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 three field experiments with over 2,000 jobseekers in Rio de Janeiro's favelas. In these experiments, jobseekers can apply and interview for real jobs. Over 80% of those jobseekers overestimate anti-favela discrimination, as we measure it in a new audit study. Jobseekers who were randomly told that their interviewer would know their name *and address* believed that their interview performance was 0.17SDs worse than those who were told that the interviewer would only know their name. Focusing on jobseekers who expected at-or-above median discrimination, we find that not only did they believe that they performed worse when told that interviewers would know their addresses, their interviewers also rated them worse by roughly 0.2SDs. Removing the need to declare an address at the application stage increases interview attendance *only* for white jobseekers, likely because they can pass for non-favela residents and ignore racial discrimination. We also present results from a complementary study in which we randomize whether we tell jobseekers that we found no discrimination in our audit study. Our findings show that expected discrimination may affect jobseekers' search, especially at the interview stage.

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1 Introduction

Employers discriminate along many dimensions, including race, ethnicity, sexuality, and criminal history (Neumark, 2018; Rich, 2014; Riach and Rich, 2002). While audit experiments cleanly identify such disparate treatment, they do not reveal the equilibrium effects of discrimination. In particular, the equilibrium effects also depend on jobseekers' beliefs and reactions to discrimination; jobseekers may hold miscalibrated beliefs, and theory shows that expected discrimination beliefs can become self-fulfilling (Coate and Loury, 1993). Miscalibration may be problematic if individuals overestimate discrimination, becoming discouraged or too nervous to give their best performance at an interview. Belief combinations may be self-perpetuating if they make some group appear different on average to recruiters (e.g., discouraged applicants may invest less in their applications), allowing initial misperceptions to evolve into actuality. Furthermore, understanding expected discrimination may open new policy angles. For example, if jobseekers overestimate discrimination, it might be desirable to disseminate information on actual discrimination rates, and for employers to credibly signal commitment to anti-discrimination policies.

This paper presents results from three interconnected field experiments with jobseekers, designed to identify how they anticipate and react to discrimination during the application and interview stages. Jobseekers in our sample (N=2,200) are *favela* (urban slum) residents in Rio de Janeiro, Brazil, where favela residents are negatively stereotyped. We partner with a large cosmetics company to advertise real sales jobs and observe favela jobseekers applying and interviewing for such jobs. Jobseekers enter our study's pipeline through our door-to-door baseline survey, where we find that about 87% of jobseekers overestimate anti-favela discrimination – as measured in an audit study we ran by sending 1,400 job applications. By experimentally varying whether jobseekers expect their addresses to be visible and how much discrimination jobseekers may expect, our three labor supply-side experiments reveal that expected discrimination negatively affects interview performance but has a muted effect on job application rates. At the same time, we see that white jobseekers, who are a minority inside favelas but a majority outside, apply more often and perform better in interviews when they believe their addresses are hidden. This could be because, with hidden addresses, white jobseekers can pass for non-favela residents. Passing is harder for non-whites, who might also expect racial discrimination anyway.

In Rio, about 1.5 million people, or 22% of the city's population, live in favelas. In most favelas, criminal organizations hold a monopoly over violence. Favela residents are more likely to be non-white, immigrants, less educated, and poorer than non-favela residents. The deroga-

tory term "favelado", meaning "slummed", is widely used. In this context, most recruiting firms collect home address information from applicants. While this is meant to gauge how hard the worker's daily commute might be, recruiters can also use it to discriminate regardless of distance to work. In our door-to-door survey, over 60% of jobseekers mention violent police raids, racial and cultural prejudice, antipathy for favela residents, and fear of crime and violence as important reasons why firms avoid hiring people from favelas.

Our focus on expected *anti-favela* discrimination provides two main advantages. The first is that we can manipulate stigma visibility to randomize expected discrimination – and by stigmas, we mean the applicants' characteristics that employers use to discriminate, associated with negative stereotypes (Loury, 2002). Manipulating the expected visibility of a more visible stigma, like race, would not be as effective since jobseekers would expect it to quickly become visible (e.g., at the interview stage). This also let us study how visible and invisible stigmas interact: the stigma of living in a favela may be visible or not (similar to a criminal history stigma), and it might compound with or substitute for other stigmas. Here, we study how address visibility interacts with a racial stigma. The second advantage is that we can study a type of discrimination that may be relevant in perpetuating poverty traps in many contexts. Almost a billion people live in urban slums (UN, 2016), and even in developed countries, we see urban divides (e.g., public housing projects in the US). Can expected discrimination play a role in perpetuating such divides?

Most favela jobseekers overestimate the anti-favela discrimination in callback rates we find in our audit study. For the audit study, we created a set of fictitious workers' profiles and résumés and then made copies that only differed in name, phone, and address. We used them to apply for 700 sales jobs in Rio, sending two different-profile applications to each. We find very similar callback rates, 19.3 and 19.6%, for favela and non-favela résumés (p=0.38 to 0.87 for the difference). We incentivized jobseekers in our door-to-door survey to predict our audit study's callback rates. Over 85% predict anti-favela discrimination, while about 60% predict that having a favela address would cause callback rates to drop 50% or more.

To measure how jobseekers actually respond to expected discrimination, we set up an HR firm that advertised real sales job opportunities in a large cosmetics firm.[1] At the beginning of the door-to-door survey, after some background questions, jobseekers could agree to share their professional details with this HR firm (described as a partner in the study). Within the next few days, the HR firm texted the jobseeker with an invitation to apply. Then, since all participants met minimal requirements, the HR firm invited all applicants for interviews at its office in

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[1]These are not strongly gendered jobs: in our study, the application rate for males was 37%, and for non-males, it was 44%.

Downtown Rio. We used this structure to run three field experiments. In each experiment, we randomized an intervention to shift perceptions of discrimination, with two experiments at the job application stage and one at the interview stage.

Our experiments used two complementary strategies to explore how expected discrimination affects application rates. Two of our experiments – the Address Omission and the Interview Experiment – randomized expected address visibility, at the application and interview stages respectively. The idea behind randomizing expected address visibility is that if jobseekers think the employer does not know their address, they should not expect anti-favela discrimination. The other strategy was to shift beliefs about market-level discrimination by randomly informing some jobseekers about our audit study findings. We describe the Interview Experiment first, and we will explain how these approaches complement each other along the way.

We ran the Interview Experiment (N=422, out of the 2,200 invited to apply) in an office staffed with one receptionist and up to two interviewers. We scripted interviews and interactions. On arrival, the receptionist asked jobseekers to confirm their name, date of birth, and address, then told them to wait. Moments later, the receptionist told the jobseeker that the interviewer was ready, and that, to keep the process objective, "the interviewer will only know your name" (*Name-Only* condition) or "your name and address" (*Name-and-Address*). The two conditions differ only by two words: "and address". The interviewer evaluated the candidate immediately after the interview, and jobseekers filled out a form with self-assessment questions at the reception desk before leaving. Interviewers were blind to the whole procedure and learned about the jobseekers' neighborhood of origin only after the end of the experiment, so any differences must be triggered by changes in the interviewees' behaviors or beliefs.

Our main interview performance measures are aggregates of the interviewers' and interviewees' evaluations. Interviewers coded, on 0–10 scales, i) how well the interviewee performed overall, ii) how nervous the interviewee was, and iii) how professionally the interviewee behaved. Interviewees filled out self-assessments for the same three dimensions. 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 interviewee (Anderson, 2008). As our primary aggregate measure, we average the two.

Hearing that the interviewer will only know one's name increases the aggregate performance index by 0.13SD (p=0.03). The effects are stronger on the self-assessment index (0.17SD, p<0.01). The effect size on the interviewer's evaluation index is 0.09SD, and it is not statistically significant (p=0.28) nor different from the effects on self-assessment (p=0.34). Nevertheless, when we split the sample into groups that expected below-median and at-or-above-median discrimination when predicting the audit study, we see that expected stigma visibility has a sta-

tistically significant negative effect of about 0.2SD on the interviewer's evaluation index among those expecting high discrimination, consistent with high expected discrimination actually damaging interviewer-assessed performance. Hence, in interviews outside our experiment, expected discrimination can exacerbate the effects of whatever discrimination exists. It can lead to self-fulfilling prophecies, at least in the narrow sense that if a jobseeker expects a worse evaluation (because of their address), they indeed get one, even if there was no discrimination.

There is little reason to believe that expected discrimination would have the same effects at different points of the application procedure. The visibility of certain characteristics, stakes, costs, and psychological pressure can differ widely from when filling out an application to the time of the job interview. To understand the role of expected discrimination at earlier job search stages, and selection into interviews, we conducted two complementary experiments: the Address Omission Experiment and the Information Experiment. Together, they provide evidence that expected discrimination may not play a major role in job application decisions.

In the Address Omission Experiment (N=1,303), we manipulate stigma visibility by randomizing the the content of the application invite message and the application form. In our main treatment condition, *Address Omission*, the text message stated that address information was unnecessary at that stage, and the form did not mention address at all. In our *Status Quo* condition, the text message listed the home address as necessary information for applying, and people need to fill it in. The address requirement does not affect jobseeker behavior; we find an application rate of 42.7% in *Status Quo* and 41% in *Address Omission* (p=0.62 for the difference). Considering all the invited applicants, 19.3% of those in *Status Quo* show up for the interview, and 19.8% in the *Address Omission* (p=0.64 for the difference).

One possible explanation for the null effect is that people "pass" as non-favela residents (e.g., by declaring a different neighborhood or a relative's address) in the *Status Quo* condition. Consistent with this, 28% of the *Status Quo* applications obfuscate their address. To explore this channel, our design included an additional condition, *Known Address*, in which we shut down the possibility of obfuscation. *Known Address* was the same as *Status Quo* except that the online application form already contained the applicant's home address, and applicants just needed to double-check it. Nevertheless, this third experimental condition generated application rates similar to the others. It could be that such differences in expected address visibility matter only for those who expect substantial discrimination, but we also see no effect heterogeneity on that dimension.

The manipulation in the *Address Omission* condition might have been too weak. For instance, jobseekers might have thought that their address would eventually be required anyway. Another possibility is that our manipulations changed how jobseekers saw the HR firm. For

instance, those in the *Known Address* arm might have believed the HR firm preferred hiring people from favelas, since they were being contacted despite their addresses. We designed the Information Experiment (N=690) to circumvent these issues.

