# Expected Discrimination and Job Search*

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**Abstract**

The impacts of labor market discrimination depend not only on whether employers discriminate, but also on jobseekers' responses to expected discrimination. To study these responses, we ran a set of field experiments in Rio de Janeiro's favelas (urban slums). In an audit study, we find little difference between callback rates for favela and non-favela resumes. Yet, over 87% of favela jobseekers in our study (N=2,167) expect discrimination in callbacks. Our main strategy to randomize expected discrimination is to vary whether favela jobseekers may expect an employer to know their address. We find that (i) removing the need to declare address encourages applications only among white jobseekers, and (ii) jobseekers perform on average worse in real job interviews when expecting interviewers to know their name and address, as opposed to only their name, the only information actually known by the interviewer. The effects on interview performance also concentrate on white jobseekers, likely because they can more easily pass as non-favela residents and ignore racial discrimination. Hence, expected discrimination can shape job market outcomes through interview performance and applicant pool composition.

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# 1 Introduction

Employers discriminate along many dimensions, including race, sexual orientation, and criminal history.[1] However, jobseekers' *reactions* to expected discrimination, i.e., their expectations about receiving different treatment due to negative stereotypes, remain understudied. These reactions can play an important role in determining the labor market equilibrium, regardless of how often employers discriminate. For instance, expected discrimination can amplify the effects of employer discrimination and create self-reinforcing loops (Coate and Loury, 1993). Jobseekers may feel discouraged from applying to jobs, especially if they overestimate discrimination (e.g., because of its salience in the public debate). But even if they are not discouraged, expected discrimination can make one too nervous to perform well in interviews – reinforcing interviewers' biases against discriminated groups. Hence, beliefs about expected discrimination may matter for policy: It might be desirable to disseminate information on actual discrimination rates to encourage applications, or for firms to train their interviewers to credibly signal commitment to anti-discrimination policies.

This paper uses field experiments to estimate how expected discrimination affects jobseeking behaviors, from job application decisions to interview performance. We focus on the address-based discrimination perceived by favela (urban slum) residents in Rio de Janeiro, Brazil. This setting allows us to randomize expected anti-favela discrimination by manipulating expected address visibility: We vary whether favela jobseekers may expect an HR firm (in a first experiment) or an interviewer (in a second one) to know their address. To emulate real job application procedures, we run this HR firm ourselves, recruiting and interviewing for real full-time sales jobs. In a third experiment, we test whether lowering expected discrimination by providing information about market-level discrimination rates, an arguably more policy-relevant intervention, could be better than lowering expected address visibility in encouraging job applications. As favela residence strongly correlates with race, a visible source of discrimination, we can also speak to how intersectionalities can play a key role in determining the effects of expected discrimination.[2]

We begin by documenting that most favela jobseekers overestimate anti-favela discrimination. In a door-to-door survey, we told N=2,167 jobseekers, who would later participate in our field experiments, that we were also running an audit study. We explained that we created ficti-

[1] See Neumark (2018), Rich (2014), and Riach and Rich (2002) for reviews of experimental evidence, and Kline et al. (2022) for a recent large-scale study documenting employer discrimination.

[2] Another reason to study expected address-based discrimination is its potential role in perpetuating geographical poverty traps. In Rio, about 1.5 million people, 22% of the city's population, live in favelas; worldwide, almost one billion people live in urban slums (UN, 2016). Urban divides are common even in wealthier countries (e.g., public housing projects in the US).

tious résumés, randomized their addresses, and submitted them to 700 sales jobs in Rio. Then, we asked jobseekers to guess our findings, paying them based on accuracy. Over 85% predicted anti-favela discrimination, while about 60% predicted that a favela address would cause callback rates to drop 50% or more. In the audit, we find no statistically significant discrimination: The callback rates were 19.3 and 19.6% for favela and non-favela résumés (p=0.38 for the difference). These similar callback rates do not imply there is no anti-favela discrimination at all (Kessler et al., 2019; Neumark, 2018). Still, the gap between prediction and reality shows that favela jobseekers are too pessimistic about address-based discrimination in callbacks.

In a first experiment with jobseekers, to test whether expected discrimination affects job applications, we subtly manipulated expected address visibility during the job application procedure (Address Omission Experiment; N = 1,303). There were three experimental conditions: (i) *Address Omission*, in which the address was unnecessary for applying, (ii) *Status Quo*, in which jobseekers had to fill in their addresses in the online application form, and (iii) *Known Address*, in which true favela addresses were pre-filled in the form. *Address Omission* was the main treatment condition, so we may think of this experiment as estimating the effect of an address blinding policy on labor supply. *Status Quo* allows address obfuscation, e.g., declaring a different neighborhood or a relative's address, which we anticipated given anecdotal evidence. So, to understand the value of obfuscation, we included *Known Address*, which prevented it. These conditions appear successful in randomizing expected address visibility: 28% of the *Status Quo* applicants obfuscated their addresses, suggesting that applicants were aware of their address visibility, and no applicant in *Known Address* corrected their pre-filled neighborhood.

Dropping the address requirement from the applications does not increase average application rates, but it does encourage white jobseekers to apply more often.[3] Average application rates are similar across arms: About 40% of jobseekers finished the job application form, and 20% attended a job interview, with no statistically significant differences across conditions. Nevertheless, white jobseekers are 57% (10 pp) more likely to attend interviews under *Address Omission* than in the other two arms pooled (p=0.05). This is consistent with white jobseekers expecting to pass as non-favela residents while non-white jobseekers expect discrimination either way – because their race will be eventually visible or because observers will associate their race with residing in favelas. As only one-third of favela residents in Rio are white, this suggests that making firms blind to address may not be enough to reduce social inequalities.

In a second experiment, we test whether expected discrimination can hurt interview performance (Interview Experiment; N=422). Jobseekers who completed the application form

[3]We pre-registered four pre-registered heterogeneity cuts: expected discrimination, skill, race, and gender, which leads to testing multiple hypothesis testing. Taking together the results of our three experiments, the race heterogeneity in the effects survives multiple-hypothesis testing corrections. See Section 4.4.

were invited for an interview in Downtown Rio. On arrival, the receptionist asked jobseekers to confirm their name, date of birth, and address. They then tell the jobseeker that, to keep the process objective, "the interviewer will only know your name" (*Name-Only* condition) or "your name and address" (*Name-and-Address* condition). The two conditions differ only by two words: "and address". Interviewers followed a script, did not know about the randomization, and learned about the jobseekers' neighborhoods of origin only after submitting all their assessments. Hence, similar to the first experiment, we only vary expected address visibility, and any treatment effects must be triggered by changes in the interviewees' behaviors or beliefs.[4] After each interview, interviewers rated candidates on overall performance, nervousness, and professionalism, and candidates filled out self-assessments on the same three dimensions. To maximize power and reduce multiple hypothesis testing, we construct an index of performance for each point-of-view (Anderson, 2008). As our primary outcome, we average the two.

Expecting address to be known (*Name-and-Address* condition) decreases aggregate performance by 0.13SD, compared to the *Name-Only* condition. Unbundling the aggregate index, we find that *Name-and-Address* leads to a larger decrease on the self-assessed (0.17SD, $p<0.01$) than on the interviewer-assessed performance index (0.09SD, $p=0.28$). Nevertheless, those effects are statistically identical ($p=0.33$ for the difference), and, among those who expected at-or-above-median discrimination at baseline, *Name-and-Address* decreases interviewer-assessed performance by 0.22SD ($p=0.04$). Hence, expected discrimination seems to indeed damage interviewer-assessed performance, specially among those expecting so from the beginning. This is consistent with a self-fulfilling prophecy: If a jobseeker simply expects a worse evaluation due to her home address, she gets one, even when her interviewer only knows her name.

While the Interview Experiment was not designed to pin down which mechanism leads to impaired performance, our data is consistent with the idea that a mix of stress and heightened stakes leads jobseekers to "choke under pressure" (Baumeister, 1984; Böheim et al., 2019; Harb-Wu and Krumer, 2019; Teeselink et al., 2020). First, this mechanism fits the pattern of null average effects of expected discrimination on application decisions (lower pressure and private) and negative effects on interview performance (higher stakes, with face-to-face interactions). Second, those who expected high discrimination reported feeling much more nervous when their addresses were known, consistent with an account of perceived stereotyping or injustice leading to stress (Berger and Sarnyai, 2015; Schmader et al., 2008). Third, jobseekers seem to find it harder to be strategic or control their behavior at the interview office: They obfuscate their addresses less often than on the application form, and expected address visibility negatively

[4]Note that our manipulation rules out self-signaling effects (e.g., that reminding candidates of their address leads them to update negatively about their skill) since all jobseekers are asked about their address before treatment.

affects their self-reported professional behavior.[5]

Similar to the results from the Address Omission Experiment, we also find that reducing expected address visibility at the interview is better for white jobseekers. Hearing that the interviewer knows *Name-Only* increases performance by 0.31SD for white jobseekers (p=0.01) while only increasing performance by a non-significant 0.07SD (p=0.28) for non-white candidates (p=0.09 for the difference in effects). This is again consistent with white jobseekers being more able to pass as non-favela residents and not suffering racial discrimination.[6]

We ran a third experiment to understand whether a more policy-relevant intervention – providing information about true market-level discrimination – could encourage applications more than blinding policies (Information Experiment; N=690). We randomized whether a jobseeker answering our baseline survey would be told about the results of our audit study, i.e., that there was little anti-favela discrimination in callbacks. Note that such intervention does not require regulating what information firms can require from candidates, and can be transferred to any context in which it is possible to benchmark (misperceived) discrimination levels. There were three experimental conditions: i) *No Info*, reveling no information ii) *Favela Info*, revealing the audit study callback rate for a favela, and iii) *Full Info*, revealing the favela and non-favela callback rates. As *Full Info* reveals both the discrimination rate and callback level, *Favela Info* works as an alternative control, holding constant the knowledge of the favela callback level.

On average, telling jobseekers that our audit study found little to no discrimination does not encourage them: Interview show-up is again near 20% for all conditions, without statistically significant differences. As we also have evidence that *Full Info* significantly decreases expected discrimination, the average null suggests that average application rates are inelastic to expected anti-favela discrimination. Nevertheless, the racial heterogeneity now appears flipped: White jobseekers were relatively discouraged after learning that we found no discrimination in callbacks for sales jobs. The most natural interpretation for this reversal is that low anti-favela discrimination implies i) low returns for passing and ii) low racial discrimination – since employers do not use address as a predictor of race to discriminate. Hence, learning the audit results might imply more opportunities, but, for white jobseekers, it also implies more compe-

[5]While other mechanisms such as reduced perceived returns to effort or overcompensating (e.g., by overselling their qualities) are possible, it is not obvious that these would lead to the same empirical patterns. For instance, lower perceived returns to effort should not cause stress responses, and overcompensating should not cause lower professionalism in *Name-and-Address*.

[6]A Bayesian should assign a random white person in Rio a 13% chance of residing in a favela, and twice that chance for a non-white. Hence, if candidates are careful not to reveal address information directly or through how they speak, a Bayesian interviewer should not guess that they are favela residents. Nevertheless, even a moderate probability of being from a favela may lead to discrimination, and a literature on stereotypes suggests that observers exaggerate group differences, making the calculus even more favorable to white jobseekers (Bordalo et al., 2016). Only 4% of all interviewees directly revealed that they were favela residents.

tition and the loss of an edge (passing).

Overall, we provide evidence that expected discrimination can exacerbate the impacts of employer discrimination on jobseekers and create self-reinforcing loops, mainly by hurting interview performance. Also, as race heterogeneities are present regardless of how and when we randomize expected discrimination, our findings suggest that policymakers should be mindful of intersectionalities when using expected discrimination as a policy lever.

To the best of our knowledge, our field experiments are the first to directly estimate the effects of expected discrimination in the labor market.[7] While many experiments measure whether employers discriminate (Neumark, 2018; Rich, 2014; Riach and Rich, 2002), the supply side received much less experimental attention. Three recent field experiments randomizing either the language in job ads (Del Carpio and Fujiwara, 2023; Burn et al., 2023) or how the selection procedure is described (Avery et al., 2023) provide suggestive but inconclusive evidence that expected discrimination can affect the job applicant mix. For instance, the non-gendered (as opposed to gendered) job ads in Del Carpio and Fujiwara (2023) may encourage female applicants, but also signal better work-life balance and an inclusive culture, which can appeal differently to females. Two design elements let us identify the effects of expected discrimination more sharply. First, we elicit beliefs about discrimination at baseline, allowing us to estimate whether expected discrimination predicts effect sizes. Second, we designed our experiments to vary only i) expected address visibility, keeping job desirability and other factors as constant as possible, or ii) expected market-level discrimination, avoiding job-level confounders.[8]

We contribute to the study of discrimination as a self-fulfilling prophecy. In theory, even with no differences in initial endowments, pessimistic beliefs about returns to investment can make a group of workers acquire less human capital in response to expected discrimination, justifying statistical discrimination (Coate and Loury, 1993; Lundberg and Startz, 1983). Glover et al. (2017) shows that a similar self-fulfilling prophecy can stem from managers' beliefs, which lead them to exert less effort in supervising minority cashiers over a trial period, making those cashiers less productive and less likely to be hired. We show how expected discrimination can

[7] Some observational studies find evidence consistent with expected discrimination affecting jobseekers. For instance, people who expect to be stereotyped might try to give clearer signals of their productivity (Lepage et al., 2022; Dickerson et al., 2022; Lang and Manove, 2011), or costly hide information (Agüero et al., 2023). Pager and Pedulla (2015) uses administrative and survey data to show that Black jobseekers cast wider nets in their job searches and that breadth correlates with having suffered discrimination. See also Kuhn and Shen (2023).

[8] We also build on lab studies showing that jobseekers may obfuscation to deal with response to expected discrimination: Kang et al. (2016) and Charness et al. (2020). For instance, Charness et al. (2020) finds that female college students are less likely to pick gender-matching avatars in a virtual labor market when competing for a male-dominated task. There is also lab-in-the-field evidence that expected discrimination may affect on-the-job outcomes like retention (Ruebeck, 2024) and productivity (Hoff and Pandey, 2006). See also Fryer et al. (2005) and Aksoy et al. (2023).

also generate a self-fulfilling prophecy in the matching process, exclusively through jobseeker's beliefs, as we hold the HR firm's actions constant.

While it is possible that employers discriminate more at the interview than in callbacks (Quillian et al., 2020; Shukla, 2024), interview performance has received little attention in the study of discrimination or labor supply. The main finding in Goldin and Rouse (2000) hints at the potential relevance of expected discrimination for interview performance: female hiring increases after orchestras adopt "blind" auditions. That effect could be both because evaluators lose the ability to discriminate and because females might perform better knowing that they will be judged only on merit. Our findings suggest that there is some merit to the latter account.[9]

Finally, our experiments randomizing expected address visibility brings a literature on stereotype threat – the idea that feeling at risk of confirming a negative stereotype may prompt a self-fulfilling prophecy (Steele and Aronson, 1995) – to the labor market. That literature overwhelmingly considers test performance or other laboratory outcomes (Spencer et al., 2016; Liu et al., 2021), and we provide field evidence of its relevance in the job search.

# 2 Context, Sample, and Misperceived Discrimination

## 2.1 Favelas in Rio de Janeiro

Brazilian favelas are areas of dense informal settlements. In Rio de Janeiro, the state has been unable to hold the monopoly of violence over favelas, which are home to 1.5 million people (one-fifth of the population). According to the 2010 Census, 66% of favela households had a per capita income of one minimum wage (≈10 USD/day) or less. Outside the favela, that rate is 30%, and per capita income is 3.5 times larger. Favela residents are also less likely to be literate (84% are literate inside favelas, 92% outside them), to have completed high school or an advanced degree, or to self-identify as white (33% in favelas and 57% outside).

Jobseekers in our study lived in Maré, Manguinhos, or Jacarezinho, three large adjacent favelas in Rio, home to about 200,000 people. These neighborhoods grew to their current boundaries without proper urban planning or public services, and now are part of a contiguous metropolitan area, sharing borders with other favelas and regular "asphalt" neighborhoods. We conducted most of our fieldwork in Maré, which is the most populous favela in Rio.

There are limited formal work opportunities in favelas. For instance, according to a census of Maré's Businesses, 75% of those businesses were entirely informal. In total, they employed

[9] In a small experiment with students, Word et al. (1974) articulates how even non-verbal interviewer cues triggered by a racial mismatch between interviewer and interviewees can lead to worse interview performance.

only 9% of the favela's working-age population (REDES, 2014). Hence, most favela jobseekers must look outside for formal jobs. Such jobs are attractive since they provide more benefits and stability, but many favela jobseekers expect discrimination when applying to them. Formal employers typically require applicants to list a home address, meant for assessing distance to work. In Section 2.3, we show that many favela jobseekers believe that such address information is used for discriminating regardless of distance to work.

Residents in all three favelas are regularly exposed to violence or its imminent risk. In Maré, three criminal groups – two of which exploit the illegal drug market, and another working mainly as an extortion racket – hold the monopoly of violence. Criminal groups were also present in the two other favelas during our fieldwork, but the police were also somewhat present.[10] Over our five months of fieldwork, police raids interrupted our survey activities 14 times. These police raids are generally unpredictable and violent. During a raid, favela residents will take refuge at their homes to avoid the crossfire. Workers miss work days, businesses shut their doors, and communications are hampered (internet or telephone). It is typically unclear when a police raid ends, so disruptions may persist for several days.

When there is no police raid in progress, favela residents can typically go in and out without issues. Some may work in the asphalt neighborhoods adjacent to their favela or commute to wealthier areas of the city. Commuting to these richer areas (e.g., Rio's Downtown or South Zone) using public transportation may take 30 to 90 minutes. The Downtown office of our HR firm, where we held interviews, was within a 50-minute commute for most participants.

## 2.2 Audit Study: Measuring Anti-favela Discrimination

There is little experimental evidence on whether employers discriminate against favela jobseekers. In Rio, Westphal (2014) conducted an audit study with résumés from different favelas and found no discrimination on average – but with some geographical heterogeneity.[11] Since the Westphal (2014) estimates were ten years old, we conducted a new audit study estimating anti-favela discrimination in callbacks for entry-level sales jobs – similar to the jobs used in our experiments with jobseekers.

We created four fictitious worker profiles with complete high school, two male and two female. Age, job experiences, sales-related certifications, and résumé templates varied slightly across profiles. A local consultant with experience matching young favela residents with formal

[10] See Lessing (2021) for a conceptualization of the symbiotic interaction of such criminal groups and the state. See also Monteiro et al. (2022) for an empirical account discussing the economic trade-off these gangs face, and Barnes (2022) for an ethnographic account of how gangs have responded to state action in recent years.

[11] Zanoni et al. (2023) used the incentivized résumé rating method (Kessler et al., 2019) to measure anti-favela discrimination in Argentina, finding substantial discrimination.

jobs revised these profiles to ensure they were competitive but not unrealistic.

For each profile, we created two copies that differed in name, email, phone number, and address – one from Maré and one from Bonsucesso, which is a non-favela neighborhood adjacent to Maré (see Appendix C for an example). We selected addresses that unambiguously mapped to either Maré or Bonsucesso, keeping the estimated commuting time constant. Maré is a widely recognized favela in Rio, so employers can immediately tell the neighborhood is a favela. Information about the Maré-Bonsucesso callback gap is also relevant for jobseekers in Manguinhos and Jacarezinho, since they update their beliefs about their own neighborhoods similarly to Maré residents when learning about the audit study results (see Figure A.5).

We collected sales job postings (e.g., sales associate, telemarketing salesperson) no older than two weeks from five popular job search websites.[12] Then, research assistants applied to each job posting with two different profiles, with randomized addresses.[13] We submitted 1,400 applications to 700 jobs between February and May 2023. Research assistants monitored the phone numbers and emails until the end of June and coded all non-automatic, non-negative replies as callbacks.

The resulting callback rates are similar across neighborhoods: for favela resumes, it is 19.3%, while for non-favela resumes, it is 19.6%, giving a 0.3 p.p. difference between them (p=0.38 to 0.87, depending on the specification, see Table C.1 for details). These similar callback rates do not imply an absence of discrimination against favela residents. For instance, if recruiters believe favela residents are *ceteris paribus* more likely to accept a job offer, that might offset callback differences caused by, say, anti-favela taste-based discrimination (Kessler et al., 2019). Another explanation for the results is that firms are sophisticated and anticipate that some Maré residents obfuscate their neighborhood and instead say they live in Bonsucesso (as we observe in our experiments discussed below), making the declared address uninformative. Nevertheless, even if the audit study measure does not reveal the "real" discrimination level, it provides an objective benchmark for measuring whether jobseekers under- or overestimate anti-favela discrimination.

[12]The websites were Catho, Indeed, Infojobs, LinkedIn, and Riovagas. If a posting listed a requirement that one or more of our profiles did not have, or if it was more than two hours away from our addresses by public transport, we also discarded it

[13]The exact randomization procedure was that, for each job posting, we first randomly ordered the four profiles. Among the first two randomly ordered profiles, we randomly picked one for being from Maré. We did the same for the latter two, which were backups. A research assistant applied to each posting with two profiles, following the order. The backup profiles were only used for gendered jobs. If a job were gendered, the research assistant would still follow the suggested order but skip the profiles of mismatched gender. This skipping happened in 9% of postings, and results are similar if we drop those.

## 2.3 Perceived vs. Actual Discrimination

In our door-to-door survey – discussed in detail in the next section – we collected incentivized predictions of what callback rates we would find in our audit study, similar to the method used in Haaland and Roth (2021). We focus on predictions about the jobseekers' favela of residence versus the adjacent non-favela neighborhood and compare that with the observed Maré and Bonsucesso callback rates.[14]

The top panel in Figure 2 compares callback rate predictions against those found in the audit study. On average, jobseekers predict a callback rate of 63% for their adjacent non-favela neighborhood, with 81% predicting callback rates of at least 50%. Jobseekers' guesses are closer to the audit estimates when estimating callback rates for favelas but are, on average, too optimistic: The average prediction for one's favela callback rate is 30% – over 50% larger than the audit study estimates.

The bottom panel in Figure 2 shows the distribution of implied discrimination rates, i.e., the percent drop in callback induced by having a favela instead of a non-favela address. We see that 87% predict discrimination (i.e., a decrease in callback), and 84% predict decreases larger than the upper bound of our 95% confidence interval for the discrimination rate in the audit study. The median jobseeker predicts a 50% discrimination rate, substantially more than the 17.5% upper bound given by our audit study.

We consider jobseekers' predictions of the audit results to be our best measure of expected anti-favela discrimination, since it is an incentivized and objective benchmark. Reassuringly, providing information on callback rates also decreases an incentivized measure of discrimination regarding the partner HR firm (see Section 4.2), and expected discrimination in the audit study strongly correlates with i) a Likert measure of anti-favela discrimination and ii) a "personalized" measure of discrimination, comparing expected future employment probability for oneself against a "clone" of the respondent in the adjacent non-favela (see Figure A.10).

In our survey, we also asked most (N=1,497) jobseekers about the main reasons why employers would discriminate against favela residents. Jobseekers mentioned a mix of productivity- and taste-based reasons. The most common reasons were loss of workdays because of police raids (mentioned by 74%), racism (68%), dislike because of cultural differences (e.g., speech) (66%), and dislike of favela residents (65%). Hence, favela jobseekers have rich second-order beliefs about employers. Notably, jobseekers think that employers understand and act on the correlation between address and race, which might explain why white and non-white jobseekers react differently to shifts in expected discrimination (Section 4.4).

[14] We reach similar conclusions if we instead always use beliefs about Maré and Bonsucesso, which are the audit study neighborhoods, see Figure A.9.

