

The Labor Hub

Discussion Paper No. 12

DISCRIMINATION WITHOUT WAGE GAPS? RACIAL PREFERENCES AND WORKPLACE SEGREGATION IN A VIRTUAL LABOR MARKET

**Manuel Arrigo, Carlos Gomez-Gonzalez,
Alfredo Fantetti, and Markus Lang**

Discrimination Without Wage Gaps? Racial Preferences and Workplace Segregation in a Virtual Labor Market^{*}

Manuel Arrigo^{1†}, Carlos Gomez-Gonzalez¹, Alfredo Fantetti², & Markus Lang¹

¹ Faculty of Social and Political Sciences, University of Lausanne

² Sports Analytics S.r.l.

Abstract

We formalize and test an implication of Becker's (1957) model of employer taste-based discrimination: racial preferences may remain visible in workplace segregation even when no racial wage gap is observed. We leverage more than 3.4 million observations from 15,000 naturally occurring virtual labor markets with real incentives and rich productivity information, where worker-side sorting is absent, institutional heterogeneity across firms is limited, and alternative discrimination channels are restricted. We find no evidence of price discrimination: Black and Non-Black workers with similar productivity are hired at statistically indistinguishable prices. Yet workplace segregation systematically exceeds race-neutral permutation benchmarks, consistent with heterogeneous employer tastes. This segregation pattern persists across years and market sizes despite price equality, while firms hiring relatively fewer Black workers do not face a profit or performance penalty. We discuss how such preferences may sustain discrimination even under transparent information and strong competitive incentives.

Keywords

Taste-based discrimination; Wage differentials; Worker–firm sorting; Racial segregation; Market competition.

JEL classification

J71, J31, J15, D91

^{*} We thank Ceren Bengü Çıbık, Gwen-Jiro Clochard, Jan Feld, Rustamdjan Hakimov, Rafael Lalive, Thomas Peeters, Francesco Principe, Jan van Ours, and Christian Zehnder for valuable feedback on earlier drafts of this article.

[†] Corresponding author. Quartier UNIL-Centre, Bâtiment Synathlon, CH-1015 Lausanne, Switzerland.
manuel.arrigo@unil.ch

1. Introduction

Discrimination, defined as unequal treatment of individuals based on group characteristics, can alter how opportunities and resources are allocated and generate persistent inequalities. A large empirical literature documents racial discrimination in labor, housing, credit, and consumer markets (Bertrand & Duflo, 2017; Pager & Shepherd, 2008), with evidence of persistence over time (Heath & Di Stasio, 2019; Quillian et al., 2017). Yet understanding how discrimination operates and shapes market outcomes remains an empirical challenge, as different mechanisms can produce similar patterns of unequal treatment (Bertrand & Duflo, 2017).

Our paper tests an implication of Becker's (1957) model of employer taste-based discrimination that has received relatively little empirical attention. In Becker's framework, racial animus acts as an additional perceived cost of hiring a Black worker: prejudiced employers are willing to pay more for Non-Black workers to avoid cross-racial contact. Black workers are therefore hired by less prejudiced employers, while more prejudiced employers avoid them. In equilibrium, the wage gap depends on the prejudice of the marginal discriminator—the most prejudiced employer who still hires Black workers—rather than on average prejudice in the market. Charles and Guryan (2008) provide empirical support for this prediction, showing that Black–White wage gaps track prejudice at this margin. If sufficiently many employers are willing to hire Black workers at the same wage as Non-Black workers, the wage gap can disappear even though other employers remain prejudiced. Yet less prejudiced employers will still hire disproportionately more Black workers, generating racial segregation across firms. Becker's model therefore implies that wage equality need not signal the absence of discriminatory preferences: they may instead remain visible in workplace segregation.

Hiring discrimination varies substantially across employers (Kline et al., 2022; Kline & Walters, 2021), and workplace segregation is well documented (e.g., Hellerstein & Neumark, 2008). However, segregation can arise for many reasons besides employer racial preferences. Differences in skills and productivity can generate sorting even without discrimination (Hellerstein & Neumark, 2008; Kremer & Maskin, 1996; Lopes de Melo, 2018). Workplace characteristics and recruitment patterns are also associated with segregation (Bygren, 2013; Carrington & Troske, 1998). Furthermore, workers' choices (Plug et al., 2014) and social networks (Hellerstein et al., 2011) can also shape where they work. An empirical test of the implication that employer racial preferences can remain visible in workplace segregation even when no racial wage gap is observed therefore requires a market setting with no influence of

workers' preferences, rich information on worker productivity, limited institutional heterogeneity across firms, and little scope for alternative discrimination mechanisms. Such conditions are rare in conventional labor markets but can arise in virtual market settings.

We identify such a setting in a naturally occurring virtual labor market in which individuals form small groups and hire workers through open ascending-bid auctions. Workers in this setting are professional football players whom participants virtually hire, with returns determined by the players' real-world performance. This environment closely matches the conditions required for our test. First, workers have no influence over the hiring process and cannot sort across employers based on their own preferences. Second, virtual employers observe rich information on workers' past performance and expected productivity. Each worker is employed for one year, making his realized output observable. Third, employers operate under common rules and budget constraints within each market, limiting institutional heterogeneity across firms. Fourth, virtual hiring does not affect workers' real-world interactions and involves no customers, closing co-worker and customer discrimination channels that can shape workforce composition in conventional labor markets (Ahmadi et al., 2025; Combes et al., 2016; Giuliano et al., 2009).

At the same time, this virtual labor market provides strong incentives to make accurate hiring decisions. Participants compete against their peers over an entire season, often with monetary stakes. Previous studies have used similar structured platforms with real incentives to study discrimination (Alrababah et al., 2026; Antonovics et al., 2005; Bryson & Chevalier, 2015). We use proprietary data that cover 15,000 virtual markets of different sizes over three years and more than 3.4 million player-market observations. The setting therefore shares some features of laboratory studies, such as restrictions on several alternative mechanisms and behavioral transparency, while also offering real incentives and large-scale naturally occurring data typical of field settings. Taken together, these features make the setting unusually well suited to testing the predicted coexistence of wage equality and workplace segregation, satisfying the external-validity litmus test proposed by List (2020).

Our paper makes two main contributions. First, we contribute to the economics of discrimination by deriving a closed-form expression for workplace segregation under wage equality in Becker's (1957) model and empirically testing the resulting prediction. Although the underlying sorting intuition is implicit in the model, we are not aware of prior work using this allocation prediction to construct such a test. Our framework links wages, worker allocation, and firm profit, showing that wage equality is not sufficient evidence of preference neutrality, since allocation can reveal racial preferences that wages alone do not. We test this

prediction in a setting that substantially restricts several major alternative sources of segregation.

Second, we contribute to the literature on the mechanisms underlying discrimination. Two canonical economic theories have traditionally structured this literature: taste-based discrimination, which underpins our analysis, and statistical discrimination. Statistical discrimination arises when employers face imperfect information about individual productivity and use group characteristics to infer worker quality (Arrow, 1973; Phelps, 1972). Unlike taste-based discrimination, this mechanism does not require racial animus. List (2004), for instance, shows in a sports-card market that discriminatory treatment can arise from statistical inference under incomplete information. Richer individual-level information has similarly been shown to reduce ethnic disparities in digital marketplaces (Cui et al., 2020; Laouénan & Rathelot, 2022). In addition to these two accounts, Bertrand et al. (2005) argue that discrimination may also operate implicitly, without conscious animus or awareness of one's behavior, such as implicit associations affecting hiring decisions (Rooth, 2010). Furthermore, discrimination may also reflect inaccurate beliefs about group characteristics (Bohren, Haggag, et al., 2025; Lang & Spitzer, 2020), whether or not decision-makers are aware of them. The relative importance of these mechanisms remains difficult to establish empirically (Guryan & Charles, 2013; Lippens et al., 2022; Neumark, 2018). In our setting, employers observe rich individual-level information on workers' performance, substantially reducing the scope for statistical discrimination. The persistence of racial differences under these market conditions and strong competitive incentives would be difficult to reconcile with statistical discrimination alone.

Empirically, we proceed in three steps. First, we test whether Black and Non-Black workers with similar productivity profiles command different prices, using a regression framework with rich controls and fixed effects. Second, we test whether workplace segregation exceeds what would be expected under race-neutral allocation. We construct this benchmark through a permutation-based Monte Carlo procedure that randomly reshuffles racial labels among workers in the same market and position while preserving the observed market structure, similar in spirit to reshuffling approaches used in the segregation literature (e.g., Åslund & Nordström Skans, 2009; Carrington & Troske, 1998; Hellerstein & Neumark, 2008). Excess workplace segregation relative to this benchmark indicates that some employers systematically select relatively more Black workers while others select fewer, consistent with heterogeneity in racial preferences. Third, we test whether firms with different racial compositions subsequently differ in realized performance after accounting for ex ante differences in worker productivity.

Our results reveal a sharp contrast between prices and allocation. Across all specifications and measures of expected productivity, we find no statistically significant difference in prices between Black and Non-Black workers. Under Becker’s model, this absence of a racial price gap is consistent with an unprejudiced marginal discriminator. Yet employers are not indifferent to workers’ racial appearance. The observed cross-firm variance in the share of Black workers exceeds the race-neutral benchmark by approximately 19% on average. The pattern is stable across all years and similar across market sizes, and it persists under increasingly restrictive reshuffling procedures that account for observable differences in worker profiles. A placebo test based on an irrelevant worker characteristic does not produce such a pattern. Finally, firms hiring relatively fewer Black workers do not subsequently perform worse, consistent with the model’s prediction that preference-driven sorting need not carry a profit penalty. Taken together, these results are consistent with the sorting mechanism implied by Becker’s (1957) model: a zero racial price gap can coexist with racial preferences that remain visible in workplace segregation, even under transparent information and strong competitive incentives.

The remainder of the paper is structured as follows. Section 2 develops the theoretical framework and derives implications for wages, workplace segregation, and firm performance. Section 3 describes the virtual labor market and the proprietary data. Section 4 presents the empirical strategy. Section 5 reports the main results and robustness checks. Section 6 concludes.

2. Theoretical framework

2.1 Baseline model: employer taste-based discrimination

Our analysis builds on the canonical employer taste-based discrimination model of Becker (1957). Consider a labor market populated by a continuum of employers of measure one and a continuum of workers. Workers are of two types: Non-Black (type a) and Black (type b). Let $\theta \in (0, 1)$ denote the exogenously given share of Black workers in the total labor supply, so that the shares of Non-Black and Black workers are $1 - \theta$ and θ . Workers of both types are perfect substitutes in production and are equally productive, with a common marginal product p . Each employer hires a workforce of normalized size one.

Employers are heterogeneous in their racial attitudes. Employer i has a prejudice parameter $d_i \geq 0$, which represents the psychic disutility associated with employing a Black worker and reflects a taste for discrimination in Becker’s sense. The distribution of prejudice across

employers is given by the cumulative distribution function $F(d)$ defined on $[0, \bar{d}]$, where $\bar{d} > 0$. A key structural feature of this distribution is that a non-negligible mass of employers is entirely unprejudiced. Formally:

Assumption 1 (Prejudice distribution). The distribution $F(d)$ has an atom of mass $\lambda \in (0, 1)$ at $d_i = 0$, so that $F(0) = \lambda$. For $d > 0$, F is continuous and strictly increasing.

Thus, λ is the fraction of unprejudiced employers, while the remaining $1 - \lambda$ have strictly positive prejudice. Figure A1 in Appendix A.1 illustrates $F(d)$ for $\lambda = 0.4$ and $\theta = 0.2$. Following Becker (1957), employer utility is defined in terms of profits and the racial composition of the workforce. Let w_a and w_b denote the wages of Non-Black and Black workers. A prejudiced employer with $d_i > 0$ behaves as if the wage of a Black worker were $w_b + d_i$, and therefore strictly prefers a Black worker if and only if $w_b + d_i < w_a$. This generates a sorting mechanism: for any positive wage gap $w_a - w_b > 0$, there exists a threshold $d = w_a - w_b$ such that employers with $d_i < d$ hire Black workers, those with $d_i > d$ prefer Non-Black workers, and those with $d_i = d$ are indifferent. In equilibrium, this threshold is denoted by d^* and identifies the marginal discriminator. Market clearing implies $d^* = F^{-1}(\theta)$, where $F^{-1}(\theta)$ denotes the lowest prejudice level at which enough employers are willing to hire all Black workers. When $d^* > 0$, continuity implies $F(d^*) = \theta$; when $d^* = 0$, only the required share of unprejudiced employers hires Black workers. Hence, the equilibrium wage gap is

$$w_a^* - w_b^* = d^* = F^{-1}(\theta) \quad (1)$$

This is the central result of Becker's model: the equilibrium wage differential equals the prejudice of the marginal discriminator, determined by the position of the supply of Black workers in the distribution of employer prejudice. A full derivation of the baseline equilibrium, including the employer utility and profit functions, is provided in Appendix A.1.

2.2 Equilibrium without a wage gap

A central implication of Becker's framework concerns the conditions under which the equilibrium wage gap vanishes entirely, even with widespread employer prejudice. The identity of the marginal discriminator depends on the relative magnitudes of θ and λ . If the supply of Black workers is sufficiently small relative to the mass of unprejudiced employers, the market can be cleared by employers with $d_i = 0$, making the marginal discriminator unprejudiced. Formally:

Assumption 2 (Sufficient mass of unprejudiced employers). The share of Black workers in the labor supply does not exceed the mass of unprejudiced employers: $\theta \leq \lambda$.

