Inclusion is not a Slam Dunk: A Study of Differential Leadership Outcomes in the Absence of a Glass Cliff

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ABSTRACT

Racial bias continues to act as one of the most thought provoking and controversial topics in our society. Even as organizations implement steps and policies to minimize discriminatory practices, evidence of bias in organizational decision-making persists. While much research has been devoted to the study of racial bias in hiring and promotion decisions, this study focuses on the effect of biases on employment outcomes of minority leaders after they have been hired or promoted to leadership positions that are comparable in quality to those of their white peers (i.e. no glass cliff present). More specifically, we investigate how discrimination influences performance rewards and employment separation decisions pertaining to minority leaders. The study uses archival data from the National Basketball Association collected from the year 2003 to 2015. From this data set, we utilize measures of head coaches' objective performance, reward allocation, and their likelihood of employment separation to find limited support for the hypotheses that minority leaders are given less time in position to achieve success and that when they do achieve success, they may be less likely than white leaders to be recognized for their accomplishments. Our findings suggest that in addition to researching selection processes, understanding why racial minorities are underrepresented in leadership positions also requires insight into the employment outcomes experienced by minority leaders.

Keywords: Glass Cliff; Leadership Prototype; Leadership Categorization; Attribution Theory; Aversive Racism

INTRODUCTION

In February of 1987, Dr. Clifton R. Wharton, Jr. made history as he took over the helm at TIAA-CREF and became the first black CEO of a major American corporation (Weiss, 1986). The significance of this milestone cannot be easily overstated. Although diversity in organizational leadership has been linked to better decision making (Krywulak & Sisco, 2008), growth in market share (Hewlett, Marshall, & Sherbin, 2013), and increased returns on equity (Barta, Kleiner, & Neumann, 2012), the market has been slow to follow TIAA-CREF's lead. The first black CEO of a current Fortune 500 company wasn't appointed until 1999 (Isidore, 2012). In fact, minorities still hold fewer than five percent of CEO positions in Fortune 500 companies (Zillman, 2014). Ascending the organizational ladder, however, is only one of the many challenges faced by racial minorities. In this paper, we examine the differential outcomes experienced by racial minorities *after* being appointed into leadership roles.

While some research has addressed the underrepresentation of racial minorities in leadership positions by examining inequities in promotion rates (e.g. Greenhaus, Parasuraman, & Wormley, 1990; James, 2000; Sagas & Cunningham, 2005), a growing body of literature has explored how biases that persist beyond the selection process influence outcomes for occupational minorities *after* being appointed to leadership positions. For example, research has shown evidence that racial minorities in leadership positions receive lower performance ratings than their white counterparts (Cox Jr. & Nkomo, 1986; Greenhaus et al., 1990; Hekman, Johnson, Foo, & Yang, 2017). Additionally, recent work has shown that minority leaders are evaluated more critically in times of failure (Carton & Rosette, 2011; Park & Westphal, 2013). One explanation for these differential outcomes can be derived from leadership categorization theory (LCT) which states that when leaders fail to meet the demographic expectations of a leadership prototype, they may fall victim to inequitable evaluations of performance (Rosette, Leonardelli, & Phillips, 2008). This explanation is consistent with findings of differential leadership outcomes in terms of compensation (Kulich, Trojanowski, Ryan, Haslam, & Renneboog, 2011) and performance evaluations (Lyness & Heilman, 2006) from the gender literature.

A divergent stream of literature has examined how a phenomenon known as the glass cliff impacts leadership outcomes for occupational minorities (Glass & Cook, 2016; Haslam & Ryan, 2008; Kulich, Lorenzi-Cioldi, Iacoviello, Faniko, & Ryan, 2015; M. K. Ryan, Haslam, Hersby, & Bongiorno, 2011; M. K. Ryan et al., 2016). The glass cliff has been conceptualized as a condition in which occupational minorities are more likely to be promoted into leadership positions in underperforming organizations and are thus "differentially exposed to criticism and in greater danger of being apportioned blame for negative outcomes that were set in train well before they assumed their new roles" (Ryan & Haslam, 2005: 87). Such exposure can lead to negative performance evaluations, employment separation, and reputational damage that decreases the likelihood of gaining similar leadership opportunities in the future. Though initially conceived to explain negative evaluations of female leaders, this literature stream has shown evidence of minority leaders being appointed to "glass cliffs" as well (Cook & Glass, 2014).

The explanations described above are not necessarily mutually exclusive as LCT has the potential to not only explain the emergence of a glass cliff, but to also provide insight as to how biases associated with categorization compound the effects of the glass cliff. Extant research has yet to show, however, if these explanations can be disentangled. For example, in the absence of a glass cliff, will minority leaders still fall victim to differential outcomes as a result of leadership categorization? Although experimental research (Rosette et al., 2008) supports the argument that the categorization process is theoretically independent of an individual's starting point (i.e. glass cliff), one could expect that if leadership categorization does not bias a real world selection process such that a glass cliff emerges, then subsequent employment outcomes should be absent of said bias as well. In this light, equitable opportunities would precede equitable outcomes such that the glass cliff could be conceptualized as a signal. Consequently, identifying the employment outcomes experienced by minority leaders in the absence of a glass cliff is critical to our understanding of the challenges faced by these leaders as well as our ability to develop solutions to the problem of inequitable leadership outcomes.

We address this gap in the literature by integrating LCT with attribution theory to argue that minority leaders will be at greater risk than white leaders of experiencing adverse employment outcomes that result from prejudice experienced *after* receiving leadership opportunities that are comparable in quality to those received by their white counterparts. We investigate this differential effect of race on employment outcomes for leaders by analyzing archival performance data for 86 head coaches in the National Basketball Association (NBA) from 2003 to 2015. Although we found minimal support for our model in initial hypotheses testing, following the perspective of aversive racism, post-hoc analyses showed nuanced evidence of unfavorable outcomes for minority leaders. These outcomes emerged in terms of performance rewards and employment separation within an organization that both prides itself on diversity and claims to have achieved equity in leadership selection (AP, 2012a) and were empirically shown to be independent of any differential hiring conditions.

In response to the literature on the glass cliff, this study contributes to the literature on racial discrimination in leadership by examining tangible leadership outcomes for racial minorities when the quality of leadership opportunities is distributed equitably. Our findings indicate that even when racial minorities are not promoted onto glass cliffs, there may be conditions in which they still experience prejudice that results in differential employment outcomes, particularly in the case of employment separations. Consequently, efforts to embrace diversity in leadership must go beyond creating equitable leadership opportunities and must continue after leaders are appointed. We also contribute to the broader stream of research on discrimination in organizations. Following Stauffer and Buckley's (2005) observation that choices in variable definition can conceal evidence of employment discrimination, we address insignificant findings associated with using tenure as an outcome variable (Cook & Glass, 2014; Glass & Cook, 2016) with the introduction of a new employment outcome variable. Finally, by incorporating objective measures of leadership performance into an analysis of racial discrimination in tangible employment outcomes, we respond to Waldman and Avolio's (1991) critique that findings of racial discrimination may be confounded by latent performance variables.

THEORETICAL DEVELOPMENT

Negative Employment Outcomes for Minority Leaders

An extensive body of research has documented evidence that racial minority leaders face discrimination in professional settings. For example, Greenhaus, Parasuraman, and Wormley (1990) found that black managers were more likely to experience a career plateau than white managers while studies by both James (2000) and Sagas and Cunningham (2005) found that white managers experienced higher promotion rates than black managers. Weil and Kimball (1996) found a significant difference in the earnings of black and white executives that could not be explained by human capital variables or differences in positions. Other studies have shown evidence that black leaders perceive more negative interpersonal outcomes such as lower levels of emotional support (James, 2000), job discretion (Greenhaus et al., 1990), and civility (Cortina, Kabat-Farr, Leskinen, Huerta, & Magley, 2013). Such outcomes may be explained by evidence that the leadership potential of white males is evaluated more favorably than that of black males (Landau, 1995).

Recent empirical work supports the assertion that differences in assessments of leadership potential may manifest into discounted evaluations of leadership performance for racial minorities. For example, work from Hekman, Johnson, Foo, and Yang (2016) suggests that when racial minority leaders express beliefs that draw attention to their inconsistency with the leadership prototype, their performance is perceived more negatively than that of their white counterparts. Greenhaus et al. (1990) also found that white managers received higher evaluation ratings than black managers on dimensions of both relationship performance and task performance. Knight, Hebl, Foster, and Mannix (2003) found evidence that manipulating the race of a manager in an experimental design influenced evaluations of work quality.

Extant literature suggests that racial disparities in evaluation outcomes may be influenced by differences in how leaders are evaluated. Early research on leadership has shown that the criteria utilized for leadership evaluations differed based upon the race of the leader (Bartol, Evans, & Stith, 1978). Building on this research, Cox Jr. and Nkomo (1986) found that black managers were evaluated based upon different criteria than white managers. Rosette et al. (2008) found that evaluations of white leaders were more strongly influenced by the combination of positive firm performance and external endorsements than the evaluations of black leaders were.

The use of different criteria for leadership evaluations may be driven by differences in how organizational performance is attributed that are contingent upon the race of the organization's leader. Greenhaus and Parasuraman (1993) found that the satisfactory job performance of white managers was likely to be attributed to factors such as abilities and effort while the satisfactory job performance of black managers was likely to be attributed to receiving help from others. Using textual analysis of newspaper articles, Carton and Rosette (2011) found that black athletes in leadership roles were more frequently referred to as incompetent in times of failure than white athletes in leadership roles were. Consistent with this finding, Park and Westphal (2013) found that after a firm's underperformance, black CEOs were more likely than white CEOs to be subjected to criticism of their leadership abilities. Building on this body of research we attempt to explain these differential outcomes through the integration of LCT and attribution theory.

Leadership Categorization Theory

Because cognitive resources are finite, people have a limited ability to process information. As the processing of information is essential to the performance of a task, information can be processed through one of two channels: controlled processing or automatic processing. Controlled processing requires an individual to devote significant resources to the task at hand. This is most likely to occur when a task is new or fails to follow a routine pattern. Examples of activities where controlled processing is necessary could include learning to ride a motorcycle, reading, or performing a detailed evaluation of performance. Automatic processing occurs in activities such as walking, where an individual has enough familiarity with a process that the detailed consideration of each step in the process is not necessary. Because people are unable to perform controlled processing of two tasks simultaneously, the ability to toggle between automatic and controlled processing is what allows an individual to simultaneously engage in two seemingly attention-demanding activities such as changing the radio station while driving. As the individual engages in controlled processing to identify a radio station and manipulate the dial, the task of driving is temporarily performed using automatic processing. This can become problematic, however, if something unexpected (e.g. a person runs into the street) happens before controlled processing capabilities are redirected to the necessary task (e.g. driving) as automatic processing does not incorporate all of the information available to the individual. Consequently, automatic processing can introduce imperfections to processes (Lord & Maher, 1993).

Automatic processing is informed by how information is encoded as it is stored in long-term memory. As information is being retained in short-term memory, en route to being encoded for storage in long-term memory, it is simplified. The purpose of this simplification is to compensate for limitations in short-term memory. Through this simplification process, information is categorized and organized for retrieval (Lord & Maher, 1993). One component of the categorization process is the development of prototypes. Prototypes reflect the characteristics of a stimulus that are most typically associated that stimulus and serve to distinguish it from other environmental stimuli. Prototypes aid in the recall process when an individual is presented with a stimulus. For example, when presented with the description of a large gray animal with a long nose, large ears, and tusks, a person would likely draw upon their prototype of an elephant to recall information about this animal. Stimuli that evoke prototypes can be context dependent, however, as the word mouse would draw upon a different prototype when presented within the context of describing one’s computer than it would when presented within the context of describing why an individual may obtain a pet cat (Rosch, 1978).

The categorization and prototype development processes are foundational to Leadership Categorization Theory (LCT). LCT states that people identify what they believe to be patterns of the traits and characteristics displayed by successful leaders and use these beliefs to develop abstract conceptions of a leadership prototype (Lord & Maher, 1993). These abstract conceptions can be so detailed that they develop for different types and styles of leadership (Braun, Peus, & Frey, 2018). Although leadership prototypes have been shown to differ across national cultures, there is evidence of consistency in leadership prototypes within each of these cultures (Gerstner & Day, 1994). Failure to conform to the conceived leadership prototype can influence interpretations and evaluations of leadership behaviors because in the absence of the traits associated with the leadership prototype, there are no stimuli present to invoke the recall of the leadership prototype (Ensari & Murphy, 2003). If the recall of the leadership prototype is not stimulated, the evaluator will not draw an association between the leader being observed and the established positive leadership traits through automatic information processing. When no such association is made through automatic processing, positive evaluations of leadership behaviors will be contingent upon controlled processing. This may be limited to the extent that controlled processing resources are allocated to other tasks.

While the idea of a leadership prototype was initially conceived as a generalization of behavioral characteristics, Lord and Emrich (2001: 561) argued that “[p]hysical features associated with race, gender, or ethnicity, for example, may prime specific components in prototypes” such that these features communicate behavioral expectations thereby playing an important role in the categorization process. The information about leaders that is communicated through their physical features is likely to play an influential role in how leaders are perceived because of the salience of this information (Kelley & Michela, 1980). Consequently, repeated exposure to individuals with consistent physical features in leadership positions will result in these physical features being categorized as leadership traits and incorporated into the leadership prototype, particularly when these features occur more frequently within the population of leaders than they do within the overall population (Lord & Maher, 1993). Furthermore, when behavioral characteristics associated with racial or ethnic stereotypes are consistent (*or inconsistent*) with the leadership prototype, races and ethnicities associated with these stereotypes become incorporated into (*or restricted from*) the leadership prototype.

The historical underrepresentation of racial minorities in executive leadership positions in the United States (e.g. Russell Reynolds Associates, 2014; Zillman, 2014) contributes to the environmental conditions necessary to make being “white” a component of the American leadership prototype as the more frequently individuals are exposed to white leaders, the more likely they are to encode white racial status as a component of the leadership prototype. This is supported by Rosette et al.'s (2008) finding that individuals were more likely to perceive a leader as being white than they were to perceive the leader as being associated with another race. Although perceptions of increased minority representation in leadership positions should signal an evolution in the leadership prototype, that is not necessarily the case.

Kanter (1977) observed that racial minorities who achieved leadership positions were often perceived as the exception to the rule rather than evidence to refute stereotypes. Furthermore, once an individual develops a prototype, future stimuli are more likely to be considered within the context of that prototype than they are to change that prototype (Lord & Maher, 1993).