# 3 Experiment Design

## 3.1 Outline and Pre-registration

In early March 2023, before any randomization in the supply-side experiments, we pre-registered the Address Omission Experiment and the Interview Experiment. Those experiments test whether expected discrimination can affect job-seeking behaviors by randomizing whether jobseekers may expect the source of discrimination (address) to be visible or not to the employer or interviewer. As our treatments typically reduce expected address visibility in relation to the status quo, one can think of these two experiments as estimating the effects of "blinding" policies on job application rates and interview performance. We launched the Information Experiment in June 2023, as we phased out the Address Omission Experiment.[15] The Information Experiment tested whether providing information on market-level discrimination, typically overestimated in our context, can reduce expected discrimination and encourage job applications.

We also pre-registered four heterogeneity analyses: by expected discrimination, race, skill, and gender. The heterogeneity by expected discrimination is key to confirming our mechanism of interest. For comparisons, we define the group of jobseekers expecting high discrimination as those who expect 50% discrimination or more when predicting the audit study (i.e., at or above median).[16] The race heterogeneity allows us to observe how correlated sources of discrimination interact, by comparing effects among white and non-white jobseekers. The skill heterogeneity could tell us how expected discrimination changes the talent pool available to employers. Finally, the gender heterogeneity could inform us about whether favela males, who are more likely to be gang members, or females react more to expected discrimination. We discuss heterogeneity by expected discrimination together with average treatment effects (since it is our mechanism of interest). As our findings suggest race plays a major in determining the effects of discrimination, we also discuss those in the main text. Appendix A presents the remaining heterogeneity analyses.

## 3.2 Sample Recruitment

**Partners.** To advertise real jobs to participants, we partnered with one of Latin America's largest cosmetics franchise and retail chains. This firm was interested in increasing its pene-

[15] Introducing the Information Experiment was the single major change in our pre-registered plans. We amended our pre-registration, explaining our reasoning, as we introduced this new experiment. See Appendix B for more details on deviations and estimates of treatment effects on measures of application effort and other secondary outcomes listed on the pre-registration, which we do not discuss in the main text for brevity.

[16] This definition pools jobseekers who expect fairly high discrimination rates (e.g., 40%) with those who expect none. Nevertheless, results are similar when considering a cut-off of, for instance, 25%.

tration into favelas and diversity among its workers, allowing us to advertise three entry-level sales jobs.[17] They committed to giving full consideration and fast-tracking promising applicants recruited through our pipeline. We also had the support of several NGOs in each favela. These institutions were extremely important since they had access to local networks and provided feedback on our survey, logistics, and research questions. Crucially NGOs' networks allowed us to hire and train surveyors locally and facilitated obtaining the approval of residents' associations – the relevant political brokers in favelas.

**Sampling.** Surveyors worked door-to-door to identify favela jobseekers who: i) were between 18 and 40 years old, ii) had completed or were in the last year of high school, and iii) were looking for a *new* full-time formal job.[18] This strategy rules out people who were so wary of anti-favela discrimination and outsiders that they gave up on looking for a formal job or answering an academic survey about it. So, in a sense, we might be estimating lower bounds on the effects of expected discrimination. To avoid spillovers (since our randomizations are at the individual level) and maximize privacy, surveyors would interview at most one person per household, one-on-one. Every participant received R$5 (≈1 USD) and was entered into a lottery for R$500. Table A.1 presents sample summary statistics: 62% were recruited in Maré, 30% are male, 22% are white, and the average age is 26. In addition, 25% had never worked before, and 32% reported currently working full- or part-time (most in the informal sector).

**Survey.** There were four blocks of questions. The first block collected general background information and labor market experience. The second block introduced the HR firm as a partner and asked for the jobseeker's permission to share their basic background with the firm. The third block was about skills. The final block was about anti-favela discrimination and expectations about labor market prospects.

**"HR firm"**. After collecting background information, the surveyor introduced a partner HR firm which assisted large companies recruiting in Rio. The surveyor then asked for permission to share the respondent's answers until that point (i.e., basic demographics and job experience) with the HR firm, so the jobseeker could be invited to apply for jobs. We (the researchers) operated this HR firm.

**Deception**. Our choice not to present the HR firm as part of the study was deceptive to the extent that jobseekers could not have anticipated that researchers would observe their interactions with the firm. This was strictly necessary for the design, and the only element of deception in this study. This separation between HR firm and academic researchers served to emulate regular labor market interactions, as research and surveys are commonly linked with local NGOs

[17] These are not strongly gendered jobs: In our study, 37% of males and 44% of non-males applied to the jobs.

[18] See Figure D.1 for photos of in-progress interviews.

in favelas. If the research team directly invited respondents to apply for a job, jobseekers could believe they would receive special treatment. At any rate, the HR firm invited jobseekers to apply for real jobs and indeed acted as a recruitment agent.[19]

After choosing whether to share data with the HR firm, surveyors moved to a block on skills. The block started by asking jobseekers whether they had completed courses or training programs relevant to the job market and then asked for self-ratings on computer and other skills. At the end of this block, participants could take an incentivized one-minute test. The test consisted of answering as many basic algebra questions as possible to receive an extra R$0.25 for each correct answer. We use this math test as one of the three components in our skill measure. The other two components are education (self-reported) and communication skills, which are assessed privately by the surveyor on a Likert scale at the end of the survey. We standardize and average these measures to form an index and classify those above the median as "high-skill".

The fourth and final survey block asked about job market prospects and expected discrimination. Almost one-third of our sample has heard of somebody who did not get (or lost) a job only because they were from a favela, and a similar number report having personally suffered the same. Before initiating the Information Experiment, our survey also included questions on *why* jobseekers believed firms would discriminate against favela residents (mentioned in Section 2.3).

**Measuring Expected Discrimination.** As our main measure of expected discrimination, we incentivized jobseekers to predict the callback rates we would find for each neighborhood in our audit study (Section 2.2), paying an extra R$100 (≈20 USD) to the ten people who got closer to the true estimates. For both Maré and Manguinhos, we used Bonsucesso as the adjacent non-favela neighborhood. For Jacarezinho, we used Maria da Graça since Bonsucesso is not immediately adjacent (see Table A.2 for Census summary statistics for each neighborhood). As our audit study covered only Maré and Bonsucesso, we elicit incentivized predictions for these other neighborhoods by initially stating that we only knew the correct answer for some of the questions. Figure D.2 shows the complete elicitation script.

**Overview.** Surveyors completed 2,392 valid surveys, yielding 2,167 eligible participants – 167 did not share their data with the HR firm, and 61 of those who did provided an invalid phone number. All of those eligible survey participants then took part in the Address Omission Experiment or the Information Experiment, implying that the HR firm invited them to apply for

[19]Our debriefing procedures included (i) carefully debriefing those eventually hired by our partner and (ii) inviting participants who applied for the job for a meeting to discuss the study's findings and the use of their data. For the duration of the study, we kept a website and a contact email running in case any jobseeker searched online for the HR firm.

the jobs (Figure 1, arrows leading from the survey stage on the left to the application stage). We introduced the Information Experiment as we phased out the Address Omission Experiment.[20] All jobseekers who completed the application form and attended the interview participated in the Interview Experiment (Figure 1, arrows from the application invites to the interview office).

## 3.3 Address Omission Experiment (N=1,303)

As the door-to-door survey proceeded, we organized the applicants in batches for the Address Omission Experiment. Every few days, the HR firm sent personalized invitations to apply via WhatsApp to a batch of new survey respondents. Batch sizes varied from 50 to 117 to accommodate logistical constraints. Almost all jobseekers received invitations to apply up to ten days after answering the door-to-door survey.

**Treatment.** We randomized expected address visibility at the application stage. There were three experimental conditions: *Address Omission*, *Status Quo*, and *Known Address*. Applicants in *Address Omission* received a WhatsApp message from the HR firm inviting them to apply and saying that a home address **is not** needed for applying. Those in *Status Quo* and *Known Address* received a message saying an address **is** needed (see below). The difference between the two conditions in which address is needed is that in *Status Quo* the jobseeker fills in the address (the common practice in our context), allowing us to observe how often applicants obfuscate their real addresses. In *Known Address*, the form stated that the research team has already shared the jobseeker's address (besides name and phone number), so applicants just need to double-check it. Hence, in *Known Address*, we make sure that obfuscation is not possible, allowing us to test whether making one's favela address fully visible affects application behavior (see Figure D.4 for the differences across forms).

**WhatsApp Invite Message**:

*Hi [NAME], how are you? This is Vanessa from **SAM HR**. I'm contacting you because you are one of the people in our database who fits the requirements for some of our vacancies. In addition to salary, these jobs offer benefits such as daycare and health insurance.*

*You have been selected to participate in one of our streamlined processes! In this stage, you need to provide your **education and any courses or experiences. Your home address is [NOT/ALSO] required.***

[20] There was an overlap of 184 participants between the two pre-interview experiments during the phase-out. For simplicity, the main text presents results for the non-overlapping samples. See Appendix B for details and results including the overlapping sample, which are very similar to the ones in the main text. Furthermore, as we launched the fieldwork in one favela at a time, the samples for each of the pre-interview studies differ with respect to their favela of origin and some other covariates (see Table A.6 for a comparison)

*It takes just **5 minutes**! Personal link: go.samrh.com/lyhW1DS5*

The application form started with a brief description of three full-time jobs: i) (in-store) Sales Consultant, ii) Direct Sales Promoter, and iii) Direct Sales Supervisor (see Figure D.3 for job descriptions). Then, it asked about the jobseeker's name, phone number, and address – except in *Address Omission*. Then, it proceeded as a standard application form, asking about job experiences, skills, and motivations. Finally, the jobseeker declared their availability for an interview.

**Outcomes.** Our main pre-registered outcomes are whether the jobseeker completes the online application form and attends the job interview, which typically happened within ten days of each other. While not an experimental outcome per se, we also calculate the address obfuscation rate for those in the *Status Quo* arm. We consider that a jobseeker has obfuscated their address if the declared neighborhood is neither a favela nor the postal service neighborhood of the jobseeker's real address (recorded by the surveyor in the door-to-door survey).

**Conceptualization.** As experimental conditions differed only within the job application procedure, it is reasonable to assume that this treatment only affects the expected value of applying to the jobs in the experiment. We can think of a jobseeker that assigns value $V$, success probability $p$, and has an application cost $c$ to such a job, applying if $pV - c > 0$ (normalizing the outside option to zero). Then, the treatment shifts $pV$, since the differences in the application procedure are minor. For instance, in *Address Omission* perceived $pV$ might be larger both because a jobseeker perceives a higher success probability and because they will be less likely to suffer address-based discrimination on the job.

## 3.4 Information Experiment (N=690)

The Address Omission Experiment ran until May 2023. As we phased it out, we embedded the Information Experiment in our door-to-door survey to test whether providing information about market-level discrimination, typically overestimated by favela jobseekers, can encourage job applications.

The information treatment in this experiment consists of randomly revealing the findings of our audit study, and has two main advantages over the strategy of lowering expected address visibility. First, it is arguably more policy-relevant, since it does not rely on regulating employers and can be transferred to any context in which it is possible to benchmark discrimination, regardless of whether the source of discrimination is visible or not. Second, providing market-level statistics in the survey, detached from the application procedure, sidesteps confounders related to a change in the application procedure also changing how the HR firm is perceived

(in ways that are unrelated to expected discrimination). Such confounders are the reason why studies randomizing the language used in job ads like Del Carpio and Fujiwara (2023), Burn et al. (2022), or whether AI is used to review applications, like Avery et al. (2023), can not sharply identify the effects of expected discrimination. For instance, in Del Carpio and Fujiwara (2023), gender-neutral language could imply less gender-based discrimination, but it also suggested better work-life balance, which can appeal differently to males and females.

**Treatment.** We randomized participants into three treatment arms: (i) *No Info*, in which no information was displayed, (ii) *Favela Info*, in which we disclosed only the favela's callback rate (19.3%, from our audit study), and (iii) *Full Info*, in which we showed both the favela and non-favela callback rates (19.3% and 19.6%) – thus revealing that we found no discrimination in callback rates. *Full Info* was designed as our main treatment decreasing expected anti-favela discrimination, but it also reveals the favela callback level. Hence, we included the *Favela Info* condition as an alternative control, holding the knowledge of the favela callback level constant. The information was delivered during our door-to-door survey, after eliciting predictions of the audit study callback rates. Figure 3 for the graphs the surveyors used to convey the treatment.

Similar to the Address Omission Experiment, the HR company invited respondents to apply for our partner's jobs. There were only two differences. First, to emulate the most realistic application procedure, we only use *Status Quo* procedures (i.e., we ask applicants to provide their home address). Second, since there was no randomization in the application procedure, we could decrease the batch size and invite jobseekers to apply more often, one to four days after they answered the door-to-door survey.

**Endline survey.** We conducted an endline survey over WhatsApp to check whether the belief shift caused by the Information Experiment persisted and to collect a self-report on the number of job applications sent since the door-to-door survey. Participants were contacted two weeks after baseline. To maximize responses, participants entered a lottery for R$200 (≈40USD) and we only asked multiple-choice questions.

**Outcomes.** Besides the application progress outcomes used in the Address Omission Experiment, we also pre-registered as main outcomes the self-reported number of applications sent after two weeks, address obfuscation, and immediate belief updates. For the latter, we incentivized predictions of what callback rates the partner HR firm would implement in each neighborhood. There is no ground truth for these callback rates, since we operated the HR firm and invited only favela jobseekers to apply. We incentivized their elicitation by including these questions together with those eliciting beliefs about our audit study callback rates; the surveyor introduced this set of questions with a statement clarifying that we only knew the answer to *some* of the questions.

**Conceptualization.** Similar to the experimental conditions in the Address Omission Experiment, we may think of the information treatments as shifting the expected callback probability and job value – as jobseekers might value formal job offers more once they learn that they were too pessimistic about anti-favela discrimination. The main difference is that the information is relevant for *all* jobs. In this case, shifting the expected callback level $p$ can have a non-monotonic effect on application rates. Intuitively, at a low $p$, an increase in $p$ makes a marginal application much more valuable, so it is worth applying to more jobs. But, if you already expect to receive "enough" callbacks, an increase in $p$ allows you to decrease the number of costly applications while still getting enough callbacks.[21] We designed the *Favela Info* condition to fix $p$, and *Full Info* could be thought of as decreasing the expected *gap* in treatment (e.g., favela residents may now expect to receive impartial treatment during the application process and on the job), increasing the value of all matches after fixing $p$. Obfuscation rates should increase in expected discrimination and decrease in $p$.

## 3.5 Interview Experiment (N=422)

The HR firm invited all jobseekers who completed the application form for a job interview in an office in Downtown Rio. Attendees received a R$25 (≈5 USD) transport subsidy, enough to cover fares. We rented a reception desk and meeting rooms in a co-working space, so applicants first had to go through the building's reception and then arrive at the co-working floor. Interviews took ten to fifteen minutes each, and we scheduled them with enough of a gap so that jobseekers would rarely, if ever, meet or interact at the premises. See photos in Appendix D.5.

**Interview.** We hired an experienced HR consultant to revise our interview script and train our two interviewers. The script contained standard interview questions for sales jobs, including questions about strengths, weaknesses, comparative advantages, past work experiences, and an activity where the applicant had to pick an item and provide a sales pitch (see Appendix D.3 for details). Interviewers were instructed to stick to the script and act the same towards all candidates.

**Treatment.** We randomize expected address visibility at the job interview. A receptionist greeted arriving candidates and asked to confirm their name, date of birth, and address, and told them to wait. Moments later, the receptionist told the jobseeker that the interviewer was

[21] To see that, let $n$ be the number of applications to be submitted, $p$ be expected callback probability, $c$ a constant marginal cost and the callback value $V(n,p)$ be such that $V_n > 0$ and $V_{nn} < 0$. If the jobseeker maximizes $V(n,p) - nc$ finding an internal solution, the inverse function theorem yields $\frac{\partial n^*}{\partial p} = -\frac{V_{np}(n^*,p)}{V_{nn}(n^*,p)}$, which has the same sign as $V_{np}(n^*,p)$. Taking, for instance, a jobseeker that only cares about getting the first callback, i.e., $V(n,p) = 1-(1-p)^n$, then one can have $V_{np}(n^*,p) > 0$ for low $p$ and $V_{np}(n^*,p) < 0$ for high $p$.

ready, and, to keep the process objective, the interviewer "will only know your name" (*Name-Only* condition) or "will only know your name and address" (*Name-and-Address*). Hence, the conditions differed by two words only: "and address". Interviewers were blind to the whole procedure until the end of all interviews, so any effects on the interview must initiate with the candidate. Later, we debriefed the interviewers both to learn their impressions and to avoid participant deception – i.e., the receptionist's statement was ambiguous about when the interviewer would learn about the addresses. Note that our design rules out self-signaling mechanisms (e.g., that when a person is reminded of their address, they lose confidence in their abilities) since all candidates are asked to confirm the address before treatment.

**Outcomes.** The interviewer evaluated candidates immediately after each interview, and interviewees filled out a form with self-assessment questions at the reception desk before receiving the transport subsidy. Interviewers coded, on 0–10 scales, i) how well the interviewee performed overall, ii) how nervous the interviewee was (reverse-coded as calmness), and iii) how professionally the interviewee behaved. Interviewees filled out self-assessments for the same three dimensions. We construct z-scores for each of the six dimensions by normalizing the scores by the mean and standard deviation of those in the *Name-and-Address* condition. For the interviewer-assessed dimensions, we normalize interviewer-wise to account for fixed effects and dispersion differences across interviewers.

To maximize statistical power and reduce the risk of multiple hypothesis testing, we construct an inverse-covariance-weighted index of impressions for the interviewers and for the interviewees (Anderson, 2008). As our primary aggregate performance measure, we average the two. While this averaging mixes impressions of different relevances – i.e., the interviewer's impressions matter for the jobs at hand, while the candidate's impressions matter for their future beliefs, say about whether they should apply to similar jobs again –, it allows us to extract a more accurate signal. We also present broken-down estimates.

**Conceptualization.** The treatment shifts the candidate's second-order beliefs about how the interviewer might see them. Candidates might respond to that strategically or involuntarily. For instance, thinking that the interviewer knows one's address might generate feelings of unfairness due to stereotyping, leading to automatic stress responses (Berger and Sarnyai, 2015; Schmader et al., 2008). Such stress, along with the high stakes of a job interview, could lead to choking under pressure, a phenomenon documented in multiple sports competitions (Böheim et al., 2019; Harb-Wu and Krumer, 2019; Teeselink et al., 2020). If a candidate simply believes their performance will be heavily discounted due to discrimination (i.e., lower returns to effort), the optimal response might be to try harder to impress, which can also lead to stress, or to disengage and reduce effort if the barrier is perceived to be insurmountable. Depending on how

interviewers deal with candidate's reactions, an initial effect may compound or dissipate: For instance, an interviewer could try to calm down an interviewee who was too nervous, dissipating the effects of an increase in stress.

### 3.6 Randomization, Balance, and Estimation

Randomization for the Address Omission Experiment proceeded in batches. We stratified by expected discrimination (batch-wise), with equal probability of each treatment within and across strata. We proceeded similarly for the Interview Experiment, randomizing in batches after jobseekers completed the application form. The offline survey app on the surveyors' tablets implemented the randomization for the Information Experiment on the spot, also with equal probabilities. Randomizations were independent across experiments.

Tables A.3, A.4, and A.5 display randomization balance checks. Given the necessity of randomizing batch-wise (for the Address Omission Experiment and Interview Experiment) or on the spot (for the Information Experiment), we could not stratify on multiple variables or at all in the latter case. Hence, we see some imbalances. Out of the 45 comparisons to the "control" groups in tables A.3, A.4, and A.5, one is significant at the 1% level, three at the 5% level, and four at the 10% level, which is not far from what one would expect from randomness. Results with controls are very similar; versions of the main tables and figures using double-lasso selected controls can be found in Appendix A.

To test for the effect of expected address visibility in the application procedure and plot the average outcomes of each experimental group, we estimate a saturated model:

$$y_i = \beta_{SQ} Status\ Quo_i + \beta_{KA} Known\ Address_i + \beta_{AO} Address\ Omission_i + \varepsilon_i \quad (1)$$

where $y_i \in \{0, 100\}$ (to yield percentages), and each coefficient captures the outcome level for each treatment group. For the Information Experiment, we use the same specification as in Equation 1 (i.e., one indicator for each treatment).

Our interview performance outcomes are normalized z-scores, or their inverse-covariance-weighted averages (Anderson, 2008). Hence, only differences across groups are informative, and we simply regress each outcome on a treatment indicator for *Name-Only* and a constant (i.e., our estimates show the effect of reducing expected discrimination on interview performance).

We estimate robust standard errors using the HC3 variance-covariance matrix estimator (Long and Ervin, 2000), which has better coverage in smaller samples.

# 4 Results

## 4.1 *Address Omission Experiment*

Expected address visibility does not affect average job application rates (left panel, Figure 4). If expected address visibility leads to expected discrimination, discouraging applications, *Address Omission* should have the highest application rate, and *Known Address* the lowest. Instead, we see little variation across conditions: form completion rates hover from 41% to 45% and interview show-up rates are just below or at 20%. The p-values for equality tests between any two conditions for application outcomes are all above the conventional significance thresholds. We see a similar pattern even when conditioning on those expecting high discrimination at baseline (right panel in Figure 4), providing no evidence that expected discrimination affects average application rates.

At the same time, we see evidence that jobseekers expected discrimination when applying, and that our treatments shifted expected address visibility to some degree. In *Status Quo*, applicants were free to obfuscate their addresses, and we see 45% of applicants doing so. This suggests that jobseekers indeed expected some address-based discrimination. At the same time, the *Known Address* treatment was effective in preventing obfuscation, since no jobseekers in that condition even tried to provide a "corrected" address with an obfuscated neighborhood (which they could do in the application form). Hence, we have some evidence that our manipulations worked as intended, shifting expected address visibility. Further evidence, discussed next, suggests that the average null effects are due to (i) lowering expected address visibility not always lowering expected discrimination, and (ii) application decisions not always being elastic to expected discrimination.

Racial heterogeneity in treatment effects suggests that our treatments meaningfully decreased expected discrimination only for white jobseekers. We find effects of *Address Omission* on show-up only in the white subsample (Section 4.4). This suggests that lowering expected address visibility lowers expected discrimination for white jobseekers, who can more easily pass as non-favela residents (since the white population is a majority outside but a minority inside favelas). Non-white jobseekers might expect to be stereotyped once their race becomes visible, and might expect discrimination either way. Hence, lowering expected address visibility does not seem to be enough to lower expected discrimination for the non-white majority of our sample.

Two other pieces of evidence suggest that application decisions are not always elastic to expected discrimination. First two exploratory questions later introduced in the door-to-door survey suggest that jobseekers try to ignore discrimination when searching: (i) 70% of respon-

dents (N=670) agree that one should apply for all possible postings to do well in the labor market, and (ii) 80% agree that to do well, one should not ruminate about employer discrimination. If a significant share of jobseekers ignores hints about discrimination, this should dull any effects of expected address visibility. Second, we also find null average effects on applications in our Information Experiment, reaffirming a small elasticity of average application rates to expected discrimination.[22]

## 4.2 Information Experiment

In this experiment, we find that the information treatments successfully shift expected discrimination in callbacks by correcting the beliefs about the callback rate for favela and non-favela neighborhood. Nevertheless, we do not find an effect on average application rates.