In the Information Experiment, we manipulated expected *market-level* discrimination. There were three experimental conditions: i) *No Info*, ii) *Favela Info* (revealing the audit study callback rate for a favela), and iii) *Full Info* (revealing that favela and non-favela callback rates were the same). Full Info reveals both the discrimination and callback level, so Favela Info works as an alternative control condition, holding constant the knowledge of favela callback rates. We verify that both information treatments shift beliefs by immediately eliciting incentivized posterior beliefs about the callback rates the partner HR firm would implement in different neighborhoods. Further, in an endline survey, we see some evidence that Full Info decreases expected discrimination even after two weeks (at least in relation to *Favela Info*). Regardless, jobseekers in the three conditions make it to the interview stage at the same rate of about 20%. We also estimate null effects on self-reported applications for other jobs, as measured in our endline survey. We conclude that expected discrimination does not affect average application decisions.

Race, a stigma correlated with favela residence, strongly predicts treatment effects in the Interview Experiment and partially explains the null results on application rates. In the interview stage, white applicants benefit more from believing their interviewers knew only their name: for all three performance indexes, we see statistically significant positive effects of expecting to have a hidden stigma, and these effects are at least 0.18SD larger than the effects on non-whites. One interpretation of these results is that when white applicants can hide their addresses, it is easier to pass for a non-favela resident.[2] For non-white applicants, even if their addresses are hidden, they might believe interviewers would still associate them with favela residents. Another possibility is that visible stigmas are "substitutes", i.e., once one stigma is exposed (either race or address), that is enough for jobseekers to expect significant discrimination, leading to similar reactions. In our pre-interview experiment (the Address Omission Experiment), we also find that reducing expected address visibility affects white jobseekers: they are 57% (9 p.p.) more likely to show up for an interview if they did not have to declare an address to apply (p=0.05 for the test of a heterogeneous effect of *Address Omission* on white jobseekers, against the other two conditions pooled).

Our field experiments are the first focusing on estimating the effects of expected discrimi-

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[2] A third of the favela population in Rio self-identifies as white, according to the 2010 Census. Outside the favela, that number is 56%. Hence, if white jobseekers are careful not to hint at their home address by revealing information directly or through how they speak, an interviewer should not guess that they are favela residents. In interviews, only 4% of all jobseekers revealed that they were favela residents.

nation. While many experiments measure whether agents discriminate in the labor market (see Neumark 2018; Rich 2014; Riach and Rich 2002 for reviews) and other contexts (reviewed in Bertrand and Duflo 2017), the supply side has received much less experimental attention.[3]

Three related field experiments experimentally vary the language used in job ads (Del Carpio and Fujiwara, 2023; Burn et al., 2023), or how they describe the selection process (Avery et al., 2023), finding effects on the composition of the applicant pool which could be explained by expected discrimination. Nevertheless, these studies cannot provide decisive evidence about how expected discrimination changes behavior. For instance, the non-gendered (as opposed to gendered) job ads in Del Carpio and Fujiwara (2023) also signal different job values, statuses, or amenities, which can appeal differently to males and females. We go further than these experiments in three main ways. First, we elicit incentivized beliefs about discrimination at baseline, allowing us to estimate whether expected discrimination predicts effect intensity. Second, we designed our experiments to vary only expected stigma *visibility*, while keeping job desirability and other factors as constant as possible, and our Information Experiment manipulates market-level expected discrimination, which is not subject to those same issues. Third, we provide a more comprehensive picture by also studying face-to-face interview performance.

We build on two lab studies that test whether jobseekers change how they present themselves in response to expected discrimination. Kang et al. (2016) shows that non-white college students craft "whitened" résumés (e.g., listing a Western name or omitting some job experience that could reveal ethnicity) but decrease the use of such strategies when asked to craft a résumé for a pro-diversity employer. A different lab experiment with UK college students finds that females are less likely to pick gender-matching avatars in a virtual labor market if they know they will compete for a male-dominated task (Charness et al. 2020). Both studies show people may change how they present themselves when expecting discrimination. We go beyond these studies by studying actual job application and interview performance, and by observing obfuscation strategies in the field.[4]

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[3]A few observational studies find evidence consistent with expected discrimination affecting human capital acquisition or job search decisions. Several studies document behavior consistent with strategic signaling in response to discrimination. That could be, for instance, disclosing more information to separate oneself (Lepage et al., 2022), investing in easily-observed human capital (Dickerson et al., 2022; Lang and Manove, 2011), or hiding a stigma even when it is costly (Agüero et al., 2023). Pager and Pedulla (2015) uses administrative data, complementing it with a survey on earlier experiences with discrimination, and finds that Black jobseekers cast wider nets in their job searches and that breadth correlates with past discrimination experiences. Findings from natural experiments in Glover et al. (2017) and Kuhn and Shen (2023) could also be consistent with expected discrimination, but it is not possible to pin it down as a mechanism.

[4]In a lab-in-the-field experiment, Hoff and Pandey (2006) shows that having a stigma (caste, in their case) made visible can lead to drops in productivity and risk-taking, which is also consistent with expected discrimination. See also Fryer et al. (2005) for a classroom game using the Coate and Loury (1993) framework, and Aksoy et al. (2023) for an experiment on anticipated discrimination against LGBTQ+ supporters in the context of prosocial behavior.

We also see our study of interview performance as a major contribution. Early work measuring employer discrimination (see Riach and Rich 2002) found little discrimination at the interview phase, and more recent work focused almost exclusively on measuring discrimination at the callback stage.[5] The role of interviews has remained understudied, and even if employers discriminate less at the interview stage, it can still be the case that anticipated discrimination plays a significant role. For instance, Goldin and Rouse (2000) find that 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 music knowing that they will be evaluated only on merit.

Our study speaks to a broader literature on how beliefs about discrimination can be important. Theoretical work has shown that discrimination can appear without differences in group endowments: beliefs might be enough to make a group of workers acquire less human capital in response to expected discrimination (Coate and Loury 1993; Lundberg and Startz 1983). While human capital accumulation decisions are out of the scope of this paper, we show how anticipated discrimination can be detrimental later on in the matching process.[6] As we randomize stigma visibility in two experiments, our study also has a connection with stereotype threat, which is the idea that when people feel at risk of confirming some negative stereotype (e.g., females being worse at math), they may perform worse and confirm that prophecy (Steele and Aronson 1995). While the stereotype threat literature overwhelmingly considers test performance or other laboratory outcomes (see Spencer et al. 2016 and Liu et al. 2021 for recent reviews), we provide evidence that it can be relevant in a high-stakes job market context.

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 one-fifth of the population. According to the 2010 Census, 66% of favela households had a per capita income of one minimum wage ($\approx$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

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[5]In a study with college and high-school students, Word et al. (1974) provides a thought-provoking study of how even non-verbal interviewer cues triggered by a racial mismatch between interviewer and interviewees can lead to worse interview performance. The effects of expected discrimination on the job can also be important. See Glover et al. (2017) and Hoff and Pandey (2006) for empirical studies of on-the-job/productivity contexts.

[6]In this sense, we join a recent literature focus on understanding the importance of jobseekers' beliefs and misperceptions (Spinnewijn 2015; Mueller et al. 2021; Bandiera et al. 2023; Jäger et al. 2022).

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 one of three large adjacent favelas in the North Zone of Rio, home to about 200,000 people, or 3% of the city's population. These neighborhoods grew to occupy their current areas throughout the 20th century, without proper urban planning or public services. They are now 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 and is usually referred to "Maré's Complex", as it is composed of 16 (sub-)favelas.

Favela jobseekers have limited formal work opportunities in their own neighborhoods. For instance, according to a Census of Maré's Businesses conducted by a local NGO from 2011 to 2013, 75% of these businesses were entirely informal. In total, they employed 9% of the favela's working-age population (REDES, 2014). Hence, most jobseekers, and especially those aiming to build a career, must go outside the favela to find jobs.

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 police were sometimes present in some of their areas.[7] Over our five months of fieldwork, police raids interrupted our survey activities 14 times and prevented us from including an extra region in this study. These police raids are generally unpredictable and violent. During a raid, favela residents will take refuge at their homes to avoid the crossfire. Workers may miss work days, favela businesses will close, and communication will be hampered as internet connections may stop working. Furthermore, it is usually unclear when a police raid ends, typically disrupting residents' lives for several days.

When there is no police raid in progress, favela residents can typically go in and out of the favela without any issues. Some may work in the asphalt neighborhoods adjacent to their favela or commute to wealthier areas of the city for work. 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 almost all participants.

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[7]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.

2.2 Audit Study: Measuring Anti-favela Discrimination

There is little experimental evidence on whether employers discriminate against favela jobseekers. In Brazil, Westphal (2014) conducted an audit study with résumés from different favelas and found no discrimination on average – but with some heterogeneity.[8] Since the Westphal (2014) estimates are ten years old, we conducted an audit study to estimate anti-favela discrimination in callbacks for entry-level sales jobs – similar to the real jobs used in our experiments.

We created four fictitious workers' profiles, two male and two female. Age, job experiences, certifications, and résumé templates varied across profiles. All profiles displayed complete high school, some job experience, and some professional certificates related to sales. With the help of a local consultant, we picked characteristics that would not be unrealistic for an unemployed favela resident.

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. We selected addresses that unambiguously mapped to either Maré or Bonsucesso, and that kept the estimated commuting difference similar between résumés from the two neighborhoods (see example résumés in Appendix C). Maré is a widely recognized favela in Rio, so employers can immediately tell the Maré résumé is from a favela. Also, jobseekers from Manguinhos and Jacarezinho in our Information Experiment acknowledge that information about Maré and Bonsucesso is relevant for them since they update their beliefs about their own neighborhoods similarly to Maré residents when learning about the Maré and Bonsucesso callback rates (see Figure A.11).

We collected sales job postings (e.g., sales associate, telemarketing salesperson) no older than two weeks from five popular job search websites.[9] We discarded positions requiring some skill, experience, or course that any fictitious profiles did not have. We also discarded positions in neighborhoods more than two hours away by public transport from our set of addresses. Then, research assistants applied to each job posting with two different profiles, with randomized addresses.[10] We submitted 1,400 applications for 700 jobs between February and May 2023. Research assistants monitored the phone numbers and emails until the end of June and coded

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[8]Zanoni et al. (2023) hired recruiters to evaluate favela and non-favela résumés in Argentina, finding substantial discrimination.

[9]Catho, Indeed, Infojobs, LinkedIn, and Riovagas.