Under Assumption 2, the equilibrium threshold is $d^* = F^{-1}(\theta) = 0$, since $F(0) = \lambda \geq \theta$. The following proposition characterizes the resulting equilibrium.

Proposition 1 (Zero wage gap). Under Assumptions 1 and 2, the competitive equilibrium features $w_a^* = w_b^* \equiv p$. Black workers are employed exclusively by employers with $d_i = 0$, that is, by unprejudiced employers. Prejudiced employers hire only Non-Black workers.

The economic intuition is straightforward. Competition among unprejudiced employers for Black workers drives up Black wages to the point where no further wage concessions are needed to place all Black workers. The marginal discriminator has $d^* = 0$ and therefore demands no compensating differential. Crucially, some or all prejudiced employers may still operate in equilibrium and employ only Non-Black workers, but their preferences are inconsequential for wage determination. Wage equality between racial groups thus does not require the absence of prejudice; it requires only that the supply of Black workers can be absorbed by unprejudiced employers. The formal proof is provided in Appendix A.2.

2.3 Workplace segregation

While Proposition 1 establishes that wage gaps may be absent even when employer prejudice is widespread, the same equilibrium can still generate workplace segregation. The implication that wage equality does not imply race-neutral worker allocation has received comparatively little attention. Under Proposition 1, unprejudiced employers ($d_i = 0$) hire all Black workers, while prejudiced employers ($d_i > 0$) hire only Non-Black workers. Among otherwise identical unprejudiced employers, we focus on the symmetric equilibrium allocation in which Black labor is distributed evenly across firms. Since all θ Black workers are allocated across the mass λ of unprejudiced employers, the share of Black workers is θ/λ in each unprejudiced firm and zero in each prejudiced firm. This allocation is the sharpest form of sorting in the baseline model. If workers differ in productivity or employers value individual workers differently, a prejudiced employer may still hire a Black worker whose additional value to the employer exceeds d_i . The zero wage gap in Proposition 1 is preserved as long as the employers who set prices are unprejudiced. We therefore allow unprejudiced and prejudiced employers to have Black worker shares s_u^b and $s_p^b < s_u^b$, with the baseline allocation as the special case $s_p^b = 0$.

We measure the resulting workplace segregation through the cross-firm variance in the share of Black workers, $\text{Var}(s_i^b)$. Under race-neutral allocation, each employer has a share of

Black workers equal to θ , so $\text{Var}_r(s^b) = 0$. Under preference-driven sorting, racial composition differs systematically across firms. Proposition 2 characterizes the resulting segregation.

Proposition 2 (Workplace segregation). Consider a zero-wage-gap allocation under Assumptions 1 and 2, with symmetric racial composition within employer types. Let unprejudiced employers have a Black worker share s_u^b and prejudiced employers a share $s_p^b < s_u^b$, and let $D \equiv s_u^b - s_p^b$. Market clearing, $\lambda s_u^b + (1 - \lambda)s_p^b = \theta$, implies $s_u^b = \theta + (1 - \lambda)D$ and $s_p^b = \theta - \lambda D$, with $0 < D \leq \theta/\lambda$. Preference-driven sorting generates positive workplace segregation despite the absence of a wage gap: $\text{Var}_m(s^b) - \text{Var}_r(s^b) = \lambda(1 - \lambda)D^2 > 0$. Segregation is maximal when prejudiced employers hire no Black workers ($s_p^b = 0$, $D = \theta/\lambda$), in which case $\text{Var}_m(s^b) - \text{Var}_r(s^b) = \theta^2(1 - \lambda)/\lambda$.

The variance has a direct interpretation as a measure of workplace segregation. With equally sized firms, as in the model, normalizing $\text{Var}(s^b)$ by $\theta(1 - \theta)$ yields the standard co-worker segregation measure: the difference between the average Black coworker share faced by Black and Non-Black workers. Thus, the variance provides a tractable representation of workplace segregation rather than a separate concept. The full derivation is provided in Appendix A.3.

The comparative statics are economically intuitive. For a given λ , segregation increases with the gap D between the two employer types. In the baseline case in which prejudiced employers hire no Black workers, segregation increases with θ , because more workers must be concentrated among unprejudiced employers, and decreases with λ , as Black workers spread more evenly across a larger set of firms. In the limit $\lambda \rightarrow 1$, prejudice disappears, and allocation becomes effectively race-neutral, so segregation vanishes. In this baseline case, as $\lambda \rightarrow \theta$ (the knife-edge case where Assumption 2 binds), unprejudiced firms employ only Black workers while prejudiced firms employ only Non-Black workers, producing complete segregation.

Because a prejudiced employer can at most avoid Black workers entirely, a given share of prejudiced employers can generate only a limited amount of segregation. Observed excess segregation therefore implies a minimum share of prejudiced employers. The proof is provided in Appendix A.3.

Corollary 1 (Share of prejudiced employers). Under the conditions of Proposition 2, an excess variance $E \equiv \text{Var}_m(s^b) - \text{Var}_r(s^b) > 0$ implies $1 - \lambda \geq E/(\theta^2 + E)$. For a given λ , the gap between the Black worker shares of the two employer types is $D = [E/(\lambda(1 - \lambda))]^{1/2}$.

2.4 Sorting and firm performance

The preference-driven sorting characterized above need not generate differences in firm

performance or profits. Let Y_i denote the output of employer i . In the baseline model, Black and Non-Black workers are equally productive and perfect substitutes in production. Since each employer has a workforce of normalized size one, $L_{ai} + L_{bi} = 1$. Firm output is therefore $Y_i = f(L_{ai} + L_{bi}) = f(1)$. Profits are $\pi_i = f(1) - w_a L_{ai} - w_b L_{bi}$. Using $L_{ai} + L_{bi} = 1$ and $w_a - w_b = d^*$, profits can be written as $\pi_i = f(1) - w_a + s_{bi}^b d^*$, so they increase with the share of Black workers at rate d^* .

Proposition 3 (No profit penalty). In the baseline model, under Assumptions 1 and 2, firm output and profits are independent of workforce racial composition: $Y_i = f(1)$ and $\pi_i = f(1) - p$ for all i . More generally, profits increase with the share of Black workers at rate d^* , the prejudice of the marginal discriminator, so employing fewer Black workers is costly only when the wage gap is positive.

Proposition 3 links the wage gap and the marginal profit cost of discrimination: both equal d^* . When $\theta > \lambda$, $d^* > 0$ and employers who hire fewer Black workers earn lower profits, so competition on profits can select against prejudiced employers (Arrow, 1972). Under Assumption 2, this selection pressure is absent, and prejudiced employers act on their preferences at no monetary cost. Employer prejudice may therefore generate workplace segregation even when it produces neither a racial wage gap nor a profit or performance penalty. A formal proof is provided in Appendix A.4.

2.5 Empirical implications

Propositions 1–3 motivate three empirical tests. First, we test whether Black and Non-Black workers with similar observable productivity command different prices. A zero conditional price gap is consistent with $d^* = 0$ but does not imply that other employers are unprejudiced.

Second, we test whether workplace segregation exceeds a race-neutral benchmark. Because finite markets generate cross-firm variation even under random assignment, we compare observed cross-firm variance with its race-neutral distribution rather than zero. By Corollary 1, the magnitude of excess segregation also bounds the share of prejudiced employers from below.

Third, we test whether firms with different racial compositions differ in realized performance, both at a given budget and after accounting for ex ante worker productivity.

Together, prices are informative about the marginal discriminator, workplace segregation reveals preference heterogeneity away from the margin and realized performance tests whether such segregation carries a profit or performance penalty.

3. Institutional setting and data

3.1 Institutional setting

Our virtual labor market setting is based on Italian fantasy football. An estimated 6 million people participate, highlighting its widespread popularity and relevance in Italy.¹

To participate, individuals must join a private league of 6–14 participants and create a team. Each participant receives the same fictional budget and acquires a roster of about 25 professional football players competing in Italy’s top division, Serie A. Player allocation takes place around the start of the season, typically in August or early September, through open ascending-bid auctions that start at one unit of the fictional budget per player. Participants nominate football players sequentially, and the platform does not prescribe the nomination order. Each player is assigned to the highest bidder before the next player is nominated. Within a league, each player can be acquired by at most one team. Competitive bidding therefore jointly determines prices and team assignments within each league, which constitutes a separate market and is referred to as such throughout the paper. Our analysis focuses exclusively on this initial auction.

This market structure distinguishes our setting from other similar platforms. In Fantasy Premier League, for example, the platform sets player prices and participants independently assemble their teams subject to a budget constraint, allowing the same player to appear on multiple teams (Bryson & Chevalier, 2015). The auction format in our setting instead creates a within-market allocation problem in which participants compete for mutually exclusive workers. This market dimension is essential for studying prices and worker–firm allocation jointly, as required by the predictions developed in Section 2.

After the auction, participants compete over the Serie A season. For each matchday, they select a starting lineup from their roster, with football players’ real-world performances generating points according to predetermined rules. Appendix B describes the scoring system in greater detail. Roster adjustments after the initial auction are typically limited to trades between participants, giving initial hiring decisions substantial season-long consequences. Some leagues also offer monetary rewards based on final standings, creating direct performance incentives. Even without such rewards, the game unfolds over an entire season and takes place among peers, creating strong incentives to bid carefully.

¹ Source: Milano Finanza, September 7, 2023 ([link](#)). This estimate has not been independently verified.

Following Bryson and Chevalier (2015), we interpret the initial auction as a virtual labor market. Participants act as employers recruiting workers (football players) based on their expected productivity (virtual points). The auction price serves as the empirical analog of a wage, reflecting participants' valuations of expected productivity. Participants therefore resemble firm managers allocating a fixed wage budget across workers with heterogeneous abilities.

We examine whether perceived race affects hiring decisions. Because workers are professional players, their appearance is publicly known, and most participants are likely familiar with many of the players in the market. Moreover, the platforms often display player avatars that retain visible phenotypic cues, including skin tone, making players' racial appearance salient. For less-known players or when photographs are not salient at the moment of bidding, players' names may provide additional racial or ethnic cues, as documented in hiring settings (Bertrand & Mullainathan, 2004).

This setting offers several advantages for identifying employer racial preferences. Employers observe rich information about past performance and expected productivity, and the production function (i.e., the scoring rules) is known in advance. This substantially reduces uncertainty about individual productivity, limiting the scope for statistical discrimination, which arises when employers use group characteristics to infer individual productivity under imperfect information (e.g., Lepage, 2024). Furthermore, customer and co-worker discrimination channels are absent within the virtual market. In other settings, decision-makers may discriminate in response to anticipated customer or co-worker preferences (Ahmadi et al., 2025; Gomez-Gonzalez et al., 2026; Rooth, 2009). In this virtual market, workers have no direct customers and do not interact with their virtual co-workers.² Moreover, workers cannot sort across employers according to their own preferences, for example, by avoiding prejudiced workplaces (Plug et al., 2014). This restriction is especially important for our segregation test. Finally, employers operate under common rules and identical budget constraints, and compete directly against one another within each market, limiting institutional heterogeneity across firms.

² Discrimination by coaches, referees, teammates, or spectators may still affect football players' observed productivity. If such effects are stable across seasons, however, they are already reflected in the lagged performance information available to participants. Our estimates can therefore capture differential treatment within the virtual labor market conditional on the productivity signals observed by participants, without requiring us to observe the actual discrimination experienced by players in professional football.

3.2 Data and variables

Our sample comprises 5,000 virtual leagues in each of the 2023/24, 2024/25, and 2025/26 seasons. We treat each league observed in a given season as a separate market, yielding 15,000 markets and 3,440,593 player-market observations. Sports Analytics S.r.l.³ randomly selected the auctions within each year and provided the data for research purposes. Each observation records a player's auction price and team assignment within a market. At the player-season level, the sample contains 1,784 observations for 1,000 unique football players. Markets are indexed by the year of the initial auction; for example, 2023 corresponds to the 2023/24 season.

Information on participants was limited, similar to many labor-market settings where worker characteristics are observed more precisely than employer characteristics. Because the platform operates in Italian, participants likely reside in Italy or other Italian-speaking regions.

Our outcome variable, *Budget Share*, measures the share of the initial team budget spent on a player.⁴ We used its logarithm because the distribution was strongly right-skewed. Figure 1 shows that players with better previous-season performance command larger budget shares, confirming that auction prices respond to expected productivity, as in standard labor markets.

We measured perceived race from player photographs on Transfermarkt, following previous work (e.g., Principe & van Ours, 2022). Three independent coders classified each player as Black or Non-Black, and majority rule determined the final classification. The sample contains 738 unique Non-Black and 262 unique Black players. Across auction years, the mean share of Black players on team rosters ranges from approximately 22% to 25%, with substantial variation across teams (within-year SDs of 8.5–9.0 percentage points). We validated the human classification against FairFace (Kärkkäinen & Joo, 2021), a machine-learning model for facial attribute classification.⁵ The algorithm assigns each individual to one of four or seven racial categories, depending on the taxonomy. We classified a player as Black when FairFace assigned him to the Black category under either taxonomy. Agreement was high both across human coders and between the human classification and FairFace (Appendix Table C1). These checks reduce concerns about classification error; we nevertheless recognize that racial categories are socially constructed and context-dependent.

³ Sports Analytics Srl is a high-tech startup providing sports data insights to customers and companies by leveraging AI. They provided the auction information used in this study.

⁴ Each league may choose different initial budgets. We therefore use budget shares rather than absolute auction prices to ensure comparability across markets.