The information treatments successfully "corrects" beliefs about the callback rate that the HR would implement in favelas: Those who initially under- and over-estimate callback converge in their posterior beliefs. Learning *Favela Info*, i.e., that the callback rate for the favela was 19.3%, does not change the *average* expected callback rate for jobseekers' own neighborhoods (Figure 5), but that is due to the belief convergence. For instance, considering only those who initially overestimate the favela callback rate, the average expected callback rate goes from 41% in *No Info* to 37% in *Favela Info* (p=0.09 for the difference, see Figure A.4 for effects on under- and overestimators of the favela callback rate). When jobseekers learn both callback rates in *Full Info*, underestimators become even more optimistic about their own favela callback rates, and overestimators become more pessimistic. *Full Info* causes statistically significant corrections in expected own-neighborhood callback rates both for under- and overestimators, indicating that jobseekers use favela *and* non-favela information to update about favela callback rates.

Considering beliefs about the adjacent non-favela callback rate, we also see that jobseekers use the information on both favela and non-favela callback rates to update. Since 92% of the sample overestimates the non-favela callback rate, that update is evident even when looking at the full sample in the top-right of Figure 5. Hence, both *Favela Info* and *Full Info* decrease expected discrimination, and the decrease is larger for *Full Info* since it provides more information. The average posterior discrimination rate for the *No Info*, *Favela Info*, and *Full Info* groups are, respectively, 35%, 28%, and 15%, with group differences significant at the 5% or 1% level.

[22]The manipulations in the application procedure might have generated other forms of employer signaling, offsetting the effects of expected discrimination. For instance, *Known Address* might have led some jobseekers to believe the HR firm would favor favela jobseekers, since they were invited *despite* the firm knowing their addresses. Nevertheless, we also find an average null effect on applications in the Information Experiment, which holds the job application procedure fixed, sidestepping such issues.

The top-right graph in Figure 5 shows a similar pattern for the subsample that expected high discrimination at baseline.

Nevertheless, we do not find statistically significant differences in application rates across information conditions, even for the high expected discrimination group (Figure 5, bottom row). In the same figure, considering obfuscation rates, we see the hypothesized pattern, (i.e., highest obfuscation in the *Favela Info* condition, when most people learn they were too optimistic about their neighborhood's callback rate). That said, the only (marginally) statistically significant difference in obfuscation rates is between *No Info* and *Favela Info*, conditional on expecting high discrimination. In that case, those receiving *Favela Info* obfuscate 13% of the time, more than double the share in *No Info* (p=0.1).[23] Hence, correcting expected anti-favela discrimination fails to encourage applications, and we have only weak evidence that it can affect obfuscation rates.

Our endline survey generally confirms the findings above. There was no differential attrition in participation (column (1) in Table 1), so sample selection into the endline should not be an issue. There is evidence that the decrease in expected discrimination caused by *Full Info* persists for at least two weeks, at least in comparison with *Favela Info* (p=0.06, see column (2) in Table 1). In a pooled comparison of *Full Info* against the two other arms (not shown in the table), we see p=0.09. Nevertheless, we still see null effects on (self-reported) application rates after two weeks.

## 4.3 Interview Experiment

Even if expected discrimination does not affect average application rates, it could still damage interview performance since there are many differences between making application decisions and controlling behavior in interviews. For instance, during the interview, the jobseeker must quickly adjust in response to the interviewer, who directly judges performance. This makes the interview behavior very different from the "cold" decision of whether to apply.

Decreasing expected address visibility, i.e., hearing that the interviewer will only know the candidate's name, increases the average aggregate performance index by 0.13SD (Figure 6, first estimate). That index averages the interviewer's and the candidate's opinions, and, when we break it up by those two components, we see that the increase in the self-assessed index (0.17SD) is statistically significant at the 1% level while the increase in the interviewer-assessed

[23] If we focus on those who underestimate the favela callback rate, then learning *Full Info* indeed decreases obfuscation, consistent with strategic behavior (see Figure A.4, bottom left). If we assume that the only way our treatments affect applications is through beliefs about callback rates, we can estimate the effect of those beliefs with two-stage least squares. Taking that approach lets us exploit variation in how both information treatments affect callback beliefs for favela and non-favela neighborhoods, but leads to similar takeaways (see Table A.9).

index (0.09SD) is not (p=0.28). While this suggests that effects concentrate on self-assessments, we cannot reject that they are equal (p=0.33). Hence, we have evidence that expected discrimination is, on average, detrimental to candidates' self-assessments, which may determine their beliefs about getting this job or a similar one in the future, but we are less confident about the effects on average interviewers' opinions, which matter directly for hiring decisions. Notably, reducing expected address visibility appears beneficial across the board, as estimates of the average effects on each of the six index components always go in the same direction (Figure 6, gray circles).

Breaking up the estimates by the groups who expect higher or lower anti-favela discrimination at baseline gives stronger evidence for a self-fulfilling prophecy powered by expected discrimination. For the group expecting high discrimination at baseline we estimate statistically significant increases in performance over 0.2SD in response to *Name-Only*, in both the candidate's and the interviewer's points-of-view (red diamonds in Figure 6, $p<0.05$ for both). While these effects are large for a treatment changing only two words said by the receptionist, they are not unreasonable in magnitude, since having some college education is correlated with a 0.55SD increase in performance. For the group expecting lower discrimination to begin with, we cannot reject the null of no effects at the 5% level for any index or their components (orange squares, Figure 6). Hence expecting to have a visible address leads to worse performance among those who expect high discrimination from the start, even when interviewers do not have the information to discriminate. Further, the size of the differential effect on people who expect high discrimination is stable when we allow for other heterogeneity dimensions (Table A.7), suggesting that other characteristics correlated with expected discrimination are not responsible for the observed effect heterogeneity.

Expected discrimination also seems to hurt interviewer-assessed performance at the right tail of the performance distribution among those who expect high discrimination, so it can generate gaps in the composition of final hires.[24] Specifically, we can show that among those who expect high discrimination, there is a drop in the share of candidates above different cut-offs in the performance distribution. For instance, take as a benchmark the cut-off corresponding to the 90$^{th}$ percentile of interviewer-assessed performance index among those in *Name-Only* (mimicking a world where firms are blind to address). Then, among candidates expecting high discrimination, there are only half as many above that cut-off in *Name-and-Addres* than in *Name-Only* (p=0.09, see Figure 7 for the empirical CDFs). Put another way, among those who expect high discrimination, *Name-and-Address* ejects about half of the candidates from the top

[24]The ideal exercise here would use data on who the cosmetics firm actually hired. Nevertheless, we only received sparse and incomplete information on which candidates were further contacted and hired, making this exercise unfeasible.

10% of the distribution. No candidate who expected high discrimination in *Name-and-Address* reached the top 1%.

While the Interview Experiment was not designed to identify the mechanisms through which expected discrimination affects interview performance, three empirical patterns suggest that a mix of stress and heightened stakes lead candidates to choke under pressure and do worse when their addresses are visible. First, since there is little pressure when a jobseeker is deciding whether to apply or not, choking under pressure is consistent with our observation of effects being more pronounced in the interview rather than at the job application stage.

The second empirical pattern consistent with a role for choking under pressure is how the heterogeneous effects by expected discrimination level are distributed across the six index components. We see that (i) the effect of *Name-Only* on self-assessed calmness among those who expect high discrimination is 0.4SD larger than the effect on those who expect lower discrimination at baseline (p=0.04), (ii) there is no effect heterogeneity in the other two types of self-assessed perceptions, and, (iii) from the point of view of the interviewer, effects on all three component concentrate in the high expected discrimination group. These suggest that increasing expected discrimination leads to a stress response, which then reflects badly on how the interviewer assesses the candidate.[25] While not ruled out, a mechanism in which discrimination reduces the perceived returns to effort during interviews would not necessarily generate such increases in nervousness when addresses are visible in the high-expected discrimination group. A similar argument applies to motivated reasoning.

Third, we see some evidence that jobseekers might find it difficult to be strategic at the office, consistent with stress making it hard for candidates to perform. For instance, if we look at jobseekers who went through a *Status Quo* application process (manually filling their addresses in the online form) and made it to the interview, the same jobseekers are 20% (5.7 p.p.) less likely to obfuscate their addresses at the interview office ($p<0.01$). Also, we see that the index component that is most affected (on average) by *Name-Only* is the self-perceived measure of professional behavior, suggesting that jobseekers do not self-regulate their behaviors as much when they believe their address is visible. While a mechanism involving candidates trying to overcompensate for their address visibility could increase nervousness, it would not explain why we see candidates considering their behavior *less* professional when their addresses are visible.

[25]Note that the effect on nervousness could have gone in the opposite direction if *Name-Only* led to a higher cognitive load among those who try to pass as non-favela residents. That is, the pressure to be careful and not reveal any hints you are actually from the favela could overpower the nervousness induced by expecting discrimination, but we see the opposite.

## 4.4 Correlated Sources of Discrimination: Race and Address

Race and favela residence are correlated stigmas (sources of discrimination), and jobseekers understand that. In Rio, address is highly predictive of race, as white people are a majority of the population outside favelas but only one-third of the favela population. As such, an employer who discriminates racially could use address information to rule out potentially non-white candidates. Favela jobseekers anticipate this employer behavior, as 68% of our survey respondents mentioned racism as an important reason why employers discriminate against favela residents. Then, it is reasonable to expect that manipulating expected address discrimination would affect white and non-white jobseekers differently. For instance, reducing expected address visibility might encourage white jobseekers more than non-white jobseekers, since a white jobseeker of an unknown address is half as likely to reside in a favela than a non-white one, and the latter may expect racial discrimination either way.

Table 2 summarizes how the treatments reducing expected discrimination differentially affected white and non-white jobseekers across the experiments. First, note that being told that the interviewer would only know the candidate's name increases the aggregate performance index only for white jobseekers (see column (1); p=0.09 when testing the difference in the effects). This is consistent with the idea of white jobseekers benefiting from passing as non-favela residents, and non-white jobseekers expecting discrimination either way, as mentioned above.[26]

Next in Table 2, we summarize the racially heterogeneous effects of removing the need to declare address in the Address Omission Experiment – against the other two conditions pooled to increase power and simplify interpretation.[27] The most relevant pattern is the statistically significant difference in how removing the need to declare address affects white and non-white jobseekers' application rates (p=0.01) and show-up rates (p=0.05) – last row, columns (2) and (3). Consistent with the story above, reduced expected address visibility encourages white jobseekers relatively more. While there is a statistically significant *negative* effect of reducing expected address visibility on the application rate of non-white jobseekers, that negative effect fades when we consider the interview attendance outcome, which is more economically relevant. As the point estimates of the effects go in different directions for the white and non-white subsamples, and as the white subsample is relatively smaller, we end up with the average null effects discussed in Section 4.1.

Finally, in the last two columns in Table 2, we show heterogeneous effects of telling people that we found no market-level address-based discrimination in callbacks – again, comparing

[26] The racial heterogeneity in interview performance is more accentuated in the interviewer-assessed component of the index (see Table A.7).

[27] We pre-registered this comparison as a way to increase power.

the main treatment against the two other conditions pooled to improve power. Here, the pattern flips: white jobseekers become relatively discouraged, applying and showing up for interviews relatively less (p=0.11 and p=0.02) when told about the negligible level of discrimination in callbacks. A natural interpretation for non-white jobseekers becoming relatively encouraged could be that observing no address-based discrimination also leads to decreasing expected *racial* discrimination (because it is evidence that firms do not use address, which strongly predicts race, as a filtering mechanism). For white jobseekers, while the audit results suggest that they might expect lower anti-favela discrimination, low anti-favela discrimination can also imply (i) lower returns for passing as a non-favela resident and (ii) increased expected competition with non-white applicants. Considering interview show-up, we see evidence of strong discouragement among white jobseekers, but not much encouragement among non-white jobseekers.

One major concern with interpreting the race-heterogeneous treatment effects is that race could be correlated with other factors predictive of effect heterogeneity. Indeed, white jobseekers in our sample tend to be more skilled, younger, and have less experience in the labor market (but predict the same amount of discrimination in the audit, see Table A.10). Nevertheless, these observable racial differences cannot explain the results: the differential effects associated with being white are similar in direction and magnitude when we include interactions with other covariates (Table A.11).

The race heterogeneity in treatment effects across experiments is also robust to multiple hypothesis testing. Considering that we test four different types of effect heterogeneities (by expected discrimination, race, skill level, and gender), the Bonferroni-corrected p-value for there being a differential effect on white jobseekers across *all* columns of Table 2 is 0.033. Considering the experiments lowering expected address visibility (the Interview Experiment and the Address Omission Experiment), the Bonferroni-corrected p-value for the differential effect is 0.075, and for the Information Experiment, it is 0.215. Hence, the role of race as a mediator of the effects of expected discrimination does not seem to be due to chance when testing multiple heterogeneities, except perhaps in the Information Experiment.[28]

[28] We conduct these tests by stacking data from the different experiments and outcomes shown in Table 2, then interacting the treatment, a dummy for white, and a set of dummies indicating which outcome and experiment is being considered. Then, we calculate the F-statistic associated with the differential effects for whites being zero throughout, with a variance-covariance matrix clustered at the individual level. The Bonferroni correction simply multiplies the associated p-value by four (the number of pre-registered heterogeneities).

# 5 Discussion

**Relevance of the effects on self-assessment**. Even if the effects of stigma visibility were restricted to self-assessment, they may still have important implications. For instance, after a negative interview experience, jobseekers might be reticent to apply for other jobs that require formal interviews. Also, note that in "regular" interviews, discriminatory behavior among interviewers can exacerbate any effects of expected discrimination – in our interviews, that channel was shut down since interviewers knew only names and stayed on script. Finally, even if we disregard discouragement effects or interview performance completely, expected discrimination can undermine jobseekers' psychological welfare (Pascoe and Smart Richman, 2009; Schmitt et al., 2014), as we show that it leads to negative interview experiences.

**Policy implications**. Our experiments have implications for policies that restrict the information recruiters may access. First, consider policies that reduce stigma visibility at the callback stage, such as résumé anonymization or forbidding employers from requesting some specific information. Our results suggest we should not expect such policies to change applicant behavior across the board. Our analysis of the interaction between race and address visibility indicates that such policies might only encourage applications for groups who can keep on hiding their stigmas later on, as was the case with white jobseekers in our sample. Since there is also evidence that such procedures can backfire when they lead recruiters to make decisions with incomplete information (e.g., Behaghel et al. 2015; Doleac and Hansen 2020), our results suggest these policies should be treated with even more caution.

On the other hand, there are reasons for being optimistic about "blind" interviews (as in Goldin and Rouse 2000), since we show evidence that simply expecting a blind procedure can improve performance. Our study highlights the importance of jobseekers' *second-order* beliefs. If jobseekers are made aware of credible blinding or anti-discrimination policies, those who expected discrimination might perform better in interviews. Furthermore, even if a policy hides one stigma, it may fail to have an effect because another stigma may act as a substitute – as we show in Section 4.4. Hence, policies that hide all stigmas during interviews (e.g., audio-only, text, or metaverse interviews) could dominate alternatives. AI-intermediated candidate selection is also a promising alternative, as shown in Avery et al. (2023).

If choking under pressure, stress, and difficulty in managing behavior are to blame for the observed effects on interview performance, there is also a case to be made for interventions helping jobseekers to signal their skills more confidently. Abebe et al. (2021) and Carranza et al. (2020) show that giving jobseekers an easy way to show their skills (e.g., with an informative letter of recommendation) has positive effects on employment. Access to such sig-

naling mechanisms may reduce the weight put on interview performance and give jobseekers more confidence. Interventions focused on controlling behavior and decreasing anxiety, such as cognitive-behavioral therapy, could also counter such negative effects in interviews.

**Intersectionality**. Our results show that interventions ignoring race as a correlated source of discrimination can lead to heterogeneous and unintended results, such as increasing racial inequalities. This resonates with the idea that overlapping sources of discrimination compound in ways that simple additive effects cannot summarize, and that first-best policies are personalized (Carvalho et al., 2022; Crenshaw, 1989). Nevertheless, our findings also suggest that predicting this intersectionality (the more personalized policy effects) is difficult. For instance, a policymaker could expect that hiding address at the interview would benefit non-white more than white jobseekers, which would happen if jobseekers thought they could overcome one but not two "strikes" against them – but we find the opposite. Another implication of our findings is that the status quo policy of asking for address at the application stage is damaging only for white jobseekers, which might go against many's intuitions. Hence, an information-constrained policymaker could justifiably pick race-blind policies to address anti-favela expected discrimination as a first-order approximation and then iterate based on its results.

**Implications for firms**. Firms may also play a role in decreasing expected discrimination and creating an environment where they can extract a better signal from interviews. For instance, making the candidate-selection process more transparent and credibly committing to non-discriminatory practices (such as diversity, equity, and inclusion). While firms need to consider the trade-offs involved in adopting these policies (e.g., a firm might need to hire staff to develop and implement such policies), our evidence on interview performance suggests that such policies may help firms hire the best talent.

# 6 Conclusion

This paper provides evidence that expected discrimination can exacerbate the impacts of employer discrimination on jobseekers and work as a self-fulfilling prophecy in job interviews, potentially contributing to labor market inequalities observed in administrative data. We document a novel mechanism through which expected discrimination can act as a self-fulfilling prophecy: By hurting interview performance, instead of lowering returns to human capital investment Coate and Loury 1993 or on-the-job discrimination Glover et al. 2017. Also, our findings that different policies tackling expected anti-favela discrimination generate race-heterogeneous effects suggest that policymakers should be mindful of intersectionalities.

Given the relevance of the topic for firms and policymakers, we see an avenue for future

research aiming to understand precisely why expected discrimination is (more) relevant at the interview stage. Our results suggest that choking under pressure might be behind the negative effects on interview performance, and experiments varying pressure, or whether the interview has a face-to-face element, could shed light on mechanisms. Moreover, since many institutions have become committed to diversity, equity, and inclusion (DEI) in recent years (Pew Research, 2021; Fath, 2023), an immediate question is whether making such public commitments can indeed decrease jobseekers' expected discrimination regarding those firms. If DEI commitments indeed remove a handicap faced by jobseekers who anticipate discrimination and help recruiters in talent identification, they could become more attractive to a broader range of firms.

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Table 1: Information Does Not Affect Application Rates at Endline

| | (1) Responded to endline (0/1) | (2) Exp. discrimination (categorical, 1-4) | (3) # of sent apps (categorical, 1-4) |
|---|---|---|---|
| Favela Info | 0.02 | 0.06 | -0.02 |
| | (0.05) | (0.10) | (0.13) |
| Full Info | 0.02 | -0.12 | 0.02 |
| | (0.05) | (0.10) | (0.14) |
| Observations | 690 | 389 | 389 |
| Controls | No | No | No |
| *No Info* Mean | 0.6 | 2.3 | 2.5 |
| Favela=Full $p$ | 0.96 | 0.06 | 0.76 |

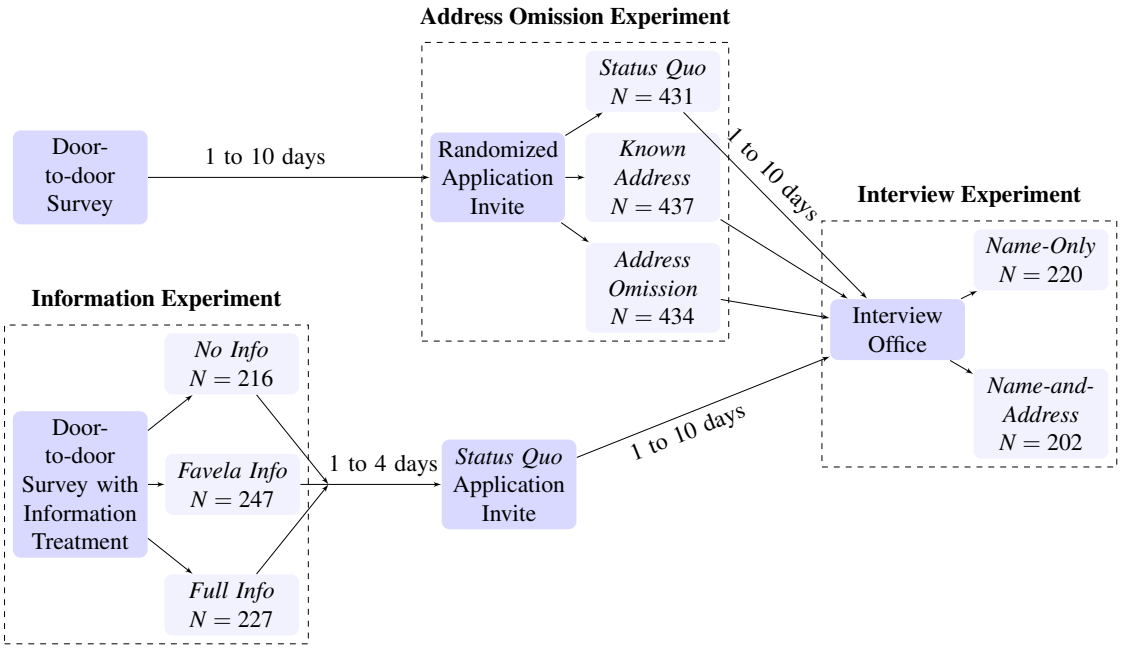
*Note*: Information Experiment treatment effects on endline survey outcomes. The outcome in column (1) is a dummy for responding the endline survey. The outcome in column (2) takes values from one to four, coding for believing that a favela jobseeker would [NOT suffer=1/suffer A BIT more=2/ suffer A LOT more=3/suffer EXTREMELY more=4] discrimination than someone from the adjacent non-favela when applying to jobs. The outcome in column (3) equals 1 if the jobseeker applied for zero jobs, 2 if applied for a single job, 3 if applied from two to five, and 4 if applied for more jobs than that over the last two weeks. Robust standard errors are shown in parentheses.

Table 2: Race Moderates the Effects of Varying Expected Address Discrimination

| | (1) Agg. Performance Index (SDs) | (2) Applied (%) | (3) Showed Up (%) | (4) Applied (%) | (5) Showed Up (%) |
|---|---|---|---|---|---|
| $\downarrow \mathbb{E}$[Disc.] $\times$ white | 0.31** | 9.63 | 10.24** | -9.37 | -15.26** |
| | (0.13) | (6.05) | (5.19) | (8.71) | (6.25) |
| $\downarrow \mathbb{E}$[Disc.] $\times$ non-white | 0.07 | -7.21** | -1.16 | 6.43 | 1.90 |
| | (0.07) | (3.30) | (2.56) | (4.57) | (3.73) |
| White jobseeker (0/1) | -0.05 | -4.30 | -0.49 | 6.34 | 5.70 |
| | (0.10) | (4.06) | (3.15) | (5.78) | (4.96) |
| Observations | 422 | 1303 | 1303 | 690 | 690 |
| Treatment | $\downarrow \mathbb{E}$[address visibility] | $\downarrow \mathbb{E}$[address visibility] | $\downarrow \mathbb{E}$[address visibility] | $\downarrow \mathbb{E}$[mkt-level disc.] | $\downarrow \mathbb{E}$[mkt-level disc.] |
| Control Average | -0.0 | 44.2 | 18.2 | 42.3 | 20.7 |
| White=non-white p-value | 0.09 | 0.01 | 0.05 | 0.11 | 0.02 |

*Note*: $\downarrow \mathbb{E}[Disc.]$ stands for a dummy for the experimental condition that in principle should reduce expected discrimination the most in each experiment. That is, $\downarrow \mathbb{E}[Disc.]$ is *Name-Only* in column (1), *Address Omission* in columns (2) and (3), and *Full Info* in columns (4) and (5). In columns (2) to (5), the omitted categories include both other experimental conditions in each experiment. * p<0.1, ** p<0.05.