[10]The exact randomization procedure was that, for each job posting, we first randomly ordered the four profiles. Then, we randomly picked one of the first two and one of the second two randomly ordered profiles to have favela addresses. A research assistant applied to each posting with two profiles, following the order. The third and fourth profiles were backups, and 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 the "wrong" gender. This skipping happened in 9% of the selected jobs, and results are similar if we drop those jobs.

all non-automatic, non-negative replies as callbacks.

The resulting callback rates are very similar across both groups: 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 a total 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 anti-favela taste-based discrimination (Kessler et al., 2019). Another possibility is that employers 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. Even if the audit study measure is imperfect for measuring whether discrimination exists or not, it provides a real benchmark for jobseekers' beliefs, allowing us to measure whether they under- or overestimate discrimination in this setting.

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' neighborhood and the adjacent non-favela neighborhood, which are more directly relevant to the perceived discrimination one might suffer, and compare that with our audit study findings.[11]

The top panel in Figure 1 compares callback rate predictions against those estimated 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.
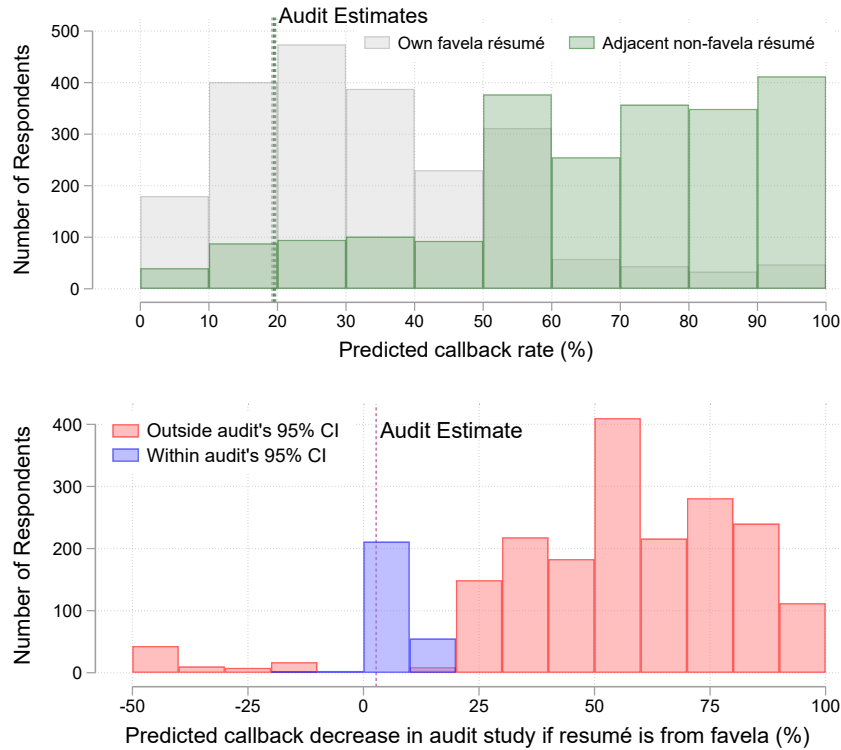
The bottom panel in Figure 1 shows the distribution of implied discrimination rates, i.e., the percent drop in callback induced by having a favela instead of a non-favela address. Here, we see that 87% predict discrimination (i.e., a decrease in callback), and 84% predict decreases larger than the upper 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.

While audit study measures of discrimination generally do not capture the full picture of

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

employers' discriminatory behaviors (e.g., because they focus only on the callback stage and cover only certain jobs), we expect jobseekers' predictions of the audit results to be still informative of the extent to which they expect to face discriminatory behavior. That is because i) expected discrimination in the audit study strongly correlates with a Likert measure of discrimination and discrimination in relation to a hypothetical "clone" of the respondent in the adjacent non-favela (see Figure A.10), and ii) providing information on callback rates also decreases an incentivized measure of discrimination regarding the HR firm (see Section 4.2).

Figure 1: 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 discrimination against Maré (vs. Bonsucesso) résumés (calculated using our audit study).

In our survey, we also asked some (N=1,497) jobseekers about the main reasons why employers would discriminate against favela residents. Jobseekers mentioned a mix of productivity-related 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%).

3 Experiment Design

3.1 Sample Recruitment

Field Team. We recruited all our surveyors locally, in each favela, through local NGOs networks. This strategy guaranteed our door-to-door survey could be conducted safely since favela residents are more likely to trust other residents, and the local surveyors were able to quickly identify and avoid risks related to criminal activity or police actions. Our local teams also facilitated obtaining the approval of multiple residents' associations, the relevant political brokers between the local powers.

   Sampling. Surveyors worked door-to-door to identify favela jobseekers who: i) were between 18 and 40 years old, ii) had completed high school or would complete it by 2023, and iii) were looking for a full-time formal job, even if they were employed. To avoid spillovers (since all our randomizations are at the individual level) and maximize privacy, surveyors would a) interview at most one person per household, b) conduct surveys one-on-one, without listeners, and iii) would not knock on homes adjacent to a former participant. Every participant received R$5 ($\approx$1 USD) and was entered into a lottery for R$500 (see Figure D.1 for photos of in-progress interviews).

   Survey. Surveyors completed 2,392 valid interviews. 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, and the final block was about anti-favela discrimination and expectations about one's future in the labor market.

   After collecting background information, the surveyor introduced the existence of a partner HR firm, which operated in Rio, assisting large companies with their recruitment. The surveyor then asked permission to share the respondent's basic profile information with the HR firm so the jobseeker could receive invitations to apply for available vacancies. We, as the researchers, operated this HR firm. 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. We presented the HR firm as separated from the study to emulate regular labor market interactions. That is because researchers and research activities are commonly linked with local NGOs in that context, and so were some of our surveyors. Hence, if the

surveyors said that the research team directly invited respondents to apply for a job, jobseekers might believe they would receive special treatment. At any rate, the HR firm invited jobseekers to apply for real jobs and indeed acted as an intermediary in the recruitment process.[12,13]

To describe our survey and sample in more detail, we focus on the 2,167 eligible to participate in our experiments – 167 did not share their data with the HR firm, and 61 of those who did provided an invalid phone number. Table A.1 presents summary statistics for this sample: 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).

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 soft skills (e.g., punctuality, salesmanship, and leadership). 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".

Finally, we move to questions about job market prospects and anti-favela labor market discrimination in the fourth block. 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.

Measuring Expected Discrimination. As our main measure of expected discrimination, we incentivized jobseekers to predict the callback rates we would find in our audit study, paying an extra R$100 ($\approx$20 USD) to the ten people who got closer to the true estimates (see Figure D.2 for the full elicitation script). 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 compared only Maré and Bonsucesso, we elicit incentivized predictions for

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[12]Our debriefing procedures include (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.

[13]For the duration of the study, we kept a website and a contact email running, in case any jobseeker searched online for the firm.

these other neighborhoods by initially stating that we only knew the correct answer for some of the questions and would pay incentives based on those.

Partners. To advertise real jobs to participants, we partnered with one of Latin America's largest cosmetics franchise and retailer chains. This firm is interested in increasing diversity among its workers and allowed us to advertise three entry-level sales jobs. They committed to giving full consideration and fast-tracking promising applicants recruited through our pipeline. We partnered or kept in touch with several NGOs in each favela. These institutions were extremely important since they could provide recommendations on locals who could work as surveyors, as well as feedback and advice on our survey, logistics, and research questions.[14]

Overview of the experiments. Figure 2 shows how the Address Omission, Information, and Interview experiments fit together. We introduced the Information Experiment as we phased out the Address Omission Experiment.[15] Hence, the sample in each of those pre-interview studies differ with respect to their favela of origin and some other covariates (see Table A.6 for a comparison). All jobseekers who completed the application form and attended the interview participated in the Interview Experiment.

Figure 2: Experimental Design
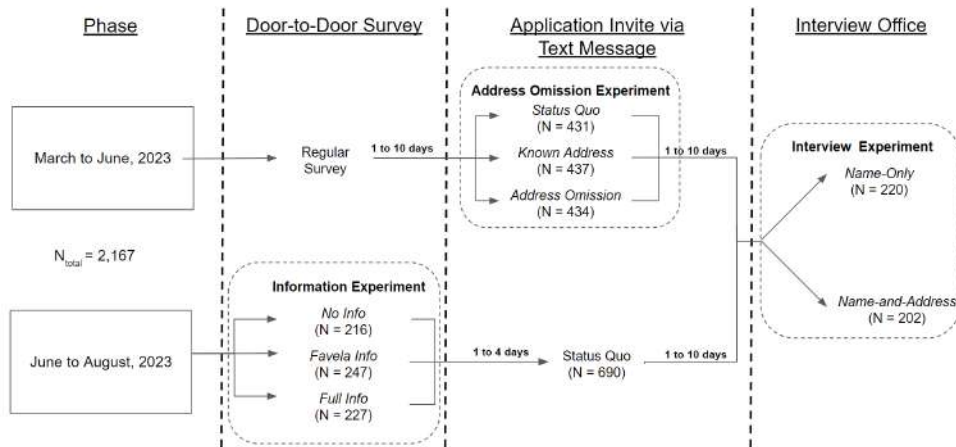
Note: The figure shows a simplified diagram of the flow of participants from door-to-door survey to job interview, for the earlier and later fieldwork periods. See Section 3.1 for details.

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[14]We are also working with a Jacarezinho NGO to produce a policy report and disseminate our findings locally, on the media, and inform policymakers.

[15]There was an overlap of 174 participants between the two pre-interview experiments when phasing out the Address Omission Experiment and launching the Information Experiment. For simplicity, the main text presents results for the non-overlapping samples.

3.2 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 would send personalized invitations to apply via WhatsApp to a new batch of applicants, each applicant receiving a unique link. Batch sizes varied from 50 to 117 to accommodate logistical capacity. Given that, most jobseekers received invitations to apply up to ten days after answering the door-to-door survey.

Treatment. In this experiment, we randomize the expected stigma (address) visibility at the application stage. The application invite and application form sent to each applicant could belong to one of three experimental conditions i) *Address Omission*, ii) *Status Quo*, and iii) *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* receive a message saying an address **is** needed. See the exact messages below. The difference between the experimental conditions in which the address is necessary 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 states that the research team has shared the jobseeker's address (besides name and phone number), so they just need to double-check it. Hence, in *Known Address*, we make sure that obfuscation is not possible, allowing us to test whether making address visible affects application behavior (see Figure D.4 for the differences across forms).