⁵ See Kärkkäinen and Joo (2021) for details on the FairFace algorithm and its validation.

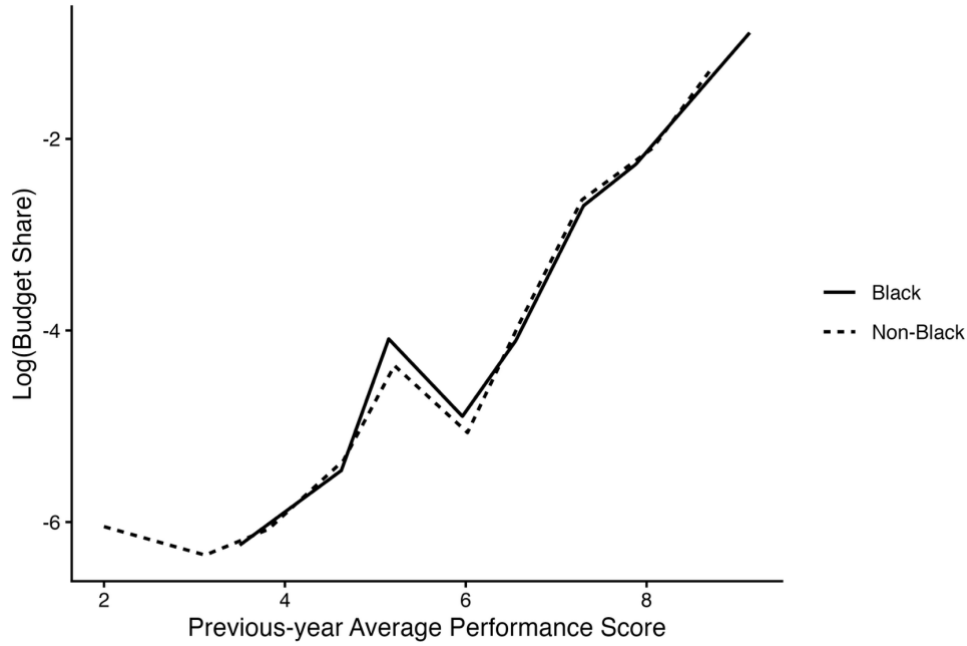


Figure 1. Better-performing players command higher auction prices.

Notes: Players are grouped into ten common equal-width intervals of previous-year Average Performance Score. The lines plot mean log Budget Share against mean performance within each interval, separately for Black and Non-Black players. Observations are at the player-market level. The local increase around 5.5 reflects top-performing goalkeepers, who cluster in this range and command higher prices because of positional scarcity.

We measured expected productivity in three alternative ways: using the platform's valuation, previous-season performance measures, or performance measures from the previous three seasons. *Listed Value* is the platform's expert-based assessment of player value, updated weekly. It combines past performance with information not easily summarized by historical statistics, such as managerial changes or off-field developments. *Average Performance Score* equals a player's total virtual points divided by the number of rated matches. We combined it with *Rated Matches* because player availability increases value to participants, who must field a full lineup of 11 players every week. We constructed these performance measures using both the previous season and the previous three seasons, capturing short- and longer-run productivity signals. For each look-back period, we set lagged Average Performance Score and Rated Matches to zero in the regressions for players without Serie A observations and identified them with a *Not in Serie A* indicator, consistent with previous research (Bryson & Chevalier, 2015).

Additional controls included *Continental Cup*, *Age*, *Age Squared*, and *Italian*. Continental Cup identifies players expected to miss Serie A matches because of participation in the Africa Cup of Nations or AFC Asian Cup. Italian equals one for players whose primary citizenship on Transfermarkt is Italian and captures potential in-group preferences. Table 1 reports

descriptive statistics separately for Black and Non-Black players, while Appendix Table C2 reports overall descriptive statistics.

Table 1

Descriptive statistics.

	Black		Non-Black	
	Mean	SD	Mean	SD
Budget Share	0.04	0.08	0.04	0.06
Listed Value	12.54	7.26	11.70	6.65
Average Performance Score (1-year)	6.39	0.75	6.25	0.71
Rated Matches (1-year)	25.97	8.78	27.16	8.87
Not in Serie A (1-year)	0.27	0.45	0.21	0.40
Average Performance Score (3-year)	6.42	0.69	6.24	0.70
Rated Matches (3-year)	25.74	7.10	25.86	8.18
Not in Serie A (3-year)	0.25	0.44	0.15	0.36
Age	25.80	3.41	26.84	4.08
Italian	0.04	0.21	0.43	0.49
Continental Cup	0.15	0.36	0.01	0.09
Goalkeeper	0.05	0.22	0.14	0.35
Defender	0.35	0.48	0.31	0.46
Midfielder	0.28	0.45	0.33	0.47
Forward	0.32	0.47	0.21	0.41
Number of observations	829,109		2,611,484	

Notes: Observations are at the player-market level. Statistics for lagged performance measures use available observations only.

4. Empirical strategy

4.1 Testing price discrimination

We first tested whether Black and Non-Black players with similar expected productivity received different auction prices. The auction price served as the empirical analog of the wage

in the model. We estimated:

$$\log(\text{BudgetShare}_{jsm}) = \beta \text{Black}_j + \gamma_1 \text{Productivity}_{js} + \gamma_2 Z_{js} + \delta_p + \delta_t + \delta_m + \varepsilon_{jsm} \quad (2)$$

BudgetShare_{jsm} denotes the share of the total budget spent on player j in season s in market m . Black_j equals one for players classified as Black. Productivity_{js} contains alternative measures of expected productivity, while Z_{js} contains the controls Age, Age Squared, Italian, and Continental Cup. The performance-history specifications additionally include the Not in Serie A indicator for the corresponding look-back period. All price specifications included position fixed effects (δ_p), club fixed effects (δ_t), and market fixed effects (δ_m). Standard errors were two-way clustered by player-season and market.

In addition to a baseline with fixed effects only, we estimated three specifications using alternative productivity measures and baseline controls. The first used Average Performance Score and Rated Matches from the previous season, together with the Not in Serie A (1-year) indicator. The second used the corresponding measures over the previous three seasons. The third used Listed Value, the platform’s assessment of expected player quality.

The coefficient β measured the conditional racial price gap. A coefficient statistically indistinguishable from zero indicated no detectable price difference conditional on observed productivity and player characteristics and, in the model, was consistent with an unprejudiced marginal discriminator.

4.2 Testing workplace segregation

We next turned to workplace segregation. Following Proposition 2, we measured segregation using the cross-firm variance in the share of Black players. Because finite rosters generate cross-firm variation even under random assignment, the observed variance alone does not identify systematic segregation. We therefore constructed a race-neutral benchmark using a permutation-based Monte Carlo procedure (also referred to as a re-randomization or shuffle test) (e.g., Ritzwoller et al., 2026), in line with random-allocation approaches in the workplace-segregation literature (e.g., Åslund & Nordström Skans, 2009; Carrington & Troske, 1998; Hellerstein & Neumark, 2008). Within each market and position, we randomly reassigned racial labels across players while leaving team assignments unchanged.⁶ Each permutation therefore generated a counterfactual version of the same market in which we randomly assigned race within position, while the players, team rosters, positional structure, and overall

⁶ For example, if a market contains 25 Black defenders and 60 Black midfielders, every permutation preserves those exact counts while randomly reassigning racial labels within each position. The same constraint applies separately to every position in every market.

racial composition of the market remained unchanged. We generated $B = 200$ counterfactual versions of each observed market and recomputed the cross-firm variance in Black player shares in each one. For market m , we compared the observed variance with the distribution generated by its 200 counterfactual allocations and calculated the Monte Carlo p-value as:

$$p_m = \frac{1 + \sum_{b=1}^B \mathbb{1}(T_m^{(b)} \geq T_m^{obs})}{B+1} \quad (3)$$

T_m^{obs} denotes the observed variance in market m , while $T_m^{(b)}$ denotes the corresponding variance in counterfactual allocation b . Small p -values indicated that observed workplace segregation was unusually high relative to the race-neutral benchmark. Because ties are counted as exceedances, the test is conservative. This approach allowed us to test for deviations from race-neutral allocation without imposing parametric assumptions on the data-generating process or functional form.

We then tested whether statistically significant segregation occurred more frequently across markets than expected under the null. Let K denote the number of markets with $p_m < 0.05$ among N total markets. Using 5% as the nominal rejection benchmark, we conducted the one-sided test:

$$H_0: \Pr(p_m < 0.05) = 0.05 \quad \text{vs} \quad H_1: \Pr(p_m < 0.05) > 0.05 \quad (4)$$

We evaluated K against a $\text{Binomial}(N, 0.05)$ benchmark, assuming independent market-level rejection decisions. We measured the magnitude of excess segregation as the percentage by which mean observed variance exceeded mean simulated variance. A rejection of the null indicated systematic workplace segregation beyond that generated by race-neutral allocation; under the model, such a pattern was consistent with heterogeneous employer preferences.

4.3 Testing realized performance

Finally, we tested whether racial composition predicted subsequent team performance after accounting for ex ante player productivity. Team performance denotes how well participants perform in the virtual market. We measured *Team Performance* as the average realized contribution of players on the roster, with each player's contribution calculated as his Average Performance Score multiplied by his number of Rated Matches.

We regressed Team Performance on the share of Black players in the team, expressed in percentage points. We used three alternative measures of ex ante productivity parallel to the price analysis. The first used the roster mean of each player's previous-season Average Performance Score $\times$ Rated Matches (*Average 1-year productivity*), together with the share of

players with Not in Serie A (1-year) = 1. The second used the corresponding measures over the previous three seasons (*Average 3-year productivity* and *Share not in Serie A (3-year)*). The third used *Average Listed Value*. The controlled specifications also included the same baseline player characteristics as in the price analysis, aggregated to team averages or shares, together with market fixed effects.

We estimated the regressions on player-team observations using inverse roster-size weights, giving each team equal total weight. This produced the same coefficients as team-level regressions while allowing two-way clustering by player-season and market to account for players appearing in multiple markets. The coefficient on the Black player share measured the conditional association between racial composition and subsequent team performance. A coefficient statistically indistinguishable from zero was consistent with Proposition 3: the segregation generated by preference-driven sorting need not carry a performance penalty. Because all teams in a market have the same budget, comparing realized performance within markets (market fixed effects only) compares output at equal cost, the empirical counterpart of profits in Proposition 3.

5. Results

5.1 Price discrimination

We first examined whether prices differed between Black and Non-Black players conditional on expected productivity (Table 2). Across all specifications, the Black coefficient is statistically indistinguishable from zero. Once expected productivity and other observed player characteristics are included, the point estimates are also close to zero. In columns (2)–(4), the estimated Black coefficients are 0.012 ($p = 0.89$) using previous-year performance, -0.042 ($p = 0.64$) using previous-three-year performance, and 0.010 ($p = 0.86$) using Listed Value. The absence of a detectable price penalty thus persists across different measures of expected productivity and look-back periods. These productivity measures are themselves strongly associated with auction prices. Average Performance Score and Rated Matches have positive and statistically significant coefficients using both one- and three-year histories, while Listed Value is also strongly predictive of prices. Participants' valuations thus respond systematically to observable productivity signals. Appendix Table D1 reports confidence intervals and one-sided upper bounds on potential Black price penalties across the alternative productivity specifications.

Table 2

No detectable Black price gap conditional on productivity.

	(1)	(2)	(3)	(4)
Black	-0.082 (0.086)	0.012 (0.089)	-0.042 (0.091)	0.010 (0.056)
Average Performance Score (1-year)		0.837*** (0.076)		
Rated Matches (1-year)		0.042*** (0.004)		
Not in Serie A (1-year)		6.060*** (0.469)		
Average Performance Score (3-year)			0.922*** (0.086)	
Rated Matches (3-year)			0.047*** (0.005)	
Not in Serie A (3-year)			6.690*** (0.527)	
Listed Value				0.187*** (0.005)
Age		0.242* (0.097)	0.141 (0.096)	-0.158** (0.054)
Age Squared		-0.004* (0.002)	-0.003 (0.002)	0.003** (0.001)
Italian		-0.020 (0.081)	-0.016 (0.080)	0.041 (0.047)
Continental Cup		-0.299* (0.146)	-0.178 (0.154)	-0.305* (0.126)
Position fixed effects	YES	YES	YES	YES
Club fixed effects	YES	YES	YES	YES
Market fixed effects	YES	YES	YES	YES
Observations	3,440,593	3,440,593	3,440,593	3,440,593
Adjusted R ²	0.2091	0.3381	0.3340	0.6014

Notes: The dependent variable is the log of Budget Share (auction price divided by the initial team budget). Standard errors, two-way clustered by player-season and market, are reported in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

We further examined whether the pooled result masks differences across the player-value distribution. We divided players into Listed Value deciles within each market and position and re-estimated the price specification separately for each decile. Appendix Figure D1 reports the estimated Black coefficients and their 95% confidence intervals. The coefficient remains statistically indistinguishable from zero in every decile, and we find no detectable adverse price gap among either lower- or higher-valued players. The absence of a detectable penalty therefore extends beyond the pooled estimate to individual player-value segments.

Taken together, these findings are consistent with the zero-gap equilibrium in Proposition 1. In that equilibrium, unprejudiced employers can absorb the supply of Black workers, leaving an unprejudiced marginal discriminator ($d^* = 0$). Black players account for 24.1% of player selections in the sample. For a market with this composition, the model's condition $\lambda \geq \theta$ would

require at least roughly one-quarter of employers to be unprejudiced. In this equilibrium, racial preferences among other employers can still shape worker allocation without generating a price penalty.