Figure 1: Experimental Design



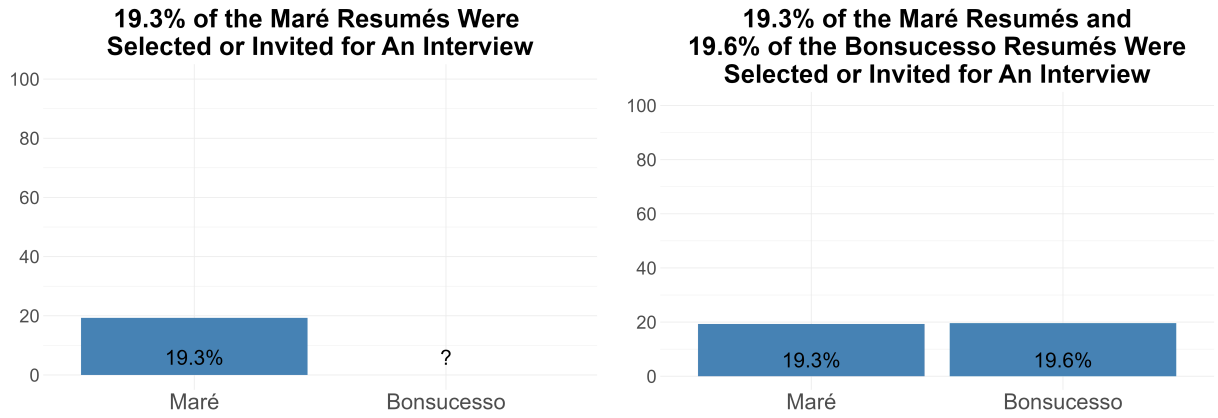

*Note*: The figure shows how the three supply-side experiments fit together. Arrows indicate the flow of jobseekers who answer a door-to-door survey, receive an invite to apply, and can also interview for a set of real jobs. The upper pipeline, which includes the Address Omission Experiment, happened from March to May 2023. The bottom pipeline was implemented after that. Time intervals over arrows exclude outliers. Dashed boxes delineate experiments. Lighter-blue nodes are randomized experimental conditions, each with its number of participants. See Section 3.2 for details.

Figure 2: Predicted *vs.* Actual Discrimination Rates




*Note*: The top panel shows the distribution of the guesses for the callback rates in an audit study using résumés with addresses from the respondent's favela or with that favela's adjacent neighborhood. The bottom panel plots the distribution of the implied discrimination rates, measured as the percent drop in callback rate caused by using a favela address. Predictions of more than 50% negative discrimination (i.e., discrimination against non-favela residents) are bunched at the leftmost bin. Vertical dashed lines show the audit study point-estimates. In the bottom graph, guesses are color-coded by whether they fall into the 95% confidence interval of the discrimination estimated in the audit study.

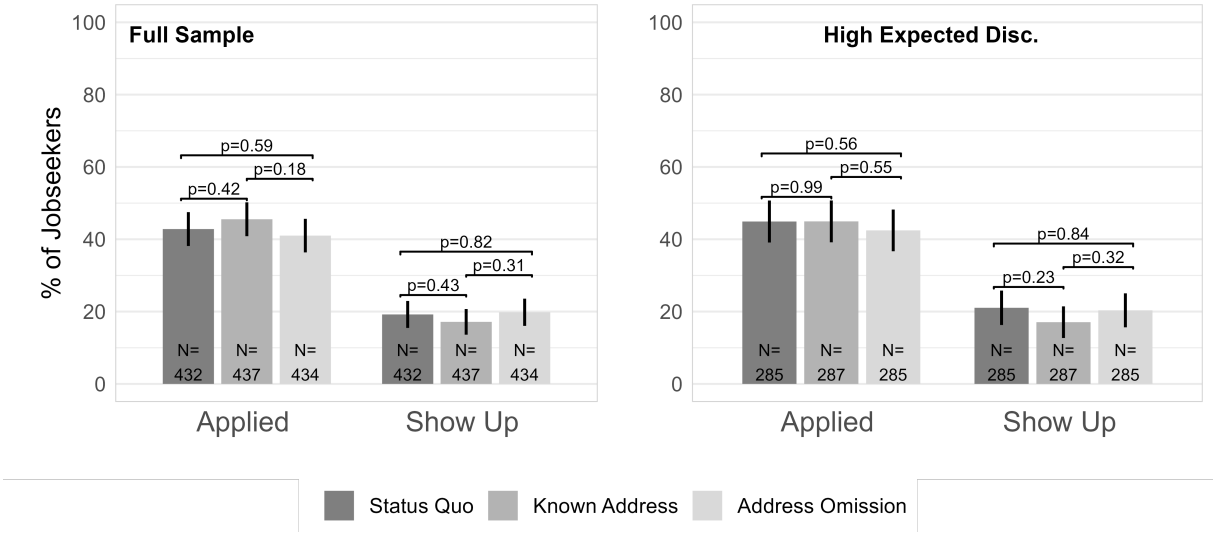
Figure 3: Information Treatment Delivery




(a) *Favela Info* (b) *Full Info*

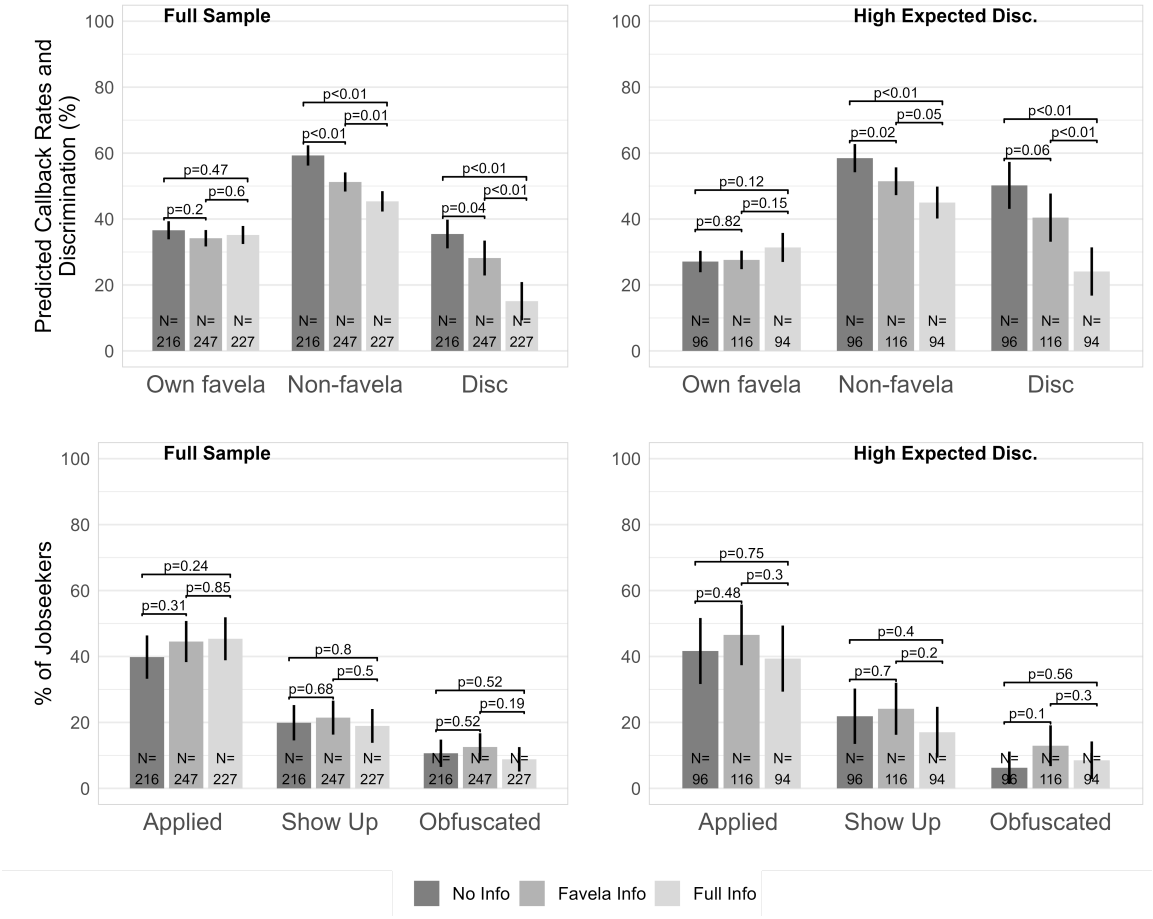
*Note*: This Figure shows the images we used to convey the Information Experiment. We showed either one of the plots (or none) to participants immediately after the belief elicitation presented in Figure D.2. The surveyor read the text above each graph when showing it to the respondent.

Figure 4: Address Omission Experiment: No Differences in Application Rates Across Arms



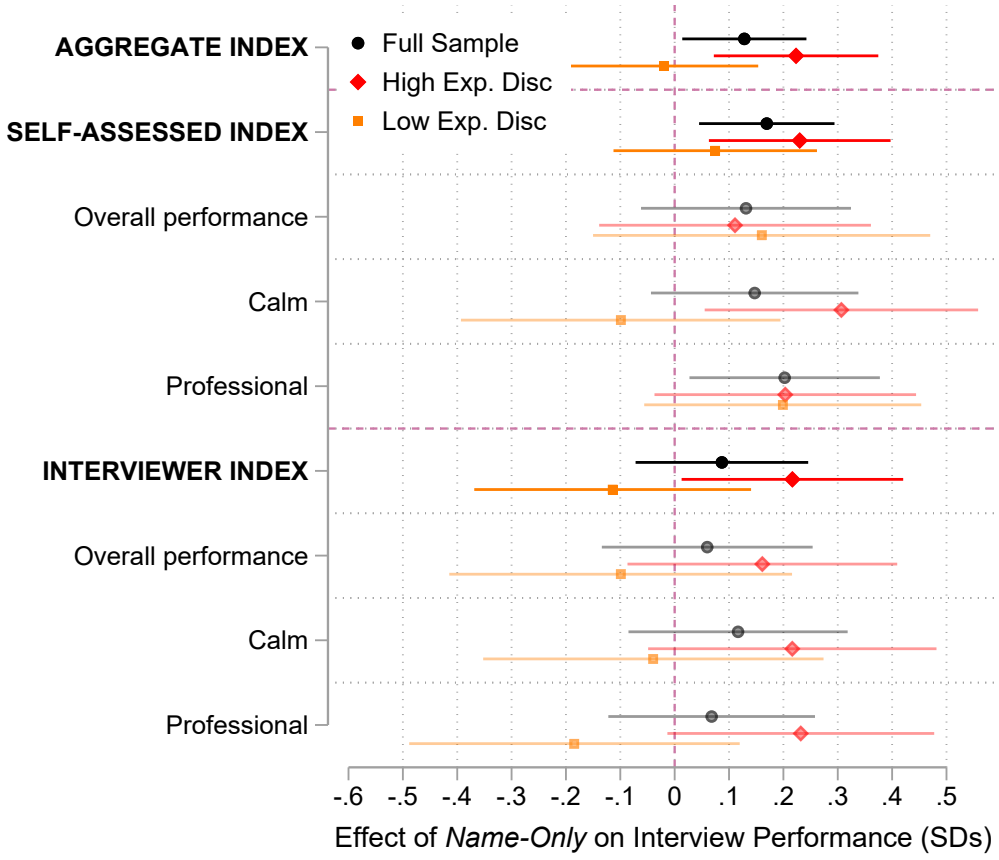

*Note*: This figure displays shares of all jobseekers in the Address Omission Experiment reaching each stage of the application process. Applied means finishing the online application form, and Show Up means showing up at the interview. The left panel shows results for the full sample, and the right panel shows results conditional on expecting 50% (median) discrimination or more when predicting the audit study. Sample size in each arm is shown at the bottom of each bar. Vertical error bars display 95% confidence intervals, and horizontal bars with tips show p-values for pairwise comparisons above them.

Figure 5: Information Treatment Shifts Beliefs, But Not Interview Show-up



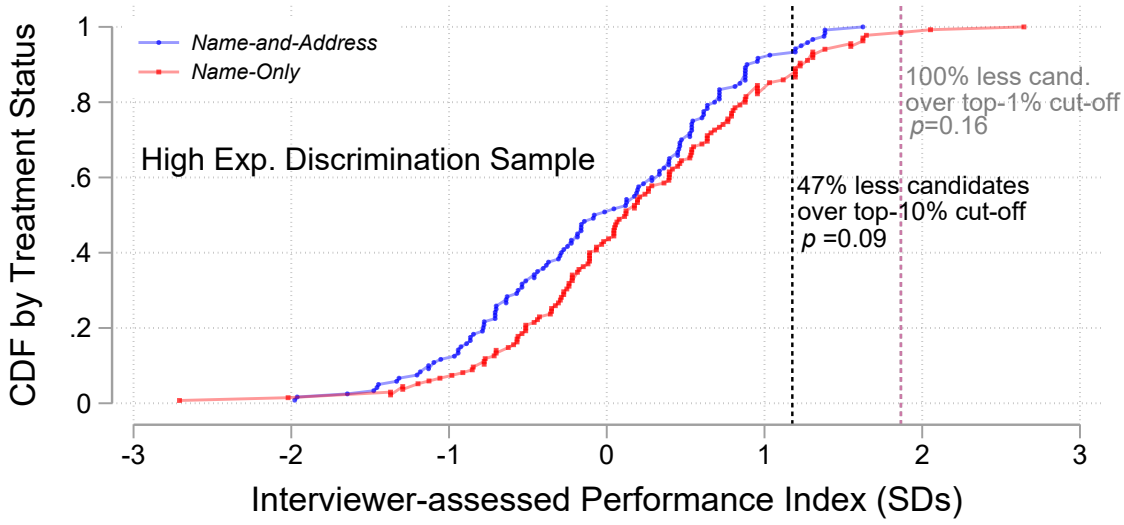

*Note*: The top row of graphs displays average posterior beliefs of what callback rates the HR firm would implement for jobseekers in each experimental condition. Non-favela and Own favela stands for the callback rate prediction for a respondent's favela and adjacent non-favela. Disc is the implied percent drop in callback rate due to the favela address. The bottom row displays outcomes from the application process. Applied means finishing the online application form, Show Up means attending the interview, and Obfuscated means declaring a neighborhood that is neither a favela nor the postal service neighborhood of the true address. The left column of graphs shows results for the full sample, and the right column shows results conditional on expecting 50% (median) discrimination or more when predicting the audit study. Sample size in each arm is shown at the bottom of each bar. Vertical error bars display 95% confidence intervals, and horizontal bars with tips show p-values for pairwise comparisons above them.

Figure 6: Expected Stigma Visibility Affects Interview Performance, Especially for the Group Expecting High Discrimination




*Note*: The graph shows treatment effect estimates (without controls) for the full sample and conditional on expecting below or at-or-above median discrimination at baseline. The interview performance outcomes are listed on the left-hand side and described in Section 3.5. The aggregate index is an average of the self-assessed and interviewer indexes. Horizontal bars indicate 95% intervals, using robust standard errors.

Figure 7: Expected Address Visibility Decreases the Share of Individuals in the Right Tail of Interviewer-assessed Performance Among Those Who Expect High Anti-favela Discrimination




*Notes*: Lines show the empirical cumulative distribution of interviewer-assessed performance for each experimental condition. P-values for the decrease in representation were calculated using regression with robust SEs, without controls.

# A Supporting Tables And Figures

Table A.1: Baseline Statistics

| | Mean | SD | Min | Max | N |
|---|---|---|---|---|---|
| Maré resident (0/1) | 0.62 | 0.48 | 0 | 1 | 2,167 |
| Jacarezinho resident (0/1) | 0.19 | 0.39 | 0 | 1 | 2,167 |
| Manguinhos resident (0/1) | 0.19 | 0.39 | 0 | 1 | 2,167 |
| Age | 26.32 | 6.23 | 18 | 42 | 2,167 |
| Male (0/1) | 0.30 | 0.46 | 0 | 1 | 2,167 |
| White jobseeker (0/1) | 0.22 | 0.42 | 0 | 1 | 2,167 |
| Some college (0/1) | 0.08 | 0.27 | 0 | 1 | 2,167 |
| Completed regular high-school (0/1) | 0.80 | 0.40 | 0 | 1 | 2,167 |
| Working now (0/1) | 0.32 | 0.47 | 0 | 1 | 2,167 |
| Holds a formal job (0/1) | 0.13 | 0.34 | 0 | 1 | 2,167 |
| Ever worked (0/1) | 0.75 | 0.43 | 0 | 1 | 2,167 |
| Actively searched in the last 7 days | 0.49 | 0.50 | 0 | 1 | 2,167 |
| Surveyor-assessed comm skills (Likert scale, 0-5) | 2.79 | 1.10 | 0 | 4 | 2,167 |
| Math test score | 6.96 | 2.50 | 0 | 17 | 2,167 |
| Heard of people refused job/fired due to address (0/1) | 0.32 | 0.47 | 0 | 1 | 2,167 |
| Believes has been refused job/fired due to address (0/1) | 0.28 | 0.45 | 0 | 1 | 2,167 |
| Own-favela expected Audit Study callback rate (%) | 30.30 | 20.23 | 0 | 100 | 2,167 |
| Adjacent non-favela expected Audit Study callback rate (%) | 63.24 | 24.54 | 0 | 100 | 2,167 |
| Reservation wage (USD) | 251.75 | 106.87 | -20 | 2,200 | 2,166 |
| Racism (is reason, 0/1) | 0.68 | 0.47 | 0 | 1 | 1,497 |
| Having a different culture/speech (is reason, 0/1) | 0.66 | 0.47 | 0 | 1 | 1,497 |
| Dislike of favela residents (is reason, 0/1) | 0.65 | 0.48 | 0 | 1 | 1,497 |
| Distance to work (is reason, 0/1) | 0.45 | 0.50 | 0 | 1 | 1,497 |
| Missing days because of police raids (is reason, 0/1) | 0.75 | 0.44 | 0 | 1 | 1,497 |
| Lower skill (is reason, 0/1) | 0.50 | 0.50 | 0 | 1 | 1,497 |
| Difficulty adapting to work (is reason, 0/1) | 0.47 | 0.50 | 0 | 1 | 1,497 |
| Fear or violence (is reason, 0/1) | 0.60 | 0.49 | 0 | 1 | 1,497 |

*Note*: This table presents descriptive statistics for the door-to-door baseline survey. Age was calculated based on the declared date of birth. Race, gender, education, and work experience were declared. "Actively searched last in the last 7 days" refers to taking any specific action to find a job (e.g., submitting a résumé) in the last seven days. "Surveyor assessed comm skills" comes from Likert-scale questions about how easily the jobseeker understood and answered the survey. Math test score is the number of multiple-choice math questions answered correctly within a minute during the survey. Reservation wage was elicited by asking for the lowest wage for which a person would accept a full-time job in their area of expertise in Downtown Rio. The last eight variables are dummies for whether the jobseekers agreed a specific reason was important for explaining why jobseekers might avoid hiring from favelas. Those eight questions were removed after we introduce the Information Experiment, to control survey duration.

Table A.2: Census (2010) Summary Statistics

| Location | Population | Literate Share | White Population Share | Income per Capita in R$ (2010) |
|---|---|---|---|---|
| All non-favela neighborhoods in Rio | 4,888,663 | 0.92 | 0.57 | 1376.35 |
| All favela neighborhoods in Rio | 1,391,953 | 0.84 | 0.33 | 382.87 |
| Jacarezinho (favela) | 37,792 | 0.87 | 0.33 | 349.63 |
| Manguinhos (favela) | 36,151 | 0.83 | 0.34 | 346.86 |
| Maré (favela) | 129,715 | 0.83 | 0.38 | 395.38 |
| Bonsucesso (non-favela) | 18,341 | 0.93 | 0.60 | 897.97 |
| Maria da Graça (non-favela) | 7,967 | 0.93 | 0.67 | 1126.26 |

*Note*: This table presents summary statistics from the 2010 Census for relevant neighborhoods in Rio. Bonsucesso was the adjacent non-favela for surveys in Maré and Manguinhos. Maria da Graça was the adjacent non-favela for Jacarezinho.

Table A.3: Address Omission Experiment: Randomization Balance

| | (1) Expects >%50 disc in audit | (2) White jobseeker (0/1) | (3) Male (0/1) | (4) Skill index | (5) Maré resident (0/1) | (6) Completed regular high schoool | (7) Working now (0/1) | (8) Ever worked (0/1) | (9) Age |
|---|---|---|---|---|---|---|---|---|---|
| *Address Omission* | -0.00 | 0.01 | 0.08*** | 0.09 | -0.02 | 0.03 | -0.00 | 0.05* | 0.57 |
| | (0.03) | (0.03) | (0.03) | (0.06) | (0.03) | (0.03) | (0.03) | (0.03) | (0.42) |
| *Known Address* | -0.00 | -0.03 | -0.00 | -0.02 | -0.01 | 0.02 | -0.01 | 0.04 | 0.73* |
| | (0.03) | (0.03) | (0.03) | (0.06) | (0.03) | (0.03) | (0.03) | (0.03) | (0.42) |
| Observations | 1303 | 1303 | 1303 | 1303 | 1303 | 1303 | 1303 | 1303 | 1303 |
| *Status Quo* Mean | 0.66 | 0.23 | 0.27 | -0.07 | 0.80 | 0.76 | 0.33 | 0.69 | 25.77 |
| *Address Omission=Known Address p* | 1.00 | 0.15 | 0.01 | 0.05 | 0.76 | 0.91 | 0.83 | 0.63 | 0.71 |

*Note*: Outcomes in each column are: (1) a dummy for being classified as expecting high discrimination at baseline; (2) a dummy for self-identifying as white; (3) a dummy for self-identifying as male; (4) a normalized skill index aggregating math test score, education, and communication skills; (5) a dummy for residing in Maré, as opposed to Manguinhos or Jacarezinho; (6) a dummy for having completed regular high school; (7) a dummy for self-declaring as being "working now"; (8) a dummy for self-declaring as having ever worked; and (9) age in years calculated based on declared birthday. Last row shows the p-value for the equality test of the average in *Address Omission* being equal to the one in *Known Address*. * p<0.1, ** p<0.05, *** p<0.01.

Table A.4: Information Experiment: Randomization Balance

| | (1) Expects >%50 disc in audit | (2) White jobseeker (0/1) | (3) Male (0/1) | (4) Skill index | (5) Maré resident (0/1) | (6) Completed regular high schoool | (7) Working now (0/1) | (8) Ever worked (0/1) | (9) Age |
|---|---|---|---|---|---|---|---|---|---|
| Favela Info | 0.03 | 0.00 | 0.02 | -0.02 | 0.04 | -0.00 | 0.02 | 0.06 | 0.92 |
| | (0.05) | (0.04) | (0.04) | (0.08) | (0.04) | (0.03) | (0.04) | (0.04) | (0.58) |
| Full Info | -0.03 | 0.02 | 0.03 | -0.17** | 0.04 | -0.07* | -0.03 | -0.01 | -0.39 |
| | (0.05) | (0.04) | (0.04) | (0.08) | (0.05) | (0.04) | (0.04) | (0.04) | (0.57) |
| Observations | 690 | 690 | 690 | 690 | 690 | 690 | 690 | 690 | 690 |
| *No Info* Mean | 0.4 | 0.2 | 0.3 | 0.1 | 0.3 | 0.8 | 0.3 | 0.8 | 26.0 |
| Favela=Full *p* | 0.23 | 0.72 | 0.91 | 0.07 | 0.98 | 0.05 | 0.29 | 0.07 | 0.02 |

*Note*: See notes to Table A.3 for outcomes. Last row shows the p-value for the equality test of the average in *Favela Info* being equal to the one in *Full Info*. * p<0.1, ** p<0.05, *** p<0.01.