WhatsApp Invite Messages:

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.**

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 with our partner (see Figure D.3 for full job descriptions). Then, it confirmed the jobseeker's name, phone number, and address (or not, as implied by each treatment arm). Then, it proceeded as a standard

application form and ended with a screen in which the jobseeker had to declare their availability for an interview.

Outcomes. Our main pre-registered outcomes relate to application progress: (i) clicking the link in the WhatsApp message to open the application form, (ii) application completion rates, and iii) interview show-up rates. The latter typically takes place up to two weeks after application since the HR firm could always schedule an interview within days of the application completion. The click-through outcome happens before the differentiation between *Status Quo* and *Known Address*, so we should not expect any difference in click-through rates between those arms.

Address Obfuscation. We also calculate the address obfuscation rate for those in the *Status Quo* arm. We consider that a favela 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).

3.3 Information Experiment (N=690)

The Address Omission Experiment ran until May 2023. In the following month, we embedded the Information Experiment in our door-to-door survey (which proceeded to cover all three favelas) to address two limitations of the Address Omission Experiment. First, jobseekers in the *Address Omission* arm might have believed their addresses would be required or revealed anyway in later stages of the application procedure, leading to a weak treatment. Second, while we only vary the information required for applying, there could still be a concern that this changed perceptions of other characteristics of the selection procedure or the job. Hence, the Information Experiment aimed to manipulate beliefs about *market-level* discrimination.

Treatment. We randomized participants into three treatment arms: (i) *Favela Info*, in which we disclosed only the favela's callback rate (19.3%, from our audit study), (ii) *Full Info*, in which we showed both the favela and non-favela callback rates (19.6%) – thus revealing that we find no discrimination in callback rates, and (iii) *No Info*, in which no information was displayed. See Figure 3 for the graphs the surveyors used to convey the treatment.

Similar to the Address Omission Experiment, the HR company later invites respondents to apply for our partner's jobs, with two main 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 is no randomization in the application procedure, we can decrease the batch size and invite jobseekers to apply more often, one to four days after they answer the door-to-door survey.

Outcomes. Besides the application progress outcomes used in the Address Omission Ex-

Figure 3: Information Treatment Delivery

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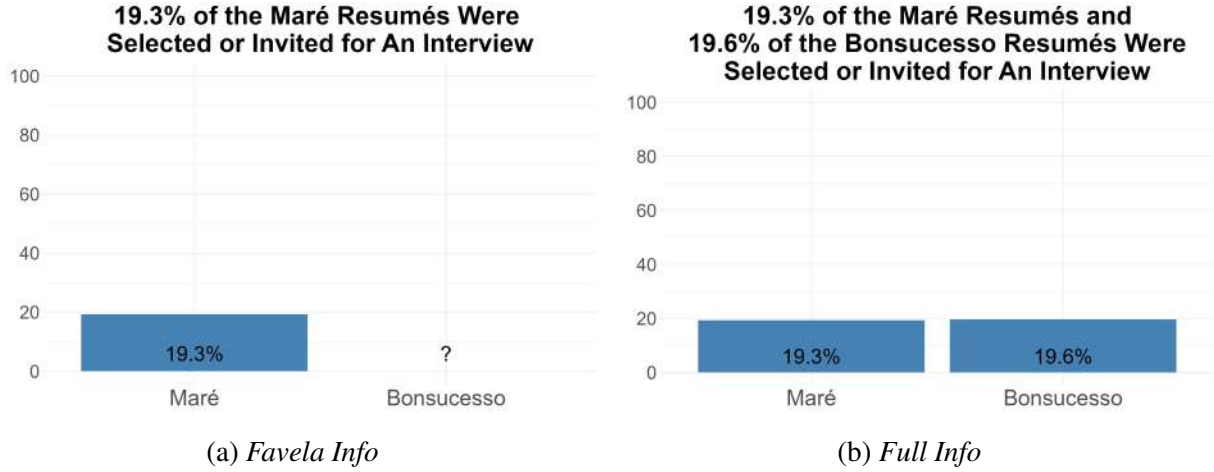
Note: This Figure shows the images we used to convey the Information Experiment. We showed either one of the plots (or none) to participants immediately after the belief elicitation presented in Figure D.2. The surveyor read the text above each graph when showing it to the respondent.

periment, we also pre-registered address obfuscation and immediate belief updates as main outcomes. As updated beliefs, we chose the 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 these beliefs by including them in the set of questions in which we elicited 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, and accuracy would be calculated based on those.

Endline survey. We conducted an endline survey to check whether the belief shift caused by the Information Experiment persisted and to collect a self-reported number of job applications sent after answering the door-to-door survey. To minimize attrition, we only asked multiple-choice questions with four possible choices each. We asked these questions over WhatsApp two weeks after each jobseekers participated in the survey. As a participation incentive, respondents were entered into a lottery for R$200 ($\approx$40USD).

3.4   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$25 ($\approx$5 USD) transport subsidy – enough to cover bus fares and a meal. We rented a reception desk and interview rooms in a co-working

space, so applicants first had to go through the building's reception and then take the elevator up to the co-working floor. Interviews took about 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. Appendix D.5 presents pictures of the co-working space.

Treatment. In this experiment, we randomize expected stigma visibility at the job interview. A receptionist greeted jobseekers when they reached the right floor. Next, the receptionist asked to confirm the applicant's name, date of birth, and address and told them to wait. Moments later, the receptionist told the jobseeker that the interviewer was ready, and, to keep the process objective, "the interviewer will only know your name" (*Name-Only* condition) or "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. Later, we debriefed the interviewers to learn their impressions and avoid participant deception – i.e., "the interviewer will know your name and address" was an ambiguous statement with respect to timing and the exact address information the interviewer would eventually receive.

Interview. We hired an experienced HR consultant to revise our interview script and train our two interviewers. The script contained a set of standard interview questions for sales jobs. For instance, it included questions about strengths, weaknesses, the candidate's comparative advantages, and past work experiences. The interview also included an activity where the applicant had to pick an item and provide a sales pitch for it (see Appendix D.2 for the complete script).

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, 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 measure, we average the two. While this averaging risks mixing different dimensions, it allows us to extract a more accurate signal. At any rate, we also present broken-down estimates in the main text.

3.5 Randomization, Balance, and Estimation

Randomization for the Address Omission Experiment proceeded in batches. We assigned treatments with the same probability, stratifying by expected discrimination (batch-wise), with equal probability of each treatment within each stratum. We proceeded similarly for the Interview Experiment, randomizing in batches after jobseekers completed the application form. Nevertheless, due to logistical issues, we had to randomize the treatment status of some participants as they arrived at the interview office. The offline survey app on the surveyors' tablets implemented the randomization for the Information Experiment on the spot – also with the same probabilities. All 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.

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

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

where $y_i \in \{0, 100\}$ (to yield percentages), and each coefficient captures the outcome level for each treatment group. Given randomization, results with controls are very similar. We show results with double-lasso selected controls in Appendix A. We present robust standard errors for all models, calculating the variance-covariance matrix using the HC3 approach (Long and Ervin, 2000).

We use the same specification as in Equation 1 to estimate average treatment effects in the Information Experiment (i.e., one indicator for each treatment). We also conduct an additional exercise to estimate the effects of shifts in expected discrimination and expected own-favela callback rate on application outcomes. That is, assuming our treatments only affect application through beliefs, we use *Favela Info* and *Full Info* to instrument the posterior beliefs about the discrimination rate the HR firm to estimate:

$$y_i = \alpha + \beta_{disc} Posterior\ Discrimination_i + \varepsilon_i, \tag{2}$$

where *Posterior Discrimination$_i$* is the expected HR firm callback percentage decrease due to a

favela address. Under the IV assumptions, estimating β_{disc} yields a quantitative test of how expected discrimination affects application outcomes, leveraging variation from both treatments. As our information treatments can shift the expected callback rate level, we also estimate:

$$y_i = \alpha + \tilde{\beta}_{disc} Posterior\ Discrimination_i + \beta_{favela} Posterior\ favela\ callback\ rate_i + \varepsilon_i, \quad (3)$$

allowing us to estimate the effects of expected discrimination rates and the expected callback level. For the IV specifications, we focus on overestimators of both *Posterior Discrimination$_i$* and *Posterior favela callback rate$_i$* to guarantee our instruments have a monotonic effect on the endogenous variable.

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 estimate:

$$y_i = \alpha + \beta_{NO} Name\text{-}Only_i + \varepsilon_i. \quad (4)$$

To show robustness to the inclusion of controls, we pick them flexibly using double-lasso.

Finally, we 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 the favela stigma interacts with an always-visible stigma correlated with favela residence. The skill heterogeneity could tell us how expected discrimination changes the talent pool available to employers, and the gender heterogeneity can inform us about whether favela males – who are more likely to be gang members – or females react more to expected discrimination. We discuss the heterogeneity by expected discrimination together with our main results (since it is our mechanism of interest), and we present all four heterogeneity breakdowns in Appendix A.

4   Results
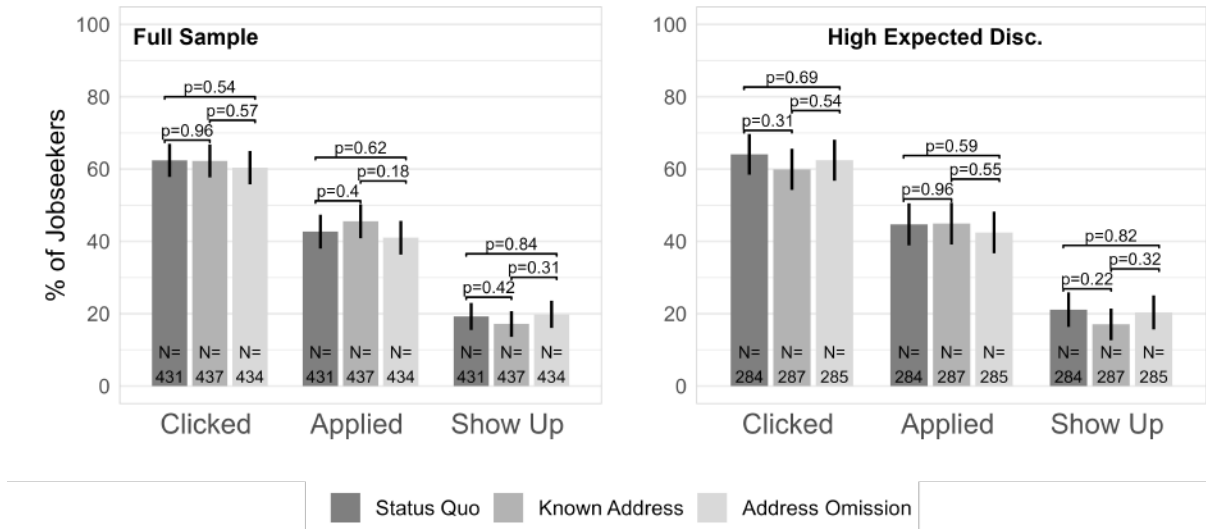
4.1   *Address Omission Experiment*

Address visibility does not affect average job application rates (left panel, Figure 4). If expected discrimination discourages applications and expected stigma visibility dictates whether

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

the jobseeker should expect discrimination, we should see *Address Omission* increasing application rates in comparison with *Status Quo* (unless jobseeekers use strategies like obfuscation to fully avoid expected discrimination under *Status Quo*). *Known Address* should do the reverse, except for the clicking outcome, since the difference between *Status Quo* and *Known Address* is whether the application form address field is pre-filled or not. Instead, we see little variation across treatments: click-through rates hover just over 60%, form completion rates hover from 41% to 45%, and interview show-up rates are just below or at 20%. The p-values for tests of equality between any two conditions for all three application outcomes are all above conventional significance thresholds.