We argue that if being white is a component of the leadership prototype, white organizational leaders will benefit from their consistency with this prototype. Recognizing that evaluation can result from a blend of controlled and automatic processing, we contend that while racial minority and white leaders may be evaluated more equitably in controlled processing, when evaluation of performance is driven by automatic processes, white leaders will benefit from their appearance serving as a stimulus for the recall of the leadership prototype which will, in turn, cause them to be associated with positive leadership traits and have their performance evaluated more favorably. Unlike previous work that would explain differential outcomes through the automatic processing of negative stereotypes regarding various minority groups (Devine, 1989), this LCT argument states that white leaders will benefit from favorable automatic categorization. Although the two explanations are not mutually exclusive, the LCT argument explains why white leaders would have a collective advantage over members of other races, whereas the negative stereotype explanation requires the presence of a negative stereotype for each individual race in order explain any collective advantage experienced by white leaders.

As rewards are typically conceived as a reinforcement for positively perceived performance (Byron & Khazanchi, 2012; Joshi, Son, & Roh, 2015), we propose that the more negative perceptions of leadership performance experienced by minority leaders will result in lower allocations of organizational rewards. In addition to prototypical leaders benefitting from favorable reward allocations, it is likely that the differential performance evaluations described above also lead to more negative employment outcomes, such as increased likelihood of employment separation, for minority leaders.

Hypothesis 1: Racial status that is inconsistent with the leadership prototype will have a negative relationship with leadership rewards.

Hypothesis 2: Racial status that is inconsistent with the leadership prototype will have a positive relationship with the likelihood of employment separation.

Attribution Theory

Causal attributions for organizational performance are likely to influence the impact of performance on leadership outcomes. Attribution theory encompasses the body of social science research that examines how perceptions of responsibility for an outcome are formed (Kelley & Michela, 1980). These causal attributions are commonly derived from limited information as the search for information ceases once a satisfactory causal inference has been reached (Lord & Smith, 1983). Achievement outcomes are frequently attributed to one of four core factors: ability, effort, task difficulty, and luck (Frieze & Weiner, 1971). Attribution, however, is not a perfect process as social biases can result in overattributing responsibility for an outcome to one cause (Harvey & Weary, 1984).

The emphasis placed on one cause of an outcome can lead to the devaluing, or “discounting” of another cause. Attributions of causality are often conceptualized as a zero-sum game, such that the perceived effect of one cause (e.g. personal control) on outcomes is inversely related to the perceived effect of another cause (e.g. external factors) on outcomes (Kay, Gaucher, Napier, Callan, & Laurin, 2008). The formation of causal attributions is thought to be dependent upon the assumptions of the evaluator such that if an evaluator enters a situation with the assumption that a certain variable is the probable cause of an outcome, other potential explanations may be subject to discounting (Morris & Larrick, 1995). When assumptions regarding causality are incompatible with alternative explanations, the likelihood of discounting occurring increases (McClure, 1998).

Valence of causal explanations is critical to their compatibility. While explanations with similar valence may be considered jointly responsible for an outcome, inconsistency in the valence of explanations contributes to perceived incompatibility, thus leading to discounting of at least one plausible causal explanation. Roese and Morris (1999) explained the role of valence in terms of causal explanations for “the often bewildering array of circumstantial evidence” (p. 445) in the 1995 criminal trial of O.J. Simpson. They argued that of the three most prominent explanations for said evidence, the two most often taken into consideration together were a police conspiracy and ineptitude of law enforcement because the valence of each of these explanations was compatible with a positive impression of the defendant. Unlike these explanations, the valence of the third explanation, that the defendant was guilty, was consistent with a negative impression of the defendant and thus incompatible with the other explanations. Roese and Morris (1999: 446) argued that "the conjunction of guilt plus ineptitude [was] more plausible…than the conjunction of conspiracy plus ineptitude," but was rarely considered as a causal explanation because of their incompatible valences.

The Influence of Categorization on Attribution

This valence argument can be easily applied to the role of leadership categorization in causal attributions for organizational success. When evaluating organizational success, individuals may consider a multitude of factors including, but not limited to, market conditions, resource availability, leadership effort, leadership competence, and leadership ability. Leadership traits that are consistent with the leadership prototype (i.e. being "white") should stimulate recall of the leadership prototype, thus invoking a positive perception of an individual as a leader (Lord & Emrich, 2001; Lord & Maher, 1993) that would be compatible with positive perceptions of leadership competence, effort, and ability, allowing all three of these explanations to contribute to causal attributions for organizational success. Conversely, the valence of traits that are inconsistent with the leadership prototype would not stimulate recall of the leadership prototype and could potentially stimulate recall of traits that are incompatible with the valence of leadership competence, effort, and ability, potentially causing these explanations to be discounted. Consequently, organizational success is likely to be most heavily attributed to leadership when the leader's traits are consistent with those of the leadership prototype.

The potential for demographic characteristics to play a role in discounting is problematic in terms of leadership employment outcomes as organizational performance is often a strong consideration in terms of outcomes such as reward allocation, evaluations, promotions, etc. If the contributions of those who do not conform to the leadership prototype are discounted, these individuals are also likely to benefit less from their organizations' success than leaders who do reflect the prototype. We argue that when a leader's race does not conform to the leadership prototype (i.e. the leader is not white), the leader's impact on organizational success will be discounted and, consequently, the relationship between organizational performance and leadership rewards will be weaker for racial minority leaders than it is for white leaders.

Hypothesis 3: The relationship between organizational performance and leadership rewards will be attenuated by a leader's status as a racial minority.

The role of discounting in attributions of responsibility for organizational performance is likely to differ based upon the performance of the organization. As discussed above, consistency with the leadership prototype should invoke positive perceptions of leadership traits and abilities. The valence of these positive perceptions would be inconsistent with the negative valence of unsuccessful organizational performance. Consequently, when organizational performance is negative, the causal role of leadership traits and abilities is more likely to be discounted if the leader conforms to the leadership prototype (i.e. the leader is white). This discounting process would result in racial minority leaders being perceived as more responsible for organizational failure than their white counterparts.

We argue that if racial minority leaders are seen as more responsible for negative organizational performance than white leaders, the relationship between organizational performance and negative employment outcomes, such as employment separation, will be stronger for racial minority leaders than it will be for white leaders.

Hypothesis 4: The relationship between organizational performance and the likelihood of employment separation will be strengthened by a leader's status as a racial minority.

DATA COLLECTION AND METHODS

Research Context

This study uses archival data on the attributes and performance of coaches and teams in the National Basketball Association (NBA). While most research in this domain has used experimental or survey data, the use of archival data increases the external validity of our findings (Carton & Rosette, 2011) while also allowing us to view the relationship between objective measures of performance and employment outcomes. The parallel growth of professional athletics, sports media and the internet has created an environment where accurate unique individual performance data are readily available for analysis. Prior leadership research (e.g. Giambatista, 2004; Goodall & Pogrebna, 2015; Hunter, Cushenbery, Thoroughgood, Johnson, & Ligon, 2011) has utilized data from athletic organizations because of the important role that leaders play within such contexts. Additionally, this context is appropriate for studying factors that influence employment separations as there is a relatively high employment separation rate for head coaches in the NBA, providing the variability necessary in our dependent variable in order to detect an effect. Nearly 35 percent of the observations in our study resulted in an employment separation. This high separation rate, which demonstrates the appropriateness of our context for testing the impact of leader race on employment separations, is also demonstrated by the average tenure of 3.30 years ($SD = 3.46$) for coaches in our sample which was significantly lower ($p<0.001$) than the median tenure of 6.3 years for managers in the United States (Bureau of Labor Statistics, 2016). The comparatively high turnover rate in our study's context is not unexpected as there are relatively few head coaching positions available in the NBA, therefore creating a unique competitive dynamic that likely increases the probability of employment separations.

The NBA is an athletic league that generates approximately $5 billion in annual revenue and has global impact as its games are broadcast to over 200 countries and territories (AP, 2012b). The league is comprised of 29 franchised members from throughout the United States and one Canadian member that have an average valuation over $1 billion (Badenhausen, 2015). According to a recent study, the addition of an NBA franchise to an urban community has the potential to have an annual economic impact of over $500 million and create 3,700 jobs (Chumura, 2012).

The NBA prides itself on achieving the most diverse leadership in major American professional sports without having to regulate integration (AP, 2012a). Even after achieving this recognition, though, the NBA has still been subject to very public racially-driven conflicts (Lee, 2014). This evidence of racial tension in an organization that is typically viewed as culturally progressive makes the NBA an intriguing environment for exploring our research questions.

Furthermore, as of 2015, while only 33.3 percent of the population of players in the NBA were classified as white, 91.2 percent of team presidents and CEOs in the league were white (Lapchick & Guiao, 2015), indicating that the conditions necessary for the sub-categorical leadership prototype of a white male to be relevant are present within the context of the NBA. Even at the subcategory of coaches within the NBA, frequency of white leaders in our sample (62.4 percent) was significantly ($p<0.001$) higher than the frequency of white players, who represent highly visible non-leaders, within the NBA. Given that in addition to the presence of a trait amongst leaders, differences in the frequency with which a trait is present amongst leaders as compared to non-leaders plays an important role in the development and persistence of a mental prototype (Lord & Maher, 1993), this provides further support for the argument that the white leadership prototype is relevant to our research context. Additionally, Lord and Maher (1993) argued that once categorization has occurred, categorization influences future information processing. Applied to this context, this means that even if the demographic composition of coaches has changed in recent years, categorization that occurred when stakeholders were first introduced to the NBA has the potential to influence the leadership prototype referenced in this context today.

Measures

Performance Rewards. The primary performance rewards measure used in this study was NBA Coach of the Year (COY) voting. The NBA COY is an annual award that is sanctioned by the NBA and given to one of thirty head coaches in the league. Upon the conclusion of the regular season, the winner is determined based upon voting conducted amongst selected members of the national media who cover the NBA and represent subject matter experts. Each selected member is allowed three votes: one for first place, one for second place, and one for third place. Votes are tabulated by an independent accounting firm and coaches are awarded five points for each first-place vote, three points for each second-place vote, and one point for each third-place vote. The coach with the highest number of points is awarded as COY. These data were collected from nba.com, the official website of the NBA. We incorporated total COY award points, first place points, second place points, and third place points into our analyses. The use of COY award points as a measure of performance rewards is consistent with prior work's definition of rewards as extrinsic recognition or reinforcement (Byron & Khazanchi, 2012; Joshi et al., 2015). While we recognize that COY voting is conducted amongst individuals whose employment is external to the organization being studied, we contend that it is highly relevant because the award is sanctioned by the NBA and media members must qualify to participate in voting. Additionally, external stakeholders such as customers (Lynn & Sturman, 2011) and the media (Wade, Porac, Pollock, & Graffin, 2006) can play an important role in an organization's future personnel decisions.

The total number of COY votes collected and points awarded varied slightly by year *(votes: min = 339, max = 390,* $\bar{x}$ *= 366.08, SD = 13.35; points: min = 1035, max = 1170,* $\bar{x}$ *= 1099.39, SD = 41.25*), most likely due to reasons such as incomplete and unreturned ballots. In order to account for this variation, COY votes were scaled so that the total number of votes collected for each year was equal to 400, with first, second, and third place votes adjusted accordingly. We then multiplied adjusted first, second, and third place points by five, three, and one, respectively, to replicate the NBA's COY point structure. Finally, we recalculated total COY points by taking the sum of adjusted first, second, and third place points earned for each coach in a given year. This resulted in the adjusted total of votes cast being 400 and the adjusted total of COY points awarded being equal to 1200 for every year in our sample.

Employment Separation. We identified employment separations as cases in which an individual was replaced in his position as head coach. Coaches for each year in our sample were identified through espn.com and nba.com. In cases where a coach could not be identified on either of these sites, information was supplemented from basketball-reference.com. Basketball-reference.com is an online database of NBA statistics that has been used for both academic research (e.g. Price & Wolfers, 2010) and mainstream media articles (e.g. Washburn, 2015). The coach of a team at the end of a season was compared to the coach at the end of the following season. If the coaches at the end of these two time periods were not the same, it was coded as an employment separation (*separation = 1*). We compared coaches at the end of sequential seasons rather than comparing the coach at the end of one season to the coach at the beginning of the next because mid-season terminations are often a residual effect of performance the previous season. Through media reports we were able to confirm that at least 85 percent of the employment separations constituted involuntary separations such as firings, team decisions not to renew a contract, or a negotiated contract buyout.

Race. In order to test hypotheses regarding consistency with demographic characteristics of the leadership prototype, a binary variable was used to record whether a coach was of racial minority status (race = 1) or white (race = 0). Both black (35.5% of sample) and Asian (2.1% of sample) coaches were classified as racial minorities. All other coaches (62.4% of sample) were classified as white. This information was obtained by observing pictures of coaches' profiles on the sites mentioned above. In cases where either no picture was available on these sites or an accurate determination could not be made from a picture, extensive internet searches were done until an article that could more clearly identify race was found. Multiracial coaches were recorded as minorities (U.S. Census Bureau, 2012). There were no full-time head coaches of Hispanic ethnicity in our sample (Wright, 2017).

Objective Measures of Performance. Objective measures of performance refer to factual performance indicators that cannot be distorted by personal feelings or biases. We collected the number of regular season wins that were earned each season from nba.com. Because coaches are responsible for the collective performance of their teams, regular season wins represent an important measure of their performance as leaders. This is particularly relevant to COY voting as ballots are cast after the regular season ends, but before the post-season begins. The NBA regular season is 82 games long, but the 2012 season was shortened by a contract dispute, so wins for 2012 were adjusted to fit a scale of 0 to 82.

We calculated a variable for improvement which represented the change in performance from one season to the next (Nakauchi & Wiersema, 2015). This variable was calculated by subtracting regular season wins in the previous season from regular season wins in the observed season. This variable allowed us to distinguish between the effects of sustaining successful organizational performance and achieving successful organizational performance through growth. Because this variable is a linear function of regular season wins in the observed time period and regular season wins in the previous time period, it also served to control for the effect of wins in the previous time period.

While regular season wins serve as an important measure of objective performance, they do not provide information regarding progress towards the ultimate goal of coaches in the NBA, which is to win an NBA Title. Each year, 16 teams compete in a tournament for the opportunity to achieve this ultimate measure of success. In the final round of the tournament, two conference champions compete for this distinction. As our final measure of objective performance, we recorded how many years have passed since a coach had won a conference championship in his current position. We chose this instead of years without winning an NBA Title because so few teams win NBA Titles that it would have limited the variability of this measure. Also, winning the conference championship is considered a high achievement, as indicated by the tradition of conference champions hanging commemorative banners from the rafters in their home arenas. This variable was calculated by subtracting the year of a coach's most recent conference championship from the year of the observation. If a coach had never won a conference championship, this value was equal to the coach's tenure in position. This value was then multiplied by negative one so that its interpretation would be consistent with other performance variables (higher numbers = favorable performance). This variable is hereby referred to as post-season performance.

Control Variables. Resources represent an important component to organizational support and in athletics, there is no greater resource than quality players. Because players' experience is often associated with team success (Tarlow, 2012), we collected the average age of players. Also, each year the top players in the league are named to All-NBA teams. We considered players to be All-NBA if they were named to either the All-NBA first team or second team, earning recognition as one of the top ten players in the league for the entire season. Data on both average player age and the number of All-NBA players on a team was also collected from basketball-reference.com.

