)		
Team's past win–loss record	-2 .488***	-2 .448***	-1 .162*	-1 .222*	- .487	- .021
	(0.702)	(0.7)	(0.477)	(0.478)	(0.477)	(0.575)
Team diversity	.316	.469#	.118	.134	.139	.188
	(0.283)	(0.282)	(0.187)	(0.186)	(0.188)	(0.182)
Percentage same-race teammates	.496	.437	.333	.297	.226	.193
	(0.638)	(0.635)	(0.430)	(0.430)	(0.440)	(0.420)
Seniority on the team	.388***	.385***	.216***	.214***	.191***	.194***
	(0.020)	(0.020)	(0.013)	(0.013)	(0.012)	(0.013)
Years in the league	.073	.069	.172#	.174#	- .014#	.135
	(0.160)	(0.159)	(0.099)	(0.1)	(0.099)	(0.095)
Fouls per 48 minutes (in previous games during season)	- .126	- .126	- .066	- .062	- .008	- .006
	(0.081)	(0.081)	(0.043)	(0.041)	(0.005)	(0.004)
Coach's past win–loss record	-2 .711*	-2 .668*	-1 .562*	-1 .635*	-1 .585*	-1 .233#
	(1.057)	(1.054)	(0.718)	(0.722)	(0.723)	(0.749)
Defensive-oriented player	-2 .779***	-2 .785***	-1 .493***	-1 .453***	-1 .267***	-1 .245***
	(0.311)	(0.309)	(0.194)	(0.188)	(0.166)	(0.164)
Years of collaboration (log)	.226#	.232#	.117	.123	.138	.134
	(0.136)	(0.136)	(0.088)	(0.088)	(0.089)	(0.097)
Coach experience (log)	- .198**	- .194**	- .104*	- .105*	- .068	- .257#
	(0.072)	(0.072)	(0.049)	(0.049)	(0.049)	(0.132)
Coach is also the GM	.838**	.839**	.414*	.421*	.353#	.534*
	(0.256)	(0.255)	(0.180)	(0.181)	(0.181)	(0.234)
Western Conference	- .508**	- .488**	- .391**	- .399**	- .208#	.850
	(0.186)	(0.185)	(0.125)	(0.125)	(0.126)	(0.562)
Player and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Team and coach fixed effects	No	No	No	No	No	Yes
Number of observations	847,429	847,429	847,429	847,429	847,429	847,429
R-squared (overall)	0.477	0.480	0.615	0.623	0.623	0.627
Robust standard errors clustered on players	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors are in parentheses. Years indicate the year in which a given season starts (e.g. 2016 = 2016/17 season).

Variables in light gray are Zhang's explanatory variables (2017). Variables in dark gray test our alternative explanations. Variables in white are Zhang's (2017) controls.