5.2 Workplace segregation

We next tested for workplace segregation. Figure 2 displays the distribution of permutation-based Monte Carlo p -values across markets. Under race-neutral allocation, this distribution should be approximately uniform. Instead, the figure shows a clear excess mass at low p -values, indicating systematic departures from the race-neutral benchmark.

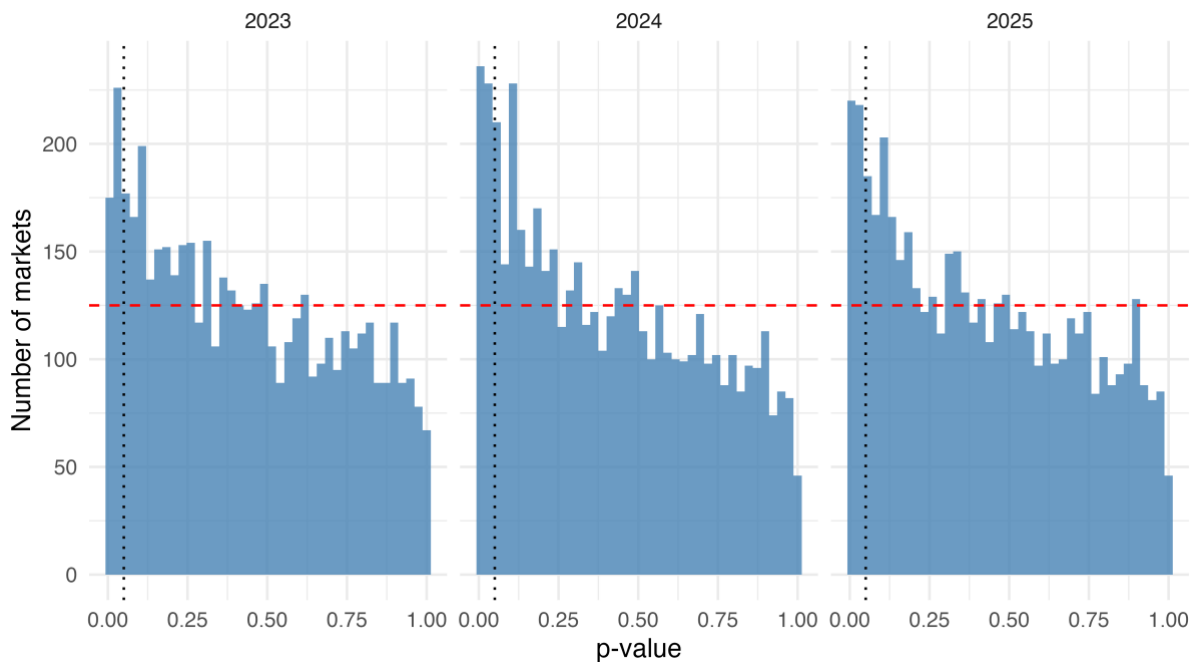


Figure 2. Permutation p -values indicate systematic workplace segregation.

Notes: Panels show market-level permutation p -values by auction year. The dotted vertical line marks $p = 0.05$; the dashed horizontal line shows the expected bin frequency under a uniform distribution.

Table 3 quantifies this pattern. The share of markets with p -values below 5% is consistently around 10% across auction years, approximately twice the level expected under race-neutral allocation. The binomial tests strongly reject the null in every auction year (all $p < .001$). Economically, the excess variance means that Black players are distributed more unevenly across teams than under race-neutral allocation: some teams employ relatively more Black players, while others employ relatively fewer. This systematic allocation pattern is consistent with the preference-driven sorting generated by heterogeneous racial preferences in the model.

In terms of magnitude, the observed cross-firm variance in the share of Black players exceeds its race-neutral benchmark by about 19.1% on average. This corresponds to about 0.24

standard deviations of the cross-market variation in the segregation measure. Appendix D shows that the conclusion is virtually unchanged when using the co-worker segregation measure commonly employed in the workplace-segregation literature. With this measure, the Black–Non-Black gap in exposure to Black co-workers exceeds its race-neutral benchmark by 0.68 percentage points (Appendix Table D2). The findings are also similar when separating markets with equal and unequal team sizes (Appendix Table D3).

Interpreted through Proposition 2, these magnitudes are informative about the composition of employers. With an excess variance of 13.7 squared percentage points ($85.4 - 71.7$; Table 3) and $\theta = 24.1\%$, applying Corollary 1 yields an illustrative, model-based lower bound of 2.3% on the share of prejudiced employers. This lower bound corresponds to prejudiced employers hiring no Black players. The same excess variance is also consistent with a larger share of employers with milder preferences: for example, half of all employers hiring Black players at a rate about 7 percentage points lower than other employers.⁷

Table 3

Observed workplace segregation exceeds the race-neutral benchmark.

Auction year	Observed variance	Simulated variance	Share of markets with $p < 0.05$	Binomial p -value	Excess variance (%)
2023	77.5	66.9	9.5%	$< .001$	15.9%
2024	89.8	73.8	10.9%	$< .001$	21.8%
2025	88.8	74.4	10.2%	$< .001$	19.3%
All	85.4	71.7	10.2%	$< .001$	19.1%

Notes: Reported variances are averages across markets of the within-market variance in Black-player shares, expressed in squared percentage points. Binomial p -values test whether the share of markets with permutation $p < 0.05$ exceeds 5%.

5.3 Racial composition and realized performance

We further examined whether teams’ racial composition is associated with subsequent realized performance. Proposition 3 implies that, under the zero-gap equilibrium, preference-driven sorting need not generate performance differences once worker productivity is held fixed. Table 4 reports the results.

⁷ Setting $\lambda = 0.5$ in Corollary 1 gives $D = \sqrt{[13.7/(0.5 \times 0.5)]} \approx 7.4$ percentage points.

Table 4

No performance penalty associated with racial composition, conditional on ex ante productivity.

	(1)	(2)	(3)	(4)
Black player share (%)	-0.121* (0.054)	-0.060 (0.063)	-0.069 (0.063)	-0.079 (0.064)
Average 1-year productivity		0.199*** (0.050)		
Share not in Serie A (1-year)		0.146 (0.100)		
Average 3-year productivity			0.234*** (0.057)	
Share not in Serie A (3-year)			0.185 (0.107)	
Average Listed Value				1.042* (0.531)
Average Age		4.572 (6.593)	3.684 (6.751)	10.208 (6.626)
Average Age Squared		-0.128 (0.121)	-0.120 (0.123)	-0.219 (0.121)
Italian share		0.043 (0.056)	0.044 (0.056)	0.089 (0.056)
Continental Cup share		-0.218* (0.094)	-0.195* (0.094)	-0.207* (0.094)
Market fixed effects	YES	YES	YES	YES
Player–team observations	3,440,593	3,440,593	3,440,593	3,440,593
Number of teams	136,348	136,348	136,348	136,348
Adjusted R ² (team level)	0.1524	0.2134	0.2130	0.1809

Notes: Team performance is the roster mean of realized player productivity, measured as Average Performance Score $\times$ Rated Matches. Controls are team means or shares; shares are measured in percentage points. Regressions use player-team observations with inverse roster-size weights, giving each team equal weight. Standard errors are two-way clustered by player-season and market. Adjusted R² is from the equivalent team-level regression. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Without additional controls, a one-percentage-point higher share of Black players is associated with 0.121 fewer realized points. Once we account for differences in ex ante player composition, however, the coefficient becomes substantially smaller in absolute magnitude and is statistically insignificant in columns (2)–(4). We therefore find no evidence of systematic differences in subsequent performance between teams with different racial compositions. Because all teams in a market face the same budget constraint, column (1) of Table 4 also offers a market-test perspective (Szymanski, 2000): for a given budget, teams with more Black players do not outperform, which is inconsistent with Black players being underpriced. In terms of Proposition 3, employers who hire fewer Black players incur no profit penalty: at a given budget, their teams do not perform worse within markets.

These findings complete the joint pattern predicted by the theoretical framework. Together, the price, allocation, and performance results are consistent with the model's zero-gap equilibrium. When unprejudiced employers can absorb the supply of Black workers, the marginal discriminator is unprejudiced, so racial preferences do not generate a price gap. Preferences among other employers can nevertheless shape whom they hire, producing workplace segregation without a detectable performance penalty conditional on ex ante productivity.

5.4 Market size

The baseline model is formulated for a continuum of employers and therefore does not assign a direct role to the absolute number of employers. Its central condition is instead compositional: the equilibrium wage gap vanishes when the share of Black workers, θ , does not exceed the share of unprejudiced employers, λ . Holding these two parameters fixed, the condition $\theta \leq \lambda$ is unaffected by the number of employers operating in the market. The model therefore does not predict that larger markets should necessarily display smaller conditional price gaps.

The same reasoning applies to allocation outcomes. Proposition 2 characterizes excess cross-firm variance as $\lambda(1 - \lambda)D^2$, which depends on the composition of employer types and the gap in their Black worker shares, but not on the absolute number of employers. Thus, provided that θ , λ , and D are broadly stable across market sizes, the model predicts that the same joint pattern should emerge in both relatively thin and relatively thick markets.

A distinctive feature of our data is that markets vary substantially in the number of participants, ranging from 6 to 14 participants. We therefore re-estimated the price specification separately by market size and repeated the allocation analysis using a race-neutral benchmark constructed separately within each size category (Table 5).

Across all market sizes, we continue to find no evidence of price discrimination conditional on productivity. The estimated Black coefficients remain close to zero and statistically insignificant, with no systematic relationship between the conditional price gap and market size. At the same time, workplace segregation remains stable: observed cross-firm variance exceeds the corresponding race-neutral benchmark by approximately 16–20% in every market-size category.⁸

⁸ The category with 14 participants contains only 29 markets. Its point estimates remain closely aligned with the other market sizes: 10.3% of markets have p-values below 0.05, and excess variance equals 18.5%, although the binomial test is imprecise.

Table 5

The price–segregation pattern is stable across market sizes.

Market size (teams)	Markets	Price regressions		Segregation statistics				
		Black coefficient	Coefficient <i>p</i> - value	Observed variance	Simulated variance	Markets with $p < 0.05$ (%)	Binomial <i>p</i> -value	Excess variance (%)
6	419	0.053 (0.055)	0.341	84.6	71.5	7.9%	0.007	18.2%
8	6,816	0.005 (0.060)	0.934	86.2	71.9	10.1%	< .001	19.9%
10	6,966	0.013 (0.057)	0.816	84.9	71.6	10.5%	< .001	18.6%
12	770	0.008 (0.052)	0.884	83.1	71.1	9.5%	< .001	16.8%
14	29	0.015 (0.065)	0.815	83.3	70.3	10.3%	0.175	18.5%

Notes: Results pool all auction years within each market size. Price regressions use the specification in Table 2, column (4), with the same controls, fixed effects, and clustering; standard errors are in parentheses. Segregation statistics follow Table 3.

The same joint pattern is already visible in markets with only six participants: even with relatively few employers, we observe no detectable conditional price gap alongside excess workplace segregation. Its stability across market sizes indicates that this pattern is not confined to thick markets, consistent with the mechanism characterized by the model.

5.5 Robustness checks

As a robustness check, we first implemented a placebo test replacing the Black indicator with an arbitrary characteristic—i.e., whether the day of birth was odd or even—and repeated the permutation-based analysis. Figure 3 shows the distribution of the resulting placebo *p*-values across markets. In contrast to the baseline results, the distribution appears close to uniform, with no systematic excess mass at low values. This indicates that the procedure does not mechanically generate excess cross-firm variance in the absence of a meaningful sorting dimension. Appendix Table D4.1 confirms that the share of significant markets remains close to 5%, excess variance is approximately zero, and the binomial tests do not reject the null of a 5% rejection rate.

As an additional placebo test, we randomly reassigned Black/Non-Black labels within each market and position and compared the resulting artificial allocation with the 200 permutations used in the main test. This procedure preserved the observed number of Black players in every cell and kept team assignments fixed. Again, the results show no evidence of excess segregation (Appendix Table D4.2 and Appendix Figure D2).

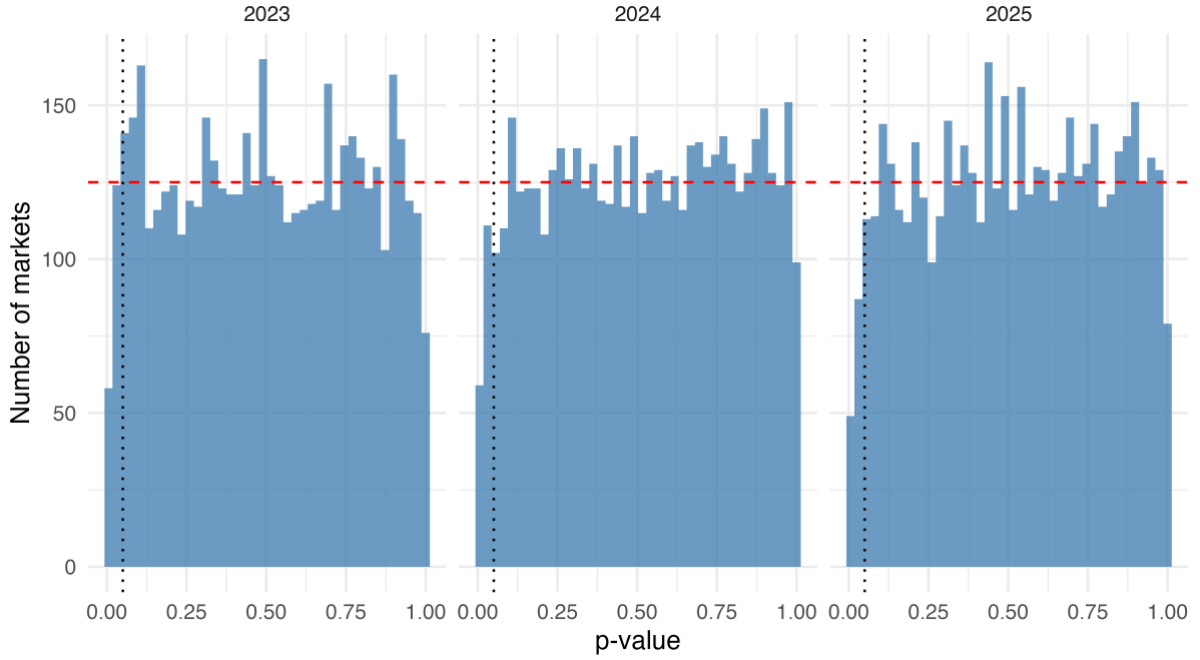


Figure 3. The placebo test shows no excess segregation.