Table A.5: Interview Experiment: Randomization Balance

| | (1) Expects >%50 disc in audit | (2) White jobseeker (0/1) | (3) Male (0/1) | (4) Skill index | (5) Maré resident (0/1) | (6) Completed regular high schoool | (7) Working now (0/1) | (8) Ever worked (0/1) | (9) Age |
|---|---|---|---|---|---|---|---|---|---|
| Name-Only | 0.02 | -0.00 | 0.01 | 0.05 | -0.10** | 0.02 | 0.06* | 0.06 | -0.13 |
| | (0.05) | (0.04) | (0.04) | (0.08) | (0.05) | (0.04) | (0.03) | (0.04) | (0.57) |
| Observations | 422 | 422 | 422 | 422 | 422 | 422 | 422 | 422 | 422 |
| Control Mean | 0.59 | 0.24 | 0.26 | -0.01 | 0.66 | 0.77 | 0.10 | 0.71 | 25.12 |

*Note*: See notes to Table A.3 for outcomes. * p<0.1, ** p<0.05, *** p<0.01.

## Table A.6: Comparison of Samples Across the Three Experiments

| | (1) Address Omission Experiment | | (2) Information Experiment | | (3) Interview Experiment | | (1)-(2) | | (1)-(3) Pairwise t-test | | (2)-(3) | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | N | Mean/(SE) | N | Mean/(SE) | N | Mean/(SE) | N | Mean difference | N | Mean difference | N | Mean difference |
| Maré resident (0/1) | 1303 | 0.789<br>(0.011) | 690 | 0.354<br>(0.018) | 422 | 0.614<br>(0.024) | 1993 | 0.435*** | 1725 | 0.175*** | 1112 | -0.260*** |
| Jacarezinho resident (0/1) | 1303 | 0.184<br>(0.011) | 690 | 0.193<br>(0.015) | 422 | 0.204<br>(0.020) | 1993 | -0.009 | 1725 | -0.020 | 1112 | -0.011 |
| Manguinhos resident (0/1) | 1303 | 0.027<br>(0.004) | 690 | 0.454<br>(0.019) | 422 | 0.182<br>(0.019) | 1993 | -0.427*** | 1725 | -0.156*** | 1112 | 0.271*** |
| Age | 1303 | 26.200<br>(0.173) | 690 | 26.412<br>(0.236) | 422 | 25.249<br>(0.281) | 1993 | -0.212 | 1725 | 0.951*** | 1112 | 1.163*** |
| Male (0/1) | 1303 | 0.295<br>(0.013) | 690 | 0.303<br>(0.018) | 422 | 0.265<br>(0.022) | 1993 | -0.008 | 1725 | 0.029 | 1112 | 0.037 |
| White jobseeker (0/1) | 1303 | 0.229<br>(0.012) | 690 | 0.210<br>(0.016) | 422 | 0.237<br>(0.021) | 1993 | 0.019 | 1725 | -0.008 | 1112 | -0.027 |
| Some college (0/1) | 1303 | 0.064<br>(0.007) | 690 | 0.080<br>(0.010) | 422 | 0.071<br>(0.013) | 1993 | -0.016 | 1725 | -0.007 | 1112 | 0.009 |
| Completed regular high-school (0/1) | 1303 | 0.776<br>(0.012) | 690 | 0.823<br>(0.015) | 422 | 0.777<br>(0.020) | 1993 | -0.047** | 1725 | -0.001 | 1112 | 0.046* |
| Working now (0/1) | 1303 | 0.326<br>(0.013) | 690 | 0.284<br>(0.017) | 422 | 0.135<br>(0.017) | 1993 | 0.042* | 1725 | 0.191*** | 1112 | 0.149*** |
| Holds a formal job (0/1) | 1303 | 0.118<br>(0.009) | 690 | 0.135<br>(0.013) | 422 | 0.047<br>(0.010) | 1993 | -0.017 | 1725 | 0.071*** | 1112 | 0.087*** |
| Ever worked (0/1) | 1303 | 0.722<br>(0.012) | 690 | 0.786<br>(0.016) | 422 | 0.737<br>(0.021) | 1993 | -0.063*** | 1725 | -0.015 | 1112 | 0.049* |
| Actively searched in the last 7 days | 1303 | 0.531<br>(0.014) | 690 | 0.425<br>(0.019) | 422 | 0.649<br>(0.023) | 1993 | 0.106*** | 1725 | -0.118*** | 1112 | -0.225*** |
| Surveyor-assessed comm skills (Likert scale, 0-5) | 1303 | 2.794<br>(0.029) | 690 | 2.797<br>(0.045) | 422 | 3.001<br>(0.051) | 1993 | -0.003 | 1725 | -0.207*** | 1112 | -0.204*** |
| Math test score | 1303 | 6.957<br>(0.072) | 690 | 6.945<br>(0.091) | 422 | 6.919<br>(0.115) | 1993 | 0.012 | 1725 | 0.038 | 1112 | 0.026 |
| Heard of people refused job/fired due to address (0/1) | 1303 | 0.335<br>(0.013) | 690 | 0.303<br>(0.018) | 422 | 0.351<br>(0.023) | 1993 | 0.032 | 1725 | -0.015 | 1112 | -0.048* |
| Believes has been refused job/fired due to address (0/1) | 1303 | 0.308<br>(0.013) | 690 | 0.255<br>(0.017) | 422 | 0.301<br>(0.022) | 1993 | 0.053** | 1725 | 0.007 | 1112 | -0.046* |
| Expected discrimination predicting audit results | 1303 | 53.771<br>(0.908) | 690 | 33.307<br>(2.406) | 422 | 45.022<br>(2.807) | 1993 | 20.463*** | 1725 | 8.748*** | 1112 | -11.715*** |
| Reservation wage (USD) | 1302 | 253.099<br>(3.014) | 690 | 246.173<br>(3.215) | 422 | 231.962<br>(2.736) | 1992 | 6.927 | 1724 | 21.137*** | 1112 | 14.211*** |

*Notes*: Pair-wise comparisons of average baseline characteristics across experiments. See notes to Table A.1 for details on variables. * p<0.1, ** p<0.05, *** p<0.01.

Table A.7: Effects on Interview Performance for the High Expected Discrimination and White Individuals Are Robust to Including Other Interacted Covariates

| | (1) Interviewer-assessed performance | (2) Interviewer-assessed performance | (3) Interviewer-assessed performance | (4) Interviewer-assessed performance | (5) Self-assessed performance | (6) Self-assessed performance | (7) Self-assessed performance | (8) Self-assessed performance |
|---|---|---|---|---|---|---|---|---|
| *Name-Only* | -0.11 | 0.02 | -0.18 | 0.44 | 0.07 | 0.12* | 0.03 | 0.64* |
| | (0.13) | (0.09) | (0.14) | (0.46) | (0.09) | (0.07) | (0.10) | (0.37) |
| *Name-Only* × High Exp. Disc. | 0.33** | | 0.32* | 0.24 | 0.16 | | 0.16 | 0.14 |
| | (0.17) | | (0.16) | (0.15) | (0.13) | | (0.13) | (0.13) |
| *Name-Only* × White jobsee. | | 0.29 | 0.29 | 0.31* | | 0.19 | 0.19 | 0.17 |
| | | (0.20) | (0.20) | (0.19) | | (0.15) | (0.15) | (0.15) |
| Observations | 422 | 422 | 422 | 422 | 422 | 422 | 422 | 422 |
| Other Interactions? | No | No | No | Yes | No | No | No | Yes |

*Notes*: We estimate $y_i = \alpha + \beta \text{Name-Only}_i + \sum_j \gamma^j \text{Name-Only}_i \times X_i^j + \nu^j X_i^j + \varepsilon_i$ for different sets of covariates. Other Interactions include skill, having completed regular high school, employment status, having ever worked, age, and a dummy for having reservation wage over R$1,500. * p<0.1, ** p<0.05, *** p<0.01.

Table A.8: Table 2 with Double-Lasso Controls

| | (1) Agg. Performance Index (SDs) | (2) Applied (%) | (3) Showed Up (%) | (4) Applied (%) | (5) Showed Up (%) |
|---|---|---|---|---|---|
| ↓ 𝔼[Disc.] × white | 0.32*** | 7.14 | 8.47* | -11.47 | -17.96*** |
| | (0.11) | (5.88) | (5.09) | (8.13) | (6.20) |
| ↓ 𝔼[Disc.] × non-white | 0.04 | -6.66** | -0.82 | 7.10* | 1.94 |
| | (0.06) | (3.19) | (2.50) | (4.22) | (3.46) |
| Observations | 422 | 1303 | 1303 | 690 | 690 |
| Treatment | ↓ 𝔼[address visibility] | ↓ 𝔼[address visibility] | ↓ 𝔼[address visibility] | ↓ 𝔼[mkt-level disc.] | ↓ 𝔼[mkt-level disc.] |
| Control Average | -0.0 | 44.2 | 18.2 | 42.3 | 20.7 |
| White=non-white p-value | 0.02 | 0.04 | 0.10 | 0.04 | 0.01 |

*Notes*: The double-lasso procedure could pick controls including expected discrimination, gender, residing in Maré, education, communication skills, math test score, having ever worked, working now, age, and whether the jobseeker has looked for a job in the last 7 days. A dummy for being white is always included. See Table 2 for more details.

Table A.9: Two-stage Least Squares Estimates of The Effect of the Expected Callback Rate and Anti-Favela Discrimination on Application Decisions

| | (1) Applied (%) | (2) Show Up (%) | (3) Obfuscated in application (%) |
|---|---|---|---|
| Posterior Expected Callback for Own Favela (%) | -0.45 | -0.14 | -0.55* |
| | (0.51) | (0.41) | (0.33) |
| Posterior Expected Discrimination Rate (%) | -0.11 | 0.05 | 0.11 |
| | (0.21) | (0.18) | (0.14) |
| Observations | 690 | 690 | 690 |
| *No Info* Mean | 39.8 | 19.9 | 10.6 |

*Note*: This table uses variation in beliefs induced by the information treatments *Favela Info* and *Full Info* to estimate their effects on application decisions. Instrumented variables are the expected callback rate the HR firm would implement in the person's favela of residence and the implied discrimination rate (percent drop in callback caused by being from the favela instead of living just outside it). Instruments are the treatment assignment interacted with i) dummy for overestimating the favela callback rate when predicting the audit study, ii) prediction error when predicting that callback rate for each audit study neighborhood, iii) dummy for overestimating the discrimination in callbacks when predicting the audit study iv) prediction error in predicting that discrimination rate. Outcomes are completing the online application form, attending the interview, and obfuscating address in the online application form.

Table A.10: Race Correlates: College Attendance, Work Experience, Age, and Skill

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Some college (0/1) | 0.08* | 0.10*** | | | | | | | | |
| | (0.04) | (0.04) | | | | | | | | |
| Completed regular high-school (0/1) | -0.02 | | 0.02 | | | | | | | |
| | (0.04) | | (0.02) | | | | | | | |
| Working now (0/1) | -0.05** | | | -0.05*** | | | | | | |
| | (0.02) | | | (0.02) | | | | | | |
| Ever worked (0/1) | -0.02 | | | | -0.05** | | | | | |
| | (0.02) | | | | (0.02) | | | | | |
| Male (0/1) | 0.03 | | | | | 0.03 | | | | |
| | (0.02) | | | | | (0.02) | | | | |
| Age | -0.00** | | | | | | -0.00*** | | | |
| | (0.00) | | | | | | (0.00) | | | |
| Reservation wage (USD) | 0.00 | | | | | | | 0.00 | | |
| | (0.00) | | | | | | | (0.00) | | |
| Skill Index | 0.04* | | | | | | | | 0.03*** | |
| | (0.02) | | | | | | | | (0.01) | |
| Expects high discrimination (0/1) | -0.03 | | | | | | | | | -0.02 |
| | (0.02) | | | | | | | | | (0.02) |
| Observations | 2166 | 2167 | 2167 | 2167 | 2167 | 2167 | 2167 | 2166 | 2167 | 2167 |

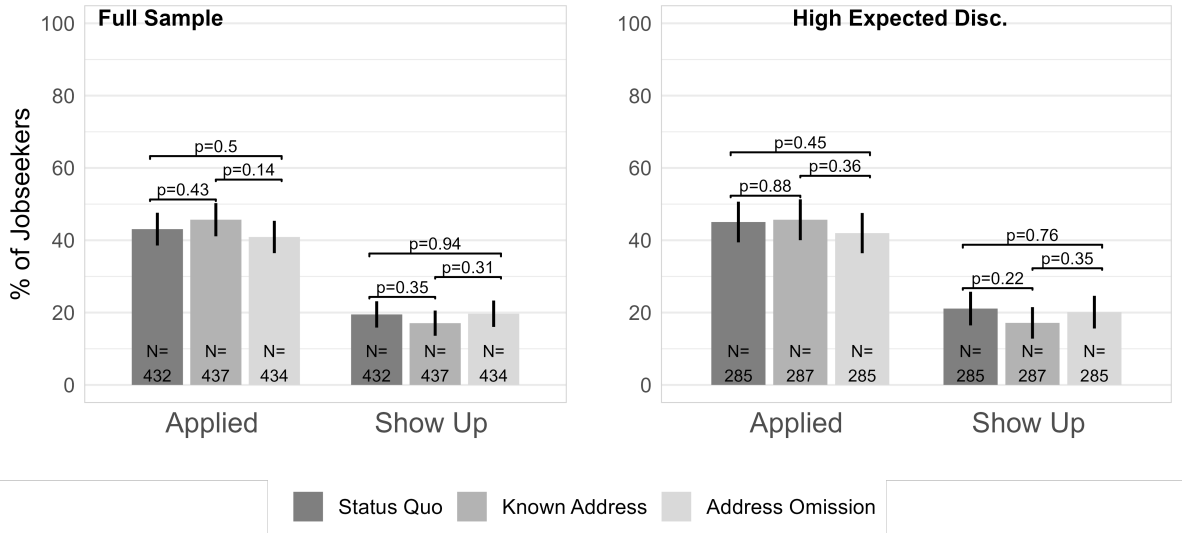
*Note*: * p<0.1, ** p<0.05, *** p<0.01. Outcome is a dummy (0/1) for the jobseekers being white. Robust standard errors shown between parenthesis.

Table A.11: Race Still Predicts Effect Sizes After Including Other Interaction Terms

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Agg. Performance Index (SDs) | Applied (%) | Showed Up (%) | Applied (%) | Showed Up (%) |
| ↓ Exp Disc. | 0.33 | -3.59 | 9.92 | 1.70 | 6.26 |
| | (0.30) | (12.95) | (10.14) | (18.00) | (13.80) |
| ↓ $\mathbb{E}$[Disc.] × white | 0.21 | 14.10** | 9.10 | -14.86 | -15.97** |
| | (0.13) | (6.92) | (5.80) | (9.67) | (7.41) |
| White jobseeker (0/1) | -0.07 | -5.48 | -1.05 | 4.48 | 4.97 |
| | (0.10) | (4.05) | (3.11) | (5.65) | (4.92) |
| Observations | 422 | 1303 | 1303 | 690 | 690 |
| Treatment | ↓ $\mathbb{E}$[address visibility] | ↓ $\mathbb{E}$[address visibility] | ↓ $\mathbb{E}$[address visibility] | ↓ $\mathbb{E}$[mkt-level disc.] | ↓ $\mathbb{E}$[mkt-level disc.] |
| Control Average | -0.0 | 44.2 | 18.2 | 42.3 | 20.7 |

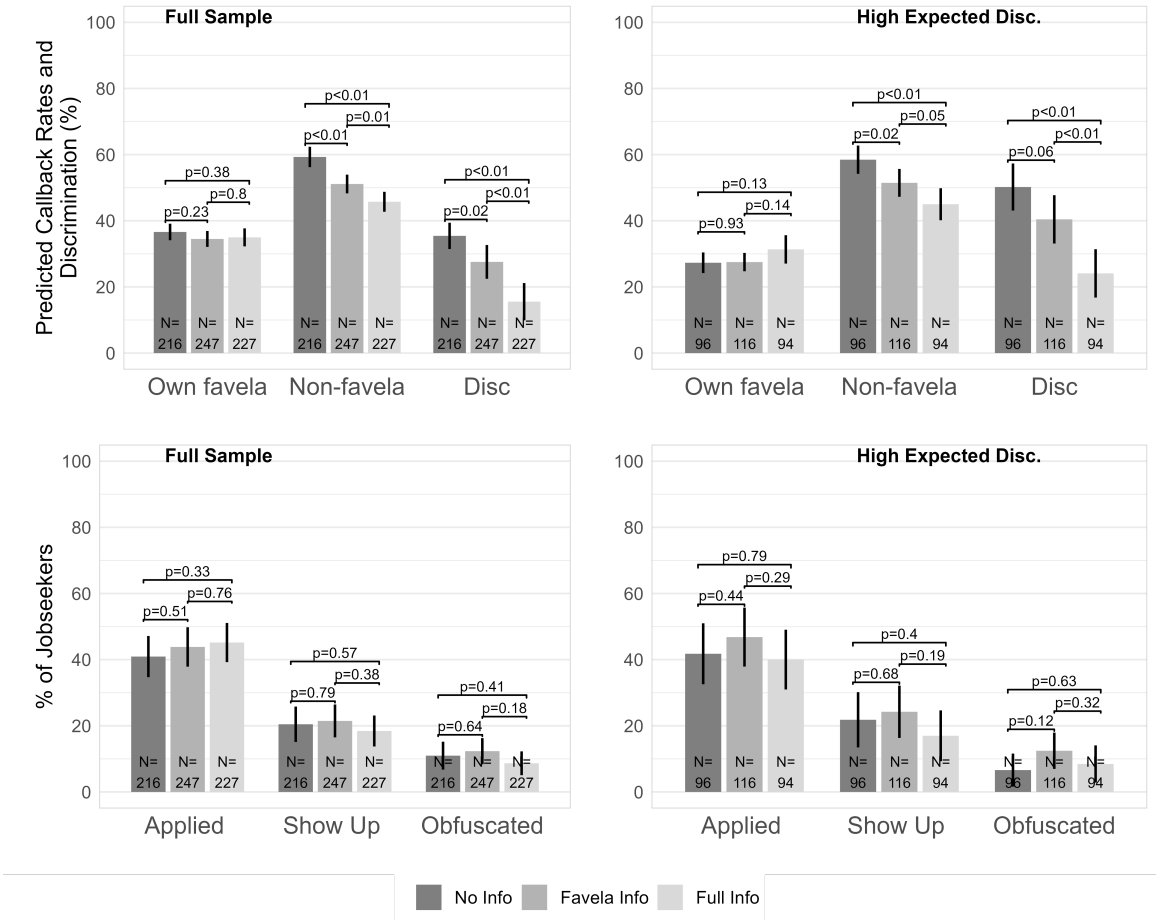
*Note*: * p<0.1, ** p<0.05, *** p<0.01. Outcome is a dummy (0/1) for the jobseekers being white. Robust standard errors shown between parenthesis.

Figure A.1: Figure 4 with Lasso-selected Controls



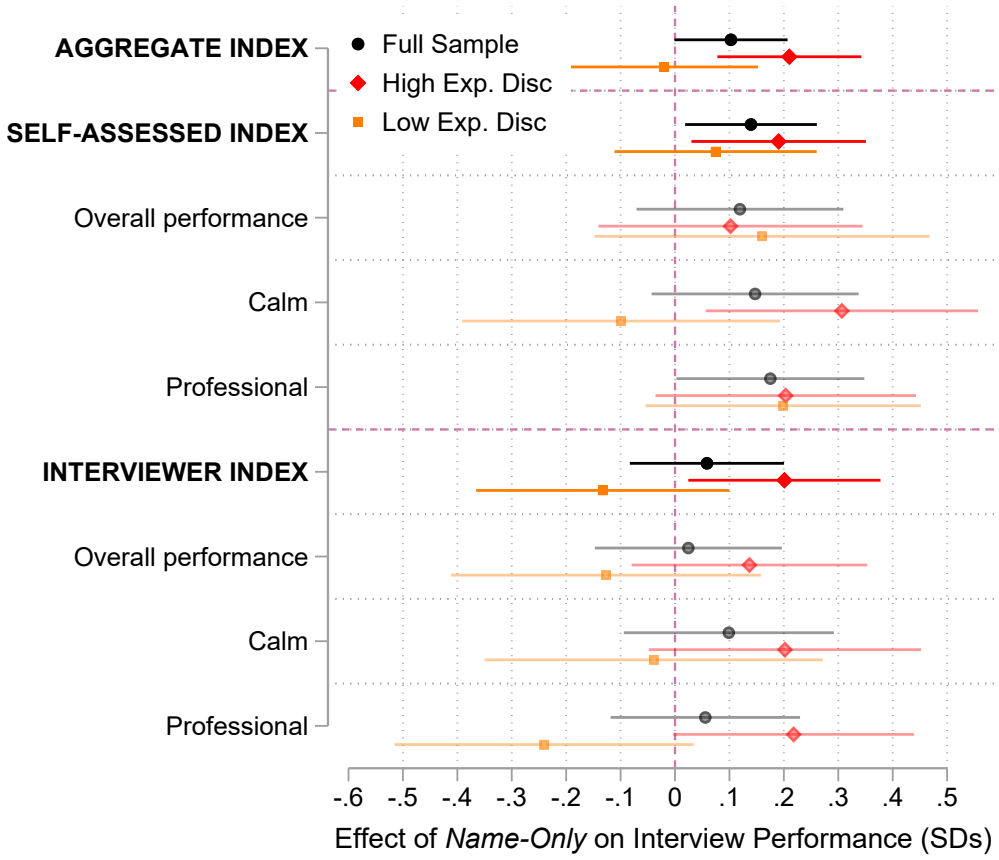

*Notes*: The double-lasso procedure could pick controls including expected discrimination, race, gender, residing in Maré, education, communication skills, math test score, having ever worked, working now, age, and whether the jobseeker has looked for a job in the last 7 days. After picking controls, we demean them and interacting them with treatment status, so the bar heights reflect control-corrected means and differences reflect ATE between arms. Variance-covariance matrix was estimated using the HC1 approach since the extra controls and sample-splitting generate ill-defined entries; see notes for Figure 4 for other details.

Figure A.2: Figure 5 with Lasso-selected Controls



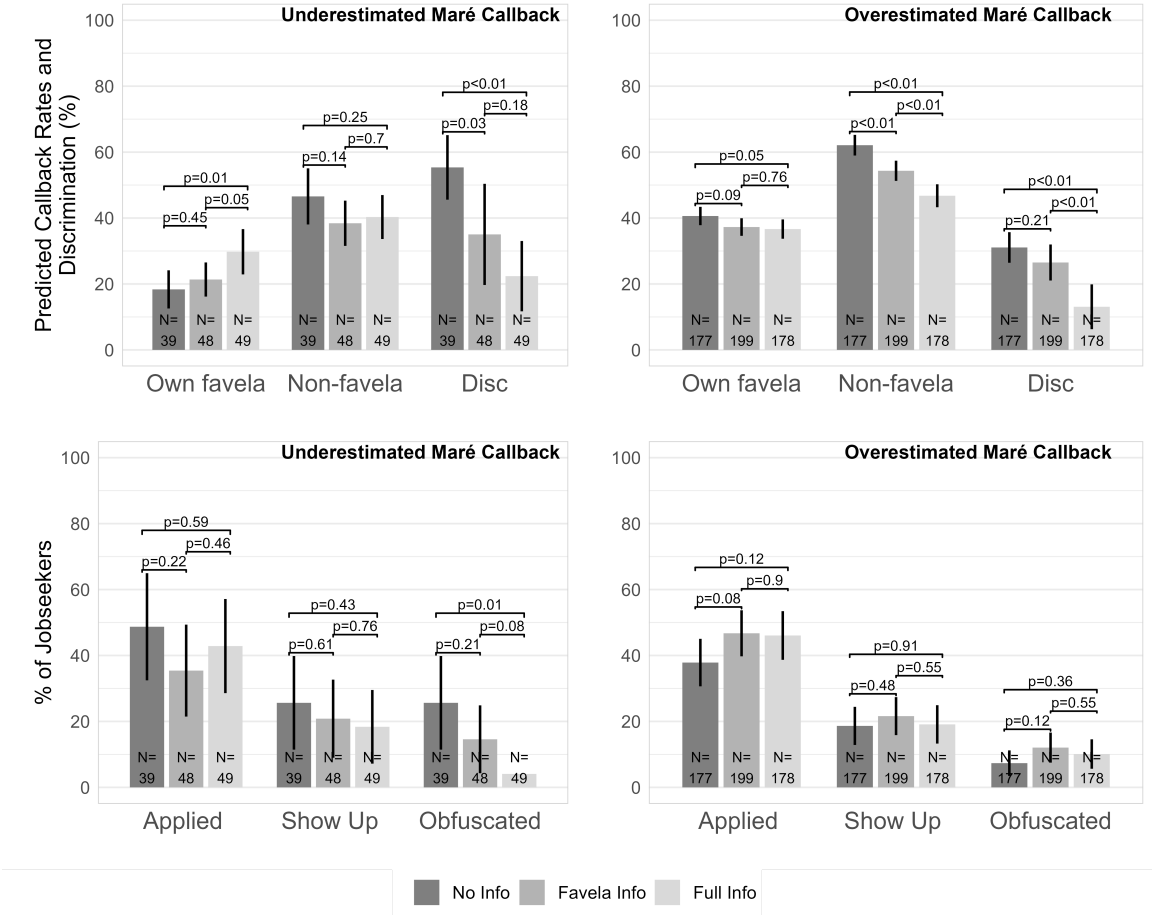

The double-lasso procedure could pick controls including expected discrimination, race, gender, residing in Maré, education, communication skills, math test score, computer skills, having ever worked, working now, age, and whether the jobseeker has looked for a job in the last 7 days. After picking controls, we demean them and interacting them with treatment status, so the bar heights reflect control-corrected means and differences reflect ATE between arms. Variance-covariance matrix was estimated using the HC1 approach since the extra controls and sample-splitting generate ill-defined entries; see notes for Figure 5 for other details.