# $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; two-tailed tests.

TABLE 5. Analysis of same-race coach effect at the game level, by race

	Model 12 1981-2016	Model 13 1981-2016	Model 14 1981-2016	Model 15 1981-2016
Black coach-Black player	.151 (0.180)	.018 (0.121)	.016 (0.121)	.099 (1.276)
White coach-White player	.381 (0.360)	.204 (0.249)	.188 (0.250)	.056 (1.262)
Absolute difference in final score (centered)		- .084*** (0.005)	.037*** (0.003)	.037*** (0.003)
Black coach-Black player x Absolute difference in final score (cent.)		.002 (0.007)	.003 (0.004)	.003 (0.004)
White coach-White player x Absolute difference in final score (cent.)		.037*** (0.009)	.006 (0.005)	.006 (0.005)
Starting the game		11 .640*** (0.1)	11 .680*** (0.101)	11 .550*** (0.099)
Starting the game x Absolute difference in final score (cent.)			- .204*** (0.004)	- .204*** (0.004)
Relative PER	3 .848*** (0.097)	2 .390*** (0.067)	2 .432*** (-0.000)	2 .418*** (0.066)
Relative PER x Absolute difference in final score (cent.)			- .056*** (0.002)	- .056*** (0.002)
Team's past win–loss record	-1 .296# (0.689)	- .430 (0.475)	- .478 (0.477)	- .021 (0.575)
Team diversity	.298 (0.283)	.112 (0.187)	.130 (0.187)	.189 (0.182)
Percentage same-race teammates	.393 (0.647)	.245 (0.437)	.219 (0.439)	.193 (0.419)
Seniority on the team	.346*** (0.018)	.192*** (0.012)	.191*** (0.012)	.194*** (0.013)
Years in the league	.026 (0.155)	.144 (0.098)	.143 (0.099)	.135 (0.095)
Fouls per 48 minutes (in previous games during season)	- .038# (0.021)	- .009 (0.005)	- .008 (0.005)	- .006 (0.004)
Coach's past win–loss record	-2 .784** (1.043)	-1 .572* (0.717)	-1 .618* (0.722)	-1 .233# (0.749)
Defensive-oriented player	-2 .452*** (0.263)	-1 .275*** (0.165)	-1 .266*** (0.166)	-1 .245*** (0.164)
Years of collaboration (log)	.245# (0.134)	.126 (0.089)	.138 (0.089)	.134 (0.097)
Coach experience (log)	- .144* (0.072)	- .069 (0.049)	- .071 (0.049)	- .257# (0.132)
Coach is also the GM	.713** (0.255)	.344# (0.180)	.350# (0.181)	.534* (0.234)
Western Conference	- .224 (0.187)	- .210# (0.126)	- .213# (0.126)	.849 (0.562)
Player and year fixed effects	Yes	Yes	Yes	Yes
Team and coach fixed effects	No	No	No	Yes
Number of observations	847,429	847,429	847,429	847,429
R-squared (overall)	0.481	0.616	0.623	0.627
Robust standard errors clustered on players	Yes	Yes	Yes	Yes

Robust standard errors are in parentheses. Years indicate the year in which a given season starts (e.g. 2016 = 2016/17 season)
Variables in light gray are Zhang's explanatory variables (2017). Variables in dark gray test our alternative explanations. Variables in white are Zhang's (2017) controls.