Notes: The procedure and reference lines are as in Figure 2, replacing perceived race with an indicator for an odd day of birth.

We next repeated the permutation analysis under more restrictive benchmarks. We separately imposed additional constraints to assess whether the observed excess variance could be explained by factors correlated with the Black indicator. First, we restricted permutations within Serie A clubs to account for club-specific selection patterns. Second, we controlled for expected performance by constructing bins based on Listed Value, ensuring that permutations occurred only among players of comparable expected quality. Third, we introduced a nationality constraint by permuting only within Italian and non-Italian players. Finally, to account for heterogeneous preferences over player profiles, we restricted permutations within age quartiles crossed with Not in Serie A (1-year) status. Appendix Table D5 shows that the results remain stable. More than 9% of markets have p -values below 5% under every specification, all binomial tests reject the null ($p < .001$), and excess variance ranges from 16.3% to 18.3%, compared with 19.1% in the baseline. The results therefore remain stable when the race-neutral benchmark accounts separately for these observable player characteristics.

Finally, we used conditional reshuffling to account jointly for observable player characteristics. Adapting the approach of Åslund and Nordström Skans (2009), we first estimated each player's probability of being Black using the productivity measures and other player characteristics reported in Appendix Table D6 (McFadden pseudo- $R^2 = 0.26$; AUC =

0.83). Within each market $\times$ position cell, we sampled players without replacement using predicted probabilities as weights. We fixed the sample size at the observed number of Black players and assigned Black status to the selected players. This creates a substantially more demanding benchmark than unconditional reshuffling, because the counterfactual allocation preserves the tendency of Black players to have particular observable characteristics.

Table 6 shows that the main result survives this benchmark. The share of markets with p -values below 5% remains above the 5% rate expected under race-neutral allocation (8.6%), and the binomial tests continue to strongly reject the null (all $p < .001$). Excess variance falls from 19.1% to 14.5% but remains economically meaningful, corresponding to approximately 0.19 standard deviations. Observable player characteristics therefore explain part of the baseline segregation, but they do not account for it. The persistence of excess variance under a benchmark that explicitly incorporates the strong correlation between race and observable player profiles makes it difficult to attribute the main result solely to participants sorting on those observables. Combined with the restrictions on worker-side sorting, institutional heterogeneity across firms, and customer or co-worker discrimination, these results are consistent with heterogeneous racial preferences as the source of the remaining workplace segregation.

Table 6

Excess segregation persists under conditional reshuffling.

Auction year	Simulated mean variance	Share of markets with $p < 0.05$	Binomial p -value	Excess variance (%)
2023	69.7	7.8%	$< .001$	11.2%
2024	76.7	9.3%	$< .001$	17.1%
2025	77.2	8.7%	$< .001$	15.0%
All	74.5	8.6%	$< .001$	14.5%

Notes: The benchmark averages 200 reshufflings within market $\times$ position, using sampling without replacement weighted by the predicted Black-status probabilities from Table D6. Each reshuffling preserves the observed number of Black players in each cell. Observed variance is unchanged from Table 3; remaining statistics follow its definitions.

6. Discussion and conclusion

We formalized and tested an underexamined implication of Becker’s (1957) model of employer taste-based discrimination. Using more than 3.4 million observations from 15,000 naturally occurring virtual labor markets, we found no systematic price difference between Black and

Non-Black workers conditional on productivity. Yet workplace segregation exceeded a race-neutral permutation benchmark by approximately 19% on average, equivalent to about 0.24 standard deviations of the observed cross-market variation, pointing to heterogeneous racial preferences across employers. This pattern is consistent with Becker's mechanism: when unprejudiced employers are sufficiently numerous relative to the supply of Black workers, competition among them can bid up Black workers' wages until the racial wage gap disappears, leaving an unprejudiced employer at the margin. Other employers may nevertheless continue to act on racial preferences, generating systematic sorting across firms. We found no detectable profit penalty associated with this sorting: employers hiring relatively fewer Black workers did not subsequently perform worse after accounting for *ex ante* worker productivity. Taken together, our findings suggest that the absence of a detectable price gap is not sufficient to rule out racial discrimination. Racial preferences may still shape worker allocation and be reflected in workplace segregation even when neither a price gap nor a performance penalty is observed.

Charles and Guryan (2008) showed that racial wage gaps are more closely related to the prejudice of the marginal discriminator than to average prejudice in the market. A separate literature has documented substantial workplace segregation and deviations from race-neutral allocation (Hellerstein & Neumark, 2008). The empirical challenge has been to connect these findings and show that employer racial preferences can shape worker allocation even when no racial wage gap is observed. Our virtual labor market provides a setting for this test: workers have no influence over where they are hired, employers observe rich information about expected productivity, firms operate under common rules and identical budget constraints, and customer or co-worker discrimination channels are absent.

The findings suggest that wage equality should not be interpreted as the absence of discrimination. Settings in which racial wage gaps largely disappear (Deschamps & De Sousa, 2021; Neal & Johnson, 1996) may still conceal a substantial share of employers with racial preferences, consistent with documented heterogeneity in hiring discrimination (Kline et al., 2022; Kline & Walters, 2021). Such preferences can have discriminatory consequences in several ways. First, discriminatory workplace sorting can contribute to what recent work defines as systemic discrimination (Bohren, Hull, & Imas, 2025). If workplaces differ in the resources and opportunities they provide, segregation can generate differences in subsequent skill accumulation and productivity. These differences may then translate into inequalities in later employment, even when subsequent employers pay workers equally conditional on their observed productivity. Second, because workers face uncertainty about employers' preferences *ex ante*, minority workers may incur additional search costs when they encounter prejudiced

employers before reaching an unprejudiced match (Black, 1995). They can therefore face individual discriminatory episodes and costs that are not fully reflected in aggregate wage gaps. Third, racial preferences may affect workers after hiring through dimensions such as evaluation and recognition (Rim et al., 2024). These channels imply that wage equality may mask economically meaningful forms of unequal treatment.

The absence of a profit penalty also speaks to a classic criticism of Becker's model. Arrow (1972) argued that, under perfect competition, prejudiced employers should eventually be driven out of the market because they forgo cheaper Black labor and earn lower profits. Subsequent work has shown that prejudice can persist under imperfect information, imperfect competition, adjustment costs, or alternative formulations of discriminatory preferences (Black, 1995; Charles & Guryan, 2007; Goldberg, 1982; Lang et al., 2005). Related empirical work examines the economic costs of discrimination by relating workforce composition to firm performance, profitability, or survival (Hellerstein et al., 2002; Szymanski, 2000; Weber & Zulehner, 2014). Our results point to a complementary case: when the marginal discriminator is unprejudiced, other employers may act on racial preferences without an observable profit or performance penalty, even under rich information and strong competition. Such preferences may therefore be harder to detect and eradicate.

In our setting, employers can consult readily available individual performance information, substantially reducing the scope for statistical discrimination. Our results are therefore difficult to reconcile with statistical discrimination alone and instead point to a role for racial preferences, consistent with experimental evidence of costly taste-based discrimination (Hedegaard & Tyran, 2018). Moreover, unlike in traditional workplaces, employers do not physically interact with workers, so the segregation we detect may represent a lower bound on the behavioral expression of such preferences (Combes et al., 2016). This distance is also compatible with preferences operating implicitly, without conscious animus or awareness (Bertrand et al., 2005; Price & Wolfers, 2010). From a policy perspective, improving transparency about workers' performance alone may therefore be insufficient, whereas interventions that increase awareness of racial bias or promote cross-group exposure may prove more effective (Mousa, 2020; Pope et al., 2018).

Our data leave some dimensions of these preferences unresolved. The segregation test identifies non-neutral racial sorting, but not its direction: excess segregation may reflect avoidance of Black workers, favoritism toward Non-Black workers, preferences in the opposite direction among some employers, or a combination of these forces. These mechanisms may have different welfare and long-run implications (Feld et al., 2016; Salamanca & Feld, 2017),

but do not alter the central result that racial preferences can remain visible in worker allocation without a racial price gap. Limited information on participants also prevents us from relating racial sorting to employer characteristics or local racial composition, which is associated with hiring discrimination (Agan & Starr, 2024). Finally, because we observe final prices and allocations rather than bid sequences, we cannot distinguish whether racial preferences affect the extensive margin of bidding for a player or the intensive margin of bidding aggressively. Future work could separate these mechanisms more directly.

Funding

This research received no external funding.

Acknowledgments

The authors used ChatGPT to improve the language and readability of the manuscript. The authors reviewed and edited the text and take full responsibility for its content.

References

- Agan, A., & Starr, S. (2024). Employers' neighborhoods and racial discrimination. *The Journal of Legal Studies*, 53(1), 115–158. <https://doi.org/10.1086/728242>
- Ahmadi, M., Clochard, G.-J., Lachman, J., & List, J. A. (2025). *Toward an understanding of discrimination when multiple channels exist* (Working Paper No. 33391). National Bureau of Economic Research. <https://doi.org/10.3386/w33391>
- Alrababah, A., Marble, W., Mousa, S., & Siegel, A. A. (2026). *Bad days, not bad groups: Parasocial contact, performance, and prejudice in the English Premier League* [Unpublished manuscript]. https://static1.squarespace.com/static/59a360bacd0f681b1cb27caa/t/6a1f328bf79ba0524d0e841c/1780429451548/fantasy_paper_science-15.pdf
- Antonovics, K., Arcidiacono, P., & Walsh, R. (2005). Games and discrimination: Lessons from The Weakest Link. *Journal of Human Resources*, 40(4), 918–947. <https://doi.org/10.3368/jhr.xl.4.918>
- Arrow, K. J. (1972). Some mathematical models of race discrimination in the labor market. In A. H. Pascal (Ed.), *Racial discrimination in economic life* (pp. 187–204). Lexington Books.
- Arrow, K. J. (1973). The theory of discrimination. In O. Ashenfelter & A. Rees (Eds.), *Discrimination in labor markets* (pp. 3–33). Princeton University Press.
- Åslund, O., & Nordström Skans, O. (2009). How to measure segregation conditional on the distribution of covariates. *Journal of Population Economics*, 22(4), 971–981. <https://doi.org/10.1007/s00148-008-0189-4>
- Becker, G. S. (1957). *The economics of discrimination*. University of Chicago Press.
- Bertrand, M., Chugh, D., & Mullainathan, S. (2005). Implicit discrimination. *American Economic Review*, 95(2), 94–98. <https://doi.org/10.1257/000282805774670365>
- Bertrand, M., & Duflo, E. (2017). Field experiments on discrimination. In A. V. Banerjee & E. Duflo (Eds.), *Handbook of economic field experiments* (Vol. 1, pp. 309–393). North-Holland. <https://doi.org/10.1016/bs.hefe.2016.08.004>
- Bertrand, M., & Mullainathan, S. (2004). Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination. *American Economic Review*, 94(4), 991–1013. <https://doi.org/10.1257/0002828042002561>
- Black, D. A. (1995). Discrimination in an equilibrium search model. *Journal of Labor Economics*, 13(2), 309–334. <https://doi.org/10.1086/298376>

- Bohren, J. A., Haggag, K., Imas, A., & Pope, D. G. (2025). Inaccurate statistical discrimination: An identification problem. *Review of Economics and Statistics*, 107(3), 605–620. https://doi.org/10.1162/rest_a_01367
- Bohren, J. A., Hull, P., & Imas, A. (2025). Systemic discrimination: Theory and measurement. *The Quarterly Journal of Economics*, 140(3), 1743–1799. <https://doi.org/10.1093/qje/qjaf022>
- Bryson, A., & Chevalier, A. (2015). Is there a taste for racial discrimination amongst employers? *Labour Economics*, 34, 51–63. <https://doi.org/10.1016/j.labeco.2015.03.002>
- Bygren, M. (2013). Unpacking the causes of segregation across workplaces. *Acta Sociologica*, 56(1), 3–19. <https://doi.org/10.1177/0001699312468802>
- Carrington, W. J., & Troske, K. R. (1998). Interfirm segregation and the Black/White wage gap. *Journal of Labor Economics*, 16(2), 231–260. <https://doi.org/10.1086/209888>
- Charles, K. K., & Guryan, J. (2007). *Prejudice and the economics of discrimination* (Working Paper No. 13661). National Bureau of Economic Research. <https://doi.org/10.3386/w13661>
- Charles, K. K., & Guryan, J. (2008). Prejudice and wages: An empirical assessment of Becker's The economics of discrimination. *Journal of Political Economy*, 116(5), 773–809. <https://doi.org/10.1086/593073>
- Combes, P.-P., Decreuse, B., Laouénan, M., & Trannoy, A. (2016). Customer discrimination and employment outcomes: Theory and evidence from the French labor market. *Journal of Labor Economics*, 34(1), 107–160. <https://doi.org/10.1086/682332>
- Cui, R., Li, J., & Zhang, D. J. (2020). Reducing discrimination with reviews in the sharing economy: Evidence from field experiments on Airbnb. *Management Science*, 66(3), 1071–1094. <https://doi.org/10.1287/mnsc.2018.3273>
- Deschamps, P., & De Sousa, J. (2021). Labor mobility and racial discrimination. *European Economic Review*, 135, Article 103738. <https://doi.org/10.1016/j.euroecorev.2021.103738>
- Feld, J., Salamanca, N., & Hamermesh, D. S. (2016). Endophilia or exophobia: Beyond discrimination. *The Economic Journal*, 126(594), 1503–1527. <https://doi.org/10.1111/eoj.12289>
- Giuliano, L., Levine, D. I., & Leonard, J. (2009). Manager race and the race of new hires. *Journal of Labor Economics*, 27(4), 589–631. <https://doi.org/10.1086/605946>