The right panel in Figure 4 presents results for the subgroup that should react the most to stigma visibility: those who expect discrimination of 50% or more in the audit study. We see a very similar pattern, providing no evidence that expected discrimination affects average application rates.

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. Clicked, means clicking the link in the WhatsApp invite. Applied stage 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.

At the same time, we observe address obfuscation in the *Status Quo* arm, consistent with a strategic reaction to expected discrimination. In that arm, applicants were free to declare their addresses, and 25% declared obfuscated addresses. Conditional on applying, that rate is 45%. We also verify that the *Known Address* treatment is effective in preventing obfuscation

since only 8% of the applicants in that condition provide a corrected address in place of the one recorded by the surveyor, and none tried to obfuscate their neighborhood. Hence, the treatment arms changed the address the jobseekers expected the firm to know, at least at the moment of application.

Three theories could explain the null results. First, jobseekers might have believed that recruiters would eventually figure out their neighborhood of origin, and that, in such case, any gains from hiding address in the initial stage would be erased. Second, jobseekers may have inferred more than just variation in stigma visibility when reading the ads. For instance, some in the *Known Address* arm might have inferred that the HR firm was especially interested in favela workers, since they were invited *despite* the firm knowing their addresses. Third, expected anti-favela discrimination might not have been marginal in the application decision. For instance, jobseekers might have used simple heuristics to decide whether to apply, e.g., whether they need a job, and whether the job opening fits their schedule or skills. Our Information Experiment avoids the issues related to the first two explanations, since it shifts beliefs about *market-level* discrimination.

4.2 Information Experiment

We begin by discussing the "first-stage" effects of learning the callback rate estimate of 19.3% for the favela résumés (taken from our audit study) in *Favela Info*. Learning *Favela Info* does not change the average expected callback rate for jobseekers' own neighborhoods – see Figure 5. That is because the effects on under- and overestimators of the favela callback rate balance out. For instance, considering only overestimators, the average expected callback rate goes from 41% in *No Info* to 37% in *Favela Info* (p=0.09, see Figure A.3 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. For both subgroups, there is a statistically significant shift in expected callback for one's own favela when learning *Full Info*. Hence, jobseekers use favela *and* non-favela information to update about favela callback rates.

Considering beliefs about the non-favela callback rate, we also see that jobseekers use information on both favela and non-favela callback rates to update. Since 92% of the sample overestimate 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.

The top-right graph in Figure 5 shows a similar pattern for the subsample who expected high discrimination from the start.

Before proceeding, consider what would be the effect of shifting beliefs about one's own favela callback rate on application. A simple model in which agents do not care about non-favela callback rates shows that applications may either increase or decrease with callback probability. Let n be the number of applications chosen by the jobseeker, 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. Intuitively, at a low p, an increase in p makes a marginal application more valuable. 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.

Learning how the non-favela callback rate compares to the favela callback might also change application decisions in different ways. For instance, for a jobseeker with initially accurate beliefs about callback rates, information can still increase applications if it decreases expected discrimination in later stages (e.g., the interview). And for a jobseeker that has overly optimistic beliefs but acts according to the model above, *Favela Info* and *Full Info* can have reinforcing or opposite effects on applications since *Full Info* can lead to a stronger update in p, with a potentially non-monotonic application response.

In the bottom-left of Figure 5, we see that the average click rates are 60% for the *No Info* group, 68% for the *Favela Info* group (p=0.1 in comparison to *No Info*), and 67% for the *Full Info* group (p=0.14 in comparison to *No Info*). We see a similar pattern (i.e., information increases application) considering the shares of jobseekers completing the application form, but those increases are not significant. These increases in the initial interest in the job concentrate in the group that initially overestimate the Maré callback rate (see Figure A.3). Still, the pattern vanishes when considering interview attendance. These results suggest that some jobseekers behave as in the model above, in which they might initially expect to have "enough" callbacks, but increase application rates when they learn the callback rate is lower than expected.

The bottom row in Figure 5 also includes average obfuscation rates by information condition. Applicants in *Favela Info* obfuscate the most, and the difference is larger in the subsample expecting higher discrimination. That would be consistent with strategic obfuscation: if I learn that my neighborhood's callback rate is lower than expected, obfuscation becomes more attractive. If I further learn that using an adjacent non-favela address (i.e., the typical obfuscation

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strategy) does not lead to higher callback rates, obfuscation rates can decrease again. Neverthe-less, we only see one statistically significant difference: in the group expecting high discrimina-tion, those receiving *Favela Info* apply obfuscating 13% of the time, more than double the share in *No Info* (p=0.1). When breaking up the sample by those who underestimated or overestimated discrimination, we see that *Favela Info* seems to decrease obfuscation for underestimators and increase it for overestimators, consistent with strategic obfuscation.

Figure 5: Information Treatment Shifts Beliefs, But Not Interview Show-up
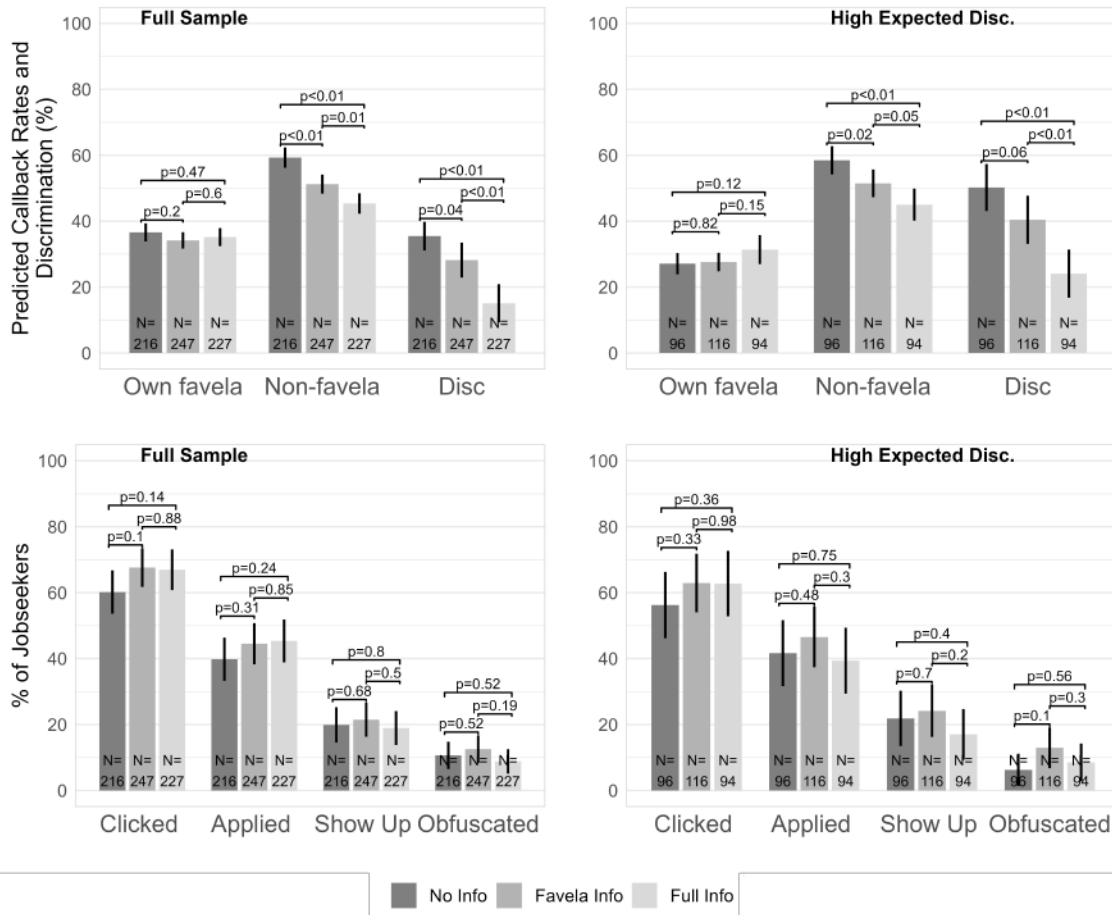
Note: The top row of graphs displays average posterior beliefs of what callback rates the HR firm would implement for jobseekers in each experimental condition. Non-favela and Own favela stands for the callback rate prediction for a respondent's favela and adjacent non-favela. Disc is the implied discrimination rate. The bottom row displays outcomes from the application process. Clicked, means clicking the link in the WhatsApp invite. Applied means finishing the online application form, and Show Up means attending the interview. Obfuscates in app means declaring (in the application form) 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.

As we are interested in describing the effects of beliefs on applications, and since both information treatments shift both types of beliefs (about callback level and discrimination), we present IV estimates of the effects of both beliefs in Table 1. To guarantee a first stage, we focus on the subsample that overestimates the favela callback and discrimination rates (as our treatments lower both beliefs). Regardless of whether we only instrument the posterior discrimination rate or also include the posterior beliefs about one's own favela callback rate, we see no statistically significant effects of beliefs on application behavior. Considering point estimates, the only cases when expected discrimination discourages application is when we consider clicking and completing the form as outcomes, without including own-favela beliefs in the estimated equation. Considering the application-progress outcomes when we include own-favela beliefs (columns (5) to (8) in Table 1), point estimates suggest that jobseekers that overestimate the favela callback (and are affected by the treatment) are in the regime in which they already expect "enough" callbacks and the difference in application rates matters less (as in the toy model previously discussed). The point estimates of the effects on obfuscation rate in the more flexible model (column 8) also suggest that jobseekers strategically declare a neighborhood that would maximize their callback rates. So, another reason we do not see people being discouraged from applying when they are told they were too optimistic may be because they have the option to obfuscate in such cases.