# $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; two-tailed tests.

TABLE 6. Analysis of same race effect, by decade

	Model 16 1981-1989	Model 17 1990-1999	Model 18 2000-2009	Model 19 2010-2016	Model 20 1986-1995	Model 21 1995-2004	Model 22 All BUT 1997-1999
Same-race coach	- .199	.932*	.123	.238	.054	.516#	.167
	(0.540)	(0.366)	(0.229)	(0.286)	(0.393)	(0.293)	(0.161)
Points per 48 minutes	.244***	.216***	.402***	.156*	.220***	.331***	.311***
	(0.020)	(0.021)	(0.022)	(0.076)	(0.023)	(0.033)	(0.061)
Rebounds per 48 minutes	.050#	.179***	.121***	.482***	.145***	.202***	.277***
	(0.029)	(0.031)	(0.035)	(0.085)	(0.039)	(0.042)	(0.060)
Assists per 48 minutes	.307***	.235**	.453***	.498***	.278***	.375***	.517***
	(0.063)	(0.071)	(0.063)	(0.138)	(0.056)	(0.101)	(0.066)
Team's past win–loss record	1 .808	-3 .915*	- .002	- .717	-4 .561*	-1 .796	-2 .561*
	(2.363)	(1.492)	(1.516)	(1.556)	(1.748)	(1.485)	(1.018)
Team diversity	.121	.789	- .040	- .404	1 .403*	.637	1 .352***
	(0.536)	(0.503)	(0.486)	(0.631)	(0.569)	(0.493)	(0.334)
Percentage same-race teammates	.438	1 .460	1 .074	1 .017	1 .128	1 .156	4 .053***
	(1.057)	(1.121)	(0.974)	(1.294)	(1.165)	(1.069)	(0.617)
Seniority on the team	.378***	.333***	.323***	.354***	.386***	.348***	.388***
	(0.053)	(0.034)	(0.031)	(0.036)	(0.040)	(0.035)	(0.023)
Years in the league	.571	.134	- .277	.214	.474	.341	- .128
	(0.374)	(0.311)	(0.301)	(0.280)	(0.354)	(0.3)	(0.159)
Fouls per 48 minutes	- .636***	- .601***	- .175***	- .032	- .810***	- .385***	- .075#
	(0.048)	(0.059)	(0.050)	(0.026)	(0.053)	(0.056)	(0.039)
Coach's past win–loss record	-5 .466#	-1 .712	-5 .555*	-2 .348	2 .369	-3 .922#	-2 .484#
	(3.143)	(1.892)	(2.640)	(2.307)	(1.955)	(1.987)	(1.416)
Defensive-oriented player	-1 .001	-2 .831***	-2 .314***	-3 .185***	-1 .554**	-2 .486***	-2 .626***
	(0.611)	(0.513)	(0.343)	(0.512)	(0.456)	(0.533)	(0.323)
Years of collaboration (log)	.653*	- .012	- .107	- .364	.076	.113	.290#
	(0.316)	(0.271)	(0.205)	(0.276)	(0.246)	(0.295)	(0.153)
Coach experience (log)	- .333#	- .353*	- .096	.189	- .413**	- .249	- .210*
	(0.195)	(0.156)	(0.128)	(0.154)	(0.148)	(0.163)	(0.086)
Coach is also the GM	.659	.702	.615	- .105	.788	.675	.798*
	(0.611)	(0.429)	(0.453)	(0.868)	(0.567)	(0.450)	(0.344)
Western Conference	- .830	- .116	- .244	-1 .425**	.075	- .754*	- .591**
	(0.571)	(0.382)	(0.371)	(0.422)	(0.448)	(0.347)	(0.219)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Player fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	No
Number of observations	181,111	225,844	246,531	194,024	220,659	232,579	860,987
R-squared (overall)	0.461	0.514	0.528	0.534	0.515	0.513	0.502
Robust standard errors clustered on players and coaches	Yes	Yes	Yes	Yes	Yes	Yes	Yes

[a] Robust standard errors are in parentheses.

Variables in light gray are Zhang's explanatory variables (2017). Variables in white are Zhang's (2017) controls.

[#] $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; two-tailed tests.