Figure A.3: Figure 6 with Lasso-Selected Controls



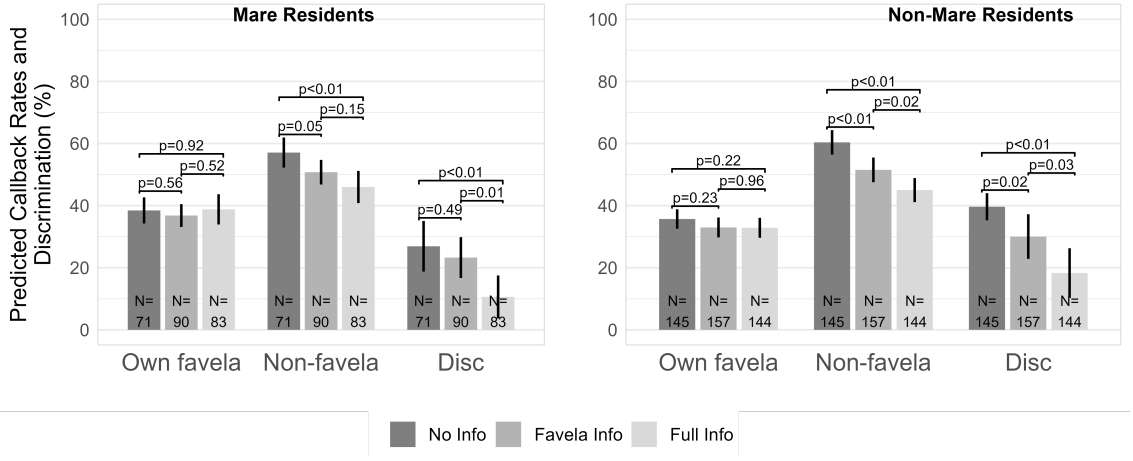

*Note*: The graph shows treatment effect estimates (using double-lasso selected controls) for the full sample and conditional on expecting below or at-or-above median discrimination at baseline. The double-lasso procedure could pick controls including expected discrimination, race, gender, residing in Maré, education, communication skills, math test score, computer skills, having ever worked, working now, age, and whether the jobseeker has looked for a job in the last 7 days. The interview performance outcomes are listed on the left-hand side and described in Section 3.5. The aggregate index is an average of the self-assessed and interviewer indexes. Horizontal bars indicate 95% intervals, using robust standard errors.

Figure A.4: Effects of Information Treatments on Beliefs and Applications by Whether Jobseekers Initially Under- or Overestimated the Favela Callback Rate



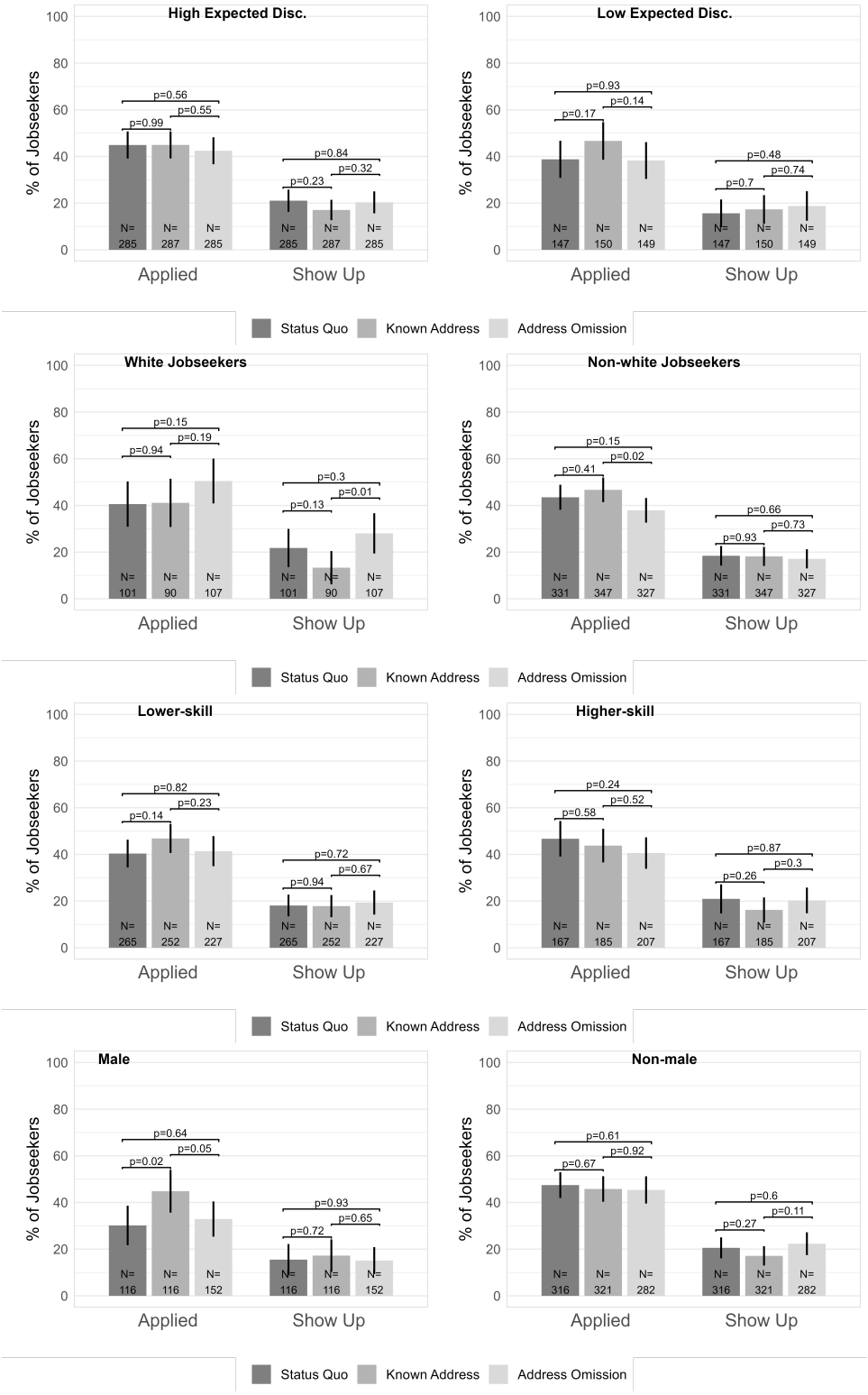

*Note*: Same as in Figure 5, but splitting the same on those who under- or over-estimated the callback rate for the favela neighborhood in the audit study. This makes it easier to see that how jobseekers adjust beliefs about callback rates for their own favela according to the direction of the information received. Note also that *Full Info* decreases the obfuscation rate for those initially too pessimistic about favela callback rates, consistent with obfuscation becoming pointless once observing there are little returns to it at the callback stage.

Figure A.5: Belief Update in Information Experiment Occurs for Maré and Non-Maré Residents



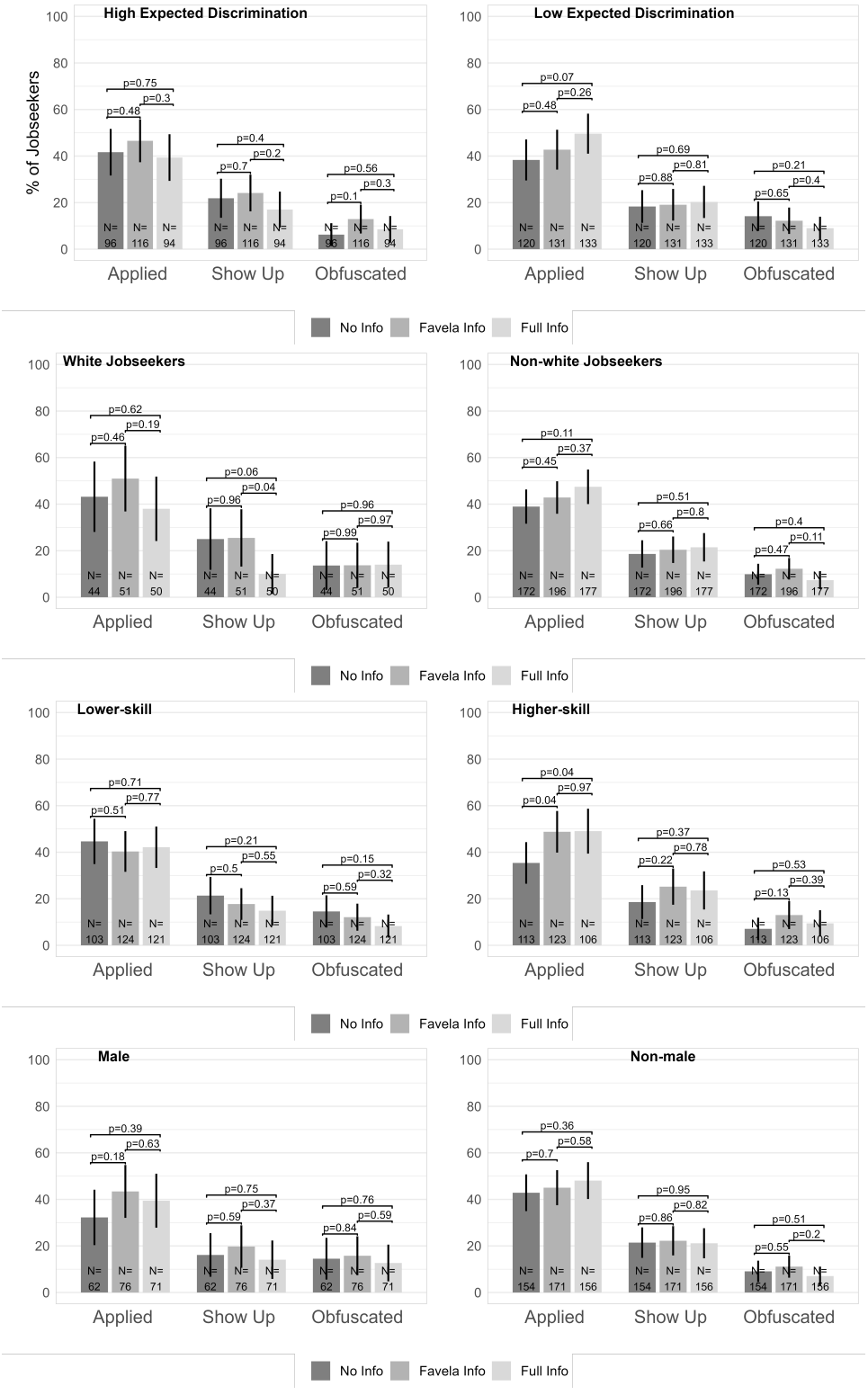

*Notes*: The the callback rates revealed in the Information Experiment were those found in the audit study, for Maré and Bonsucesso. Residents of Manguinhos and Jacarezinho make similar belief updates as the Maré residents, suggesting that they extrapolate similarly from the audit findings. See notes to Figure 5 for details on outcomes and figure features.

Figure A.6: Heterogeneous Effects in the Address Omission Experiment – No Controls



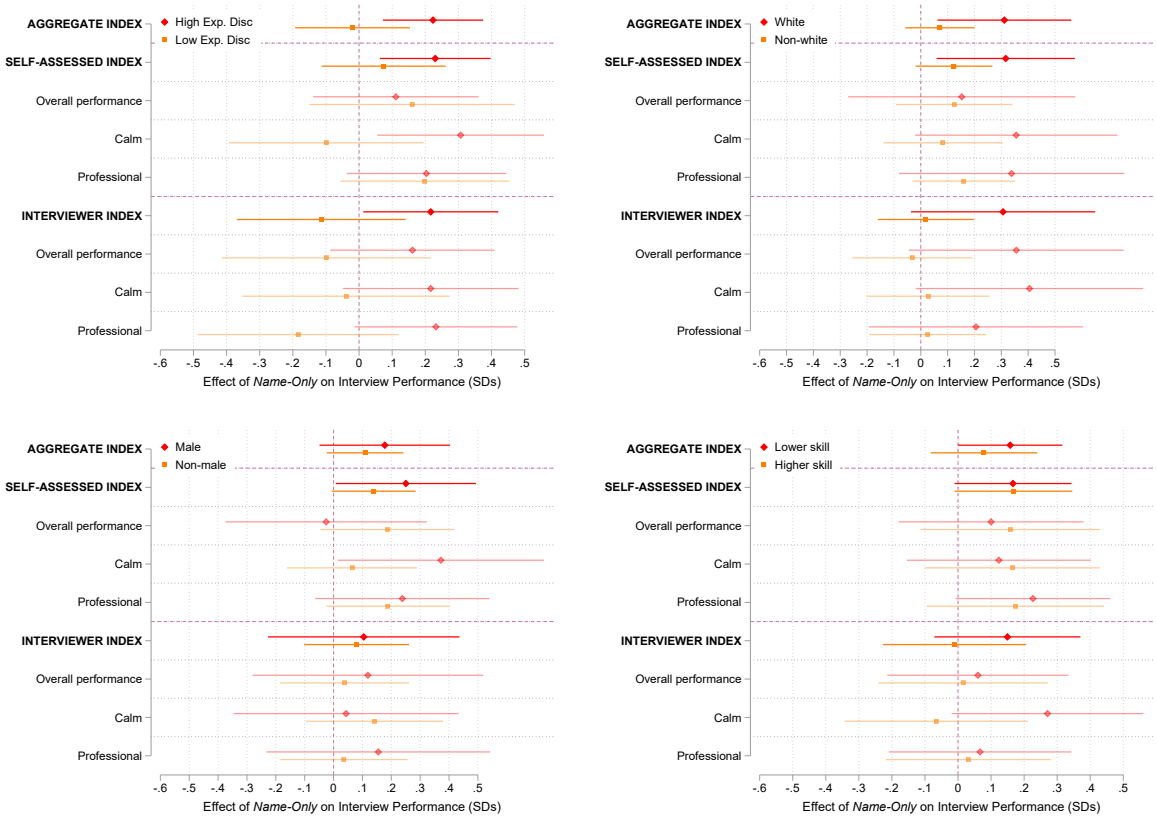

*Notes:* Pre-registered heterogeneity break-downs for the Address Omission Experiment. See notes to Figure 4 for other details.

Figure A.7: Heterogeneous Effects of Information Treatments – No Controls



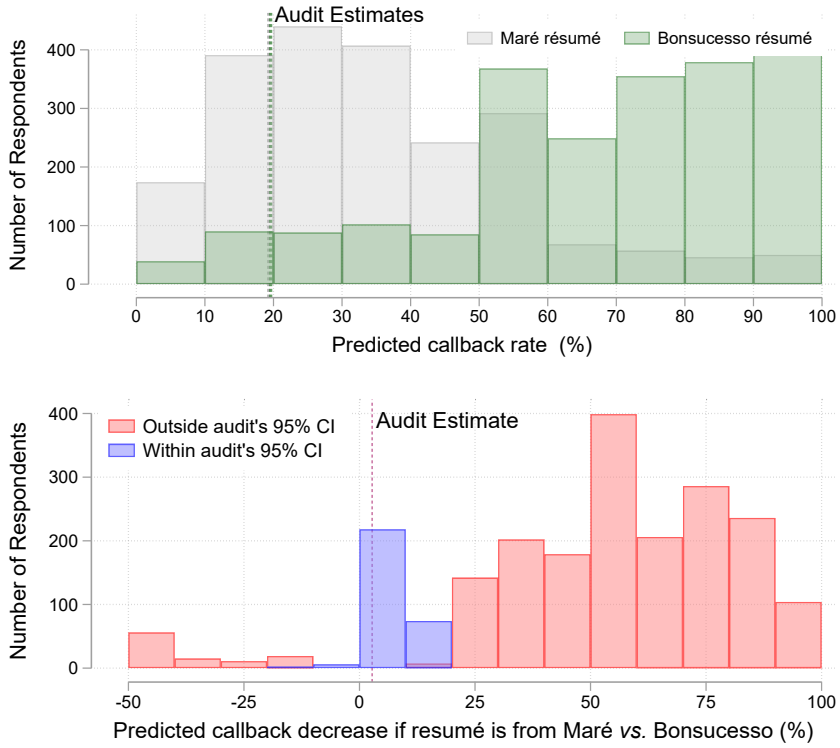

*Notes:* Pre-registered heterogeneity break-downs for the Information Experiment. See notes to Figure 5 for details.

Figure A.8: Heterogeneous Effects of *Name-Only*



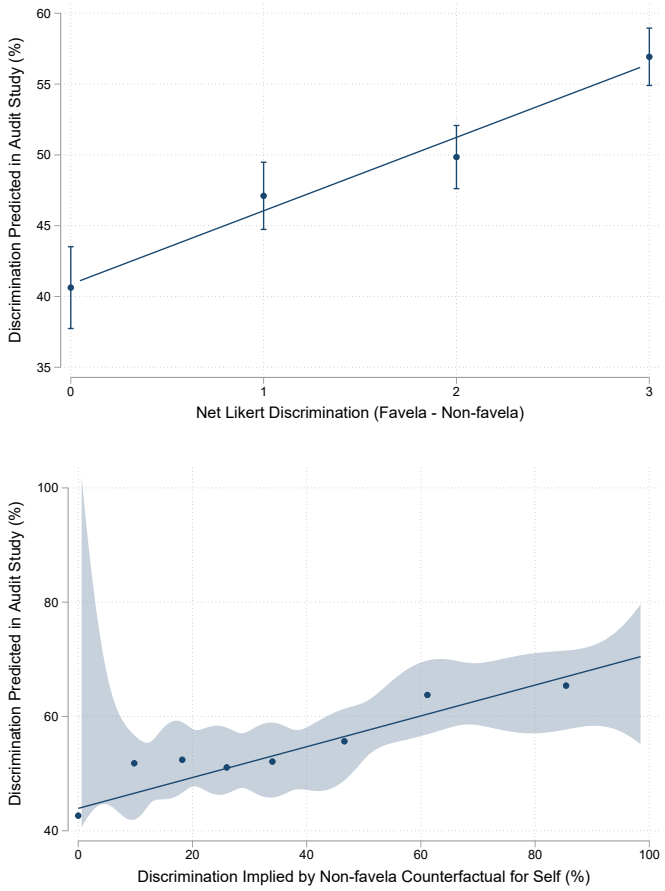

*Notes*: Pre-registered heterogeneity break-downs for the Interview Experiment. See notes to Figure 6 for details.

Figure A.9: Predicted *vs.* Actual Discrimination Rates Using Only Beliefs About the Audit Study Neighborhoods



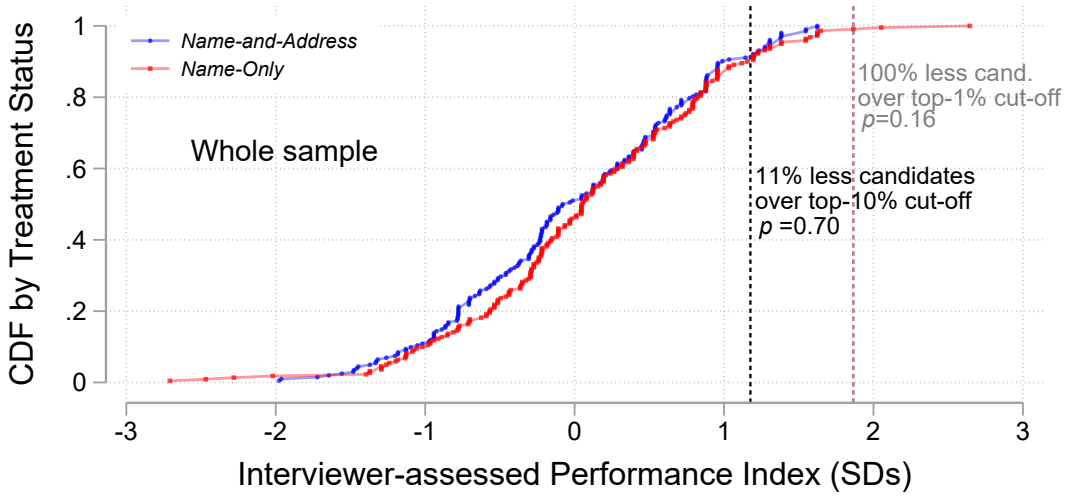

*Notes*: Same as in Figure 2, but using declared beliefs regarding Maré and Bonsucesso instead of one's own favela and its adjacent non-favela.

Figure A.10: Predicted Audit Study Discrimination Correlates with Other Measures of Expected Discrimination



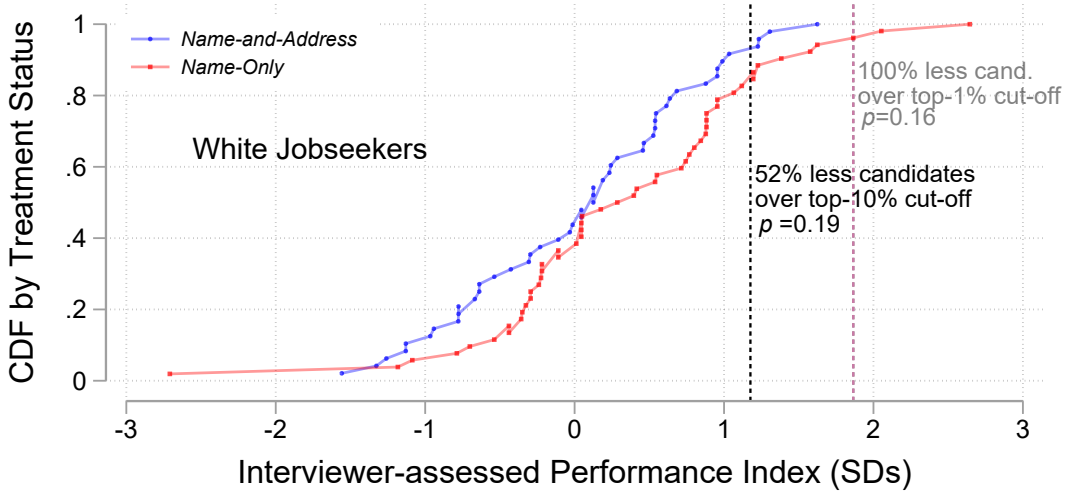

*Note*: Negative values of discrimination are pooled with zero discrimination (since there are few observation with negative discrimination, which make estimates noisy). We construct the Likert discrimination measure by taking the Liker-scale answers of how much employers discriminate against individuals in one's favela and adjacent non-favela (from no discrimination to a lot), converting them into integers from one to four, and then taking the difference between neighborhoods (as some individuals believe there is also some discrimination against the non-favela neighborhood). We calculate the discrimination for the counterfactual self by comparing the beliefs about one's job-finding probability over the next six months to predictions about "somebody just like you, but from [adjacent non-favela neighborhood]".