- Goldberg, M. S. (1982). Discrimination, nepotism, and long-run wage differentials. *The Quarterly Journal of Economics*, 97(2), 307–319. <https://doi.org/10.2307/1880760>
- Gomez-Gonzalez, C., Clochard, G.-J., Dietl, H., & Duhalde, J. C. (2026). Migration and sources of discrimination in a social context: Experimental evidence from 15 Latin American countries. *Journal of Development Economics*, 179, Article 103633. <https://doi.org/10.1016/j.jdevco.2025.103633>
- Guryan, J., & Charles, K. K. (2013). Taste-based or statistical discrimination: The economics of discrimination returns to its roots. *The Economic Journal*, 123(572), F417–F432. <https://doi.org/10.1111/eoj.12080>
- Heath, A. F., & Di Stasio, V. (2019). Racial discrimination in Britain, 1969–2017: A meta-analysis of field experiments on racial discrimination in the British labour market. *The British Journal of Sociology*, 70(5), 1774–1798. <https://doi.org/10.1111/1468-4446.12676>
- Hedegaard, M. S., & Tyran, J.-R. (2018). The price of prejudice. *American Economic Journal: Applied Economics*, 10(1), 40–63. <https://doi.org/10.1257/app.20150241>
- Hellerstein, J. K., McInerney, M., & Neumark, D. (2011). Neighbors and coworkers: The importance of residential labor market networks. *Journal of Labor Economics*, 29(4), 659–695. <https://doi.org/10.1086/660776>
- Hellerstein, J. K., & Neumark, D. (2008). Workplace segregation in the United States: Race, ethnicity, and skill. *Review of Economics and Statistics*, 90(3), 459–477. <https://doi.org/10.1162/rest.90.3.459>
- Hellerstein, J. K., Neumark, D., & Troske, K. R. (2002). Market forces and sex discrimination. *Journal of Human Resources*, 37(2), 353–380. <https://doi.org/10.2307/3069651>
- Kärkkäinen, K., & Joo, J. (2021). FairFace: Face attribute dataset for balanced race, gender, and age for bias measurement and mitigation. *Proceedings of the IEEE/CVF Winter Conference on Applications of Computer Vision*, 1548–1558. <https://doi.org/10.1109/WACV48630.2021.00159>
- Kline, P., Rose, E. K., & Walters, C. R. (2022). Systemic discrimination among large U.S. employers. *The Quarterly Journal of Economics*, 137(4), 1963–2036. <https://doi.org/10.1093/qje/qjac024>
- Kline, P., & Walters, C. (2021). Reasonable doubt: Experimental detection of job-level employment discrimination. *Econometrica*, 89(2), 765–792. <https://doi.org/10.3982/ECTA17489>

- Kremer, M., & Maskin, E. (1996). *Wage inequality and segregation by skill* (Working Paper No. 5718). National Bureau of Economic Research. <https://doi.org/10.3386/w5718>
- Lang, K., Manove, M., & Dickens, W. T. (2005). Racial discrimination in labor markets with posted wage offers. *American Economic Review*, 95(4), 1327–1340. <https://doi.org/10.1257/0002828054825547>
- Lang, K., & Spitzer, A. K.-L. (2020). Race discrimination: An economic perspective. *Journal of Economic Perspectives*, 34(2), 68–89. <https://doi.org/10.1257/jep.34.2.68>
- Laouénan, M., & Rathelot, R. (2022). Can information reduce ethnic discrimination? Evidence from Airbnb. *American Economic Journal: Applied Economics*, 14(1), 107–132. <https://doi.org/10.1257/app.20190188>
- Lepage, L.-P. (2024). Experience-based discrimination. *American Economic Journal: Applied Economics*, 16(4), 288–321. <https://doi.org/10.1257/app.20220466>
- Lippens, L., Baert, S., Ghekiere, A., Verhaeghe, P.-P., & Deros, E. (2022). Is labour market discrimination against ethnic minorities better explained by taste or statistics? A systematic review of the empirical evidence. *Journal of Ethnic and Migration Studies*, 48(17), 4243–4276. <https://doi.org/10.1080/1369183X.2022.2050191>
- List, J. A. (2004). The nature and extent of discrimination in the marketplace: Evidence from the field. *The Quarterly Journal of Economics*, 119(1), 49–89. <https://doi.org/10.1162/003355304772839524>
- List, J. A. (2020). *Non est disputandum de generalizability? A glimpse into the external validity trial* (Working Paper No. 27535). National Bureau of Economic Research. <https://doi.org/10.3386/w27535>
- Lopes de Melo, R. (2018). Firm wage differentials and labor market sorting: Reconciling theory and evidence. *Journal of Political Economy*, 126(1), 313–346. <https://doi.org/10.1086/695505>
- Mousa, S. (2020). Building social cohesion between Christians and Muslims through soccer in post-ISIS Iraq. *Science*, 369(6505), 866–870. <https://doi.org/10.1126/science.abb3153>
- Neal, D. A., & Johnson, W. R. (1996). The role of premarket factors in Black-White wage differences. *Journal of Political Economy*, 104(5), 869–895. <https://doi.org/10.1086/262045>
- Neumark, D. (2018). Experimental research on labor market discrimination. *Journal of Economic Literature*, 56(3), 799–866. <https://doi.org/10.1257/jel.20161309>

- Pager, D., & Shepherd, H. (2008). The sociology of discrimination: Racial discrimination in employment, housing, credit, and consumer markets. *Annual Review of Sociology*, 34, 181–209. <https://doi.org/10.1146/annurev.soc.33.040406.131740>
- Phelps, E. S. (1972). The statistical theory of racism and sexism. *American Economic Review*, 62(4), 659–661.
- Plug, E., Webbink, D., & Martin, N. (2014). Sexual orientation, prejudice, and segregation. *Journal of Labor Economics*, 32(1), 123–159. <https://doi.org/10.1086/673315>
- Pope, D. G., Price, J., & Wolfers, J. (2018). Awareness reduces racial bias. *Management Science*, 64(11), 4988–4995. <https://doi.org/10.1287/mnsc.2017.2901>
- Price, J., & Wolfers, J. (2010). Racial discrimination among NBA referees. *The Quarterly Journal of Economics*, 125(4), 1859–1887. <https://doi.org/10.1162/qjec.2010.125.4.1859>
- Principe, F., & van Ours, J. C. (2022). Racial bias in newspaper ratings of professional football players. *European Economic Review*, 141, Article 103980. <https://doi.org/10.1016/j.eurocorev.2021.103980>
- Quillian, L., Pager, D., Hexel, O., & Midtbøen, A. H. (2017). Meta-analysis of field experiments shows no change in racial discrimination in hiring over time. *Proceedings of the National Academy of Sciences*, 114(41), 10870–10875. <https://doi.org/10.1073/pnas.1706255114>
- Rim, N., Rivera, R., Kiss, A., & Ba, B. (2024). The Black-White recognition gap in award nominations. *Journal of Labor Economics*, 42(1), 1–23. <https://doi.org/10.1086/722412>
- Ritzwoller, D. M., Romano, J. P., & Shaikh, A. M. (2026). Randomization inference: Theory and applications. *Journal of Political Economy Microeconomics*. <https://doi.org/10.1086/743457>
- Rooth, D.-O. (2009). Obesity, attractiveness, and differential treatment in hiring: A field experiment. *Journal of Human Resources*, 44(3), 710–735.
- Rooth, D.-O. (2010). Automatic associations and discrimination in hiring: Real world evidence. *Labour Economics*, 17(3), 523–534. <https://doi.org/10.1016/j.labeco.2009.04.005>
- Salamanca, N., & Feld, J. (2017). A short note on discrimination and favoritism in the labor market. *The B.E. Journal of Theoretical Economics*, 17(1), Article 20160133. <https://doi.org/10.1515/bejte-2016-0133>

- Szymanski, S. (2000). A market test for discrimination in the English professional soccer leagues. *Journal of Political Economy*, 108(3), 590–603.
<https://doi.org/10.1086/262130>
- Weber, A., & Zulehner, C. (2014). Competition and gender prejudice: Are discriminatory employers doomed to fail? *Journal of the European Economic Association*, 12(2), 492–521. <https://doi.org/10.1111/jeea.12048>

Appendix A. Theoretical framework: derivations and proofs

This appendix provides the formal derivations and proofs underlying the theoretical framework in Section 2. Appendix A.1 derives the baseline Becker equilibrium from employer preferences. Appendix A.2 proves Proposition 1 (zero wage gap). Appendix A.3 derives the workplace-segregation result in Proposition 2, proves Corollary 1, and establishes its relation to a standard co-worker segregation measure. Appendix A.4 proves Proposition 3 (no profit penalty).

A.1 Baseline equilibrium: employer preferences and market clearing

Employer utility is defined in terms of profits and the workforce's racial composition. Following Becker (1957), employer i 's utility is:

$$U_i = \pi_i - d_i L_b \quad (\text{A.1})$$

where π_i denotes profits and L_b the quantity of Black labor employed. Profits are given by:

$$\pi_i = f(L_a + L_b) - w_a L_a - w_b L_b \quad (\text{A.2})$$

where L_a and L_b denote the quantities of Non-Black and Black labor employed, w_a and w_b their respective wages, and $f(\cdot)$ is the production function. Combining (A.1) and (A.2), a prejudiced employer with $d_i > 0$ behaves as if the wage of a Black worker were equal to $w_b + d_i$. Consequently, employer i strictly prefers to hire a Black worker if and only if $w_b + d_i < w_a$ and is indifferent when equality holds. Employers with $d_i = 0$ prefer the cheaper worker and are indifferent between worker types when $w_b = w_a$.

This generates a sorting mechanism. For any positive wage gap $w_a - w_b > 0$, there exists a threshold level of prejudice $d = w_a - w_b$ such that employers with $d_i < d$ prefer Black workers, those with $d_i > d$ prefer Non-Black workers, and those with $d_i = d$ are indifferent. In equilibrium, this threshold is denoted by d^* and identifies the marginal discriminator.

Labor market clearing requires that enough employers are willing to hire the available supply of Black workers. The marginal discriminator is therefore characterized by

$$d^* = F^{-1}(\theta) \equiv \inf\{d : F(d) \geq \theta\} \quad (\text{A.3})$$

When $d^* > 0$, continuity of F implies $F(d^*) = \theta$. When $d^* = 0$, $F(0) = \lambda$ may exceed θ , in which case only the required share of unprejudiced employers hires Black workers. Since the equilibrium threshold also satisfies $d^* = w_a^* - w_b^*$, the equilibrium wage gap is $w_a^* - w_b^* = d^* = F^{-1}(\theta)$. Hence, the equilibrium wage differential equals the prejudice of the marginal

discriminator, determined by the position of the supply of Black workers in the distribution of employer prejudice.

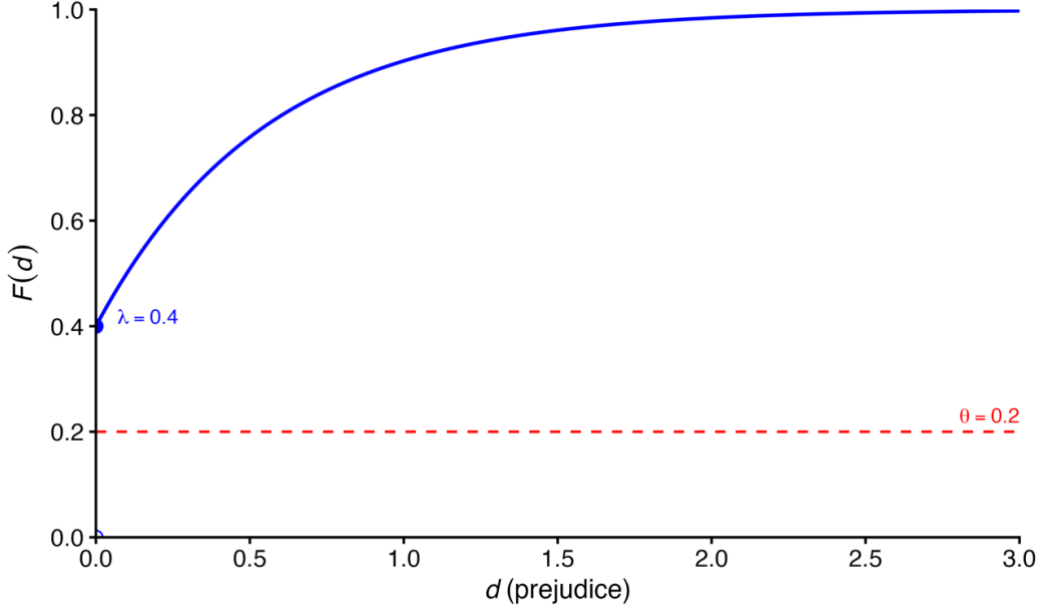


Figure A1. Prejudice distribution function with $\lambda = 0.4$ and $\theta = 0.2$. Since $\theta < \lambda$, the marginal discriminator has $d^* = 0$.