In order to account for the unique pressures associated with visibility in top media markets, our employment separation analyses included a control variable for top 10 media markets in the United States. Our binary variable (1 = yes, 0 = no) for whether or not a team resides in one of these markets was constructed based upon Nielsen ratings for 2013 and 2014.

Finally, in our employment separation analysis, we also controlled for team salary. This variable refers to the amount of money that the team invested in players each year and not only serves as a proxy for player ability, but also for the investment that ownership has made into resources. This was measured by collecting individual player salaries from basketball-reference.com and aggregating them at the team level. To account for changes in spending as well as the league salary cap from year to year, we adjusted all salaries to match the salary scale for the year 2015. This methodology allowed salaries to be viewed as comparisons from team to team across years on a consistent scale.

Sample

Our data set includes observations that draw from the 2002-03 basketball season through the 2014-15 season.[1] Coaches included in the data set were identified through their association with NBA teams during this time period. The 2003 season marked the first year that some of the data necessary for our analysis became publicly available. 2015 was the last year for which data were available at the time of data collection. Data availability did not raise any concerns regarding selection bias as internet use did not begin to grow rapidly in America until the late 1990s (Geier, 2015), so investing in hosting data publicly in 2003 is a logical progression.

The NBA had 30 teams for 11 of the 13 seasons included in this sample and it had 29 teams for the other two seasons. Because data on previous performance was not available for the first season of the team that was created during our sample selection period, one observation had to be eliminated from the sample, giving us an initial sample size of *n=387*. Of these observations, 52 included more than one person coaching games for a team in a single season. This was problematic in the sense that objective performance measures could not be accurately attributed in these cases. While it may be possible give a coach credit for achievements in the games that he is on record as coaching, doing so would not account for factors such as pre-season activities, nuances in scheduling, etc. Also, while these observations accounted for 13.4 percent of our original sample, they only accounted for 1.1 percent of total COY

[1] From here on, seasons will be referred to by the year that denotes the end of the season (i.e. 2002-03 = 2003).

points in the data. Because our employment separation variable included separations within 12 months of the end of the season, mid-season terminations were accounted for in our employment separation analysis, thus the elimination of these observations did not result in a loss of employment separation data. Taking this into consideration, these observations were eliminated from our final sample (*n=335*). Our final data set included 86 individuals in 134 unique combinations of an individual coach and an individual team, hereby referred to as a unique position.

Methods

Hypotheses 1 and 3 focused on the impact of race on leadership rewards. Rewards were measured using the COY voting subjective evaluative measures described above. While *Table 1* shows that the mean value for total adjusted COY award points in our full sample was 46.57 (*SE=6.05*), this variable had minimum values clustered at zero. To account for this, we utilized a Tobit regression model with a lower limit of zero. This censored regression model corrects for inconsistencies that can result when using OLS to analyze data where the dependent variable (DV) has a partly continuous and partly discrete distribution (Stock & Watson, 2015). Tobit models factor in both the likelihood of the DV being above the lower limit as well as the relationships between explanatory variables and the DV when the DV is above the lower limit (McDonald & Moffitt, 1980). This is a critical distinction because while only 49.6 percent of the observations in our sample had a COY award points value above zero, 89.0 percent of the observations had win totals that were greater than or equal to the minimum number of games won by coaches who received COY award points.

Insert Table 1 About Here

Hypotheses 2 and 4 discuss the impact of a coach's race on the likelihood of an employment separation. While non-linear models (*i.e. logit, probit*) are frequently used when the target of analysis is a dichotomous variable, in cases where interaction terms are used, employing a non-linear model can result in inaccurate reporting of coefficient sign, magnitude, and statistical significance (Ai & Norton, 2003). To address this concern, tests of *Hypotheses 2 and 4* were conducted using a linear probability model (LPM).

While use of this method reduces the risk of reporting false results in models that include interaction terms, the results of LPM analysis are frequently consistent with those of logit and probit (Chatla & Shmueli, 2016).

No coach has ever won the NBA’s COY Award in consecutive seasons, indicating that the votes a coach receives in one year are related to votes received the next year. The distribution of COY votes also varies from season to season, which theoretically could be linked to the apparent reluctance of voters to recognize the same coach in consecutive seasons. In our separation analyses, the likelihood of a coach being fired in one season is also subject to serial correlation of errors because of the effects of cumulative performance on termination. Additionally, error terms could be correlated by season as the termination of a coach increases the pool of available applicants which may influence the termination of other coaches. Consequently, assumptions of independence across observations are not met in this panel data set for either of our targets of analysis. Two-way clustered standard errors were used to address this concern as this method accounts for serial correlation among the errors that is caused by multiple non-nested clusters. This is an important consideration as even a small change in standard errors has the potential to influence interpretations of statistical significance (Cameron & Miller, 2015; Petersen, 2009). Though traditionally used in finance panel data, the practice of using two-way clustered standard errors has also been used in the management literature (e.g. Jonczyk, Lee, Galunic, & Bensaou, 2016).

Simple regression analysis does not allow one to infer causality from results as confounding factors, such as correlations among variables, can influence outcomes. Coarsened exact matching addresses this concern by coarsening variables into groups (i.e. converting a continuous variable into eight equal groups), exact matching treated and untreated observations on the coarsened variables, and then performing analyses on the original un-coarsened variables after matching is complete (Blackwell, Iacua, King, & Porro, 2009; Iacus, King, & Porro, 2012). After first testing our baseline hypotheses in our full sample, we applied this technique using Stata’s *cem* command with the one-to-one option to construct samples that were matched based upon the explanatory and control variables used in each equation, thus allowing for more robust analyses. The high quality of this data set and inclusion of clearly defined variables makes this study a prime candidate for utilizing matching techniques (Smith & Todd, 2001). Non-binary variables used in regression analyses were standardized for ease of interpretation.

RESULTS

Leadership Rewards

Hypothesis 1 stated that minority status would have a negative effect on the rewards received by leaders. *Table 1* shows that the mean number of COY award points for white coaches was 54.92 (*SE = 8.40*) while the mean for minority coaches was 32.71 (*SE = 7.92*), but the difference was only marginally significant ($p=.08$). To validate *Hypothesis 1*, we began by regressing COY award points on coach's race while controlling for wins, improvement, average age of players on the team, and the number of All-NBA players on the team in our full sample. As shown in *Table 2*, Model 1 was significant [$\chi^2(5) = 103.20$, $p<.001$, *pseudo* $R^2=.09$], but our variable of interest was not ($p>.10$).

Because first place votes account for over half of the COY Award points, our results were heavily driven by these votes. The theory of aversive racism states that individuals have a desire to maintain an unbiased appearance and that prejudice is most likely to occur where behavior is least conspicuous (Dovidio & Gaertner, 2000, 2004). We argue that because there were no more than three coaches in serious contention for COY in any year of our data and first place votes hold considerably more weight than any other votes, the casting of second place ballots is less conspicuous than that of first place ballots. Therefore, discrimination should be more likely to emerge in second place voting and its effect on overall COY points may be masked by a lack of discrimination in first place voting.

Table 1 indicates that white coaches received significantly ($p<.05$) more points from second place votes ($\bar{x} = 18.84, SE = 2.76$) than minority coaches did ($\bar{x} = 10.02, SE = 2.26$). To investigate if any race effects emerged specifically for second place votes, we re-ran our regression using second place points as the DV. As shown in Model 2 of *Table 2* [$\chi^2(5) = 105.88$, $p<.001$, *pseudo* $R^2 =.11$], the main effect of race had a negative and significant relationship with second place points ($\beta = -11.20, SE = 5.44, p<.05$). We also estimated the effect of race with first and third place points as the DVs respectively.

However, our results indicated that the effect of minority status was not significant on points earned from first or third place votes (*ps>.10*).[2]

There are some concerns that failure to find significance on our variable of interest could have been influenced by systematic differences in our sample that were unrelated to race. We addressed this concern by replicating our analyses in a matched sample, as specified above. While *Table 1* shows that white coaches in our full sample won more games ($p<0.05$), had older players ($p=0.12$), and had more All-NBA players on their teams ($p=.07$) than minority coaches, differences in these variables did not approach significance in our matched sample ($ps>0.10$), thus reducing concerns regarding the effects of confounding variables. Additionally, by mimicking experimental conditions, use of a matched sample allows us to estimate the causal effect of race on our DV (Rosenbaum & Rubin, 1983; Todd, 2010). As shown in *Table 2*, when using total COY points as our DV in a matched sample, Model 3 was significant [$\chi^2(5) = 66.55, p<.001, pseudo\ R^2=.11$], but leader race did not have a significant relationship with the total COY award points that a coach received ($p>.10$), failing to provide support for our hypothesis that racial minority status has a negative effect on reward allocation for leaders.

Insert Table 2 About Here

We then we re-ran our regression using second place points as the DV in our matched sample. As shown in Model 4 [$\chi^2(5) = 61.03, p<.001, pseudo\ R^2 =.12$], the main effect of race was marginally significant ($\beta = -15.85, SE = 8.54, p=.06$). We again replicated our hypotheses tests using first and third place points as the DVs. Once again, the effect of minority status was not significant on points earned from first place votes or third place votes (p>.10).[3] Collectively, we only found partial support for *Hypothesis 1* in our full data set. We found no evidence of a relationship between race and total COY

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[2] Due to space limitations, these results were not included in the paper. However, they are available from the authors upon request.
[3] It should be noted that although we found no evidence of discrimination in third place COY voting, the authors believe this may a residual function of discrimination in second place voting as second place votes are deferred to third place for minority coaches and third place votes are elevated to second place for white coaches. The authors used a simulated data set to test, and find support for, this explanation. Results of this simulation are available upon request.

award points in our full or matched sample. Conversely, our finding of a negative relationship between second place COY points and race was present in both our full data set and our matched sample, though the level of statistical significance was lower in our matched sample.

Hypothesis 3 argued that objective measures of organizational success would have a stronger positive effect on performance rewards for white leaders than racial minority leaders. This was tested by interacting variables for objective measures of performance and control variables with our binary variable for race. We then regressed total COY award points earned on race, the control variables, performance variables, and the interaction terms in our matched sample. The estimates for Model 5 in *Table 2* show that the model was significant [$\chi^2(9) = 88.19, p<.001, \text{pseudo } R^2 = .11$]. There was a positive relationship between wins and total COY award points ($\beta = 198.83, SE = 38.32, p<.001$). The interaction of wins and race was negative and marginally significant ($\beta = -77.80, SE = 45.31, p=.09$), providing some support for *Hypothesis 3.* Model 6, however, failed to provide support for our hypothesis when using second place points as the DV (*ps>.10*).

Likelihood of Employment Separation

Hypothesis 2 stated that minority leaders would have a greater likelihood of employment separation than white leaders. We first tested this hypothesis by regressing our binary variable for employment separation on race, wins, improvement, post-season performance, All-NBA players on the team, average team salary, and whether or not the team was in a top media market. Model 1 of *Table 3* shows the results of our baseline model, an analysis conducted in the full data set.[4] The baseline model was significant [$F(7, 325) = 7.22, p<.001, R^2 = .11$], but our variable of interest was not.

As discussed above, the theory of aversive racism states that discrimination is most likely to occur when a behavior is less conspicuous. High-profile athletic coaches are under significant public scrutiny after initially being hired (e.g. Gleeson, 2017; Wallace, 2015). This statement is supported by

[4] Two observations were eliminated from the full data set for this analysis as employment separations were the result of death or serious illness. Results were consistent when replicated without removing these observations.

employment separation patterns in our data showing that approximately 54 percent of the separations in our sample took place within the first two years of employment. Consequently, we posit that during those first two years of employment, when performance is highly scrutinized, discrimination is less likely to occur and that in order to identify effects of discrimination, we must focus our analysis on coaches who have remained in position beyond this initial period of high visibility. Additionally, as shown in *Figure 1,* the difference in games won between coaches who experienced an employment separation and those who didn't decreased greatly after a coach's second year in position, indicating that after this initial period of high visibility was completed, factors other than performance may have begun to influence the likelihood of separation. To test this possibility, we re-ran our analysis restricting the sample to coaches with more than two years in position. Model 2 of *Table 3* shows that both the model [$F(7, 136) = 5.99$, $p<.001$, $R^2 =.19$] and the variable for race ($\beta = 0.16$, $SE = .08$, $p<.05$) were significant, providing conditional support for *Hypothesis 3.*

Insert Table 3 About Here

In order to identify evidence of causality, we constructed two matched samples as described above: one from our full data set and one from observations that included coaches with over two years in position. Again, the significant differences between white and minority coaches in terms of wins, post-season performance, and All-NBA players in our full sample (*as* shown in *Table 1*) were not present in our matched samples. We then replicated Models 1 and 2 in these matched samples to create Models 3 [$F(7, 174) = 3.53$, $p<.01$, $R^2 =.10$] and 4 [$F(7, 48) = 5.85$, $p<.001$, $R^2 =.27$], respectively. As shown in *Table 3,* results from analyses conducted in our matched samples were consistent with those conducted in our full data set.

Hypothesis 4 stated that performance would have a greater impact on the likelihood of employment separation for minority leaders than white leaders. To test this hypothesis, we again interacted our binary term for race with all performance and asset-related control variables. *Table 3* shows that Model 5, which incorporated these terms in the matched sample constructed from the full data set, was significant [$F(12, 169) = 2.66, p<.001, R^2 =.13$]. Consistent with our expectations, wins ($\beta = -0.18, SE = .08, p<.01$) and post-season performance ($\beta = -0.10, SE = .05, p=.06$) had negative relationships with the likelihood of employment separation. No interaction terms were significant (*ps>.10*). Additionally, this model did not explain significantly more variance than Model 3 [$\Delta R^2 =.03, F(5, 169) = 1.08, p=.37$].

We then investigated this hypothesis within the matched sample that was constructed from coaches with over two years in position to create Model 6 [$F(12, 43) = 4.90, p<.01, R^2 =.41$]. The effect of minority status on likelihood of separation remained significant ($\beta = 0.37, SE = .14, p<.05$) in this model. Our analysis here showed that current wins reduced the likelihood of separation for all coaches ($\beta = -0.32, SE = .11, p<.01$). This effect, however, was attenuated for minority coaches ($\beta = 0.31, SE = .11, p<.01$). Contrary to our hypothesis, *Figure 2* shows that while positive organizational performance reduced the likelihood of separation for white leaders, minority leaders were subject to a consistently high threat of employment separation. No other interaction terms were significant. The amount of additional variance explained by this model, as compared to Model 4, only approached significance [$\Delta R^2 =.13, F(5, 43) = 1.92, p=.11$].