Figure A.11: Cumulative Distribution of the Interviewer-assessed Performance by Experimental Condition – Whole Sample




*Notes*: Lines show the empirical cumulative distribution of interviewer-assessed performance for each experimental condition. P-values for the decrease in representation were calculated using regression with robust SEs, without controls.

Figure A.12: Cumulative Distribution of the Interviewer-assessed Performance by Experimental Condition – White Jobseekers




*Notes*: Lines show the empirical cumulative distribution (for the white subsample) of interviewer-assessed performance for each experimental condition. P-values for the decrease in representation were calculated using regression with robust SEs, without controls.

# B Deviations from the Pre-Analysis Plan

Our pre-analysis plan can be found attached at the end of this document. Below, we list our deviations from that plan.

- As mentioned in footnote 15, the one major deviation from the initial pre-registration was the introduction of the Information Experiment. We amended the pre-registration, explaining our reasoning, before inviting the N=690 participants in that experiment to apply (see field "Intervention (hidden)" in the attached pre-registration).
- The first heterogeneity analysis mentioned in our pre-registration is by expected discrimination level. In that same bullet, we also mention considering heterogeneity analyses by attitudes such as "how bothered one is by discrimination" and "whether the possibility of being discriminated against in the hiring process is motivating or discouraging." As we dropped these questions from our survey when introducing the Information Experiment, we also drop this more exploratory analysis.
- We pre-registered our in-survey math test as the main skill measure, but we later judged it was too narrow with respect to a sales job. Hence, we also included education (0 for incomplete regular high school, 1 for completing regular high school, and 2 for having some college) and a measure of communication skills (Likert-scale,coded by the surveyor after the end of each survey).
- We list effort in applications, measured by length and quality of the optional cover letter applicants could write in the online application form, as primary outcomes. For brevity, we omit them from the main text and present them here.
- The receptionist randomized the treatment of ten participants at the office, and results do not change by excluding them. She conducted the on-the-spot randomization when either i) she could not locate the jobseeker's treatment status (e.g., due to internet connection issues), or ii) a candidate was mistakenly invited to the interview before being assigned a randomization batch, or iii) the number of candidates scheduled for the day was too low for make up a single strata.
- We initially planned to stratify the randomization in the Interview Experiment by predicted discrimination level *and* previous treatment assignments. During the implementation, we only stratified by the discrimination level. That is because, given the logistical constraints and lower-than-expected interview show-up rates, the batch sizes for the interview stage would be too small to fit many strata.

- The pre-registration mentions an endline phone survey with participants of the Address Omission Experiment. Its main purpose was to quality-check data collected by surveyors. As the experiment progressed, we added to this survey questions on whether the jobseeker applied to other jobs besides the ones in this study (see Table B.1). Later, we also added questions to verify what information job seekers believed that the HR firm in this study had about them prior to the application invite. We only asked the latter of about 370 jobseekers before shifting to the Information Experiment. We do not find evidence of a first stage on expected address visibility, but we believe that is due to noise and imperfect recall: These phone calls took place at least four weeks after the invite to apply, and only about 60% believe (when asked a placebo question) that the HR firm knew candidates' phone numbers – showing that recall is quite imperfect (Table B.1).

- 184 out of the main sample of 2,167 jobseekers were randomly assigned both to a condition in the Address Omission Experiment and to a condition in the Information Experiment. Out of those 184, 151 participated in a "pilot" version of the Information Experiment, in which we did not add the callbacks that could not be matched to a specific job ad (so information was that the callback rates were around 16% for both neighborhoods). The remaining 33 received the same information described in the main text. 10 of those 33 were randomly assigned to the *Status Quo* condition, so those 10 are part of the Information Experiment in the main text. To summarize, the **2,167** sample is made of: **1,303** who received only Address Omission assignments, **680** who received only Information assignments, and **184** who received both (10 of which were pre-randomization identical to the sample who only received Information assignments, so we pool them). See Appendix Section B.2

## B.1 Treatment Effects on Secondary Outcomes

Table B.1: Secondary Outcomes in the Address Omission Experiment

| | (1) Clicked application link (%) | (2) Words in cover letter | (3) Cover letter quality (0/100) | (4) Years of experience declared | (5) Declared experience in favela (0/1) | (6) Participated in endline (0/1) | (7) Applied for another job (0/1) | (8) Thought HR knew address before applying (0/1) | (9) Thought HR knew phone before applying (0/1) |
|---|---|---|---|---|---|---|---|---|---|
| *Address Omission* | -2.13 | 5.13 | 0.65 | 0.15 | -0.00 | 0.03 | -0.07 | -0.05 | -0.07 |
| | (3.31) | (7.98) | (1.08) | (0.10) | (0.01) | (0.04) | (0.06) | (0.07) | (0.07) |
| *Known Address* | -0.26 | 7.09 | 0.45 | 0.04 | -0.00 | 0.00 | -0.08 | -0.04 | -0.03 |
| | (3.29) | (7.35) | (1.03) | (0.09) | (0.01) | (0.04) | (0.06) | (0.07) | (0.07) |
| Observations | 1303 | 1303 | 1303 | 1303 | 1303 | 975 | 422 | 341 | 341 |
| *Status Quo* Average | 62.50 | 52.43 | 8.32 | 0.46 | 0.03 | 0.42 | 0.40 | 0.64 | 0.63 |

*Note*: Sample includes only those who did not participate in the Information Experiment (as in the main text). Outcome in column: (1) whether the candidate clicked the link to start the application form in the WhatsApp message; (2) how many words applicants wrote in response to optional question at the end of the application form in which they could freely introduce themselves and say why they thought they were a good fit; (3) ChatGPT 4 rating of each essay, with a prompt describing the job openings and asking it to put itself if place of an HR consultant; (4) total years of experience declared in the application form; (5) whether any courses or experiences declared in the application form could be easily linked to a favela address or institution; (6) whether the participant responded to the endline *phone* survey (smaller sample size as not all batches were contacted); (7) whether the endline participant declared applying for another job besides the ones in this study; (8) whether jobseeker thought that the HR inviting them to apply knew their address; (9) whether jobseeker thought that the HR inviting them to apply knew their phone number.

Table B.2: Secondary Outcomes in the Interview Experiment

| | (1) Nervousness cues (0/1) | (2) Gave away address (0/1) | (3) Used slang (0/1) | (4) Aggregate question-wise performance (SD) | (5) Interviewer professionalism (perceived, SD) | (6) Interviewer preparedness (perceived, SD) | (7) HR firm values diversity (SD) |
|---|---|---|---|---|---|---|---|
| *Name-Only* | 0.00 | 0.03 | -0.01 | 0.08 | -0.07 | 0.07 | 0.07 |
| | (0.03) | (0.02) | (0.01) | (0.08) | (0.11) | (0.09) | (0.08) |
| Observations | 422 | 422 | 422 | 422 | 422 | 422 | 422 |
| *Name-and-Address* Average | 0.10 | 0.02 | 0.01 | 0.00 | -0.00 | -0.00 | 0.00 |

*Note*: Outcome in column: (1) whether the interviewer noted that the candidate laughed out of nervousness, stuttered, or had a shaking voice; (2) whether the candidate explicitly gave away their neighborhood during the interview; (3) whether the candidate used slang during the itnerview; (4) ICW index of performance in the six main interview questions; (5) from the candidate's feedback form, normalized rating of the interviewer's professional behavior; (6) from the candidate's feedback form, normalized rating of how prepared the interviewer seemed to be; (7) from the candidate's feedback form, normalized rating of how much it seemed like the HR firm valued diversity. Outcomes in columns (5) to (8) had little variation, about 85% of candidates picked ten out of ten in those questions.

Table B.3: Secondary Outcomes in the Information Experiment

| | (1) Words in cover letter | (2) Cover letter quality (0/100) | (3) Exp. Disc. on the job (% chance) | (4) Exp. disc for self (%) | (5) Wage gap for self (%) | (6) # of planned future applications | (7) Exp. future callback rate (%) | (8) More excited about job search (0/1) | (9) Agrees job mkt is unfair (-2/2) | (10) Agrees job search is an opportunity (-2/2) | (11) Agrees one must ignore disc. (-2/2) | (12) Agrees one must apply to all jobs (-2/2) | (13) Plans to increase search effort (0/1) | (14) Feels is in bad position by endline (1/4) | (15) Improved search strategy by endline (0/1) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Favela Info* | 15.80 | 1.50 | -6.47** | -2.77 | -1.26 | -8.27 | -2.74 | -0.22*** | -0.01 | -0.13 | 0.21* | -0.04 | -0.02 | -0.04 | -0.10 |
| | (10.34) | (1.57) | (2.54) | (3.09) | (2.95) | (6.46) | (2.66) | (0.04) | (0.12) | (0.11) | (0.11) | (0.13) | (0.03) | (0.11) | (0.06) |
| *Full Info* | 15.41 | 0.33 | -5.29** | -14.63** | -9.29 | -3.37 | 0.72 | -0.15*** | -0.13 | 0.09 | 0.00 | 0.10 | 0.01 | 0.14 | -0.04 |
| | (10.36) | (1.54) | (2.66) | (6.62) | (8.59) | (6.92) | (2.78) | (0.04) | (0.13) | (0.11) | (0.11) | (0.13) | (0.03) | (0.11) | (0.06) |
| Observations | 690 | 690 | 690 | 689 | 690 | 690 | 690 | 670 | 670 | 670 | 670 | 670 | 690 | 389 | 389 |
| *No Info* Average | 47.89 | 8.61 | 47.64 | 13.63 | 11.60 | 50.38 | 43.36 | 0.79 | 0.80 | 1.02 | 0.98 | 0.75 | 0.87 | 2.43 | 0.56 |

*Note*: Sample includes only those who did not participate in the Address Omission Experiment, as in the main text. Outcome in column: (1) how many words applicants wrote in response to optional question at the end of the application form in which they could freely introduce themselves and say why they thought they were a good fit; (2) ChatGPT 4 rating of the essay, with a prompt describing the job openings and asking it to put itself if place of an HR consultant; (3) predicted chance of suffering anti-favela discrimination over the first year working in a formal job outside favela; (4) expected discrimination implied by comparing the respondent's predicted odds of finding a new formal job over the next six months against someone similar living just outside the favela; (5) wage gap between the two people from the outcome in column 3 in case they found a job (a negative number implies anti-favela wage discrimination); (6) number of applications the candidate plans to send in the next two months; (7) expected callback rate to the applications referred to in the previous column; (8) agreement with "the job market is extremely unfair" (Likert scale, -2=completely disagrees, 2=completely agrees); (9) agreement with "the job search is an opportunity to find the place I best fit into", same scale; (10) agreement with "to do well in the labor market, we can not think about employer discrimination all the time", same scale; (11) agreement with "to do well in the labor market, you have to apply to all possible vacancies", same scale; (12) whether one believes they will increase their job search efforts over the next two months; (13) whether the endline survey respondent thinks that someone like them, from their neighborhood, has [NO=1/SOME=2/GOOD=3/GREAT=4] odds of finding a new formal job fast, (14) whether the endline survey respondent says they have worked on their résumé and took new measures to improve the odds they will find a job.

## B.2 Main Tables and Figures Including Individuals Who Participated in Both the Address Omission and Information Experiments

Tables and figures related exclusively to the Interview Experiment or already including the full sample are omitted, since they would not change.

Table B.4: Table 1 Including Sample Overlapping with Address Omission Experiment

| | (1) Responded to endline (0/1) | (2) Exp. discrimination (categorical, 1-4) | (3) # of sent apps (categorical, 1-4) |
|---|---|---|---|
| Favela Info | -0.00 | 0.02 | -0.03 |
| | (0.04) | (0.08) | (0.12) |
| Full Info | 0.01 | -0.15* | 0.06 |
| | (0.04) | (0.09) | (0.12) |
| Observations | 864 | 506 | 508 |
| Controls | No | No | No |
| *No Info* Mean | 0.6 | 2.3 | 2.5 |
| Favela=Full *p* | 0.78 | 0.04 | 0.45 |

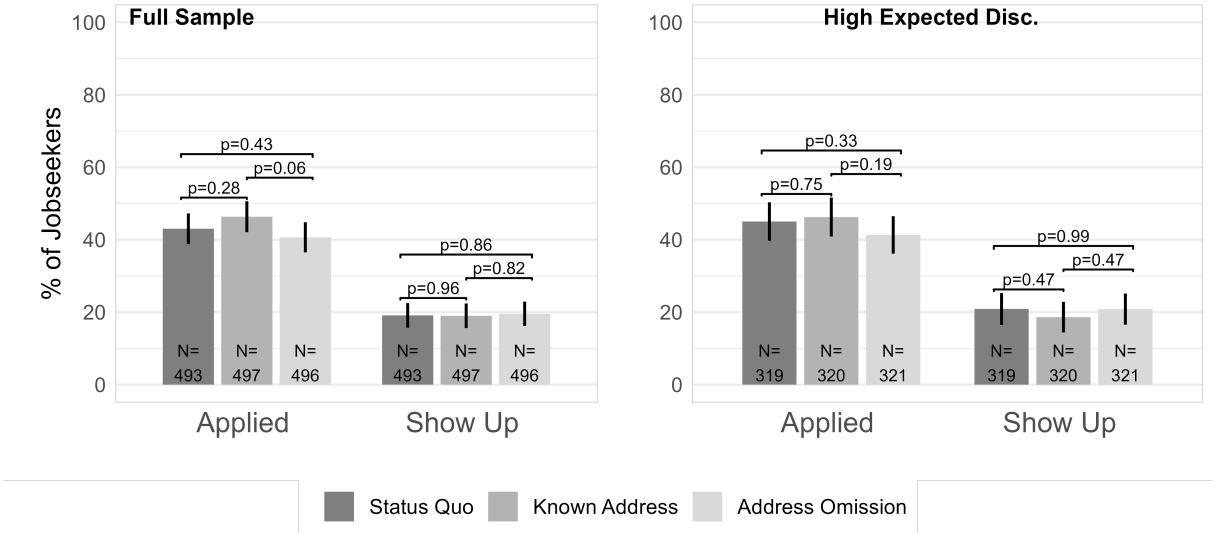
*Note*: See notes for Table 1 for details.

Table B.5: Table 2 Including Individuals Who participated in Both the Address Omission and Information Experiments

| | (1) Agg. Performance Index (SDs) | (2) Applied (%) | (3) Showed Up (%) | (4) Applied (%) | (5) Showed Up (%) |
|---|---|---|---|---|---|
| ↓ $\mathbb{E}$[Disc.] × white | 0.31** | 10.84* | 8.95* | -5.42 | -8.32 |
| | (0.13) | (5.72) | (4.87) | (7.88) | (5.84) |
| ↓ $\mathbb{E}$[Disc.] × non-white | 0.07 | -7.60** | -1.32 | 9.45** | 4.25 |
| | (0.07) | (3.08) | (2.43) | (4.10) | (3.44) |
| White jobseeker (0/1) | -0.05 | -4.60 | -1.44 | 6.01 | 2.27 |
| | (0.10) | (3.75) | (2.93) | (5.00) | (4.12) |
| Observations | 422 | 1486 | 1486 | 864 | 864 |
| Treatment | ↓ $\mathbb{E}$[address visibility] | ↓ $\mathbb{E}$[address visibility] | ↓ $\mathbb{E}$[address visibility] | ↓ $\mathbb{E}$[mkt-level disc.] | ↓ $\mathbb{E}$[mkt-level disc.] |
| Control Average | -0.0 | 44.4 | 18.9 | 41.4 | 20.1 |
| White=non-white p-value | 0.09 | 0.00 | 0.06 | 0.09 | 0.06 |

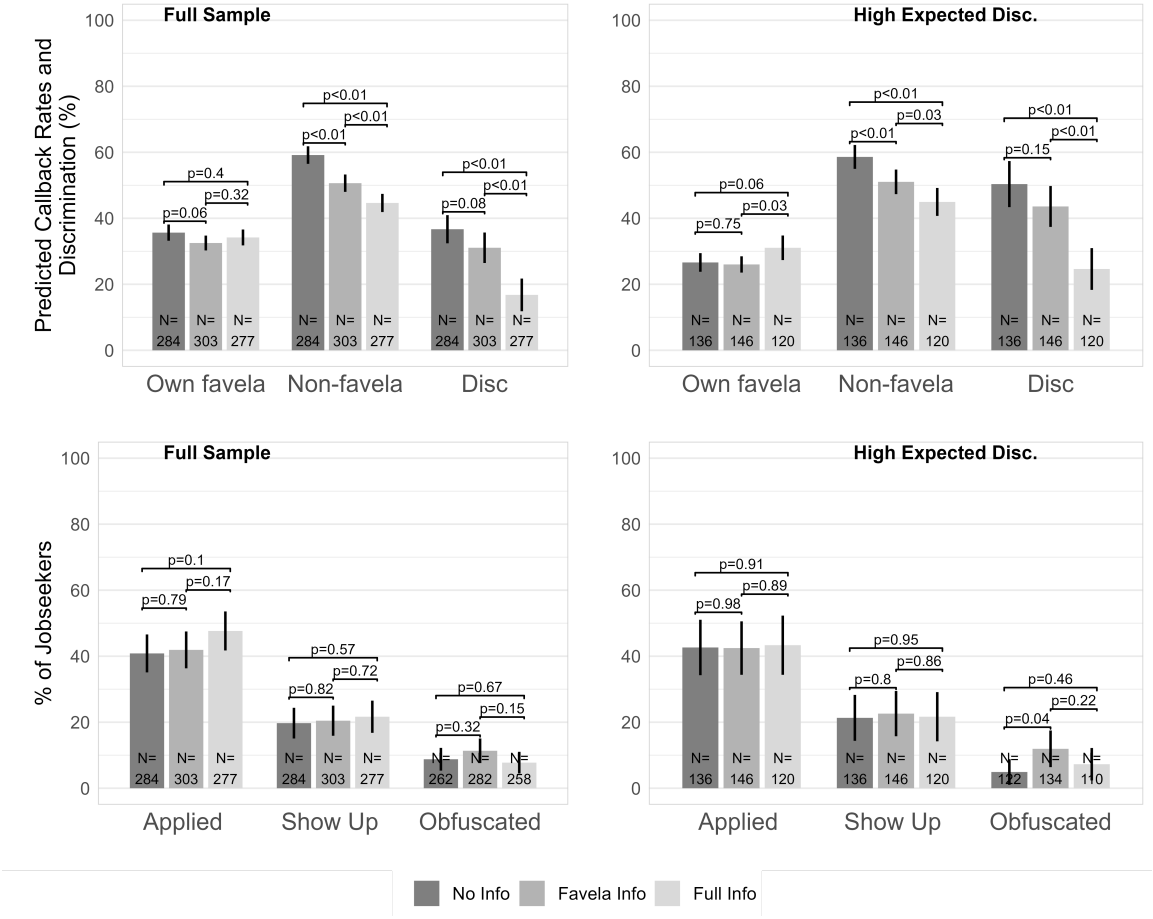
*Note*: See notes for Table 2 for details.

Figure B.1: Figure 4 Including Sample Overlapping with Information Experiment




*Note*: See notes to Figure 4 for details.

Figure B.2: Figure 5 Including Sample Overlapping with Address Omission Experiment




*Note*: See notes to Figure 5 for details.

# C Audit Study

**Picking Résumé Addresses.** For addresses in each neighborhood, we picked streets that were i) entirely contained in the neighborhood, ii) in the postal office list, and iii) up to a 15-minute walk from a bus stop in the avenue between Maré and Bonsucesso. These choices guaranteed that employers could back out neighborhood unambiguously, and keep commuting time to any job as constant as possible.

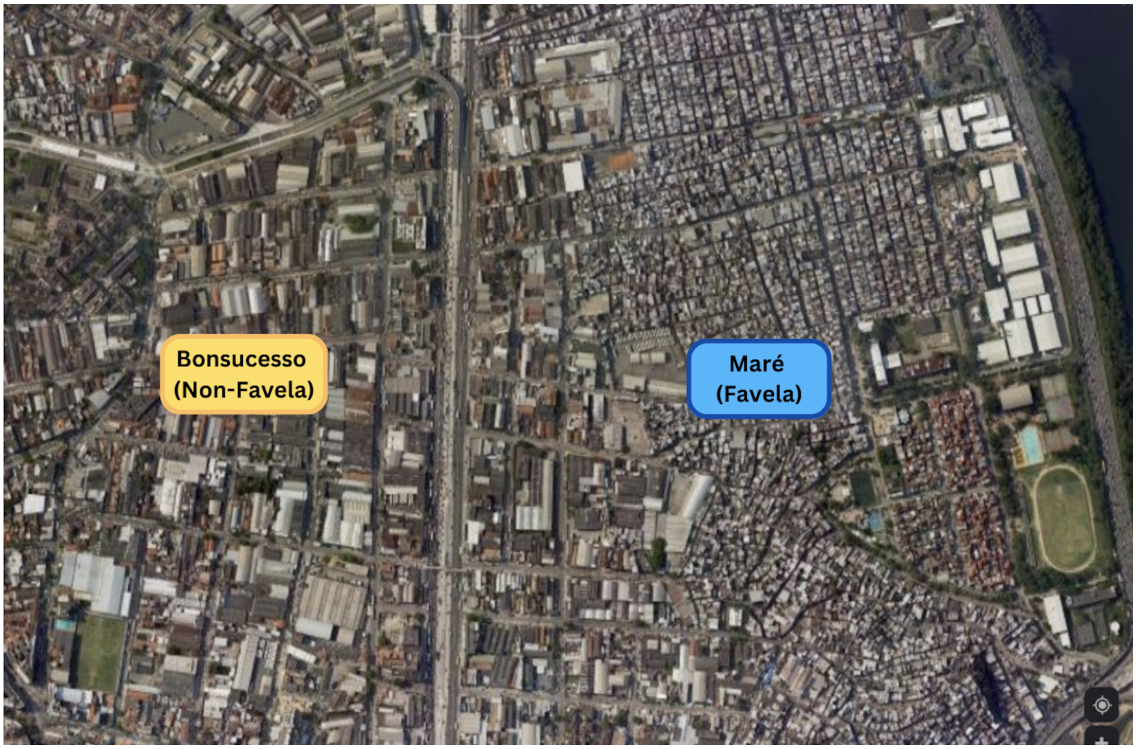
Table C.1: Audit Study Results

| | (1) Callback (%) | (2) Callback (%) | (3) Callback (%) |
|---|---|---|---|
| Maré résumé | -0.34 | -0.40 | -1.04 |
| | (1.28) | (1.29) | (1.18) |
| Observations | 1400 | 1400 | 1400 |
| *No Info* Mean | 16.96 | 16.96 | 16.96 |
| Controls | No | Yes | No |
| Job FEs | No | No | Yes |

*Note*: Outcome variable evaluates to 100 if the application received a positive response and zero otherwise. Maré résumé is a dummy for the fictitious applicant being from Maré. Controls include the job's city region, and the website in which we found it. The callback level here is about 3% lower than than the numbers used in the Information Experiment because for the regressions we only consider callbacks we could link to unique postings. Standard errors clustered at the posting level shown between parenthesis.