TABLE 7. Analysis of same race effect in closely contested games

	Model 23 1990-2014 *Final score < 5*	Model 24 Full sample *Final score < 5*	Model 25 Full sample *Final score < 5*	Model 26 Full sample *Final score < 5*	Model 27 Full sample *Final score < 5*	Model 28 Full sample *Final score < 10*
Same-race coach	.462**	.287	.254	.158	.031	.075
	(0.169)	(0.174)	(0.176)	(0.167)	(0.117)	(0.106)
Relative PER				4 .026***	2 .582***	2 .520***
				(0.114)	(0.081)	(0.074)
Starting the game					12 .510***	12 .350***
					(0.196)	(0.177)
Team record in current season (centered)	-5 .548***	-6 .149***	-5 .941***	-3 .306***	-1 .596***	-1 .670***
	(0.493)	(0.512)	(0.482)	(0.533)	(0.429)	(0.349)
Team record in previous ten games (centered)	-1 .882***	-1 .457***	-1 .535***	-1 .047***	- .699**	- .757***
	(0.269)	(0.266)	(0.255)	(0.262)	(0.212)	(0.160)
Points per min	6 .819***	8 .090***	8 .086***			
	(1.835)	(1.613)	(1.624)			
Rebounds per min	-1 .549	6 .127**	6 .234**			
	(2.358)	(1.931)	(1.870)			
Assists per min	1 .350**	.714	.387			
	(3.932)	(6.348)	(6.307)			
Points per min (last 10 games)	6 .693***	6 .335***	6 .432***			
	(1.087)	(0.832)	(0.815)			
Rebounds per min (last 10 games)	9 .886***	2 .037	1 .973			
	(1.988)	(1.476)	(1.394)			
Assists per min (last 10 games)	13 .820***	11 .240***	11 .970***			
	(3.292)	(1.994)	(1.965)			
Fouls per min	-14 .001***	-1 .842	-1 .860	- .948	- .296	- .361
	(2.875)	(1.642)	(1.669)	(0.748)	(0.209)	(0.267)
Years in the league	1 .185***	1 .336***	2 .085***	1 .848***	1 .225***	1 .205***
	(0.095)	(0.092)	(0.217)	(0.207)	(0.142)	(0.129)
Years in the league squared	- .098***	- .105***	- .107***	- .094***	- .058***	- .058***
	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.003)
Years of coach-player collaboration (log)	.066	.4*	.375*	.333#	.234#	.216#
	(0.211)	(0.194)	(0.174)	(0.173)	(0.135)	(0.115)
Num of games played for this team	.005***	.005***	.005***	.005***	.002***	.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
First year on the team	-1 .132***	-1 .033***	- .992***	- .830***	- .419**	- .432***
	(0.202)	(0.195)	(0.182)	(0.190)	(0.145)	(0.124)
Year fixed effects	No	No	Yes	Yes	Yes	Yes
Player fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	131,005	186,546	186,546	186,546	186,546	447,074
R-squared (overall)	0.523	0.494	0.501	0.500	0.637	0.636
Robust standard errors clustered on players and c	Yes	Yes	Yes	Yes	Yes	Yes

[a] Robust standard errors are in parentheses.

Variables in light gray are Zhang's explanatory variables (2019). Variables in dark gray test our alternative explanations. Variables in white are Zhang's (2019) controls.

[#] $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; two-tailed tests.

TABLE 8. Analysis of same-race coach effect at the game level on complete data subsample