Robustness Checks

Theoretical Robustness. The glass cliff theory would suggest that our findings may be the result of white coaches being hired into more favorable conditions than minority coaches, and benefiting from those conditions as they persist (Glass & Cook, 2016; M. K. Ryan & Haslam, 2005; M. K. Ryan et al., 2011). Evidence of racial discrimination in sports has shown us that minority assistant coaches receive fewer promotions, (Sagas & Cunningham, 2005), have weaker professional networks (Davidson, 2014), and are given duties that do not prepare them for head coaching responsibilities (S. Ryan, 2015), suggesting the types of selection issues that are foundational to the glass cliff are germane to our research context. Although we have theoretically argued that the phenomenon tested in this study is not related to the glass cliff, given the susceptibility of our context to selection bias, we empirically tested this alternative explanation.

To test the difference in quality of opportunities available to coaches of different races, we looked at data from when individual coaches were hired into head coaching positions in the NBA. For each coach in our sample, we looked at the date of hire into a new position and used the team's wins during the previous season and how many All-NBA players a team had on its roster to represent quality of opportunity. We controlled for coaches' characteristics (previous coaching experience, awards won, and experience as a player) as well as attributes of teams' home metropolitan areas (market size and demographic composition). White coaches had significantly more coaching experience at time of hire than minority coaches ($p<.05$), but a smaller percentage of white coaches had playing experience than minority coaches did ($p<.001$). Though the mean number of previous wins for teams that hired minority coaches was slightly higher than that of white coaches ($p<.05$), comparative t-tests showed no other significant differences in characteristics or quality of opportunity ($p>.10$). To test our alternative explanation, we started with a logistic regression of race on our quality of opportunity variables along with our control variables. The entire model was significant, $\chi^2(7) = 19.43$ ($p<.001$). The coefficient on previous wins was significant ($\beta = 0.04$, $SE = .02$, $p<.05$), indicating that minority coaches may actually receive slightly higher quality job opportunities than white coaches. The coefficient on our other quality of opportunity variable was not significant ($p>.10$). We attempted several different configurations of the logistic regression by removing highly correlated variables and achieved similar results.[2]

To further test the possibility that our results were driven by white leaders experiencing an advantage in the selection process, we examined the impact of race on the likelihood of coaches in our sample being hired into a different head coaching position after experiencing an employment separation. We regressed a binary variable for new job (1 = hired for new job, 0 = did not receive new job) on race, measures of past objective performance (career winning percentage, final season winning percentage, awards won), and experience. We found no evidence that a coach's race influenced selection for future employment opportunities. These tests provide evidence that our findings regarding discrimination in rewards and employment separation are not residual effects of discrimination in selection processes and therefore not a manifestation of the glass cliff.

Standard Error Clustering. While clustering standard errors two-ways can address issues associated with correlation of the error term, it can increase estimation variance. Consequently, using two-way clustered standard errors may result in increased reporting of statistical significance (Thompson, 2011). In order to account for this possibility, we replicated analyses using standard errors clustered on coach only. We re-estimated the effects of a coach's minority status on second place votes as this was the only rewards dependent variable where increased reporting of statistical significance was a concern. Consistent with our analysis, the effect of race on second place votes was marginally significant ($p=.067$) in our matched sample, but the change in standard errors resulted in the p-value increasing from .039 to .103 in our full sample. Replications of Models 5 and 6 were consistent with what has been reported in *Table 2.*

We then re-estimated the effects of a coach's minority status on likelihood of separation for coaches with more than two years in position. Consistent with our analyses, robustness checks with standard errors clustered on coach showed evidence that performance had a stronger effect on the likelihood of employment separation for established minority coaches than it did for established white coaches ($p<.05$) in the unmatched sample. We then replicated Models 4 and 6 in the matched sample. The main effect of race was significant in each of these models (*ps*$<.01$) and once again, we found that the negative effect of wins on likelihood of separation ($p<.001$) was attenuated for minority coaches ($p<.05$). [2]

Endogeneity (Instrumental Variable). One potential source of endogeneity is omitted variable bias. Omitted variable bias occurs when coefficients are biased due to a variable that is not accounted for in the data. The use of an instrumental variable in a 2SLS estimation attempts to separate the variance of an explanatory variable that is correlated with the error term from that which is not correlated with the error term, thus reducing bias attributable to an omitted variable (Larcker & Rusticus, 2005; Stock &

Watson, 2015; Wooldridge, 2003). One potential source of omitted variable bias in our data set is ability. Our measures of organizational performance (e.g. wins, improvement) do not allow us to differentiate between the effect of a coach's performance and the effect of player ability on organizational performance.

While a 2SLS estimation designed to account for player ability could potentially solve the endogeneity problem, this context presents a unique challenge in terms of identifying good candidates for instruments. Although there is extensive data on a multitude of variables within this context, one could reasonably argue that any instruments seeking to isolate variance attributable to player ability could influence our dependent variables and thus be theoretically correlated with the error term. For example, general managers making firing or retention decisions have access to a considerable amount of data on players' fitness, skills, performance, etc., thus they could potentially consider these factors in making decisions regarding the retention of coaches. As introduced in our discussion of control variables, player age and the number of All-NBA players on a team are associated with team performance because each of these variables serves as a signal of player ability. These variables are serve as better predictors of player ability than other available variables that have error introduced by factors such as the structure of the collective bargaining agreement at the time a contract is signed (e.g. salary), changes in the rules of the game over the course of our sampling period (e.g. data on physical attributes and raw athletic performance such as speed/jumping ability), and unpredictable potential (e.g. college attended, high school prospect ranking, draft number). Additionally, the availability of data on player age and All-NBA players on the team is consistent with the availability of other ability-related data, thus making these variables no more likely to be considered by evaluators making Coach of the Year and employment retention decisions. Consequently, these variables were the candidates for instruments that came closest to meeting the theoretical requirements for a two-stage instrumental variable (IV) model, although we cannot be sure that they are truly exogenous. With this caveat, we present the results of our IV models below. In each of these models, the variable for improvement was replaced by previous season wins because improvement is a linear function of our instrumented variable.

We began by replicating our organizational reward analyses with the IV model (*see Appendix A*). It should be noted that standard errors were not clustered as Stata's *ivtobit* function cannot calculate clustered standard errors with a two-step estimator. To address the issue of clustering standard errors, we also ran the same IV models using an MLE estimator and standard errors clustered on coach. Results were consistent with those of the two-step estimator. The results discussed in this section are from the two-step estimator, though results from both estimators are provided in *Appendices A and B.* In the full sample, we found that the relationship between race and COY award points was insignificant ($p>.10$), but that the relationship between leader race and second-place award points was marginally significant ($\beta = -14.96, SE = 8.49, p=.08$). The Amemiya-Lee-Newey statistic indicated that our model was not overidentified, $\chi^2(2) = 1.53$ ($p>.10$). Similarly, in our matched sample, race did not have a significant direct effect on COY award points ($p>.10$), though it did approach significance for second-place award points ($p=.11$). Our replications of Model 5 and 6 (*see Appendix B*) yielded no significant interaction effects ($ps>.10$).

Next, we replicated our employment separation analyses using the same IV in the full sample of coaches with more than two years in position (*see Appendix C*). In this analysis, the effect of race on likelihood of separation approached significance ($\beta = .13, SE = .08, p=.102$). In the matched sample, however, the coefficient on race was significant ($\beta = .33, SE = .13, p<.05$). The coefficient on race remained significant in our replication of Model 6 ($\beta = .33, SE = .15, p<.05$; *see Appendix D*), although none of our interaction terms were significant ($ps>.10$). In light of the previous concerns discussed regarding the challenges associated with finding appropriate instruments in our data, it should be noted that Hamilton and Nickerson (2003) argued that in cases where a suitable instrument cannot be identified, the use of a matched sample may the most appropriate alternative for addressing endogeneity, though these models may be biased due to unobserved factors.

Endogeneity (Fixed Effects--Coach). We then replicated our linear probability model using fixed effects models that address omitted variables by allowing us to control for unobserved differences across our group variable. In order to take full advantage of our panel data set, these models were analyzed using the unmatched samples. We began by controlling for the fixed effects of coaches, therefore addressing unobserved differences between individual coaches. Because race is constant within an individual, controlling for the fixed effects of coaches causes the race variable to be omitted to the model, thus main effects must be examined by comparing confidence intervals of subsamples. When comparing coaches of all tenures, the 95 percent confidence interval of the constant in the subsample of white coaches ($N=207$) overlapped with the 95 percent confidence interval of the constant in the subsample of minority coaches ($N=126$), therefore providing no evidence of a main effect for race (*see Appendix E*). In our interaction model ($N=333$), the negative relationship between wins and employment separation ($\beta = -.18, SE = .05, p<.001$) appeared to be attenuated for minority coaches, as indicated by a marginally significant interaction term ($\beta =.13, SE = .07, p=.08$). The interaction of race and post-season performance was negative and significant in this model ($\beta = -.13, SE = .06, p<.05$), suggesting that while minority coaches may benefit less from regular season wins than their white counterparts, this effect may be counteracted through successful post-season performance.

We then controlled for the fixed effects of coaches while limiting our sample to coaches with more than two years in position ($N=144$). Once again, the 95 percent confidence intervals for white ($N=91$) and minority ($N=53$) coaches overlapped, providing no indication that a direct effect of race was present. In our interaction model, however, the negative relationship between wins and employment separation was once again significant ($\beta = -.37, SE = .12, p<.01$), while the interaction of race and wins had a positive relationship with employment separation ($\beta = .46, SE = .19, p<.05$), providing further evidence that white coaches benefitted more from positive performance than their minority counterparts. While the interaction of race and post-season performance was not significant in this model, the interaction of improvement and race was significant and negative employment separation ($\beta = -.40, SE = .11, p<.001$), suggesting, once again, that minority leaders may need to perform well in multiple areas in order to counteract the discounting effect of race.

Endogeneity (Fixed Effects--Team). Controlling for the fixed effects of coaches does not necessarily address unobserved differences between teams as certain teams may place more value on different aspects of coaching performance than other teams. We addressed this by replicating the above analyses while controlling for the fixed effects of teams (*see Appendix F*). Because one team could have coaches of different races over the course of our panel data set, the main effect of race was not omitted from these models. In the full sample (*N=333),* the positive relationship between race and employment separation was marginally significant (β =*.12, SE = .06, p=.06*), but only in the model that included interaction terms. Consistent with the models described above, the negative relationship between wins and employment separation (β = -*.24, SE = .05, p<.001*) was attenuated for minority coaches as indicated by the significant interaction term (β = *.22, SE = .07, p<.01*). Other significant interaction terms, however, indicated that this effect may be counteracted for minority leaders by positive performance in improvement (β = -*.16, SE = .07, p<.05*) and post-season performance (β = -*.13, SE = .05, p<.01*).

When restricting our sample to coaches with over two years in position, we found a main effect for race in our replication of Model 2 (β = *.53, SE = .13, p<.001*), but not in our interaction model (*p>.10*). We did, however, find that once again, the main effect of wins on employment separation (β = -*.40, SE = .09, p<.001*) was attenuated for minority coaches (β = *.39, SE = .15, p<.05*). Similar to the above models, we also found the interaction of race and improvement was significant (β = -*.45, SE = .12, p<.001*) and that the interaction of race and post-season performance was marginally significant (β =-*.22, SE = .11, p=.06*).

Endogeneity (Matching Technique). Finally, as discussed above, sample matching increases the ability to infer causality from results by reducing correlations among treatment and explanatory variables, thus helping to address issues of endogeneity. The process of modifying the sample for this purpose, however, introduces the potential for reported results to be influenced by the matching process as different matching methodologies can have similar correlation-reducing effects, but result in different samples. To address this concern, we replicated our matching process using a CEM model with alternate coarsening specifications as well as a propensity score matching model. Propensity-score matching matches treated and untreated observations within a data set by using covariates to calculate scores that predict the likelihood of an observation receiving the treatment and then matching observations on these propensity scores (Rosenbaum & Rubin, 1983; Todd, 2010). We applied this technique using Stata's *psmatch2* command with the propensity-score matching caliper (.01) specifications to construct samples that were matched based upon the explanatory and control variables used in each equation. Use of this econometric technique has grown in the field of management in recent years (e.g. Boivie, Graffin, Oliver, & Withers, 2016; Cumming, Leung, & Rui, 2015).

We began by replicating our organizational rewards analyses using our alternate CEM specifications. In our replication of Model 4, we found a significant relationship between leader race and second place award points ($\beta = -17.99$, $SE = 6.77$, $p<.01$). No other variables of interest were significant in these replications ($ps>.10$). We then replicated our organizational rewards analyses using the propensity-score matching caliper (.01) specifications. None of our variables of interest were significant in these replications ($ps>.10$). Collectively, these robustness tests indicate that our organizational rewards tests were sensitive to matching specifications.

We then replicated our separation analyses for coaches with tenure greater than two years using the same matching specifications described above. In our alternate CEM model, the coefficient on the effect of leader race was in the expected direction, but the effect was not significant ($ps>.10$). We did find, however, that similar to the results reported in *Table 3*, the negative relationship between wins and employment separation in this model ($\beta = -.27$, $SE = .14$, $p=.06$), was attenuated for minority coaches ($\beta = .28$, $SE = .15$, $p=.06$). Using the propensity-score matching caliper (.01) specification, we found that the relationship between leader race and employment separation was positive and significant in our replications of Models 4 and 6 ($ps<.05$). The interaction effects in these models, however, indicate that minority leaders benefited *more* from improvement and post-season performance than their white counterparts such that for minority leaders, performance served to counteract their increased likelihood of employment separation. Collectively, these findings indicate that while the evidence of a race effect was present in all of our models, how that effect presented itself in analyses was sensitive to matching specifications.

Employment Separations. As stated earlier, we were able to confirm that 85.09 percent of the employment separations in this study could be coded as involuntary. For robustness, observations where the cause of separation was not confirmed as involuntary were removed from our sample before re-estimating our likelihood of employment separation models. Consistent with the results reported in *Table 3,* in the full model ($N=316$), the effect of race was not significant ($p>.10$). When restricting the sample to coaches with over two years in position ($N=132$), the direct effect of race was marginally significant ($\beta = .18$, $SE = .10$, $p=.07$). We then used CEM to create a matched sample excluding coaches where the cause of separation was not confirmed as involuntary. Consistent with *Table 3,* in the replications of Models 3 and 5 ($N=156$), neither race nor any interaction variables were significant ($ps>.10$). We then created a matched sample for coaches with tenure greater than two years, once again excluding coaches whose cause of separation was not confirmed as involuntary. In our replication of Model 4 ($N=46$), the relationship between race and likelihood of employment separation was marginally significant ($\beta = .22$, $SE = .11$, $p=.054$). In the replication of Model 6, the main effect of race was significant ($\beta = .23$, $SE = .09$, $p<.05$). Additionally, the negative effect of wins on likelihood of separation ($\beta = -.23$, $SE = .13$, $p=.07$) was attenuated for minority head coaches ($\beta = .36$, $SE = .18$, $p=.052$). In this model, the interaction of race and improvement was negative and significant ($\beta = -.58$, $SE = .21$, $p<.05$), indicating that minority coaches needed to improve in order to counteract the positive relationship between race and likelihood of separation. Overall, these results were consistent with our reported findings.