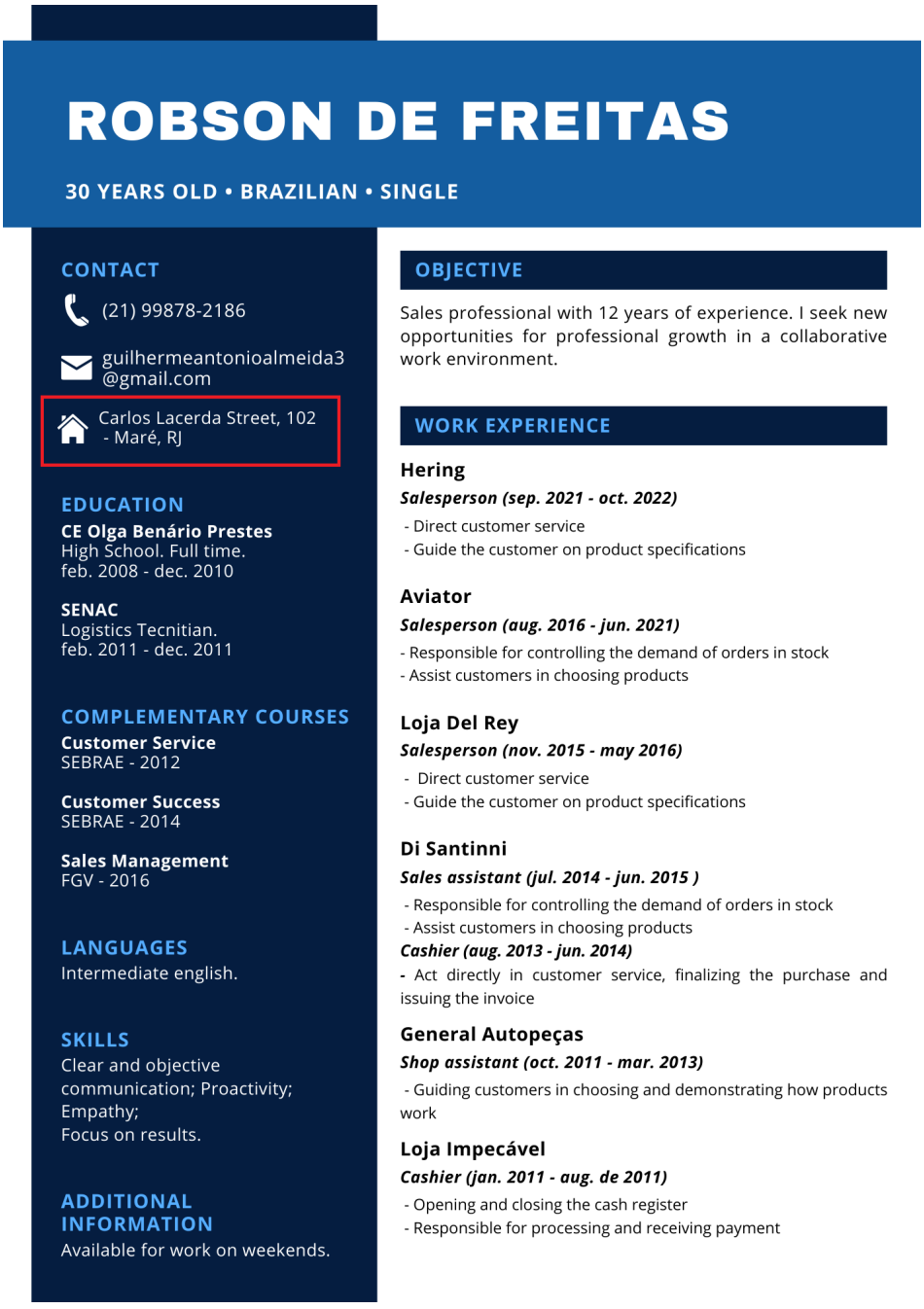
## C.1 Audit Study Neighborhoods

Figure C.1: Bonsucesso (Non-Favela) *vs.* Maré (Favela)




*Note*: This image shows the geographic location of the two neighborhoods for the audit study: Bonsucesso (Non-Favela) and Maré (Favela). The large avenue in the picture is the divide between each region.

Figure C.2: Example Résumé – Maré home address



# ROBSON DE FREITAS

30 YEARS OLD • BRAZILIAN • SINGLE

## CONTACT

(21) 99878-2186

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Carlos Lacerda Street, 102 - Maré, RJ

## EDUCATION

**CE Olga Benário Prestes**
High School. Full time.
feb. 2008 - dec. 2010

**SENAC**
Logistics Tecnitian.
feb. 2011 - dec. 2011

## COMPLEMENTARY COURSES

**Customer Service**
SEBRAE - 2012

**Customer Success**
SEBRAE - 2014

**Sales Management**
FGV - 2016

## LANGUAGES

Intermediate english.

## SKILLS

Clear and objective communication; Proactivity; Empathy;
Focus on results.

## ADDITIONAL INFORMATION

Available for work on weekends.

## OBJECTIVE

Sales professional with 12 years of experience. I seek new opportunities for professional growth in a collaborative work environment.

## WORK EXPERIENCE

**Hering**
***Salesperson (sep. 2021 - oct. 2022)***
- Direct customer service
- Guide the customer on product specifications

**Aviator**
***Salesperson (aug. 2016 - jun. 2021)***
- Responsible for controlling the demand of orders in stock
- Assist customers in choosing products

**Loja Del Rey**
***Salesperson (nov. 2015 - may 2016)***
- Direct customer service
- Guide the customer on product specifications

**Di Santinni**
***Sales assistant (jul. 2014 - jun. 2015 )***
- Responsible for controlling the demand of orders in stock
- Assist customers in choosing products

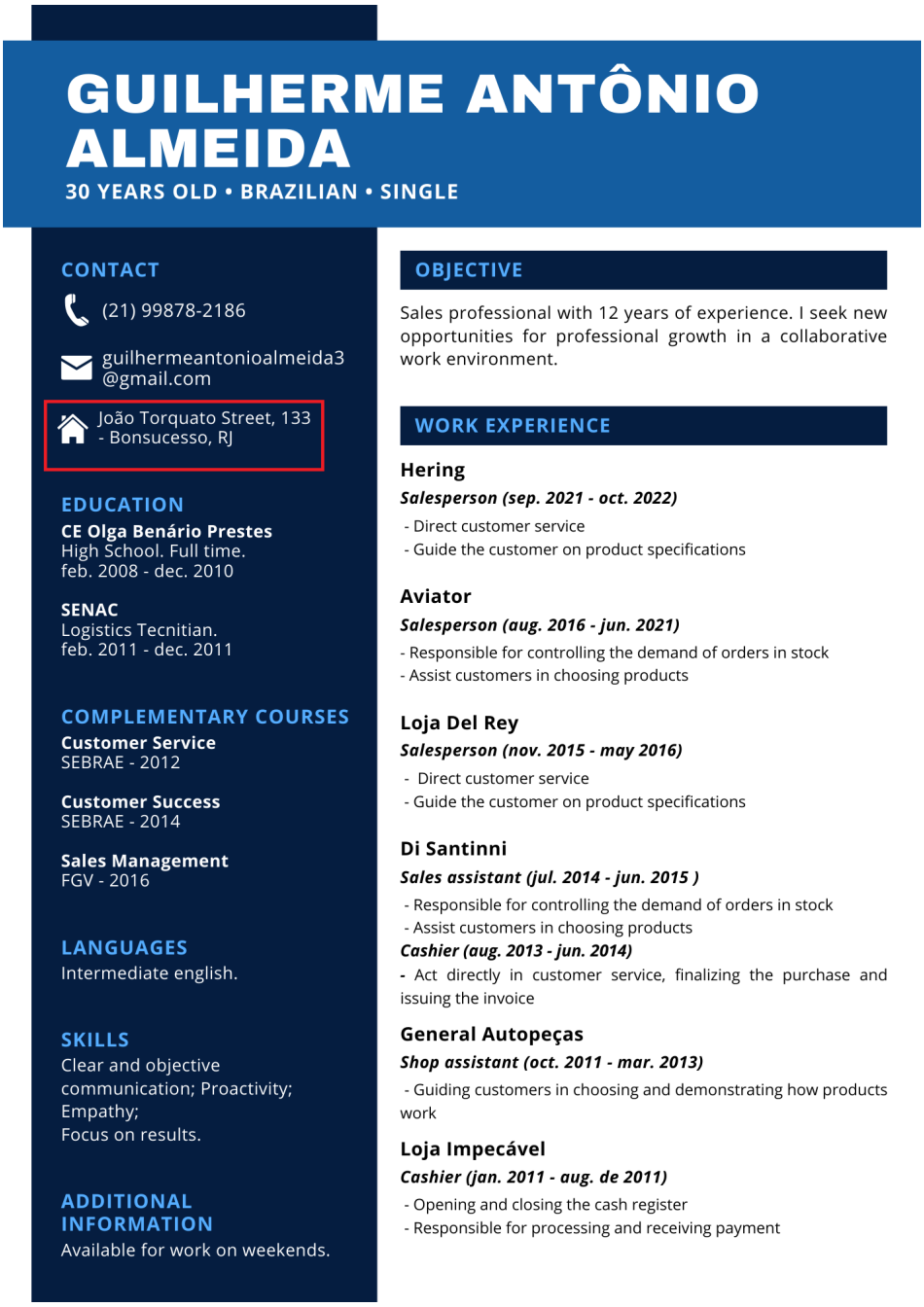
***Cashier (aug. 2013 - jun. 2014)***
- Act directly in customer service, finalizing the purchase and issuing the invoice

**General Autopeças**
***Shop assistant (oct. 2011 - mar. 2013)***
- Guiding customers in choosing and demonstrating how products work

**Loja Impecável**
***Cashier (jan. 2011 - aug. de 2011)***
- Opening and closing the cash register
- Responsible for processing and receiving payment

*Note*: This image shows one of the résumés used in the audit study. We drew the red box around the address in this picture for emphasis. It was not present in the original résumé.

Figure C.3: Example Résumé – Bonsucesso Address



# GUILHERME ANTÔNIO ALMEIDA

30 YEARS OLD • BRAZILIAN • SINGLE

## CONTACT

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João Torquato Street, 133 - Bonsucesso, RJ

## EDUCATION

**CE Olga Benário Prestes**
High School. Full time.
feb. 2008 - dec. 2010

**SENAC**
Logistics Tecnitian.
feb. 2011 - dec. 2011

## COMPLEMENTARY COURSES

**Customer Service**
SEBRAE - 2012

**Customer Success**
SEBRAE - 2014

**Sales Management**
FGV - 2016

## LANGUAGES

Intermediate english.

## SKILLS

Clear and objective communication; Proactivity; Empathy;
Focus on results.

## ADDITIONAL INFORMATION

Available for work on weekends.

## OBJECTIVE

Sales professional with 12 years of experience. I seek new opportunities for professional growth in a collaborative work environment.

## WORK EXPERIENCE

**Hering**
***Salesperson (sep. 2021 - oct. 2022)***
- Direct customer service
- Guide the customer on product specifications

**Aviator**
***Salesperson (aug. 2016 - jun. 2021)***
- Responsible for controlling the demand of orders in stock
- Assist customers in choosing products

**Loja Del Rey**
***Salesperson (nov. 2015 - may 2016)***
- Direct customer service
- Guide the customer on product specifications

**Di Santinni**
***Sales assistant (jul. 2014 - jun. 2015 )***
- Responsible for controlling the demand of orders in stock
- Assist customers in choosing products

***Cashier (aug. 2013 - jun. 2014)***
- Act directly in customer service, finalizing the purchase and issuing the invoice

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***Shop assistant (oct. 2011 - mar. 2013)***
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**Loja Impecável**
***Cashier (jan. 2011 - aug. de 2011)***
- Opening and closing the cash register
- Responsible for processing and receiving payment

*Note*: Image shows one of the résumés used in the audit study. We drew the red box around the address in this picture for emphasis. It was not present in the original résumé.

# D Supporting Materials

## D.1 Door-to-door Survey




Figure D.1: Door-to-Door Baseline Survey
*Notes*: This Figure shows surveyors interviewing research participants in Maré.

## D.2 Application Form

Figure D.2: Script for Eliciting Expected Discrimination in Baseline Survey – Maré's Version

*Now I'm going to ask you some questions about the differences between job seekers from different neighborhoods. We know the right answer to two of them.*

*If, at the end of our project, you are among the 10 people who came closest to getting these two questions right, you will receive an additional $100 Brazilian reais.*

*Let me tell you the story to start:*

*At the beginning of our project, the researchers organizing this study heard from the population of several favelas here in Rio about how it was more difficult to apply for a formal job living in a community.*

*To really understand the size of the challenge, researchers sent 1,400 applications with fake résumes, but as if they were real people, for 700 vacancies in sales in the city of Rio.*

*The résumes were from men and women, from people with experience and suitable for each vacancy.*

*The only difference between the résumes was that some said that the address was from Bonsucesso, and others said that the address was from Maré.*

*I will give you a moment to look at an example of one of the resumes sent.*

*[PASS THE TABLET TO THE INTERVIEWEE]*

*The researchers calculated WHAT PERCENTAGE of résumes sent with BONSUCESSO's address were selected (for example, for a training period) or called for an interview.*

*They also calculated this percentage for MARE's résumes.*

*To get the additional $100 Brazilian reais, I'm going to ask you to try to guess what they found, okay?*

- *WHAT PERCENTAGE of résumes sent with BONSUCESSO's address do you guess were selected or invited for an interview?*
- AND WHAT PERCENTAGE OF MARE's?

*Note*: The script above was used for surveys in Maré. Surveys completed in other favelas also included elicitations of the callback rates for that other favela (and Maria da Graça, in the case of Jacarézinho). The description of the audit study did not mention specific neighborhoods in surveys in favelas other than Maré.

Figure D.3: Job Descriptions

**Job Description 1**
**Sales Representative**

Duties:
- Trial, sale, and demo of products, focusing on customer satisfaction and loyalty, ensuring the cleanliness and organization of the store

Prerequisites:
- High School Degree
- Skills with persuasion and approaching
- Office Package

Desirable:
- Have a good beauty repertoire (knowing products, competing brands and influencers);
- Results-oriented

**Job Description 2**
**Direct Sales Promoter**

Duties:
- Responsible for attracting and prospecting new resellers in its operating unit. Fill out forms, register and deliver the documentation to the Direct Sale center.

Prerequisites:
- High School Degree

Desirable:
- Experience with negotiation and persuasion to charm customers

**Job Description 3**
**Direct Sales Supervisor**

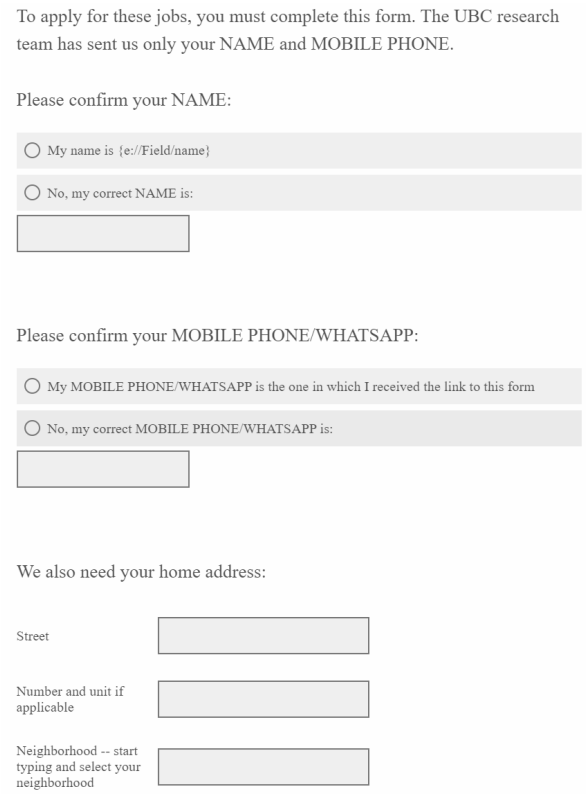
Duties:
- Responsible for receiving payments, operating sales systems, issuing invoices, making contact with resellers

Prerequisites:
- High School Degree
- Office Package
- Experience with sales and payments

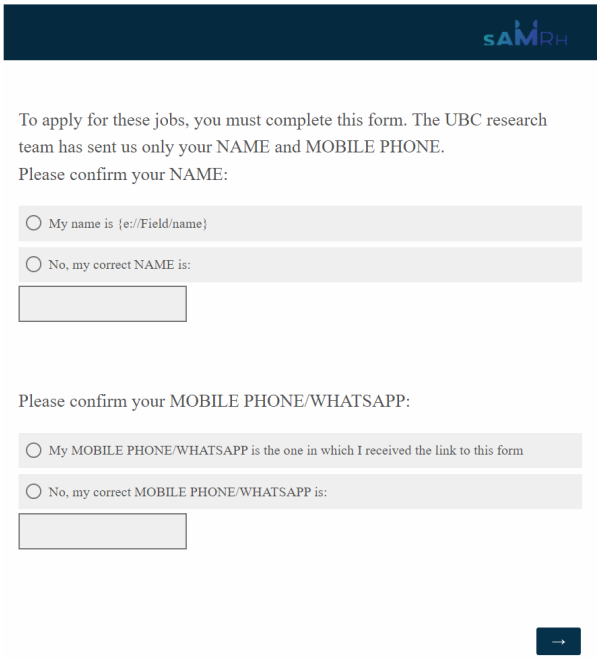
Desirable:
- Ability to do math
- Good verbal and written communication
- Detail oriented

*Note* Job descriptions as presented in the online application forms (translated from Portuguese).

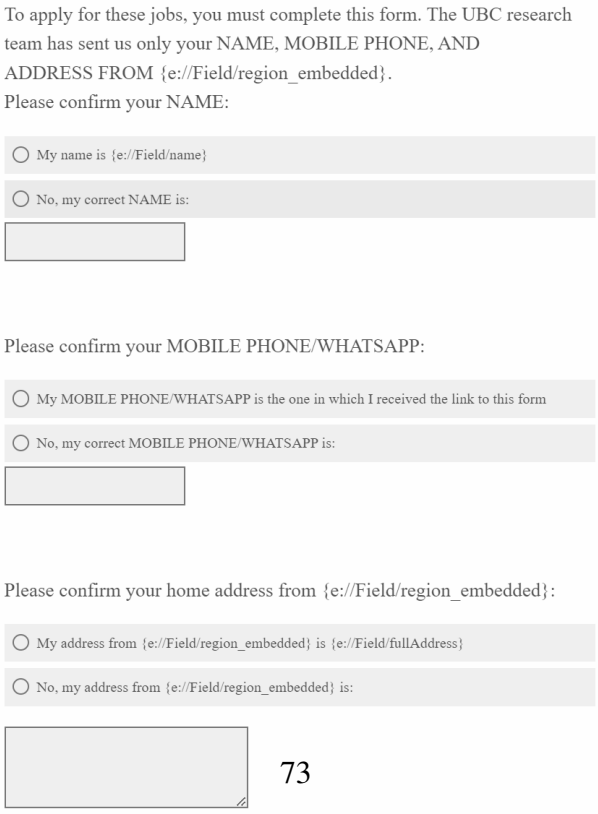
Figure D.4: Second Screen of the Application Form of Each Experimental Condition in the Address Omission Experiment




(a) *Status Quo*




(b) *Address Omission*




(c) *Known Address*



(a) Co-Working Reception (b) Interview Room

Figure D.5: Interview Co-Working Space

## D.3 Interview Script

The italicized text below was *not* read out loud.

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*You [the interviewer] must treat all candidates equally and as uniformly as possible. Ideally, your tone will be friendly and reserved.*

*Introduce yourself and confirm the candidate's name. Let the candidate know that the interview will be recorded, for quality control and training of future interviewers.*

*Stick to the script as much as possible. Then you should say that you are going to start the interview. If you have questions, you should wait until the end.*

**Q1.** How comfortable do you feel working with laptops/computers?
*(1) Very comfortable, (2) A little comfortable, (3) Indifferent, (4) A little uncomfortable, (5) Very uncomfortable*

**Q2.** Do you typically send emails or type more complex texts? Can you tell me the last time you did something like this?

*OPEN ANSWER BOX*

**Q3.** Have you ever used Word, Excel, or similar programs? If so, can you give me an example of something you have done with this program?

*OPEN ANSWER BOX*

*Interviewer evaluates how well the candidate did on this question, from 0 to 10*

**Q4.** Now I will also you to do an activity. Think of a product you like and know well. It could be a type of clothing, a cell phone, a car, anything, but preferably something that you know how to describe and sell well, ok? Can you try to convince me that I should buy this product from you or your store, instead of buying from a competitor? As if you were the seller of that product.

*OPEN ANSWER BOX*

*Interviewer evaluates how well the candidate did on this question, from 0 to 10, and notes: (i) the product sold, (ii) the main argument, and (iii) whether it was convincing.*

**Q5.** What would you say are your top 3 skills for a sales job, and why do you think you are

good at them? It could be an example showing why you are good too.

*OPEN ANSWER BOX*

*Interviewer evaluates how well the candidate did on this question, from 0 to 10*

**Q6.** And your main disadvantages? Can you explain or give examples of how they affect you?

*OPEN ANSWER*

*Interviewer evaluates how well the candidate did on this question, from 0 to 10*

**Q7.** What do you think makes you the best fit for this position, compared to your competitors?

*OPEN ANSWER BOX*

*Interviewer evaluates how well the candidate did on this question, from 0 to 10*

**Q8.** Thinking about your background and your day-to-day life, how would you say your experiences would help you to be a good fit for this position? You don't just need to give professional experiences. It could be academic, school, some leadership position, participation in social projects, volunteer work, or something else. *OPEN ANSWER*

*Interviewer evaluates how well the candidate did on this question, from 0 to 10*

**Q9.** Would you like to add any other information?

*OPEN ANSWER BOX*

**Q10. [Interviewees self-administer this question on a tablet]**

*I see myself as a person that...*

1. *Does a meticulous job*
2. *It's a little careless sometimes*
3. *It's trustworthy*
4. *Tends to be disorganized*
5. *Tends to be lazy*
6. *Perseveres until tasks are completed*
7. *Works efficiently*
8. *Make and follow plans*

9. *Is easily distracted*

*Options are: (1) Totally disagree, (2) Partially disagree, (3) Neither agree nor disagree, (4) Partially agree, (5) Totally agree.*
*Ask if the candidate has any questions, and instruct the candidate to return to the reception for payment and final orientation.*
*Immediately after saying goodbye to the candidate, the interviewer responds, on a scale from 0 to 10 to each of the questions below. 0 means "Extremely bad"and 10 means "Extremely well".*

1. *Overall, how well did the candidate perform?*
2. *How nervous did the candidate seem?*
3. *How focused did the candidate seem?*
4. *How professional was the candidate throughout the interview?*

*Now, during the interview, the candidate... [Check all that apply]*

1. *Had a shaky voice*
2. *Stuttered*
3. *Laugh nervously*
4. *Dressed in informal clothes*
5. *Used slangs*
6. *Made MANY grammar mistakes in Portuguese*
7. *Used swear words*
8. *Mentioned personal things, irrelevant to the position*
9. *Mentioned that they were religious or went to church or worship*
10. *Mentioned that they lived in a favela*
11. *Talked about where they came from (on that day of the interview)*
12. *Talked about where they lived*

13. *Talked about where they were born*
14. *Asked you personal questions*
15. *Asked you irrelevant questions for the position*
16. *Showed you know they knew something(s) about the company or the position*
17. *Used very formal language*
18. *Looked you in the eyes when answering*
19. *Avoided looking into your eyes*
20. *Was very shy*
21. *None of the above*