Table 1: IV Estimates of How Expected Discrimination Beliefs Affect Application Rates

	(1) Clicked (%)	(2) Applied (%)	(3) Show Up (%)	(4) Obfuscates in application (%)	(5) Clicked (%)	(6) Applied (%)	(7) Show Up (%)	(8) Obfuscates in application (%)
Posterior Exp. Disc. (%)	-0.289 (0.343)	-0.217 (0.346)	0.031 (0.277)	0.007 (0.192)	0.924 (1.602)	0.703 (1.290)	0.152 (0.591)	0.886 (1.193)
Posterior Cb. for Own Favela (%)					-4.457 (5.185)	-3.380 (4.107)	-0.446 (1.902)	-3.231 (3.847)
Observations	447	447	447	447	447	447	447	447
Sample	Overestimators	Overestimators	Overestimators	Overestimators	Overestimators	Overestimators	Overestimators	Overestimators
No Info Mean	56.85	37.67	19.18	6.16	56.85	37.67	19.18	6.16

Note: Two-stage-least square estimates of the effect of posterior beliefs about discrimination on application outcomes. The instrumented variable is the predicted drop in the HR firm's callback rate, and the instruments are information treatment dummies (*Favela Info* and *Full Info*). See Figure 5 notes for definitions of the outcomes. Sample includes only individuals who overestimated the audit study discrimination rate and the favela callback rate. Robust standard errors between parenthesis.

Our endline survey generally confirms the findings above. There was no differential attrition in participation – Table 2, column (1). In column (2), 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). In a pooled comparison of Full Info against the two other arms (not shown in the table), we see p=0.09. Nevertheless, in column (3), we still see null results on application rates, but now on a self-report of the total number of jobs the respondent applied to

in the last two weeks.

Table 2: Information Does Not Affect Application Rates at Endline

	(1)	(2)	(3)
	Responded to endline (0/1)	Exp. discrimination (categorical, 1-4)	# of sent apps (categorical, 1-4)
Favela Info	-0.027	0.003	-0.030
	(0.039)	(0.081)	(0.116)
Full Info	-0.001	-0.123	0.017
	(0.039)	(0.083)	(0.119)
Observations	969	558	560
Controls	No	No	No
No Info Mean	0.59	2.34	2.46
Favela=Full *p*	0.50	0.13	0.69

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.

4.3 Interview Experiment

We do not see evidence for expected discrimination affecting application rates in both pre-callback experiments. Nevertheless, expected discrimination could still damage interview performance since there are many differences between the application decision and one's behavior in an interview. During the interview, the jobseeker must quickly adjust behavior in response to the interviewer, who directly observes and judges performance, making the interview interaction very different from the "cold" decision of whether to apply. Hence, we might see expected discrimination affecting performance in the Interview Experiment.

Hearing that the interviewer will only know the interviewee's name increases interview performance (top panel, Figure 6). Regardless of whether we use double-lasso to select controls, the direction of the effects on all performance dimensions (three self- and three-interviewer assessed) is consistent with a negative relationship between stigma visibility and interview performance. When we aggregate all the self-assessed dimensions into an index, we get an average effect of 0.17SD, with p<0.01, regardless of whether we use double-lasso. For the index of the interviewer-assessed dimensions, we see a non-significant effect of 0.09SD (p=0.28) without controls and a smaller estimate with controls. We also cannot reject that the difference between

the effects on the interviewer- and self-assessment index is zero (p=0.34 or 0.33, with or without controls). The effect on the aggregate index is 0.13SD (p=0.03) without controls and 0.1SD (p=0.06) using double-lasso.

The average treatment effects leave us in a position of ambiguity since we do not have power to reject the null of no effect of stigma visibility on the interviewer assessment. One way to proceed is to consider that both the interviewer- and the self-assessed indexes are noisy measures of interview performance and then use the aggregate index as our best guess for both. Nevertheless, there might be bias in jobseekers' self-assessments. Another way to proceed is to check whether our hypothesized mechanism (i.e., expected discrimination) works for both the self- and the interviewer-assessed measures. If interviewers see those expecting high discrimination as worse performers when they believe their addresses are visible, that would be evidence that expected discrimination hurts performance.

The bottom panel in Figure 6 shows that the effects on the index outcomes strongly concentrate on the group expecting high discrimination from the start, consistent with expected discrimination hurting performance. This pattern is the same regardless of whether we look at the interviewer- or self-assessed index. When looking at the subgroup expecting 50% or more discrimination (at or above the median), we see performance increases of about 0.2SD, no matter which index we look at. These effects are always significant – one of them at the 10%-level and all others at 5% or less. Comparing the effects on the low- against high-expected discrimination group yields statistically significant differences at the 5%-level when the outcome is either the interviewer or aggregate index. Hence, expected discrimination hurts interview performance – as assessed by the interviewer – at least for those who expect high discrimination from the start.

Figure 6: Expected Stigma Visibility Affects Interview Performance, Especially for the Group Expecting High Discrimination
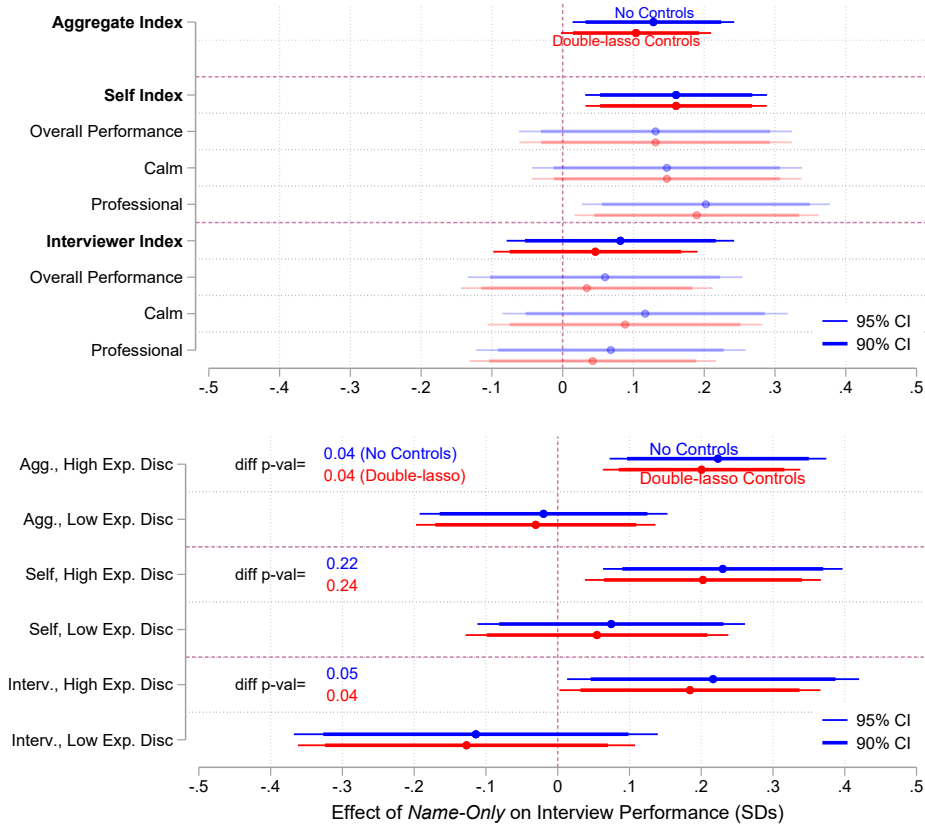
Note: The top graph shows average treatment effect estimates using either no-controls (blue) or double-lasso selected controls (red). The interview performance outcomes are listed on the left-hand side and described in Section 3.4. The bottom graph shows estimates of heterogeneous effects by expected discrimination. For each outcome, we estimate a single model with saturated dummies for expected discrimination, and we show p-values for the equality of the effects on both groups in the left-hand side. Thicker error bars show 90% confidence intervals, and thinner bars show 95% intervals.

5 Discussion

5.1 Race and Stigma Visibility

In our door-to-door survey, 68% of jobseekers mentioned racism as an important reason why employers discriminate against favela residents, and 70% believe firms discriminate a lot against

Black jobseekers.[17] Furthermore, white people are a majority of the population outside favelas, but only one-third of the favela population. Hence, race is a visible stigma correlated with favela residence, an "invisible" stigma. These stigmas can interact in different ways, and we highlight two of them. First, a visible stigma might hint at an invisible one since they are correlated. Hence, a white jobseeker may easily pass for a non-favela resident (if they are careful about what information they disclose directly and in their way of speaking), but that is harder for non-whites. This asymmetry suggests that address visibility can be more relevant for white jobseekers, since non-white jobseekers might always expect to been seen as a favela resident with high probability. Second, jobseekers might be similarly stressed or expect employers to treat them similarly no matter whether one or more stigmas are visible (e.g., one source of expected discrimination might be enough to discourage). In the latter case, since race is always eventually visible, we should further expect null effects on non-whites. In both of these mechanisms, the visibility of the race and address stigmas work as substitutes. We will show evidence of that substitutability in this section.

At the job interview, race becomes immediately visible. In the top-right panel in Figure 7, we see that expecting address to be hidden during the interview increases performance for white jobseekers by about 0.3SD for the aggregate *and* the broken-down indexes, and the effect is always significant at least at the 10%-level. Further, the effects on non-whites are at least three times smaller (but still significant at the 10% level, considering the self-assessed index), suggesting that these stigmas act more as substitutes than complements.

Moving back to when we randomized expected address visibility at the Address Omission Experiment, we see that white jobseekers applied and showed up more often when the invite message said their addresses were not necessary at the application stage – see the bottom of Figure 7. Furthermore, white jobseekers are about twice as likely to attend an interview under the *Address Omission* treatment compared to the *Known Address* treatment. In a pooled comparison of *Address Omission* against the two other arms, we see a 10 p.p. increase in the application (p=0.1) and show-up (p=0.05) rates. Those are large increases, of 25% and 57%, respectively. Looking at non-white jobseekers, we see null (or negative) effects of *Address Omission*, as if the non-white jobseekers expected any gains from applying without an address to be undone later when their race becomes visible.[18] Since 77% of jobseekers in the Address Omission Experiment are not white, the negative effects of *Address Omission* in that subsample cancel out

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[17]In the original survey, we use the word "negro", which according to the Census classification means the sum of "preto" (most closely translate to "Black"), and "pardo", which may be thought of as "mixed-race", commonly of partly African descent.