Linear Models. The use of linear models in binary dependent variables can create concerns due to their potential to produce probabilities that differ from those of non-linear models. We compared the probabilities predicted by both LPM and probit analyses in our employment separation models. The probabilities predicted by each technique were highly correlated in both the full sample ($\rho=.99$, $p<.001$)

and the subsample of coaches with tenure over two years (*ρ=.98, p<.001*). The mean difference in probabilities predicted by LPM and probit for each of these samples was less than .001 (*ps>.10*). In the full sample, five out of 333 probabilities predicted by LPM fell out of the range of zero to one while 10 out of 144 probabilities predicted by LPM fell out of the range of zero to one in the sample of coaches with over two years in position. We also re-estimated our employment separation models using a probit analyses. These results were also consistent with our reported findings. A summary of the results of robustness tests has been provided in *Appendices G and H.*

DISCUSSION

Summary of Findings

In this study, we found nuanced evidence that even in the absence of a glass cliff, the rewards allocated to organizational leaders and leaders' likelihood of employment separation could be impacted by a leader's status as a racial minority. These findings indicate that even when racial biases do not lead to differential leadership opportunities (i.e. a glass cliff), they can still lead to differential leadership outcomes, though the findings were contingent upon dependent variable specifications and sampling restrictions. Specifically, in both the full and primary matched sample analyses, we found that minority leaders received fewer second place votes for NBA COY than their white peers. Although these findings were present in several of our robustness tests, they were also shown to be somewhat sensitive to matching specifications and analytical methodology.

In both our full and matched samples, we found that once leaders' tenure had surpassed a highly conspicuous initial period of employment, minority leaders had a greater likelihood of employment separation than their white counterparts. This finding was robust to a variety of matching specifications and analytical methodologies. Furthermore, our robustness tests indicated that when controlling for fixed effects of either the leader or organization, racial effects emerged for the entire sample. While our primary findings showed that leader race interacted with organizational performance such that positive performance reduced the likelihood of employment separation for white coaches more than it did for minorities, how interaction effects manifested was somewhat influenced by model specifications.

Theoretical Contribution

Glass Cliff. Our research makes a significant contribution to the literature on discrimination in leadership because it indicates that while the phenomenon of the glass cliff may emerge within a variety of contexts, it is not a necessary condition for inequitable leadership outcomes. To our knowledge, this is the first study using archival data to accomplish this feat. Although Price and Wolfers (2010) found evidence of racial discrimination in NBA officiating, Coleman, DuMond, and Lynch (2008) showed racial parity in player reward allocation, and the NBA is generally regarded as a leader in organizational diversity (Lapchick & Guiao, 2015). Given the extensive evidence of racial equity in the NBA, our findings of discrimination in leadership employment outcomes come from a context that we were able to both theoretically argue and empirically demonstrate was absent of a “glass cliff”. This finding also suggests that even when discrimination does not present itself in the selection process through quality of opportunity, it may be deferred to subsequent employment decisions.

LCT and Attribution Theory. Our study makes further contribution to the literature by integrating LCT with attribution theory to propose a theoretical framework for the underlying mechanisms that lead to the manifestation of discrimination. Our framework suggests that the interactive effect that leader race and organizational performance have on leadership outcomes will be influenced by the extent that the decision maker is utilizing automatic processing as opposed to controlled processing. Our findings are consistent with argument. Whereas we find evidence that racial bias presents itself in second place COY voting we find no evidence of bias in first place voting. One explanation for this is that evaluators are placing great weight on their first-place decision and thus engaging in much more controlled processing than in the second-place voting process. Similarly, in our separation analyses, the emergence of bias for coaches with several years in position may indicate that in the early years of employment, performance is much more highly scrutinized and thus decision makers are engaging in a considerable amount of controlled processing as they evaluate leaders. As leaders become established, however, their evaluation becomes much more routine and thus much more reliant upon automatic processing. This reliance upon automatic processing makes evaluations at this stage more susceptible to racial biases that are primed by leadership categorization. Consequently, as time in position reaches the point that evaluations become more routine, white leaders benefit more from successful performance than minority leaders do. Future research should attempt to isolate the differential effects of automatic processing from those of controlled processing.

Although our interaction models did not provide full support for our hypotheses, in many ways, they are consistent with our theoretical framework. Model 5 of Table 2 provided some support for our argument that positive organizational performance would result in greater performance rewards for white leaders than it would for minority leaders. We did not find support for Hypothesis 4, however, as Figure 2 shows that for experienced coaches, the relationship between performance and likelihood of separation was stronger from white leaders than it was for minority leaders. One interpretation of this finding, however, is that when performance was noticeably poor (i.e. 1 SD below the mean), efforts to correct this outcome would result in more controlled processing, thus reducing the likelihood of bias emerging. As performance levels improve, however, executives can increasingly devote cognitive resources to other functions, thus utilizing more automatic processing in the evaluation of leaders. As described in our theoretical argument, this would result in white leaders benefitting more from improved performance. This interpretation of our interaction model should be considered cautiously, however. In our employment separation models, the finding that performance interacted with race to influence likelihood of separation was robust to a variety of specifications, but how these interactions presented themselves was somewhat sensitive to specifications. Future research should further explore these types of relationships in an effort to better understand exactly how leadership outcomes are influenced by the interaction of race and performance.

Leadership Categorization or Aversive Racism? Finally, although the theory of aversive racism motivated some of our exploratory analyses (i.e. second place award points, coaches with over two years in position), our findings taken in conjunction with LCT raise questions as to whether the likelihood of discrimination occurring in less conspicuous circumstances is influenced by people's motivations or if it is simply a manifestation of how information is processed in encoding, retrieval, and task completion.

The theory of aversive racism states that while people can value equality and intend to behave in procedurally just ways, ingrained and sometimes unconscious biases can still lead to discriminatory behavior (Dovidio & Gaertner, 2004). Therefore, discrimination is less likely to occur when behavior is highly visible than when actions are less apparent (Dovidio & Gaertner, 2000). In both our reward and separation analyses, we only found evidence of discrimination under less conspicuous conditions, using archival data to find support for a theory that has largely relied on support from experimental studies. Extant research has argued that discrimination is most likely to occur when prejudiced behavior can be justified (Crandall & Eshleman, 2003; Murrell, Dietz-Uhler, Dovidio, Gaertner, & Drout, 1994) and that rationalizing discriminatory behavior can reinforce a sense of self-objectivity which can lead to further discrimination (Uhlmann & Cohen, 2007).

Leadership categorization theory, however, addresses how information is encoded, stored, and retrieved, suggesting that automatic processes rely much more on categorization information that is retained in memory than controlled processes do. Consequently, tasks that rely heavily on automatic processes will be subject to biases associated with the categorization process. According to this argument, the presentation (or suppression) of bias may be influenced more by how information is processed than it is by an individual's desires. Although these two explanations are not mutually exclusive, there is a need for future research to decouple these explanations in an effort to identify solutions to the problem of differential leadership outcomes.

Methodological Contribution

Due to challenges in collecting appropriate data, one negative employment outcome that has been under-researched in the discrimination literature is the impact of race on a leader’s likelihood of employment separation. Cook and Glass (2014a) attempted to address this gap by using CEO tenure as a dependent variable, but acknowledged limitations in this measurement resulting from CEO tenures that persisted beyond the time period of their data collection. This paper addresses this concern by introducing the practice of using panel data to investigate employment separation as a binary outcome based upon changes in employment status from one year to the next.

We also contend that our robustness tests showing that matching specifications influenced our reported findings in our rewards analyses make an important methodological contribution given current concerns about replicability of research (Antonakis, 2017; Camerer et al., 2018; Loken & Gelman, 2017; Open Science Collaboration, 2015). Although the samples that yielded different results were matched using different specifications, the matching specifications yielded similar results in terms of correlations between the samples' covariates and the endogenous regressor. Consequently, there were no noticeable differences between these samples, and one could argue that the failure to replicate from one matched sample to another was simply a problem of sample selection. This speaks to the importance of utilizing replication studies to confirm our understanding of theory, even when replication would not constitute what researchers traditionally refer to as a theoretical contribution.

Practical Contribution

Although our research did find evidence of racial disparities in leadership employment outcomes, our findings were more nuanced than those of many of the studies discussed above. One possible explanation for the more nuanced findings presented in this study is that while we still found evidence of discrimination within a context that was absent of a glass cliff, the more equitable selection processes employed within this context may be a signal that stakeholders within the NBA exercise more controlled processing in evaluating leaders due to their emphasis on equity in selection processes. Accordingly, one could argue that equitable selection processes may be a good first step for reducing discriminatory leadership outcomes, but efforts should not end at selection.

The fact that findings of discrimination in NBA COY voting were somewhat susceptible to model specification may be an indication that this process may have been less biased than the process of making employment separation decisions. One possible explanation is that in addition to COY voting being conducted by external stakeholders who may have different rationales for their decisions than general managers or team presidents, the COY voting is a designated annual process where evaluators are likely to take care and engage in controlled processing, thus limiting the influence of the leadership prototype. Conversely, the decision to terminate a coach's employment is likely the manifestation of ongoing evaluations, many of which are influenced by automatic processing. Organizational leaders could attempt to mimic the controlled processing that takes place in COY voting by having a panel of external subject matter experts provide annual performance feedback and recommendations to those responsible for evaluating leadership performance. This process would be similar to a 360 review except that the feedback provided by the experts would be intended to aid the evaluator by stimulating controlled processing in the evaluation process.

Limitations

Our data, however, did not allow us to directly observe this attribution process. Furthermore, our finding that experienced minority leaders were at greater risk of employment separation than white leaders after successful performance, but not after unsuccessful performance, may appear to conflict with the findings of Carton and Rosette (2011), but it is similar to that of Rosette et al. (2008). This clearly underscores the need for future research to investigate how demographic characteristics influence the development of attributions for responsibility.

Another potential limitation in this study is that we were unable to obtain data on coaches' salaries and contract length. As NBA coaching contracts are generally guaranteed, such information would be relevant to fully understanding the economic impact that an involuntary employment separation has on the organization initiating the separation. If racial bias were to influence how contracts were constructed in terms of annual salary, length, and guaranteed payouts, it is possible that bias in contract negotiation could reduce the economic penalties that organizations experience when firing minority coaches and that our results could be capturing such an effect. This is improbable, however, as our robustness checks found no evidence of discrimination in aspects of the selection process. Also, if such disparities were to exist in something as quantifiable as contract negotiations, this would likely be addressed by the National Basketball Coaches Association. Future research, however, could examine the relationship between contract terms and manifestations of discrimination.

CONCLUSION

In summary, we examined the presence of racial bias towards those in leadership roles in two different phases of the leadership process: reward allocation and employment separation. Our study was conducted using data from an organization that is known for, and publicly celebrates, its efforts to support diversity. Even so, while our robustness checks found no evidence of discrimination in the quality of opportunities received by leaders, we found evidence of racial discrimination in post-selection leadership outcomes. Furthermore, our use of coarsened exact matching supports inferences of causality from our results. These findings suggest that even when an organization appears to be operating in an unbiased manner, discrimination can still exist. The major takeaway from these findings is that inclusion is an ongoing process that requires ongoing efforts rather than a one-time goal that can be achieved, illustrating why this topic continues to warrant attention even after decades of research.

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TABLE 1

Summary Statistics and Correlation Table

	Mean Values				Correlations for Full Sample											
	Full Sample[a]	White[b]	Minority[c]	Difference	1	2	3	4	5	6	7	8	9	10	11	12
1 Race (0 = White, 1 = Minority)	0.38				1.00											
	(0.03)															
2 Total Coach of Year Award Points	46.57	54.92	32.71	22.22 t	-0.10 t	1.00										
	(6.05)	*(8.40)*	*(7.92)*	*(12.44)*												
3 Coach of Year First Place Points	25.87	30.48	18.23	12.25	-0.08	0.97 ***	1.00									
	(4.04)	*(5.57)*	*(5.45)*	*(8.33)*												
4 Coach of Year Second Place Points	15.52	18.84	10.02	8.83 *	-0.12 *	0.93 ***	0.83 ***	1.00								
	(1.93)	*(2.76)*	*(2.26)*	*(3.97)*												
5 Coach of Year Third Place Points	5.17	5.60	4.46	1.14	-0.06	0.60 ***	0.46 ***	0.67 ***	1.00							
	(0.51)	*(0.68)*	*(0.76)*	*(1.05)*												
6 Employment Separation	0.35	0.33	0.38	-0.06	0.06	-0.19 ***	-0.16 **	-0.20 ***	-0.22 ***	1.00						
	(0.03)	*(0.03)*	*(0.04)*	*(0.05)*												
7 Wins	42.29	43.60	40.13	3.46 *	-0.14 *	0.41 ***	0.37 ***	0.41 ***	0.41 ***	-0.23 ***	1.00					
	(0.67)	*(0.84)*	*(1.10)*	*(1.38)*												
8 Improvement	0.95	1.39	0.23	1.16	-0.05	0.39 ***	0.34 ***	0.40 ***	0.40 ***	-0.14 **	0.43 ***	1.00				
	(0.60)	*(0.70)*	*(1.10)*	*(1.24)*												
9 Average Age of Players on Team	26.75	26.87	26.56	0.31	-0.09	0.02	0.01	0.03	0.05	-0.02	0.53 ***	-0.14 *	1.00			
	(0.09)	*(0.12)*	*(0.15)*	*(0.20)*												
10 All-NBA Players on Team	0.36	0.41	0.29	0.13 t	-0.10 t	0.18 ***	0.17 **	0.16 **	0.17 **	-0.05	0.60 ***	0.23 ***	0.32 ***	1.00		
	(0.03)	*(0.05)*	*(0.05)*	*(0.07)*												
11 Adjusted Team Salary	72.92	72.40	73.79	-1.39	0.05	-0.05	-0.04	-0.06	-0.04	0.05	0.23 ***	-0.15 **	0.40 ***	0.18 **	1.00	
	(0.72)	*(0.79)*	*(1.42)*	*(1.50)*												
12 Post-season performance	-2.25	-2.41	-1.98	-0.43 *	0.11 *	-0.05	-0.06	-0.02	-0.06	-0.14 **	-0.08	0.03	0.02	0.06	0.04	1.00
	(0.10)	*(0.15)*	*(0.12)*	*(0.21)*												