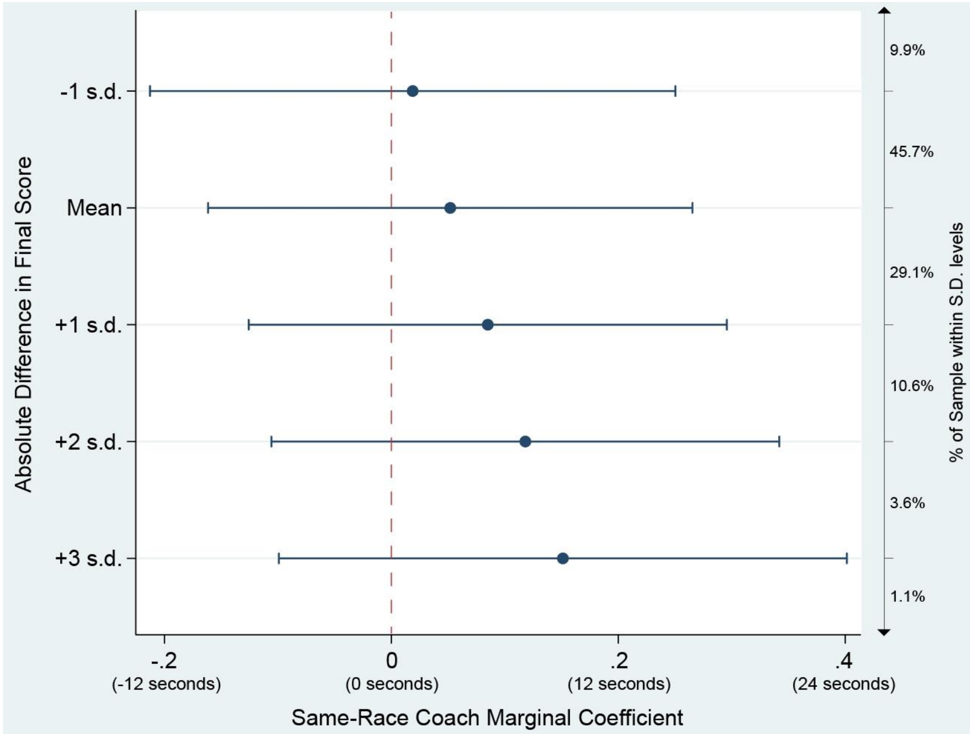
	Model 29 1996-2016	Model 30 1996-2016	Model 31 1996-2016	Model 32 1996-2016	Model 33 1996-2016	Model 34 1996-2016
Same-race coach	.344#	.343#	.339#	.081	.075	- .302#
	(0.183)	(0.183)	(0.182)	(0.128)	(0.127)	(0.176)
Absolute difference in final score (centered)		- .023***	- .007	- .009	.274***	.137***
		(0.007)	(0.007)	(0.006)	(0.008)	(0.004)
Same-race coach x Absolute difference in final score (cent.)		.004	.004	.006	- .003	.003
		(0.008)	(0.008)	(0.008)	(0.004)	(0.005)
Game went to overtime			2 .489***	2 .503***	2 .510***	2 .507***
			(0.081)	(0.074)	(0.073)	(0.073)
Starting the game				13 .410***	13 .360***	13 .490***
				(0.173)	(0.164)	(0.140)
Starting the game x Absolute difference in final score (cent.)					- .277***	- .291***
					(0.007)	(0.005)
Points per 48 mins x Absolute difference in final score (cent.)					- .007***	
					(0.001)	
Rebounds per 48 mins x Absolute difference in final score (cent.)					- .003***	
					(0.001)	
Assists per 48 mins x Absolute difference in final score (cent.)					- .010***	
					(0.001)	
Relative PER						2 .705***
						(0.094)
Relative PER x Absolute difference in final score (cent.)						- .060***
						(0.002)
Points per 48 minutes (in previous games during season)	.340***	.340***	.340***	.232***	.249***	
	(0.099)	(0.099)	(0.099)	(0.068)	(0.060)	
Rebounds per 48 minutes (in previous games during season)	.374***	.374***	.375***	.272***	.260***	
	(0.091)	(0.091)	(0.091)	(0.063)	(0.058)	
Assists per 48 minutes (in previous games during season)	.621***	.621***	.621***	.321***	.308***	
	(0.124)	(0.124)	(0.124)	(0.077)	(0.071)	
Team's past win–loss record	-1 .612#	-1 .608#	-1 .572#	- .492	- .457	.722
	(0.9)	(0.9)	(0.9)	(0.612)	(0.605)	(0.841)
Team diversity	1 .933***	1 .968***	1 .971***	1 .519***	1 .399***	1 .674***
	(0.360)	(0.359)	(0.359)	(0.252)	(0.247)	(0.267)
Percentage same-race teammates	6 .429***	6 .452***	6 .440***	5 .398***	5 .046***	5 .141***
	(0.841)	(0.841)	(0.841)	(0.6)	(0.589)	(0.618)
Seniority on the team	.384***	.384***	.384***	.191***	.186***	.203***
	(0.028)	(0.028)	(0.028)	(0.018)	(0.017)	(0.017)
Years in the league	- .317	- .318	- .318	- .093	- .014#	- .150
	(0.210)	(0.210)	(0.210)	(0.135)	(0.134)	(0.142)
Fouls per 48 minutes (in previous games during season)	- .052#	- .052#	- .052#	- .026	- .023	.000
	(0.031)	(0.031)	(0.031)	(0.017)	(0.015)	(0.010)
Coach's past win–loss record	-4 .118**	-4 .112**	-4 .164**	-2 .160*	-2 .238*	- .473
	(1.383)	(1.382)	(1.382)	(0.953)	(0.953)	(1.099)
Defensive-oriented player	-3 .121***	-3 .122***	-3 .125***	-1 .585***	-1 .539***	-1 .166***
	(0.432)	(0.431)	(0.432)	(0.280)	(0.264)	(0.216)
Years of collaboration (log)	.000	.003	- .000	.160	.164	.098
	(0.179)	(0.179)	(0.179)	(0.120)	(0.119)	(0.139)
Coach experience (log)	- .091	- .091	- .093	- .070	- .069	- .355#
	(0.091)	(0.091)	(0.091)	(0.064)	(0.063)	(0.213)
Coach is also the GM	.975**	.979**	.985**	.423#	.416#	.201
	(0.328)	(0.328)	(0.328)	(0.228)	(0.228)	(0.331)
Western Conference	- .587*	- .584*	- .585*	- .429**	- .423**	2 .189#
	(0.251)	(0.251)	(0.251)	(0.165)	(0.164)	(1.3)
Player and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Team and coach fixed effects	No	No	No	No	No	Yes
Number of observations	586,386	586,386	586,386	586,386	586,386	586,386
R-squared (overall)	0.536	0.536	0.538	0.670	0.681	0.676
Robust standard errors clustered on players	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors are in parentheses. Years indicate the year in which a given season starts (e.g. 2016 = 2016/17 season)