[18]One explanation for the negative (rather than simply null) effects of*Address Omission* on non-white jobseekers' application rates is that a small share of applicants might have suspected the legitimacy of a job posting that does not require an address (since most jobs do require one).

the positive effects on white jobseekers, yielding the average null.[19]

Figure 7: Race and Address Visibility Operate as Substitutes
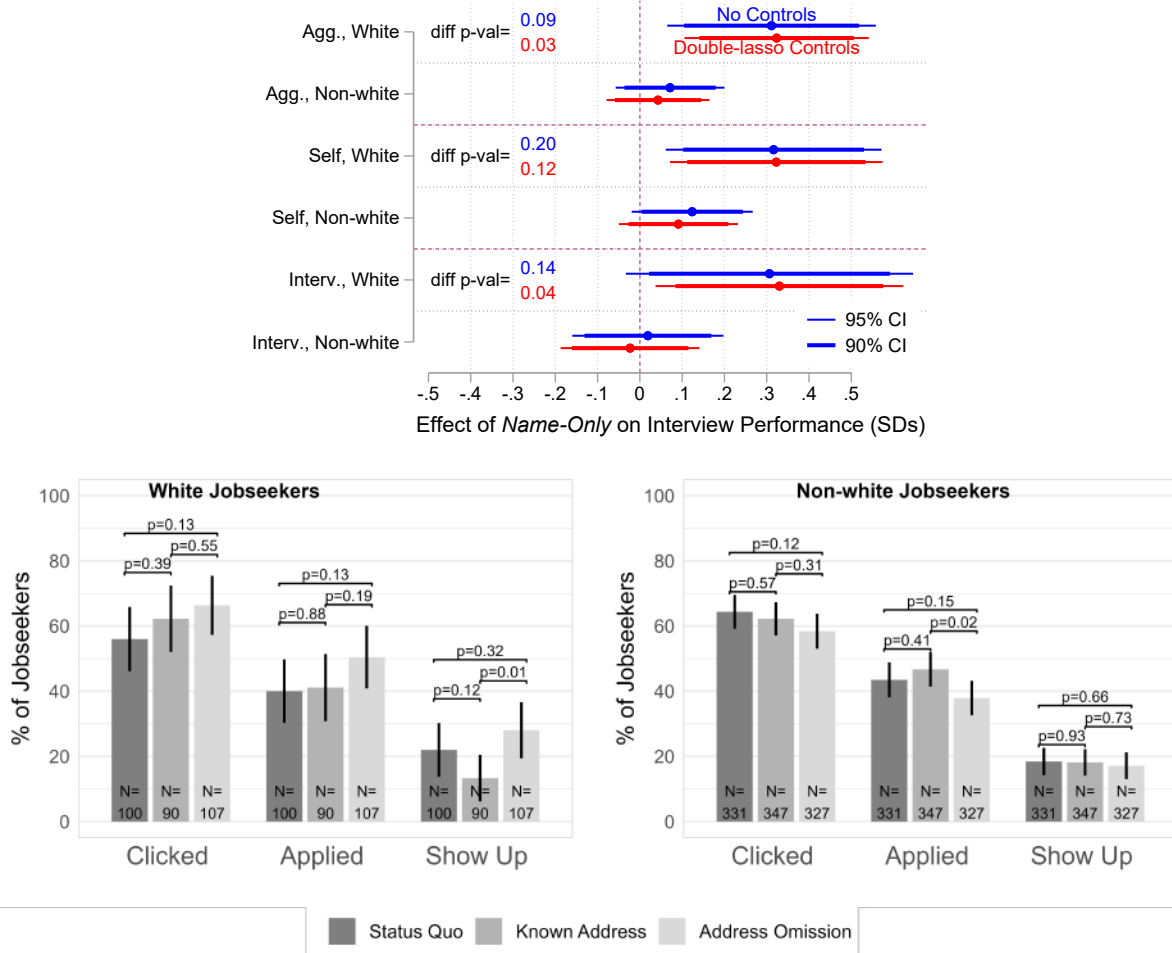
Note: Graphs show heterogeneous treatment effects by self-identified race (white vs. non-white jobseekers). See notes in Figures 6 and 4 for details on outcomes and graph elements.

5.2 Obfuscation

We see address obfuscation throughout our experiments, and it correlates positively with expected discrimination. Among all jobseekers who finished a *Status Quo* application form (i.e., those who freely declared their addresses), 28.5% obfuscated. This share was 24% among those

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[19]In the Information Experiment, as we do not vary expected stigma visibility, we do not have the same predictions for the race heterogeneity. The effects of information could depend, for instance, on how non-whites update their beliefs about racial discrimination in response to the callback information. We display the pre-registered heterogeneity cuts for the Information Experiment, including race, in Figure A.7.

who expected low discrimination and 34% for those who expected high discrimination (p=0.01 for the difference). At the interview, the receptionist also asks for an address when the applicant arrives: 17% of those expecting low discrimination obfuscate, and 29% of those expecting high discrimination do the same (p<0.01 for the difference).[20] We see this as evidence of jobseekers indeed expecting discrimination from the HR firm and attempting to avoid it.

Obfuscation might have contributed to our findings of null effects of information about market-level discrimination on application in the Information Experiment. Section 4.2 presented some suggestive evidence that jobseekers picked obfuscated addresses to maximize the expected callback rate. To see how that could induce null results on applications, consider a jobseeker such that i) if she learns no new information, she applies for the job, and ii) learning the actual favela callback rate would discourage her from applying (e.g., because she was too optimistic). Then if she learns that she was too optimistic about the favela callback rate in *Favela Info*, she might "pick" a higher callback rate by choosing to declare a non-favela address. If she instead learns both callback rates, her expected callback rate should decrease for all possible addresses, but she might also think jobs outside the favela are more attractive because now she also expects lower on-the-job discrimination. So, for this jobseeker, the option to obfuscate prevents *Favela Info* from decreasing callback rates, leading to no difference in her behavior. If jobseekers of this "type" are numerous and could not obfuscate, we might have seen *Favela Info* decreasing application rates and *Full Info* bringing it back up.

5.3 Policy Considerations

5.3.1 Relevance of the Effects on Interview Performance

One key issue for deriving policy implications from our Interview Experiment findings is whether expected discrimination also affects the interviewer's ultimate judgment of who to recommend. Above, we have shown evidence that the aggregate interview-assessed performance is negatively affected by stigma visibility in the subsamples of white jobseekers and those expecting high discrimination. While those two heterogeneities confirm that expected discrimination can affect the interviewer's impressions, we have less power to evaluate whether expected discrimination impacts the interviewers' final judgment. That judgment is coded in the overall performance rating, which is one of the three components of the interviewer assessment index.

Hearing that the interviewer would only know the jobseeker's name (*Name-Only*) has an effect of 0.06SD (without controls) or 0.03SD (with lasso-selected controls) on the overall per-

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[20]For these correlations, we use a classification based on the latest measure of expected discrimination available for each jobseeker – i.e., for those who went through the Information Experiment, we use expected discrimination regarding the HR firm instead, and split into low and high groups based on the same 50% threshold as before.

formance rating z-score, and neither is statistically significant. Nevertheless, when estimating heterogeneous effects by expected discrimination and race, we see some evidence that white jobseekers and those expecting high discrimination get better overall ratings under *Name-Only* (see Figure 8). As before, the gap is wider for the race comparison, where we see a positive and statistically significant effect of *Name-Only* on the overall interviewer rating. This evidence suggests that expected discrimination also affected the final interviewer rating in our experiment. If that is true, there is reason for policymakers and firms to consider policies such as "blinding" in interviews.

Figure 8: Expected Discrimination May Also Affect Final Interviewer Rating
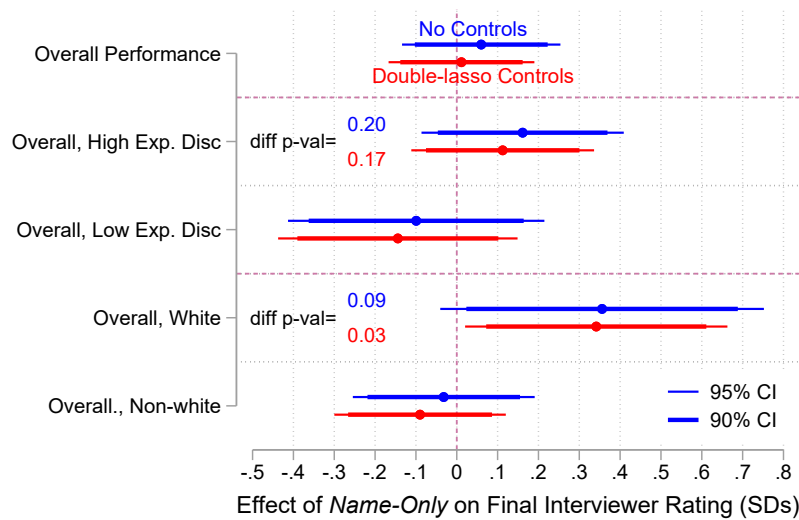
Note: The graph shows average and heterogeneous treatment effects on the final performance rating (used for making recommendations) of being told that one's interviewer would only know their name (as opposed to name and address). Thicker error bars show 90% confidence intervals, and thinner bars show 95% intervals.

But our results have implications even if all the effects of expected stigma visibility were restricted to the jobseekers' self-assessment. For instance, after a negative interview experience, jobseekers might be reticent to apply again for other jobs that require formal interviews. Also, note that interviewers in our experiment had no way to discriminate against favela residents – because we focused on the effects of expected discrimination, we kept interviewers always on script, and they did not know anything about the experimental design or question at the time of the interviews. But, in a regular interview, interviewers may actually behave differently towards a favela jobseeker, which can further change how the interviewee reacts and amplify the effects of expected discrimination on performance. Finally, even if we disregard performance

completely, there is the question of jobseekers' self-confidence: expected discrimination can undermine jobseeker's psychological welfare in general (Pascoe and Smart Richman, 2009; Schmitt et al., 2014), and we show that it leads to negative interview experiences.