[a] *n=335*

[b] *n=209*

[c] *n=126*

****p<.001, **p<.01, *p<.05, †p<.10 for two-tail test, standard errors in parentheses*

TABLE 2

Tobit Model for Effects of Race on Performance Rewards

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample	Full	Full	Matched	Matched	Matched	Matched
DV=	Total Points	2nd Place Points	Total Points	2nd Place Points	Total Points	2nd Place Points
Constant	-61.77 *** (15.20)	-33.35 *** (8.24)	-56.09 ** (18.38)	-28.65 ** (9.98)	-67.26 *** (15.19)	-33.42 ** (11.84)
Race (0 = White, 1 = Minority)	-9.28 (15.23)	-11.20 * (5.44)	-23.11 (18.76)	-15.85 ^{t} (8.54)	1.53 (26.04)	-4.00 (12.95)
Wins	168.10 *** (16.37)	61.12 *** (7.54)	156.95 *** (30.62)	52.24 *** (11.61)	198.83 *** (38.32)	60.74 *** (13.93)
Improvement	56.29 *** (15.76)	21.87 *** (6.85)	70.28 *** (21.73)	26.42 ** (10.15)	67.13 ** (23.68)	33.91 * (13.33)
Average player age	-33.00 ** (11.38)	-12.42 * (5.58)	-36.73 (23.07)	-11.59 (8.96)	-73.02 * (36.34)	-16.94 (15.24)
All-NBA players on team	-29.57 *** (8.17)	-11.96 *** (3.85)	-20.50 * (8.50)	-9.15 ^{t} (4.89)	-29.81 *** (7.90)	-14.30 *** (3.89)
Race*Wins					-77.80 ^{t} (45.31)	-18.59 (16.94)
Race*Improvement					0.77 (37.29)	-15.24 (17.79)
Race*Average player age					67.48 ^{t} (37.18)	12.60 (17.85)
Race*All-NBA players on team					17.58 (15.31)	9.40 (6.34)
Sample size	335	335	198	198	198	198
Clusters (Coach/Year)	86/13	86/13	68/13	68/13	68/13	68/13
Uncensored observations	162	120	93	68	93	68
Pseudo R-Squared	0.09	0.11	0.11	0.12	0.11	0.13
Model fit	$X^2(5) =$ *103.20* ***	$X^2(5) =$ *105.88* ***	$X^2(5) =$ *66.55* ***	$X^2(5) =$ *61.03* ***	$X^2(9) =$ *88.19* ***	$X^2(9) =$ *90.78* ***

Robust standard errors clustered on coach and year, $^{***}p<.001$, $^{**}p<.01$, $^{*}p<.05$, $^{\dagger}p<.10$ *for two-tail test*

TABLE 3

Linear Probability Model for Effects of Race on Likelihood of Employment Separation

	Model 1 Full Sample		Model 2 Full Sample, Tenure>2		Model 3 CE Matched Sample		Model 4 CE Matched Sample, Tenure>2		Model 5 CE Matched Sample		Model 6 CE Matched Sample, Tenure>2	
Constant	0.35 ***	(0.04)	0.28 ***	(0.03)	0.37 ***	(0.06)	0.09	(0.12)	0.38 ***	(0.06)	0.12	(0.13)
Race (0 = White, 1 = Minority)	0.04	(0.07)	0.16 *	(0.08)	0.02	(0.08)	0.37 **	(0.11)	0.02	(0.08)	0.37 *	(0.14)
Wins	-0.17 ***	(0.03)	-0.20 **	(0.07)	-0.12 ^{t}	(0.06)	-0.16	(0.10)	-0.18 *	(0.08)	-0.32 **	(0.11)
Improvement	0.01	(0.03)	0.05	(0.05)	-0.03	(0.04)	0.01	(0.09)	0.00	(0.05)	0.10	(0.13)
Post-season performance	-0.08 *	(0.04)	-0.13 *	(0.05)	-0.11 ***	(0.03)	-0.20 ***	(0.05)	-0.10 ^{t}	(0.05)	-0.09	(0.06)
Top 10 media market (0 = No, 1 = Yes)	-0.05	(0.05)	0.06	(0.06)	-0.08	(0.08)	0.26 ^{t}	(0.14)	-0.09	(0.09)	0.20	(0.15)
Total player salary	0.06	(0.04)	0.09 **	(0.03)	0.03	(0.04)	0.04	(0.05)	-0.01	(0.05)	0.17 ^{t}	(0.10)
All-NBA players on team	0.08 ***	(0.02)	0.15 ***	(0.04)	0.07 *	(0.03)	0.16 *	(0.06)	0.12 **	(0.05)	0.13	(0.10)
Race*Wins									0.13	(0.10)	0.31 **	(0.11)
Race*Improvement									-0.06	(0.07)	-0.19	(0.14)
Race*Post-season performance									-0.04	(0.07)	-0.21	(0.15)
Race*Total player salary									0.08	(0.08)	-0.23	(0.16)
Race*All-NBA players on team									-0.11	(0.09)	0.01	(0.13)
Sample size	333		144		182		56		182		56	
Clusters (Coach/Year)	85/13		45/13		66/13		31/13		66/13		31/13	
R-Squared	0.11		0.19		0.10		0.27		0.13		0.41	
Model fit	*F(7, 325) = 7.22* ***		*F(7, 136) = 5.99* ***		*F(7, 174) = 3.53* **		*F(7, 48) = 5.85* ***		*F(12, 169) = 2.66* **		*F(12, 43) = 4.90* **	

Robust standard errors clustered on coach and year, $^{***}p<.001$, $^{**}p<.01$, $^{*}p<.05$, $^{\dagger}p<.10$ *for two-tail test*

FIGURE 1

Games Won Per Season by Coaches' Employment Separation Status

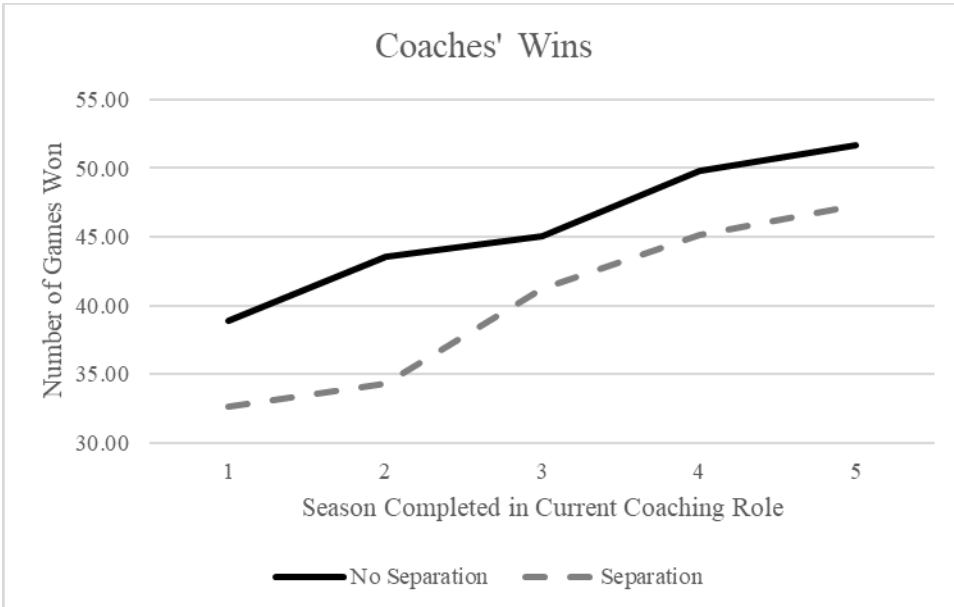
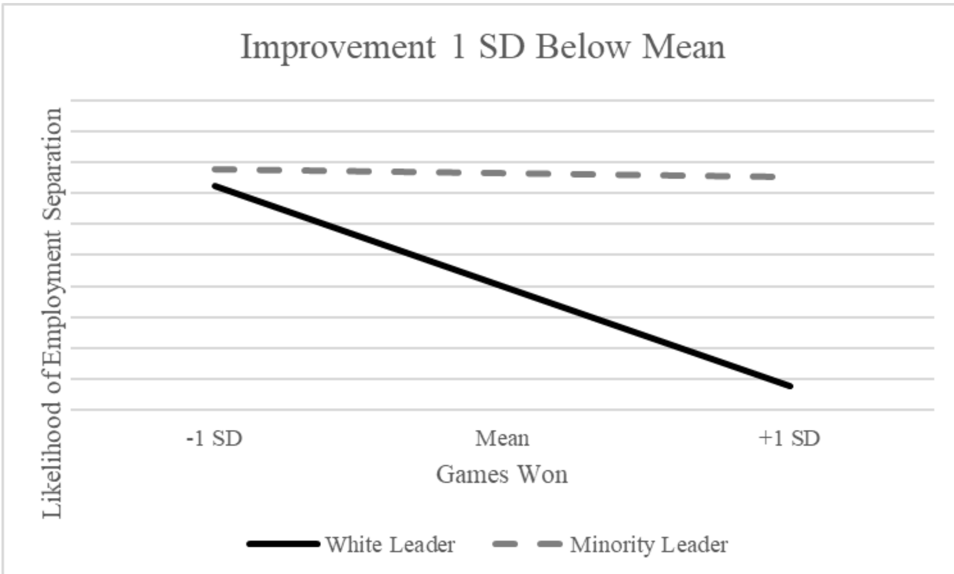


FIGURE 2

Differential Effects of Games Won on Employment Separation for Coaches with More Than Two Years in Position



APPENDIX A

IV Tobit Model for Effects of Race on Performance Rewards (Main Effects Only)

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7		Model 8	
Sample	Full		Full		Full		Full		Matched		Matched		Matched		Matched	
IV Estimator	Two-Step		MLE		Two-Step		MLE		Two-Step		MLE		Two-Step		MLE	
DV=	Total Points		Total Points		2nd Place Points		2nd Place Points		Total Points		Total Points		2nd Place Points		2nd Place Points	
First-stage regression / DV = Wins																
Constant	0.04	(0.05)	0.04	(0.05)	0.04	(0.05)	0.04	(0.05)	0.01	(0.07)	0.00	(0.08)	0.01	(0.07)	0.00	(0.08)
Race (0 = White, 1 = Minority)	-0.10	(0.08)	-0.10	(0.08)	-0.10	(0.08)	-0.10	(0.08)	-0.01	(0.10)	-0.01	(0.09)	-0.01	(0.10)	-0.01	(0.09)
Previous Wins	0.33***	(0.05)	0.32***	(0.05)	0.33***	(0.05)	0.32***	(0.05)	0.37***	(0.07)	0.35***	(0.07)	0.37***	(0.07)	0.36***	(0.07)
Average player age	0.18***	(0.05)	0.21***	(0.05)	0.18***	(0.05)	0.20***	(0.05)	0.13^{t}	(0.07)	0.16*	(0.07)	0.13^{t}	(0.07)	0.15*	(0.07)
All-NBA players on team	0.40***	(0.04)	0.39***	(0.04)	0.40***	(0.04)	0.40***	(0.04)	0.39***	(0.05)	0.38***	(0.04)	0.39***	(0.05)	0.39***	(0.04)
R-Squared	0.55				0.55				0.51				0.51			
Model fit	$F(4, 330) =$ 99.34***				$F(4, 330) =$ 99.34***				$F(4, 198) =$ 50.88***				$F(4, 198) =$ 50.88***			
Tobit with endogenous regressors																
Constant	-58.42***	(14.50)	-59.18***	(15.44)	-31.70***	(6.53)	-32.10***	(6.27)	-57.55**	(19.81)	-59.46**	(19.84)	-28.83***	(8.68)	-29.43***	(8.09)
Race (0 = White, 1 = Minority)	-18.91	(19.89)	-19.03	(20.01)	-14.96^{t}	(8.49)	-15.03^{t}	(7.99)	-21.61	(23.06)	-21.24	(22.98)	-15.27	(9.64)	-15.26^{t}	(9.07)
Wins	145.04***	(22.11)	144.45***	(25.24)	51.24***	(9.34)	51.15***	(9.97)	166.24***	(27.95)	165.61***	(35.56)	53.07***	(11.72)	53.09***	(12.94)
Previous Wins	-43.94**	(15.83)	-43.29*	(18.93)	-16.44*	(6.67)	-16.28*	(8.03)	-73.12***	(19.97)	-72.18**	(24.09)	-24.88**	(8.31)	-24.77**	(9.30)
Model fit	$X^2(3) =$ *63.05****		$X^2(3) =$ 57.10***		$X^2(3) =$ *45.64****		$X^2(3) =$ *62.43****		$X^2(3) =$ *39.58****		$X^2(3) =$ 31.00***		$X^2(3) =$ *23.97****		$X^2(3) =$ *28.90****	
Sample size	335		335		335		335		198		198		198		198	
Clusters (Coach)	N/A		86		N/A		86		N/A		68		N/A		68	
Uncensored observations	162		162		120		120		93		93		68		68	
Wald test of exogeneity	$X^2(1) =$ *15.81****		$X^2(1) =$ *15.23****		$X^2(1) =$ *13.79****		$X^2(1) =$ *11.10****		$X^2(1) =$ *6.23**		$X^2(1) =$ *7.32***		$X^2(1) =$ *5.88**		$X^2(1) =$ *6.22**	

Robust standard errors clustered on coach with MLE estimator (no clustering with Two-Step estimator), $^{***}p<.001$, $^{**}p<.01$, $^{*}p<.05$, $^{t}p<.10$ *for two-tail test*

APPENDIX B

IV Tobit Model for Effects of Race on Performance Rewards (Interaction Models)