A.2 Proof of Proposition 1 (zero wage gap)

Suppose, contrary to the claim, that $w_a^* > w_b^*$, so that $d^* = w_a^* - w_b^* > 0$. When $d^* > 0$, market clearing requires $F(d^*) = \theta$. But $F(0) = \lambda \geq \theta$ by Assumption 2, and F is strictly increasing for $d > 0$, so $F(d^*) > F(0) = \lambda \geq \theta$, contradicting $F(d^*) = \theta$. Hence, $d^* = 0$ and $w_a^* = w_b^*$.

Under competition, the common wage is bid up to the common marginal product p for both types of workers. Hence, $w_a^* = w_b^* = p$. At these wages, the mass λ of unprejudiced employers is willing to hire Black workers, and since $\lambda \geq \theta$, they can absorb the entire Black labor supply. Prejudiced employers strictly prefer Non-Black workers at equal wages and are not constrained in doing so, given the available Non-Black supply $1 - \theta \geq 1 - \lambda$. This establishes the equilibrium allocation and completes the proof. ■

A.3 Derivation of Proposition 2 (workplace segregation) and Corollary 1

Proof of Proposition 2. Under Proposition 1, preferences determine allocation across employer types. Unprejudiced employers ($d_i = 0$) hire Black workers, and some Non-Black workers when $\theta < \lambda$, while prejudiced employers ($d_i > 0$) hire only Non-Black workers. Among otherwise identical unprejudiced employers, we focus on the symmetric equilibrium allocation in which Black labor is distributed evenly across firms. Since each employer has a workforce of normalized size one, the share of Black workers in firm i 's workforce is

$$s_i^b = \theta/\lambda \text{ if } d_i = 0, \text{ and } s_i^b = 0 \text{ if } d_i > 0 \quad (\text{A.4})$$

The expression θ/λ follows because all θ Black workers are allocated across the mass λ of unprejudiced employers.

Under race-neutral allocation, worker assignment is independent of race, and every firm has a Black worker share θ . Hence,

$$\text{Var}_r(s^b) = 0, \quad (\text{A.5})$$

Under preference-driven sorting, s_i^b follows a two-point distribution: a mass λ of employers has $s_i^b = \theta/\lambda$, while a mass $1 - \lambda$ has $s_i^b = 0$. The mean is

$$E(s_i^b) = \lambda(\theta/\lambda) + (1 - \lambda)0 = \theta \quad (\text{A.6})$$

The second moment is $E((s_i^b)^2) = \theta^2/\lambda$. The cross-firm variance is therefore

$$\text{Var}_m(s^b) = E((s_i^b)^2) - [E(s_i^b)]^2 = \theta^2(1 - \lambda)/\lambda > 0 \quad (\text{A.7})$$

Thus,

$$\text{Var}_m(s^b) - \text{Var}_r(s^b) = \theta^2(1 - \lambda)/\lambda > 0, \quad (\text{A.8})$$

This variance has a direct workplace-segregation interpretation. Let BB denote the average share of Black coworkers faced by Black workers and WB the corresponding share faced by Non-Black workers. With equally sized firms, worker-weighting and firm-weighting coincide. Therefore, $BB = E((s_i^b)^2)/\theta$ and $WB = E[s_i^b(1 - s_i^b)]/(1 - \theta)$. The standard co-worker segregation measure, defined as $CW \equiv BB - WB$, can then be written as

$$CW = \text{Var}(s_i^b)/[\theta(1 - \theta)] \quad (\text{A.9})$$

Hence, cross-firm variance and co-worker segregation contain the same information in the baseline model. Under race-neutral allocation, $CW_r = 0$. Under preference-driven sorting,

$$CW_m = \theta(1 - \lambda)/[\lambda(1 - \theta)] > 0 \quad (\text{A.10})$$

As $\lambda \rightarrow 1$, $CW_m \rightarrow 0$. As $\lambda \rightarrow \theta$, $CW_m \rightarrow 1$, corresponding to complete racial segregation across firms.

General case. More generally, suppose that unprejudiced employers have a Black worker share s_u^b and prejudiced employers a share $s_p^b < s_u^b$, with symmetric allocation within each type, and let $D \equiv s_u^b - s_p^b$. Market clearing requires $\lambda s_u^b + (1 - \lambda)s_p^b = \theta$, so that

$$s_u^b = \theta + (1 - \lambda)D, \quad s_p^b = \theta - \lambda D \quad (\text{A.11})$$

Since $s_p^b \geq 0$, $D \leq \theta/\lambda$; the constraint $s_u^b \leq 1$ does not bind because $\theta \leq \lambda$. The share s_i^b again follows a two-point distribution with mean θ , and its variance is

$$\text{Var}_m(s^b) = \lambda(s_u^b - \theta)^2 + (1 - \lambda)(s_p^b - \theta)^2 = \lambda(1 - \lambda)D^2 \quad (\text{A.12})$$

where the second equality uses $s_u^b - \theta = (1 - \lambda)D$ and $s_p^b - \theta = -\lambda D$. Hence, $\text{Var}_m(s^b) - \text{Var}_r(s^b) = \lambda(1 - \lambda)D^2 > 0$ for any $D > 0$, and, by (A.9), $CW_m = \lambda(1 - \lambda)D^2/[\theta(1 - \theta)]$. Because $\lambda(1 - \lambda)D^2$ increases in D , segregation is maximal when prejudiced employers hire no Black workers ($s_p^b = 0$, $D = \theta/\lambda$), in which case (A.12) reduces to (A.7) and CW_m to (A.10). This establishes Proposition 2. ■

Proof of Corollary 1. Let $E \equiv \text{Var}_m(s^b) - \text{Var}_r(s^b)$. By (A.12) and $D \leq \theta/\lambda$, $E \leq \theta^2(1 - \lambda)/\lambda$, that is, $\lambda E \leq \theta^2(1 - \lambda)$. Rearranging yields

$$1 - \lambda \geq E/(\theta^2 + E) \quad (\text{A.13})$$

For a given λ , (A.12) gives $D = [E/(\lambda(1 - \lambda))]^{1/2}$.

A.4 Proof of Proposition 3 (no profit penalty)

Let Y_i denote the output of employer i . Black and Non-Black workers are perfect substitutes in production and equally productive in the baseline model. Since each employer has a workforce of normalized size one, $L_{ai} + L_{bi} = 1$. It follows that

$$Y_i = f(L_{ai} + L_{bi}) = f(1) \quad (\text{A.14})$$

Hence, holding worker productivity fixed, firm output is invariant to workforce racial composition. Profits of employer i , with Black worker share $s_i^b = L_{bi}$, are

$$\pi_i = f(1) - w_a(1 - s_i^b) - w_b s_i^b = f(1) - w_a + s_i^b d^* \quad (\text{A.15})$$

where the last equality uses $w_a - w_b = d^*$ from (A.3). Profits therefore increase with the Black worker share at rate d^* . Under Assumptions 1 and 2, Proposition 1 gives $d^* = 0$ and $w_a = w_b = p$, so that $\pi_i = f(1) - p$ for all i . Preference-driven sorting can therefore alter cross-firm racial composition without generating differences in firm output or profits. ■

Appendix B. Institutional details

B.1 Scoring and lineup rules

Participants select a starting lineup of 11 players for each Serie A matchday, together with substitutes on the bench. If a starter does not receive a rating, an eligible substitute can replace him under league rules. The team's matchday score is then determined by the scores of the 11 players ultimately counted. A player's score combines a base rating ranging from 1 to 10, assigned by fantasy football game providers, with bonuses and penalties for observable match events. Table B1 reports the standard scoring adjustments. A minority of leagues use algorithmically generated statistical ratings rather than editorial ratings.

A player who appears for too little time to receive an evaluation may receive no rating. Player availability therefore matters because participants need enough rated players to field a complete lineup each matchday. This motivates our use of Rated Matches alongside Average Performance Score when measuring player productivity.

Table B1

Fantasy football standard points scoring system.

Action	Points
Goal scored	+3
Penalty scored	+3
Penalty missed	-3
Yellow card	-0.5
Red card	-1
Assist	+1
Own goal	-2
Goal conceded (goalkeeper)	-1
Penalty saved (goalkeeper)	+3
Clean sheet (goalkeeper)	+1

Notes: Standard bonuses and penalties are added to the player's base rating. League administrators may customize settings.

Appendix C. Additional data and measurement details

C.1 Racial classification

Table C1

Interrater reliability for race coding.

Comparison	Kappa coefficient
Human Rater 1 & Human Rater 2	0.949
Human Rater 1 & Human Rater 3	0.945
Human Rater 2 & Human Rater 3	0.940
Human Rater 1 & FairFace	0.911
Human Rater 2 & FairFace	0.884
Human Rater 3 & FairFace	0.912
Human Majority & FairFace	0.908
Human Raters (3)	0.945
Human Raters (3) & FairFace	0.924

Notes: Two-rater comparisons use Cohen's kappa; multi-rater comparisons use Fleiss' kappa. Human Majority is the majority classification of the three human coders.

C.2 Overall descriptive statistics

Table C2

Overall descriptive statistics.

	<i>N</i>	Mean	SD	Min	Max
Black	3,440,593	0.24	0.43	0	1
Budget Share	3,440,593	0.04	0.06	0	0.98
Listed Value	3,440,593	11.9	6.81	1	45
Average Performance Score (1-year)	2,675,607	6.28	0.72	2	9.14
Rated Matches (1-year)	2,675,607	26.9	8.86	1	38
Not in Serie A (1-year)	3,440,593	0.22	0.42	0	1
Average Performance Score (3-year)	2,830,592	6.28	0.70	2	8.83
Rated Matches (3-year)	2,830,592	25.84	7.96	1	38
Not in Serie A (3-year)	3,440,593	0.18	0.38	0	1
Age	3,440,593	26.59	3.95	16	42
Italian	3,440,593	0.34	0.47	0	1
Continental Cup	3,440,593	0.04	0.20	0	1
Goalkeeper	3,440,593	0.12	0.33	0	1
Defender	3,440,593	0.32	0.47	0	1
Midfielder	3,440,593	0.32	0.47	0	1
Forward	3,440,593	0.24	0.43	0	1

Notes: Observations are at the player-market level. Statistics for lagged performance measures use available observations only and exclude the zeros assigned to unavailable values in the regressions.

Appendix D. Additional empirical results and robustness checks

D.1 Additional price results

This section supplements the price results in Section 5.1. Table D1 reports estimated price differences in percentage terms, two-sided confidence intervals, and one-sided bounds on potential Black price penalties. Figure D1 examines heterogeneity across the player-value distribution.

Table D1

Percentage estimates and confidence bounds for the Black price gap.

Table 2 specification	Productivity measure	Estimated gap (%)	Two-sided 95% CI (%)	One-sided 95% upper bound on penalty (%)
(2)	Previous-year performance	1.2	[−15.0, 20.5]	12.5
(3)	Previous three-year performance	−4.1	[−19.8, 14.7]	17.5
(4)	Listed Value	1.0	[−9.6, 12.8]	8.0

Notes: Estimates transform the Black coefficients in Table 2 as $100[\exp(\beta) - 1]$; negative values indicate a Black price penalty. Confidence intervals and bounds use the same two-way clustered standard errors. The final column reports the one-sided 95% upper confidence bound on the penalty.

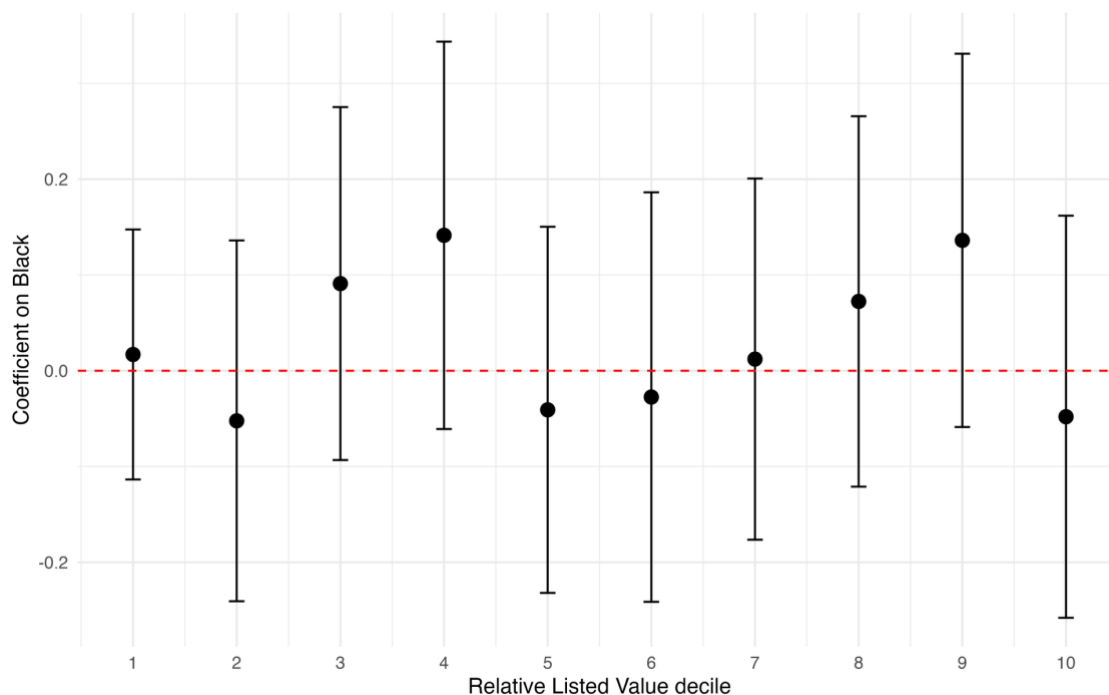


Figure D1. Estimated Black coefficient across Listed Value deciles.