Variables in light gray are Zhang's explanatory variables (2017). Variables in dark gray test our alternative explanations. Variables in white are Zhang's (2017) controls.

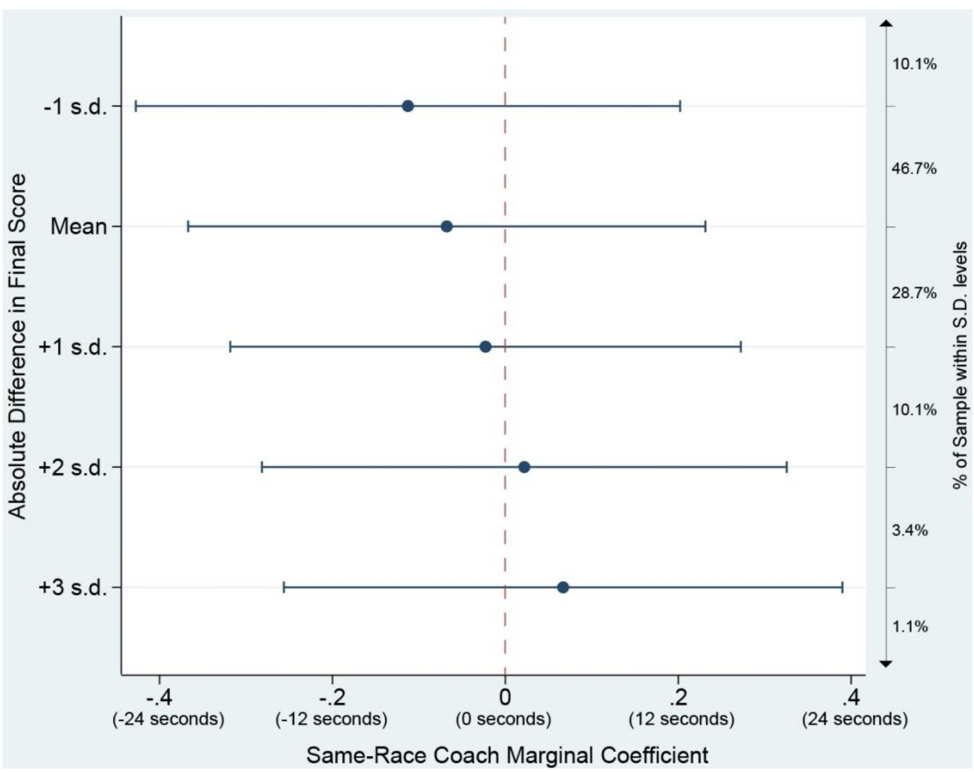
# $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; two-tailed tests.