	Model 1		Model 2		Model 3		Model 4	
Sample	Matched		Matched		Matched		Matched	
IV Estimator	Two-Step		MLE		Two-Step		MLE	
DV=	Total Points		Total Points		2nd Place Points		2nd Place Points	
First-stage regression / DV = Wins								
Constant	0.01	(0.07)	0.00	(0.08)	0.01	(0.07)	0.00	(0.08)
Race (0 = White, 1 = Minority)	-0.01	(0.10)	-0.01	(0.09)	-0.01	(0.10)	-0.01	(0.09)
Previous Wins	0.36 ***	(0.11)	0.32 ***	(0.09)	0.36 ***	(0.11)	0.34 ***	(0.09)
Average player age	0.13	(0.10)	0.19 *	(0.09)	0.13	(0.10)	0.16 t	(0.09)
All-NBA players on team	0.40 ***	(0.08)	0.38 ***	(0.07)	0.40 ***	(0.08)	0.39 ***	(0.06)
Race*Previous Wins	0.02	(0.15)	0.06	(0.13)	0.02	(0.15)	0.04	(0.13)
Race*Average player age	0.00	(0.15)	-0.06	(0.15)	0.00	(0.15)	-0.02	(0.15)
Race*All-NBA players on team	-0.01	(0.11)	0.00	(0.08)	-0.01	(0.11)	-0.01	(0.08)
R-Squared	0.51				0.51			
Model fit	*F(7, 190) =* 28.63 ***				*F(7, 190) =* 28.63 ***			
First-stage regression / DV = Race * Wins								
Constant	0.00	(0.05)	0.00	(0.00)	0.00	(0.05)	0.00	(0.00)
Race (0 = White, 1 = Minority)	-0.01	(0.07)	0.00	(0.05)	-0.01	(0.07)	0.00	(0.05)
Previous Wins	0.00	(0.07)	-0.01	(0.01)	0.00	(0.07)	0.00	(0.01)
Average player age	0.00	(0.07)	0.01	(0.02)	0.00	(0.07)	0.01	(0.01)
All-NBA players on team	0.00	(0.05)	0.00	(0.01)	0.00	(0.05)	0.00	(0.00)
Race*Previous Wins	0.38 ***	(0.10)	0.39 ***	(0.10)	0.38 ***	(0.10)	0.38 ***	(0.10)
Race*Average player age	0.13	(0.10)	0.12	(0.12)	0.13	(0.10)	0.12	(0.12)
Race*All-NBA players on team	0.39 ***	(0.08)	0.39 ***	(0.06)	0.39 ***	(0.08)	0.39 ***	(0.06)
R-Squared	0.57				0.57			
Model fit	*F(7, 190) =* 35.27 ***				*F(7, 190) =* 35.27 ***			
Tobit with endogenous regressors								
Constant	-66.81 **	(22.92)	-72.26 ***	(21.66)	-33.61 ***	(9.74)	-34.43 ***	(10.02)
Race (0 = White, 1 = Minority)	1.52	(28.76)	4.88	(30.24)	-3.55	(11.78)	-3.20	(13.00)
Wins	167.69 ***	(39.23)	166.38 ***	(46.93)	56.11 ***	(16.26)	56.11 ***	(16.85)
Previous Wins	-80.54 **	(26.77)	-78.02 *	(36.56)	-31.69 **	(11.02)	-31.55 *	(12.92)
Race*Wins	-5.65	(54.78)	-4.44	(65.33)	-8.20	(22.55)	-8.18	(23.86)
Race*Previous Wins	14.71	(39.40)	12.85	(47.25)	14.48	(16.26)	14.45	(17.31)
Model fit	$X^2(5) =$ *39.59* ***		$X^2(5) =$ 30.21 ***		$X^2(5) =$ *23.58* ***		$X^2(5) =$ *26.82* ***	
Sample size	198		198		198		198	
Clusters (Coach)	N/A		68		N/A		68	
Uncensored observations	93		93		68		68	
Wald test of exogeneity	$X^2(2) =$ *8.62* *		$X^2(2) =$ *11.84* **		$X^2(2) =$ *8.17* ***		$X^2(2) =$ *12.02* **	

*Robust standard errors clustered on coach with MLE estimator (no clustering with Two-Step estimator), ***p<.001, **p<.01, *p<.05, †p<.10 for two-tail test*

APPENDIX C

2SLS IV Model for Effects of Race on Likelihood of Employment Separation (Main Effects Only)

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7		Model 8	
Tenure	All		All		>2		>2		All		All		>2		>2	
Matching	None		None		None		None		CEM		CEM		CEM		CEM	
DV=	Employment Separation		Employment Separation		Employment Separation		Employment Separation		Employment Separation		Employment Separation		Employment Separation		Employment Separation	
SE Cluster	Coach / Year		Coach		Coach / Year		Coach		Coach / Year		Coach		Coach / Year		Coach	
First-stage regression / DV = Wins																
Constant	0.05	(0.07)	0.05	(0.06)	0.02	(0.08)	0.02	(0.10)	0.02	(0.10)	0.02	(0.10)	-0.08	(0.22)	-0.08	(0.22)
Race (0 = White, 1 = Minority)	-0.09	(0.09)	-0.09	(0.08)	0.15	(0.12)	0.15	(0.13)	-0.01	(0.09)	-0.01	(0.10)	0.21	(0.23)	0.21	(0.24)
Previous Wins	0.33 ***	(0.06)	0.33 ***	(0.06)	0.38 ***	(0.11)	0.38 ***	(0.09)	0.38 ***	(0.08)	0.38 ***	(0.08)	0.33 **	(0.12)	0.33 **	(0.11)
Post-season performance	-0.06	(0.04)	-0.06	(0.04)	-0.02	(0.09)	-0.02	(0.07)	-0.11 ^{t}	(0.06)	-0.11 ^{t}	(0.06)	-0.08	(0.17)	-0.08	(0.16)
Top 10 media market (0 = No, 1 = Yes)	-0.03	(0.09)	-0.03	(0.08)	-0.19 *	(0.08)	-0.19	(0.12)	-0.04	(0.11)	-0.04	(0.10)	-0.05	(0.22)	-0.05	(0.21)
Total player salary	-0.03	(0.03)	-0.03	(0.04)	0.02	(0.06)	0.02	(0.06)	-0.06	(0.06)	-0.06	(0.06)	0.02	(0.13)	0.02	(0.12)
Average player age	0.19 ***	(0.06)	0.19 ***	(0.05)	0.14 ^{t}	(0.07)	0.14 ^{t}	(0.08)	0.19 ***	(0.06)	0.19 *	(0.08)	0.15	(0.10)	0.15	(0.13)
All-NBA players on team	0.41 ***	(0.04)	0.41 ***	(0.03)	0.43 ***	(0.04)	0.43 ***	(0.06)	0.35 ***	(0.05)	0.35 ***	(0.05)	0.52 **	(0.17)	0.52 ***	(0.13)
F-Test of Excluded Instruments	*F(2, 12) =*		*F(2, 84) =*		*F(2, 12) =*		*F(2, 44) =*		*F(2, 12) =*		*F(2, 65) =*		*F(2, 12) =*		*F(2, 30) =*	
	68.67 ***		75.05 ***		58.64 ***		28.56 ***		23.69 ***		34.84 ***		4.91 *		8.48 **	
IV (2SLS) Estimation																
Constant	0.34 ***	(0.05)	0.34 ***	(0.04)	0.27 ***	(0.05)	0.27 ***	(0.07)	0.36 ***	(0.07)	0.36 ***	(0.06)	0.11	(0.13)	0.11	(0.11)
Race (0 = White, 1 = Minority)	0.06	(0.07)	0.06	(0.05)	0.13	(0.08)	0.13	(0.08)	0.03	(0.08)	0.03	(0.07)	0.33 *	(0.13)	0.33 **	(0.12)
Wins	0.04	(0.05)	0.04	(0.05)	0.19 ***	(0.06)	0.19 *	(0.09)	0.08	(0.08)	0.08	(0.09)	0.18 *	(0.08)	0.18	(0.15)
Previous Wins	-0.10 *	(0.05)	-0.10 *	(0.04)	-0.19 *	(0.09)	-0.19 **	(0.07)	-0.09	(0.08)	-0.09	(0.06)	-0.14	(0.11)	-0.14	(0.09)
Post-season performance	-0.08 *	(0.03)	-0.08 *	(0.03)	-0.14 **	(0.05)	-0.14 *	(0.05)	-0.09 **	(0.03)	-0.09 **	(0.03)	-0.19 ***	(0.06)	-0.19 **	(0.07)
Top 10 media market (0 = No, 1 = Yes)	-0.04	(0.05)	-0.04	(0.05)	0.12 ^{t}	(0.06)	0.12	(0.08)	-0.07	(0.07)	-0.07	(0.07)	0.26	(0.16)	0.26 ^{t}	(0.15)
Total player salary	0.06	(0.04)	0.06 *	(0.03)	0.07 ^{t}	(0.04)	0.07	(0.05)	0.04	(0.04)	0.04	(0.04)	0.03	(0.06)	0.03	(0.09)
Kleibergen-Paap RK LM statistic	$X^2(2)$ =		$X^2(2)$ =		$X^2(2)$ =		$X^2(2)$ =		$X^2(2)$ =		$X^2(2)$ =		$X^2(2)$ =		$X^2(2)$ =	
	9.75 **		29.76 ***		9.37 **		19.95 ***		8.06 *		18.75 ***		5.33 ^{t}		6.27 *	
Sample size	333		333		144		144		182		182		56		56	
Clusters	85/13		85		45/13		45		66/13		66		31/13		31	

****p<.001, **p<.01, *p<.05, ^{t}p<.10 for two-tail test*

APPENDIX D

2SLS IV Model for Effects of Race on Likelihood of Employment Separation (Interaction Models)

	Model 1		Model 2		Model 3		Model 4	
Tenure	All		All		>2		>2	
Matching	CEM		CEM		CEM		CEM	
DV=	Employment Separation		Employment Separation		Employment Separation		Employment Separation	
SE Cluster	Coach / Year		Coach		Coach / Year		Coach	
First-stage regression / DV = Wins								
Constant	0.02	(0.10)	0.02	(0.10)	-0.08	(0.27)	-0.08	(0.25)
Race (0 = White, 1 = Minority)	-0.01	(0.09)	-0.01	(0.10)	0.24	(0.23)	0.24	(0.24)
Previous Wins	0.37 ***	(0.11)	0.37 **	(0.11)	0.39 *	(0.15)	0.39 *	(0.16)
Post-season performance	-0.05	(0.09)	-0.05	(0.09)	-0.03	(0.26)	-0.03	(0.24)
Top 10 media market (0 = No, 1 = Yes)	-0.03	(0.10)	-0.03	(0.10)	-0.08	(0.22)	-0.08	(0.21)
Total player salary	-0.07	(0.06)	-0.07	(0.07)	-0.03	(0.19)	-0.03	(0.16)
Average player age	0.24 **	(0.08)	0.24 **	(0.09)	0.24	(0.17)	0.24	(0.20)
All-NBA players on team	0.32 ***	(0.05)	0.32 ***	(0.06)	0.56 **	(0.21)	0.56 **	(0.17)
Race*Previous Wins	0.02	(0.14)	0.02	(0.16)	-0.16	(0.14)	-0.16	(0.24)
Race*Post-season performance	-0.14	(0.11)	-0.14	(0.12)	-0.12	(0.26)	-0.12	(0.29)
Race*Total player salary	0.03	(0.07)	0.03	(0.11)	0.08	(0.26)	0.08	(0.24)
Race*Average player age	-0.09	(0.22)	-0.09	(0.17)	-0.06	(0.22)	-0.06	(0.27)
Race*All-NBA players on team	0.07	(0.07)	0.07	(0.10)	-0.02	(0.20)	-0.02	(0.25)
F-Test of Excluded Instruments	*F(4, 12) =* 15.27 ***		*F(4, 65) =* 16.23 ***		*F(4, 12) =* 3.33 *		*F(4, 30) =* 5.20 **	
First-stage regression / DV = Race*Wins								
Constant	0.05 t	(0.03)	0.05	(0.03)	0.02	(0.07)	0.02	(0.05)
Race (0 = White, 1 = Minority)	-0.02	(0.05)	-0.02	(0.07)	0.12 *	(0.05)	0.12	(0.12)
Previous Wins	0.01	(0.01)	0.01	(0.01)	-0.01	(0.03)	-0.01	(0.02)
Post-season performance	0.01 t	(0.00)	0.01	(0.01)	0.01	(0.04)	0.01	(0.03)
Top 10 media market (0 = No, 1 = Yes)	-0.10 t	(0.05)	-0.10	(0.07)	-0.03	(0.15)	-0.03	(0.11)
Total player salary	0.00	(0.00)	0.00	(0.01)	0.00	(0.02)	0.00	(0.02)
Average player age	-0.01	(0.01)	-0.01	(0.01)	0.01	(0.03)	0.01	(0.02)
All-NBA players on team	0.01	(0.01)	0.01	(0.01)	0.00	(0.02)	0.00	(0.01)
Race*Previous Wins	0.38 ***	(0.10)	0.38 ***	(0.11)	0.25 t	(0.14)	0.25	(0.18)
Race*Post-season performance	-0.19 *	(0.09)	-0.19 *	(0.09)	-0.16	(0.14)	-0.16	(0.16)
Race*Total player salary	-0.02	(0.08)	-0.02	(0.09)	0.05	(0.18)	0.05	(0.17)
Race*Average player age	0.16	(0.16)	0.16	(0.14)	0.16 t	(0.09)	0.16	(0.19)
Race*All-NBA players on team	0.38 ***	(0.07)	0.38 ***	(0.07)	0.54 **	(0.17)	0.54 **	(0.18)
F-Test of Excluded Instruments	*F(4, 12) =* 7.65 **		*F(4, 65) =* 7.70 ***		*F(4, 12) =*		*F(4, 30) =* 2.65 t	

continued on next page

APPENDIX D (CONTINUED)

2SLS IV Model for Effects of Race on Likelihood of Employment Separation (Interaction Models)

IV (2SLS) Estimation								
Constant	0.37 ***	(0.07)	0.37 ***	(0.07)	0.13	(0.11)	0.13	(0.10)
Race (0 = White, 1 = Minority)	0.03	(0.08)	0.03	(0.07)	0.33 *	(0.15)	0.33 **	(0.11)
Wins	0.18 *	(0.08)	0.18	(0.12)	-0.02	(0.07)	-0.02	(0.14)
Previous Wins	-0.18 *	(0.07)	-0.18 *	(0.08)	-0.20	(0.13)	-0.20 t	(0.11)
Post-season performance	-0.07	(0.05)	-0.07 *	(0.04)	-0.08	(0.07)	-0.08	(0.09)
Top 10 media market (0 = No, 1 = Yes)	-0.09	(0.09)	-0.09	(0.08)	0.19	(0.16)	0.19	(0.16)
Total player salary	0.01	(0.05)	0.01	(0.07)	0.16	(0.11)	0.16	(0.12)
Race*Wins	-0.19	(0.16)	-0.19	(0.17)	0.21	(0.20)	0.21	(0.22)
Race*Previous Wins	0.18 t	(0.10)	0.18	(0.11)	0.19	(0.16)	0.19	(0.16)
Race*Post-season performance	-0.06	(0.06)	-0.06	(0.06)	-0.20	(0.14)	-0.20	(0.13)
Race*Total player salary	0.06	(0.07)	0.06	(0.08)	-0.23	(0.17)	-0.23	(0.16)
Kleibergen-Paap RK LM statistic	$X^2(3) =$ *9.48* *		$X^2(3) =$ 15.27 **		$X^2(3) =$ 4.62		$X^2(3) =$ 7.07 t	
Sample size	182		182		56		56	
Clusters	66/13		66		31/13		31	

****p<.001, **p<.01, *p<.05, †p<.10 for two-tail test*

APPENDIX E

Fixed Effects (Coach) Model for Effects of Race on Likelihood of Employment Separation