Notes: Each point reports the Black coefficient from a separate regression for a Listed Value decile constructed within market $\times$ position. Specifications and clustering follow Table 2, column (4). Error bars show 95% confidence intervals.

D.2 Alternative measure of workplace segregation

Our main analysis measures workplace segregation using the cross-firm variance in the share of Black players, as derived in Proposition 2. We assess the robustness of this measure using the co-worker segregation measure of Hellerstein and Neumark (2008). The measure compares the racial composition of the coworkers encountered by Black and Non-Black workers and naturally accommodates differences in team size.

For each market, let n_t denote the number of players on team t and b_t the number classified as Black. Let $B = \sum_t b_t$ and $W = \sum_t (n_t - b_t)$. We calculate the average share of Black coworkers faced by Black players as

$$BB = \frac{100}{B} \sum_t b_t \frac{b_t - 1}{n_t - 1} \quad (\text{D.1})$$

and the corresponding share faced by Non-Black players as

$$WB = \frac{100}{W} \sum_t (n_t - b_t) \frac{b_t}{n_t - 1} \quad (\text{D.2})$$

Co-worker segregation is then defined as $CW = BB - WB$. Positive values indicate that Black players are exposed to a higher share of Black coworkers than Non-Black players. We construct the race-neutral benchmark using the same permutation procedure as in the main analysis. Both the cross-firm variance and CW are calculated from the same 200 counterfactual allocations.

Table D2 reports the results. The mean observed CW is 0.17 percentage points, compared with -0.51 under race-neutral allocation, yielding excess co-worker segregation of 0.68 percentage points. The negative counterfactual mean arises mechanically in finite teams: because the focal player is excluded from his own coworker set, a Black player has one fewer Black individual available as a potential coworker than an otherwise comparable Non-Black player. The permutation benchmark directly accounts for this feature.

The results closely reproduce those obtained using cross-firm variance. Excess co-worker segregation is positive in every auction year, and the share of markets with permutation p -values below .05 ranges from 9.3% to 10.9%, compared with 5% under the null. The pooled share is 10.1%, and the corresponding binomial test strongly rejects the race-neutral benchmark.

Table D2

Co-worker segregation relative to race-neutral allocation.

Auction year	Observed mean <i>CW</i>	Simulated mean <i>CW</i>	Excess <i>CW</i> (pp)	Share with $p < .05$	Binomial p -value
2023	0.02	-0.55	0.56	9.3%	< .001
2024	0.32	-0.47	0.78	10.9%	< .001
2025	0.18	-0.50	0.69	10.1%	< .001
All	0.17	-0.51	0.68	10.1%	< .001

Notes: *CW* is the difference in Black coworker exposure between Black and Non-Black players, measured in percentage points. Excess *CW* is observed minus simulated mean *CW*. Permutation and binomial tests follow Table 3, using *CW* in place of variance. Calculations use unrounded values.

D.3 Team size and alternative workplace segregation measures

The baseline model assumes equally sized firms. This condition also describes most markets in our data: all teams have the same roster size in 14,222 of the 15,000 markets (94.8%). Only 778 markets (5.2%) contain differences in roster size across teams. Among these markets, differences are generally small: the median gap between the largest and smallest roster is one player, with an interquartile range of one to three players.

We nevertheless assess whether unequal team sizes affect the main result. Table D3 compares the cross-firm variance used in the main analysis with the co-worker segregation measure separately for markets with equal and unequal roster sizes.

Table D3

Workplace segregation measures by roster-size structure.

Roster structure	Markets	Sample share	Excess variance (%)	Markets with $p < .05$	Excess <i>CW</i> (pp)	<i>CW</i> $p < .05$	p -value correlation	Same rejection decision (%)
Equal roster size	14,222	94.8%	19.0	10.1%	0.68	9.9%	0.998	99.5%
Unequal roster size	778	5.2%	20.2	12.5%	0.69	12.7%	0.999	99.7%
All	15,000	100.0%	19.1	10.2%	0.68	10.1%	0.998	99.5%

Notes: Equal roster size means that all teams within a market have the same number of players. Variance and *CW* statistics follow Tables 3 and D2 and use the same permutations. Rejection shares report markets with $p < 0.05$. “Same rejection decision” is the share of markets in which both measures reject or both fail to reject.

The two measures produce nearly identical conclusions in both subsamples. Among equal-roster markets, their permutation p -values correlate at 0.998 and yield the same 5% rejection

decision in 99.5% of markets. Among unequal-roster markets, the corresponding figures are 0.999 and 99.7%. Both measures also show similar excess segregation across the two groups. These results indicate that the equal-firm-size assumption used to obtain the tractable theoretical representation of workplace segregation does not drive the empirical findings.

D.4 Placebo tests

As a placebo test, we repeated the permutation procedure using whether a player's day of birth was odd or even instead of perceived race. This provides a check for mechanically generated excess variance using a characteristic unrelated to racial preferences. Table D4.1 shows no such evidence: the share of significant markets remains near 5%, and excess variance is near zero.

Table D4.1

Day-of-birth placebo: excess variance across teams.

Auction year	Observed mean variance	Simulated mean variance	Share of markets with $p < 0.05$	Binomial p -value	Excess variance (%)
2023	100.0	99.5	4.7%	0.826	0.5%
2024	98.3	100.1	4.2%	0.996	-1.8%
2025	98.0	100.4	3.6%	1.000	-2.3%
All	98.8	100.0	4.2%	1.000	-1.2%

Notes: Statistics follow Table 3, replacing perceived race with an indicator for an odd day of birth.

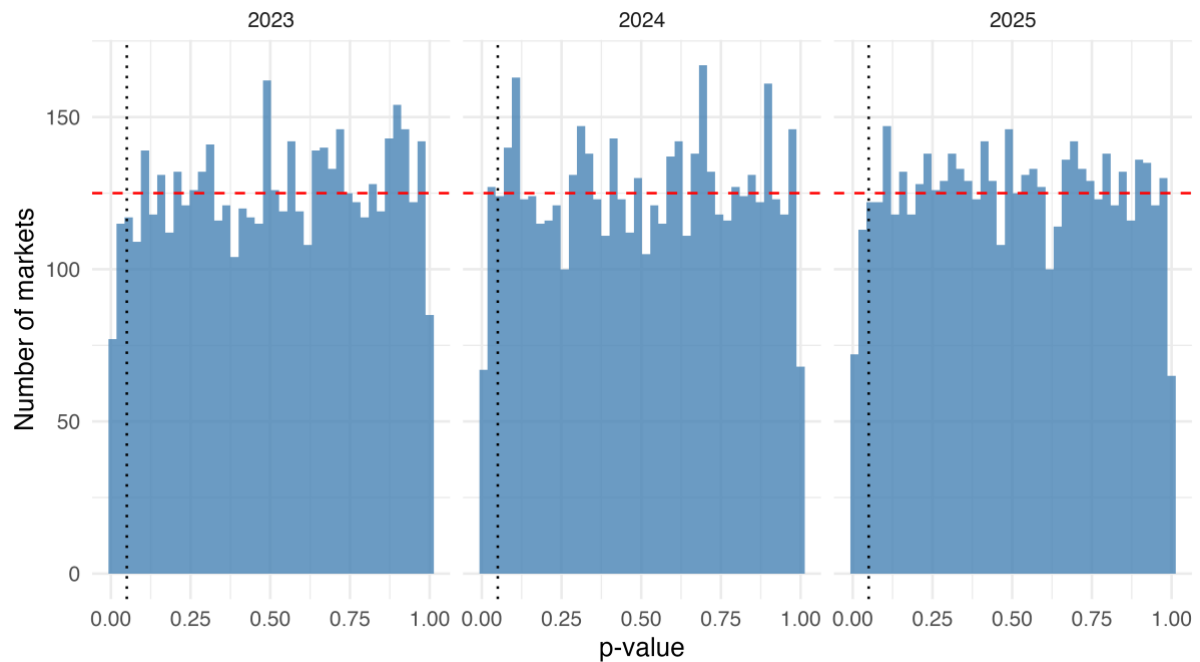
Additionally, we constructed another placebo test by randomly reassigning Black/Non-Black labels within each market and position, preserving racial composition and team assignments. Table D4.2 and Figure D2 show no evidence of excess segregation, consistent with the day-of-birth placebo.

Table D4.2

Placebo with randomly assigned Black/Non-Black labels: excess variance across teams.

Auction year	Observed mean variance	Simulated mean variance	Share of markets with $p < 0.05$	Binomial p -value	Excess variance (%)
2023	66.1	66.9	5.0%	0.561	-1.2%
2024	74.0	73.8	5.1%	0.334	0.3%
2025	74.6	74.4	4.8%	0.729	0.3%
All	71.6	71.7	5.0%	0.565	-0.2%

Notes: Statistics follow Table 3. Observed variance refers to the artificial allocation obtained by randomly permuting Black/Non-Black labels within market $\times$ position, keeping team assignments fixed.

**Figure D2.** Distribution of permutation p -values under randomly assigned racial labels.

Notes: Panels show market-level permutation p -values by auction year. The dotted vertical line marks $p = 0.05$; the dashed horizontal line shows the expected bin frequency under a uniform distribution.

D.5 Alternative permutation constraints

We next imposed alternative constraints on the permutation procedure to assess whether excess segregation could be explained by observable characteristics correlated with perceived race. We separately restricted permutations by Serie A club, Listed Value deciles, nationality, and age quartiles crossed with Not in Serie A (1-year) status. Table D5 reports the results.

Table D5

Excess variance under alternative permutation constraints.

Additional constraint	Simulated mean cross-firm variance	Share of markets with $p < 0.05$	Binomial p -value	Excess variance (%)
Serie A club constraint	73.4	9.3%	$< .001$	16.3%
Listed Value deciles	72.1	10.0%	$< .001$	18.3%
Nationality (Italian vs. non-Italian)	73.4	9.3%	$< .001$	16.3%
Age quartiles $\times$ Not in Serie A (1-year)	72.4	9.9%	$< .001$	17.9%

Notes: Results pool all auction years. Restrictions are added separately to the baseline market $\times$ position permutation procedure. Listed Value deciles and age quartiles are constructed within market $\times$ position. Statistics follow Table 3; its observed mean variance is unchanged and omitted.

D.6 Conditional reshuffling

Finally, we implemented the conditional reshuffling procedure described in Section 5.5. Table D6 reports the logit model used to predict each player's probability of being Black from observable characteristics. The model has substantial predictive power (McFadden pseudo- $R^2 = 0.26$; AUC = 0.83), confirming that perceived race is strongly correlated with observable player profiles. We use these predicted probabilities to construct the more restrictive counterfactual allocation reported in Table 6 of the main text.

Table D6

Logit model predicting Black player status.

	(1)
Intercept	-0.774*** (0.066)
Listed Value	-0.012*** (0.0003)
Average Performance Score (1-year)	-0.808*** (0.007)
Rated Matches (1-year)	-0.015*** (0.0003)
Not in Serie A (1-year)	-6.789*** (0.043)
Average Performance Score (3-year)	0.922*** (0.007)
Rated Matches (3-year)	-0.005*** (0.0004)
Not in Serie A (3-year)	7.103*** (0.042)
Age	0.057*** (0.004)
Age Squared	-0.002*** (0.00008)
Italian	-2.727*** (0.006)
Continental Cup	3.214*** (0.009)
Bologna	-0.729*** (0.009)
Cagliari	0.901*** (0.011)
Como	-2.256*** (0.015)
Cremonese	-2.753*** (0.040)
Empoli	-0.188*** (0.014)
Fiorentina	0.451*** (0.008)
Frosinone	-2.362*** (0.021)
Genoa	-0.769*** (0.011)
Inter	0.084*** (0.008)
Juventus	0.766*** (0.007)
Lazio	-0.355*** (0.009)
Lecce	-0.315*** (0.010)
Milan	1.049*** (0.007)

Monza	-2.286*** (0.030)
Napoli	-0.134*** (0.008)
Parma	0.051*** (0.010)
Pisa	0.889*** (0.015)
Roma	-0.524*** (0.008)
Salernitana	-0.765*** (0.019)
Sassuolo	0.163*** (0.012)
Torino	0.159*** (0.008)
Udinese	0.241*** (0.008)
Venezia	0.037 (0.019)
Verona	0.127*** (0.009)
Number of observations	3,440,593
Log-likelihood	-1,401,115
McFadden pseudo- R^2	0.26
AUC	0.83

Notes: Logit estimates predict Black status for the conditional reshuffling in Table 6. Observations are at the player-market level; Atalanta is the omitted club category. Conventional standard errors are reported in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.