FIGURE 1a. Marginal Effects of Same-Race Coach on Playing Time



Note: Two-tailed tests

FIGURE 1b. Marginal Effects of Same-Race Coach on Playing Time (with DNP records)



Note: Two-tailed tests

APPENDIX A – COMPARISONS OF SAMPLES

TABLE A1: Comparison of summary statistics (year-level data, 1955-2000) with Zhang (2017)

Variable	Mean		S.D.		Min		Max	
Minutes per game	21.20	(21.14)	10.76	(10.75)	0	(1)	48.53	(48.5)
Same-race coach	39.26%	(39.17%)	NA	(NA)	0	(0)	1	(1)
Years of collaboration (log)	0.50	(0.47)	0.60	(0.59)	0	(0)	2.77	(2.77)
Coach experience (log)	1.44	(1.4)	0.92	(0.93)	0	(0)	3.30	(3.3)
Other-race players coached (log)	3.04	(2.85)	1.63	(1.65)	0	(0)	5.59	(5.44)
Points per 48 minutes	19.27	(19.26)	6.89	(6.9)	0	(0)	144	(144)
Rebounds per 48 minutes	9.13	(9.12)	4.74	(4.74)	0	(0)	96	(96)
Assists per 48 minutes	4.38	(4.4)	3.00	(3.03)	0	(0)	48	(48)
Team’s past win–loss record	0.51	(0.5)	0.12	(0.12)	0.19	(0.19)	0.83	(0.83)
Team diversity	0.74	(0.74)	0.22	(0.22)	0	(0)	1	(1)
Percentage same-race teammates	0.63	(0.63)	0.22	(0.22)	0.05	(0.05)	1	(1)
Seniority on the team	17.56	(17.55)	4.13	(4.14)	3	(3)	24	(24)
Years in the league	4.87	(4.73)	3.71	(3.46)	1	(1)	22	(21)
Fouls per 48 minutes	5.27	(5.48)	1.71	(1.95)	0	(0)	32.00	(26.67)
Defensive-oriented player	49.00%	(49.12%)	NA	(NA)	0	(0)	1	(1)
Coach’s past win–loss record	0.52	(0.52)	0.07	(0.09)	0.28	(0.22)	0.78	(0.83)
Player is also the coach	0.32%	(0.26%)	NA	(NA)	0	(0)	1	(1)
Coach is also the GM	13.10%	(12.21%)	NA	(NA)	0	(0)	1	(1)
Western Conference	50.67%	(49.63%)	NA	(NA)	0	(0)	1	(1)