	Model 1				Model 2				Model 3			
Fixed Effect	Coach				Coach				Coach			
Sample	White Coaches, All Tenure				Black Coaches, All Tenure				All Coaches, All Tenure			
	β	SE	95% CI		β	SE	95% CI		β	SE	95% CI	
			LL	UL			LL	UL			LL	UL
Constant	0.22	(0.18)	-0.14	0.59	0.63 ***	(0.17)	0.28	0.98	0.35 *	(0.14)	0.07	0.62
Race (0 = White, 1 = Minority)												
Wins	-0.19 ***	(0.06)	-0.30	-0.08	-0.08	(0.05)	-0.19	0.03	-0.18 ***	(0.05)	-0.29	-0.07
Improvement	0.01	(0.04)	-0.07	0.08	-0.06	(0.04)	-0.14	0.02	0.00	(0.04)	-0.08	0.08
Post-season performance	-0.22 ***	(0.03)	-0.29	-0.16	-0.37 ***	(0.05)	-0.48	-0.26	-0.22 ***	(0.03)	-0.28	-0.16
Top 10 media market (0 = No, 1 = Yes)	0.11	(0.12)	-0.14	0.35	0.32 *	(0.15)	0.02	0.62	0.13	(0.10)	-0.07	0.33
Total player salary	-0.04	(0.04)	-0.11	0.04	0.02	(0.05)	-0.09	0.12	-0.04	(0.04)	-0.12	0.03
All-NBA players on team	0.12 **	(0.04)	0.04	0.20	0.06	(0.06)	-0.05	0.18	0.12 **	(0.04)	0.03	0.20
Race*Wins									0.13 t	(0.07)	-0.01	0.28
Race*Improvement									-0.08	(0.05)	-0.19	0.02
Race*Post-season performance									-0.13 *	(0.06)	-0.25	-0.02
Race*Total player salary									0.06	(0.06)	-0.06	0.18
Race*All-NBA players on team									-0.05	(0.07)	-0.18	0.09
Sample size	207				126				333			
Clusters (Fixed Effect)	52				33				85			
R-Squared	0.10				0.20				0.09			

*Robust standard errors clustered on fixed effect, ***p<.001, **p<.01, *p<.05, †p<.10 for two-tail test, Fixed effects for year were controlled for but not reported*

APPENDIX E (CONTINUED)

Fixed Effects (Coach) Model for Effect of Race on Likelihood of Employment Separation

	Model 4				Model 5				Model 6			
Fixed Effect	Coach				Coach				Coach			
Sample	White Coaches, Tenure>2				Black Coaches, Tenure>2				All Coaches, Tenure>2			
	β	SE	95% CI		β	SE	95% CI		β	SE	95% CI	
			LL	UL			LL	UL			LL	UL
Constant	0.26	(0.20)	-0.15	0.67	0.73 ***	(0.13)	0.45	1.02	0.32	(0.19)	-0.07	0.70
Race (0 = White, 1 = Minority)												
Wins	-0.39 ***	(0.11)	-0.61	-0.17	0.00	(0.11)	-0.23	0.24	-0.37 **	(0.12)	-0.61	-0.14
Improvement	0.30 ***	(0.08)	0.13	0.47	-0.03	(0.05)	-0.14	0.09	0.29 ***	(0.08)	0.14	0.45
Post-season performance	-0.14 t	(0.07)	-0.29	0.01	-0.42 ***	(0.10)	-0.63	-0.22	-0.16 *	(0.07)	-0.30	-0.02
Top 10 media market (0 = No, 1 = Yes)	0.04	(0.24)	-0.45	0.52	0.44 ***	(0.10)	0.23	0.65	0.14	(0.22)	-0.31	0.59
Total player salary	0.04	(0.09)	-0.14	0.22	0.04	(0.15)	-0.28	0.36	0.04	(0.08)	-0.12	0.19
All-NBA players on team	0.06	(0.08)	-0.11	0.23	0.07	(0.10)	-0.14	0.28	0.05	(0.08)	-0.10	0.21
Race*Wins									0.46 *	(0.19)	0.07	0.85
Race*Improvement									-0.40 ***	(0.11)	-0.62	-0.18
Race*Post-season performance									-0.19	(0.14)	-0.48	0.09
Race*Total player salary									-0.01	(0.17)	-0.35	0.32
Race*All-NBA players on team									0.08	(0.15)	-0.22	0.39
Sample size	91				53				144			
Clusters (Fixed Effect)	25				20				45			
R-Squared	0.25				0.35				0.24			

Robust standard errors clustered on fixed effect, $^{***}p<.001$, $^{**}p<.01$, $^{*}p<.05$, $^{\dagger}p<.10$ for two-tail test, Fixed effects for year were controlled for but not reported

APPENDIX F

Fixed Effects (Team) Model for Effect of Race on Likelihood of Employment Separation

	Model 1				Model 2				Model 3				Model 4			
Fixed Effect	Team				Team				Team				Team			
Sample	All Coaches, All Tenure				All Coaches, All Tenure				All Coaches, Tenure>2				All Coaches, Tenure>2			
	β	SE	95% CI		β	SE	95% CI		β	SE	95% CI		β	SE	95% CI	
			LL	UL			LL	UL			LL	UL			LL	UL
Constant	0.52 ***	(0.11)	0.28	0.75	0.52 ***	(0.12)	0.28	0.76	0.12	(0.15)	-0.19	0.43	0.34 *	(0.14)	0.06	0.63
Race (0 = White, 1 = Minority)	0.08	(0.06)	-0.05	0.21	0.13 ^{t}	(0.06)	0.00	0.26	0.53 ***	(0.13)	0.26	0.80	0.22	(0.17)	-0.13	0.56
Wins	-0.15 ***	(0.04)	-0.24	-0.07	-0.24 ***	(0.05)	-0.34	-0.14	-0.19 *	(0.09)	-0.37	-0.01	-0.40 ***	(0.09)	-0.59	-0.22
Improvement	0.00	(0.03)	-0.05	0.05	0.07 ^{t}	(0.04)	-0.01	0.14	0.05	(0.07)	-0.09	0.19	0.31 **	(0.09)	0.13	0.49
Post-season performance	-0.13 ***	(0.03)	-0.19	-0.07	-0.12 ***	(0.03)	-0.17	-0.06	-0.20 ***	(0.05)	-0.30	-0.10	-0.14 **	(0.05)	-0.24	-0.05
Top 10 media market (0 = No, 1 = Yes)																
Total player salary	0.06 ^{t}	(0.03)	-0.01	0.12	0.07	(0.05)	-0.02	0.16	0.09	(0.07)	-0.06	0.23	0.12	(0.10)	-0.09	0.32
All-NBA players on team	0.07 *	(0.03)	0.01	0.14	0.10 *	(0.04)	0.02	0.18	0.12 **	(0.04)	0.03	0.21	0.12 *	(0.04)	0.02	0.21
Race*Wins					0.22 **	(0.07)	0.08	0.35					0.39 *	(0.15)	0.09	0.69
Race*Improvement					-0.16 *	(0.06)	-0.29	-0.02					-0.45 **	(0.12)	-0.68	-0.21
Race*Post-season performance					-0.13 **	(0.05)	-0.22	-0.04					-0.22 ^{t}	(0.11)	-0.45	0.01
Race*Total player salary					-0.04	(0.04)	-0.13	0.05					-0.02	(0.14)	-0.29	0.26
Race*All-NBA players on team					-0.09	(0.07)	-0.23	0.06					0.03	(0.09)	-0.16	0.22
Sample size	333				333				144				144			
Clusters (Fixed Effect)	30				30				30				30			
R-Squared	0.15				0.18				0.17				0.28			

Robust standard errors clustered on fixed effect, $^{***}p<.001$, $^{**}p<.01$, $^{*}p<.05$, $^{\dagger}p<.10$ for two-tail test, Fixed effects for year were controlled for but not reported

APPENDIX G

Overview of Leadership Rewards Analyses

Location	Analytical Method	Dependent Variable	SE Clustering	Sample	H1	H3
Table 2, Model 1	Tobit	Total Points	Coach, Year	Full		N/A
Table 2, Model 2	Tobit	2nd Place Points	Coach, Year	Full	p<.05	N/A
Table 2, Model 3	Tobit	Total Points	Coach, Year	CEM (primary)		N/A
Table 2, Model 4	Tobit	2nd Place Points	Coach, Year	CEM (primary)	p<.10	N/A
Table 2, Model 5	Tobit	Total Points	Coach, Year	CEM (primary)		p<.10
Table 2, Model 6	Tobit	2nd Place Points	Coach, Year	CEM (primary)		
Robustness Test	Tobit	Total Points	Coach	Full		N/A
Robustness Test	Tobit	2nd Place Points	Coach	Full	p=.103	N/A
Robustness Test	Tobit	Total Points	Coach	CEM (primary)		N/A
Robustness Test	Tobit	2nd Place Points	Coach	CEM (primary)	p<.10	N/A
Robustness Test	Tobit	Total Points	Coach	CEM (primary)		p<.10
Robustness Test	Tobit	2nd Place Points	Coach	CEM (primary)		
Appendix C, Model 1	IV Tobit, 2-Step	Total Points		Full		N/A
Appendix C, Model 2	IV Tobit, MLE	Total Points	Coach	Full		N/A
Appendix C, Model 3	IV Tobit, 2-Step	2nd Place Points		Full	p<.10	N/A
Appendix C, Model 4	IV Tobit, MLE	2nd Place Points	Coach	Full	p<.10	N/A
Appendix C, Model 5	IV Tobit, 2-Step	Total Points		CEM (primary)		N/A
Appendix C, Model 6	IV Tobit, MLE	Total Points	Coach	CEM (primary)		N/A
Appendix C, Model 7	IV Tobit, 2-Step	2nd Place Points		CEM (primary)	p=.113	N/A
Appendix C, Model 8	IV Tobit, MLE	2nd Place Points	Coach	CEM (primary)	p<.10	N/A
Appendix D, Model 1	IV Tobit, 2-Step	Total Points		CEM (primary)		
Appendix D, Model 2	IV Tobit, MLE	Total Points	Coach	CEM (primary)		
Appendix D, Model 3	IV Tobit, 2-Step	2nd Place Points		CEM (primary)		
Appendix D, Model 4	IV Tobit, MLE	2nd Place Points	Coach	CEM (primary)		
Robustness Test	Tobit	Total Points	Coach, Year	CEM (alt spec)		N/A
Robustness Test	Tobit	2nd Place Points	Coach, Year	CEM (alt spec)	p<.01	N/A
Robustness Test	Tobit	Total Points	Coach, Year	CEM (alt spec)		p=.124
Robustness Test	Tobit	2nd Place Points	Coach, Year	CEM (alt spec)	p=.125	
Robustness Test	Tobit	Total Points	Coach, Year	PSM		N/A
Robustness Test	Tobit	2nd Place Points	Coach, Year	PSM		N/A
Robustness Test	Tobit	Total Points	Coach, Year	PSM		
Robustness Test	Tobit	2nd Place Points	Coach, Year	PSM		

APPENDIX H

Overview of Employment Separation Analyses

Location	Analytical Method	SE Clustering	Sample (Matching)	Sample (Tenure)	H2	H4
Table 3, Model 1	LPM	Coach, Year	Full	Full		N/A
Table 3, Model 2	LPM	Coach, Year	Full	Tenure > 2	p<.05	N/A
Table 3, Model 3	LPM	Coach, Year	CEM (primary)	Full		N/A
Table 3, Model 4	LPM	Coach, Year	CEM (primary)	Tenure > 2	p<.01	N/A
Table 3, Model 5	LPM	Coach, Year	CEM (primary)	Full		
Table 3, Model 6	LPM	Coach, Year	CEM (primary)	Tenure > 2	p<.05	p<.01
Robustness Test	LPM	Coach	Full	Full		N/A
Robustness Test	LPM	Coach	Full	Tenure > 2	p<.05	N/A
Robustness Test	LPM	Coach	CEM (primary)	Full		N/A
Robustness Test	LPM	Coach	CEM (primary)	Tenure > 2	p<.001	N/A
Robustness Test	LPM	Coach	CEM (primary)	Full		
Robustness Test	LPM	Coach	CEM (primary)	Tenure > 2	p<.01	p<.05
Appendix E, Model 1	IV Reg (2SLS)	Coach, Year	Full	Full		N/A
Appendix E, Model 2	IV Reg (2SLS)	Coach	Full	Full		N/A
Appendix E, Model 3	IV Reg (2SLS)	Coach, Year	Full	Tenure > 2	p=10.2	N/A
Appendix E, Model 4	IV Reg (2SLS)	Coach	Full	Tenure > 2	p=.114	N/A
Appendix E, Model 5	IV Reg (2SLS)	Coach, Year	CEM (primary)	Full		N/A
Appendix E, Model 6	IV Reg (2SLS)	Coach	CEM (primary)	Full		N/A
Appendix E, Model 7	IV Reg (2SLS)	Coach, Year	CEM (primary)	Tenure > 2	p<.05	N/A
Appendix E, Model 8	IV Reg (2SLS)	Coach	CEM (primary)	Tenure > 2	p<.01	N/A
Appendix F, Model 1	IV Reg (2SLS)	Coach, Year	CEM (primary)	Full		p<.10
Appendix F, Model 2	IV Reg (2SLS)	Coach	CEM (primary)	Full		p=10.1
Appendix F, Model 3	IV Reg (2SLS)	Coach, Year	CEM (primary)	Tenure > 2	p<.05	
Appendix F, Model 4	IV Reg (2SLS)	Coach	CEM (primary)	Tenure > 2	p<.01	
Robustness Test	LPM	Coach, Year	CEM (alt spec)	Full		N/A
Robustness Test	LPM	Coach, Year	CEM (alt spec)	Tenure > 2		N/A
Robustness Test	LPM	Coach, Year	CEM (alt spec)	Full		p<.001
Robustness Test	LPM	Coach, Year	CEM (alt spec)	Tenure > 2		p<.10
Robustness Test	LPM	Coach, Year	PSM	Full		N/A
Robustness Test	LPM	Coach, Year	PSM	Tenure > 2	p<.05	N/A
Robustness Test	LPM	Coach, Year	PSM	Full		opp direct. p<.01
Robustness Test	LPM	Coach, Year	PSM	Tenure > 2	p<.05	opp direct. p<.01
Robustness Test	Probit	Coach, Year	Full	Full		N/A
Robustness Test	Probit	Coach, Year	Full	Tenure > 2	p<.05	N/A
Robustness Test	Probit	Coach, Year	CEM (primary)	Full		N/A
Robustness Test	Probit	Coach, Year	CEM (primary)	Tenure > 2	p<.001	N/A
Robustness Test	Probit	Coach, Year	CEM (primary)	Full		
Robustness Test	Probit	Coach, Year	CEM (primary)	Tenure > 2	p<.01	p<.05
Appendix G	Fixed Effects	Coach	Full	Full		N/A
Appendix G	Fixed Effects	Coach	Full	Full		p<.10 & opp direct. p<.05
Appendix G	Fixed Effects	Coach	Full	Tenure > 2		N/A
Appendix G	Fixed Effects	Coach	Full	Tenure > 2		p<.05 & opp direct. p<.001
Appendix H	Fixed Effects	Team	Full	Full		N/A
Appendix H	Fixed Effects	Team	Full	Full	p<.10	p<.01 & opp direct. p<.01
Appendix H	Fixed Effects	Team	Full	Tenure > 2	p<.001	N/A
Appendix H	Fixed Effects	Team	Full	Tenure > 2		p<.05 & opp direct. p<.01