Statistics from Zhang (2017) in parentheses

TABLE A2: Comparison of correlation tables (year-level data, 1955-2000) with Zhang (2017)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1 Minutes per game																		
2 Same-race coach	-0.08																	
	(-0.08)																	
3 Years of collaboration (log)	0.32	-0.01																
	(0.32)	(-0.01)																
4 Coach experience (log)	-0.02	-0.08	0.22															
	(-0.01)	(-0.07)	(0.21)															
5 Other-race players coached (log)	-0.02	-0.19	0.19	0.88														
	(-0.02)	(-0.15)	(0.21)	(0.94)														
6 Points per 48 minutes	0.41	-0.07	0.13	-0.01	-0.02													
	(0.41)	(-0.07)	(0.14)	(-0.01)	(-0.02)													
7 Rebounds per 48 minutes	0.03	0.03	-0.02	-0.06	-0.09	0.04												
	(0.03)	(0.05)	(-0.01)	(-0.05)	(-0.07)	(0.03)												
8 Assists per 48 minutes	0.2	-0.03	0.1	0	0.02	0.06	-0.44											
	(0.21)	(-0.04)	(0.1)	(0)	(0.01)	(0.05)	(-0.44)											
9 Team's past win–loss record	0	-0.01	0.23	0.11	0.08	0.05	0.02	0.02										
	(0)	(0)	(0.24)	(0.12)	(0.11)	(0.05)	(0.02)	(0.02)										
10 Team diversity	0.02	0.08	0	-0.08	-0.05	0.08	0.07	0.03	0.11									
	(0.02)	(0.09)	(0.03)	(-0.06)	(-0.07)	(0.08)	(0.07)	(0.03)	(0.08)									
11 Percentage same-race teammates	0.09	-0.37	0.01	0.04	0.03	0.03	-0.09	0.05	-0.06	-0.51								
	(0.08)	(-0.37)	(-0.02)	(0.03)	(0.03)	(0.03)	(-0.1)	(0.05)	(-0.05)	(-0.5)								
12 Seniority on the team	0.44	0.04	0.6	-0.02	-0.02	0.17	0.01	0.11	0.02	0.04	-0.01							
	(0.4)	(0.03)	(0.56)	(-0.01)	(-0.02)	(0.2)	(0.02)	(0.1)	(0.01)	(0.05)	(-0.02)							
13 Years in the league	0.2	0	0.36	0.09	0.09	-0.01	-0.05	0.05	0.16	-0.08	0.05	0.5						
	(0.24)	(-0.01)	(0.36)	(0.07)	(0.09)	(0.01)	(-0.05)	(0.05)	(0.18)	(-0.06)	(0.04)	(0.3)						
14 Fouls per 48 minutes	-0.37	0.08	-0.11	-0.02	-0.02	-0.22	0.24	-0.28	-0.02	0.04	-0.11	-0.13	-0.12					
	(-0.39)	(0.08)	(-0.18)	(-0.02)	(-0.03)	(-0.2)	(0.23)	(-0.28)	(-0.04)	(0.04)	(-0.11)	(-0.21)	(-0.21)					
15 Defensive-oriented player	-0.18	0.03	-0.07	-0.01	-0.02	-0.28	0.63	-0.58	-0.01	0	-0.08	-0.08	-0.03	0.34				
	(-0.31)	(-0.01)	(-0.12)	(0.01)	(0.01)	(-0.41)	(0.17)	(-0.1)	(-0.01)	(0)	(0)	(-0.14)	(-0.13)	(0.2)				
16 Coach's past win–loss record	-0.02	-0.03	0.22	0.28	0.22	0.01	0	0	0.55	0.01	-0.01	0	0.13	-0.02	0			
	(-0.01)	(-0.02)	(0.19)	(0.25)	(0.22)	(0.02)	(0.01)	(0)	(0.55)	(0.03)	(-0.02)	(0)	(0.12)	(-0.02)	(0)			
17 Player is also the coach	0.02	0.07	0	-0.06	-0.06	0	0	0.05	0	0.02	-0.01	0.07	0.08	-0.02	-0.01	-0.02		
	(0.02)	(0.06)	(0.01)	(-0.05)	(-0.04)	(-0.01)	(0)	(0.05)	(0)	(0.02)	(-0.01)	(0.04)	(0.07)	(-0.02)	(-0.01)	(-0.01)		
18 Coach is also the GM	-0.01	0.02	0.05	0.12	0.07	-0.01	0.01	-0.02	-0.04	-0.03	0.02	-0.02	0.02	0.01	0	0.05	-0.02	
	(-0.01)	(0.02)	(0.05)	(0.12)	(0.09)	(-0.00)	(0.02)	(-0.02)	(-0.01)	(-0.04)	(0.03)	(-0.02)	(0.00)	(0.01)	(0)	(0.04)	(-0.02)	
19 Western Conference	-0.01	0.02	0.05	0.12	0.07	-0.01	0.01	-0.02	-0.04	-0.03	0.02	-0.02	0.02	0.01	0	0.05	-0.02	-0.02
	(0)	(-0.02)	(0)	(0.04)	(0.05)	(0.03)	(-0.02)	(0.03)	(0)	(0.02)	(0)	(.01)	(-0.02)	0	(-0.00)	(-0.03)	(-0.01)	(-0.04)

Correlations from Zhang (2017) in parentheses