5.3.2 Blinding and Other Policies

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 substantially or across the board. Our analysis of the interaction between race and address visibility suggests 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 is reason to become more optimistic about "blind" auditions (as in Goldin and Rouse 2000) or interviews, since we show evidence that simply expecting a blinding procedure can improve performance. Our study highlights the importance of jobseekers' *second-order* beliefs, rather than whatever other damage discriminating interviewers may impose. Hence, employers should make sure that jobseekers are fully aware of blinding policies. 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 5.1. Hence, policies that hide all stigmas during interviews (e.g., audio-only, text, or metaverse interview rounds) could dominate alternatives. AI-intermediated candidate selection is also a promising alternative, as shown in Avery et al. (2023).

Understanding exactly what is different about face-to-face interviews that leads to larger effects of expected discrimination could help inform policy, too. At a face-to-face interview, application and show-up costs are sunk, but the immediate stakes are higher, and an interviewer explicitly judges the jobseeker. Our experiments cannot measure the importance of each of those components, but we have some hints that jobseekers might find it difficult to be strategic at the office. For instance, if we look at jobseekers who went through a Status Quo application process and make it to the interview, we see the same jobseekers are 20% (5.7 p.p.) less likely to obfuscate their addresses at the interview office (p<0.01). Looking at the experimental results, we see that the index component that is most affected by *Name-Only* is a self-perceived measure of professional behavior, suggesting that jobseekers can not (or will not) self-regulate their

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behaviors as much when they believe their address stigma is visible. If the stress and difficulty in managing behavior are to blame, coaching programs could help jobseekers to prepare for facing the pressure of an interview.

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, our evidence on interview performance suggests that such policies may help them extract more accurate signals during candidate selection.

6 Conclusion

This paper provides evidence that, in a context where favela jobseekers overestimate address-based discrimination, expected discrimination damages interview performance. When we manipulate expected discrimination though expected address visibility at the interview, expected visibility leads to a significant decrease in interviewees' perceptions of their performance and a nonsignificant decrease in the interviewer-assessed performance. Nevertheless, there are statistically significant decreases in interviewer-assessed performance for those who expect high discrimination and for white jobseekers. The effects on these subgroups are consistent with i) expected discrimination leading to worse performance when a stigma is visible and ii) the race and address stigmas acting as substitutes.

Hence, expected discrimination can amplify the effects of whatever discrimination exists in interviews. It can lead to self-fulfilling prophecies, at least in sense that if a jobseeker expects a worse evaluation because of their address, they get one. As expected discrimination can make favela residents look worse on average to interviewers, it has the potential to create a self-fulfilling prophecy: as many favela residents expect discrimination and perform worse, recruiters may form opinions about favela jobseekers that do not reflect their full capacities. While we do not see significant effects on average interviewer-assessed performance, future research can test whether it is true with a higher-powered experiment and in other contexts where expected discrimination may be more important. To close the loop, it would also be necessary to verify whether interviewers perceive the discriminated group as worse on expectation.

Regarding the application decision, we show evidence from two experiments indicating that expected anti-favela discrimination plays no role in most jobseeker's decision to apply. We show that i) manipulating expected stigma visibility at the moment of the application decision and ii) shifting beliefs about expected discrimination rates (by randomly revealing to jobseekers

callback rate estimates for favela and non-favela neighborhoods) do not change application rates. White jobseekers may be an exception to that rule since they apply more often when told that their addresses are unnecessary at the application stage – an effect of stigma visibility that is again consistent with the visibility of race and address operating as substitutes. The non-responsiveness of non-whites to their address visibility may be why we see null effects on the experiment manipulating address visibility at the application stage.

The possibility of making decisions in private and in one's own time when crafting an application (but not at the interview) may be another reason why we see effects at the interview but not at the application stage. Address obfuscation may have also contributed to the null effects in the Information Experiment. At the moment of the interview, expected discrimination might be more important for several reasons like stress, difference in stakes, or even stereotype threat.

Given the importance 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. 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. These DEI commitments can be costly for firms (e.g., a firm might need to hire staff to develop and implement such policies), while their upsides are uncertain. If DEI commitments 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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A   Supporting Tables And Figures

A.1   Baseline Survey

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	25.91	6.24	18	41	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 search last week (0/1)	0.49	0.50	0	1	2,167
Microsoft Office Experience (0/1)	0.80	0.40	0	1	1,984
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
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. Differences in sample sizes occur because we dropped them after introducing the Information Experiment.

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.

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 school	(7) Working now (0/1)	(8) Ever worked (0/1)	(9) Age
Address Omission	-0.002	0.015	0.081***	0.092	-0.017	0.027	-0.005	0.053*	0.548
	(0.032)	(0.029)	(0.031)	(0.059)	(0.028)	(0.029)	(0.032)	(0.031)	(0.425)
Known Address	-0.002	-0.026	-0.004	-0.025	-0.009	0.024	-0.011	0.039	0.737*
	(0.032)	(0.028)	(0.030)	(0.058)	(0.028)	(0.029)	(0.032)	(0.031)	(0.426)
Observations	1302	1302	1302	1302	1302	1302	1302	1302	1302
Status Quo Mean	0.66	0.23	0.27	-0.07	0.80	0.76	0.33	0.69	25.35
Favela=Full *p*	1.00	0.15	0.01	0.05	0.76	0.91	0.83	0.63	0.66

* 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 school	(7) Working now (0/1)	(8) Ever worked (0/1)	(9) Age
Favela Info	0.025	0.003	0.021	-0.020	0.036	-0.001	0.017	0.057	1.008*
	(0.047)	(0.038)	(0.043)	(0.078)	(0.044)	(0.034)	(0.043)	(0.038)	(0.585)
Full Info	-0.030	0.017	0.026	-0.166**	0.037	-0.072*	-0.027	-0.011	-0.311
	(0.047)	(0.039)	(0.044)	(0.082)	(0.045)	(0.037)	(0.043)	(0.041)	(0.572)
Observations	690	690	690	690	690	690	690	690	690
No Info Mean	0.44	0.20	0.29	0.09	0.33	0.85	0.29	0.77	25.78
Favela=Full *p*	0.23	0.72	0.91	0.07	0.98	0.05	0.29	0.07	0.02

* 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	0.020	-0.001	0.006	0.051	-0.095**	0.019	0.060*	0.056	-0.106
	(0.048)	(0.042)	(0.043)	(0.085)	(0.047)	(0.041)	(0.033)	(0.043)	(0.567)
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	25.01

* 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)	1302	0.790	690	0.354	422	0.614	1992	0.436***	1724	0.176***	1112	-0.260***
		(0.011)		(0.018)		(0.024)						
Jacarezinho resident (0/1)	1302	0.184	690	0.193	422	0.204	1992	-0.009	1724	-0.020	1112	-0.011
		(0.011)		(0.015)		(0.020)						
Manguinhos resident (0/1)	1302	0.027	690	0.454	422	0.182	1992	-0.427***	1724	-0.156***	1112	0.271***
		(0.004)		(0.019)		(0.019)						
Age	1302	25.783	690	26.036	422	24.815	1992	-0.254	1724	0.967***	1112	1.221***
		(0.174)		(0.236)		(0.283)						
Male (0/1)	1302	0.295	690	0.303	422	0.265	1992	-0.008	1724	0.030	1112	0.037
		(0.013)		(0.018)		(0.022)						
White jobseeker (0/1)	1302	0.228	690	0.210	422	0.237	1992	0.018	1724	-0.009	1112	-0.027
		(0.012)		(0.016)		(0.021)						
Some college (0/1)	1302	0.064	690	0.080	422	0.071	1992	-0.016	1724	-0.007	1112	0.009
		(0.007)		(0.010)		(0.013)						
Completed regular high-school (0/1)	1302	0.776	690	0.823	422	0.777	1992	-0.047**	1724	-0.002	1112	0.046*
		(0.012)		(0.015)		(0.020)						
Working now (0/1)	1302	0.326	690	0.284	422	0.135	1992	0.042*	1724	0.191***	1112	0.149***
		(0.013)		(0.017)		(0.017)						
Holds a formal job (0/1)	1302	0.118	690	0.135	422	0.047	1992	-0.017	1724	0.071***	1112	0.087***
		(0.009)		(0.013)		(0.010)						
Ever worked (0/1)	1302	0.722	690	0.786	422	0.737	1992	-0.064***	1724	-0.015	1112	0.049*
		(0.012)		(0.016)		(0.021)						
Actively search last week (0/1)	1302	0.531	690	0.425	422	0.649	1992	0.106***	1724	-0.119***	1112	-0.225***
		(0.014)		(0.019)		(0.023)						
Surveyor-assessed comm skills (Likert scale, 0-5)	1302	2.795	690	2.797	422	3.001	1992	-0.002	1724	-0.206***	1112	-0.204***
		(0.029)		(0.045)		(0.051)						
Math test score	1302	6.960	690	6.945	422	6.919	1992	0.015	1724	0.041	1112	0.026
		(0.072)		(0.091)		(0.115)						
Reservation wage (USD)	1301	253.155	690	246.173	422	231.962	1991	6.983	1723	21.193***	1112	14.211***
		(3.016)		(3.215)		(2.736)						

* p<0.1, ** p<0.05, *** p<0.01.

Table A.7: Effects of Information on Beliefs for Under- and Overestimators

	(1) Disc (%)	(2) Cb. Own Neigh (%)	(3) Cb. Own Neigh (%)	(4) Cb. Own Neigh (%)	(5) Cb. Other Neigh (%)	(6) Cb. Other Neigh (%)	(7) Cb. Other Neigh (%)
Favela Info	-7.288**	-2.419	-3.344*	2.995	-8.049***	-8.521***	3.337
	(3.487)	(1.886)	(1.961)	(3.925)	(2.149)	(2.116)	(9.814)
Full Info	-20.371***	-1.428	-3.962*	11.417**	-13.923***	-14.918***	12.625
	(3.692)	(1.973)	(2.055)	(4.541)	(2.217)	(2.193)	(12.195)
Observations	690	690	554	136	690	637	53
Sample	All	All	Overestimators	Underestimators	All	Overestimators	Underestimators
Control Mean	35.46	36.60	40.62	18.36	59.29	61.34	29.71
Favela=Full p	0.00	0.60	0.76	0.05	0.01	0.00	0.40

* p<0.1, ** p<0.05, *** p<0.01.

Figure A.1: Figure 4 with Lasso-selected Controls
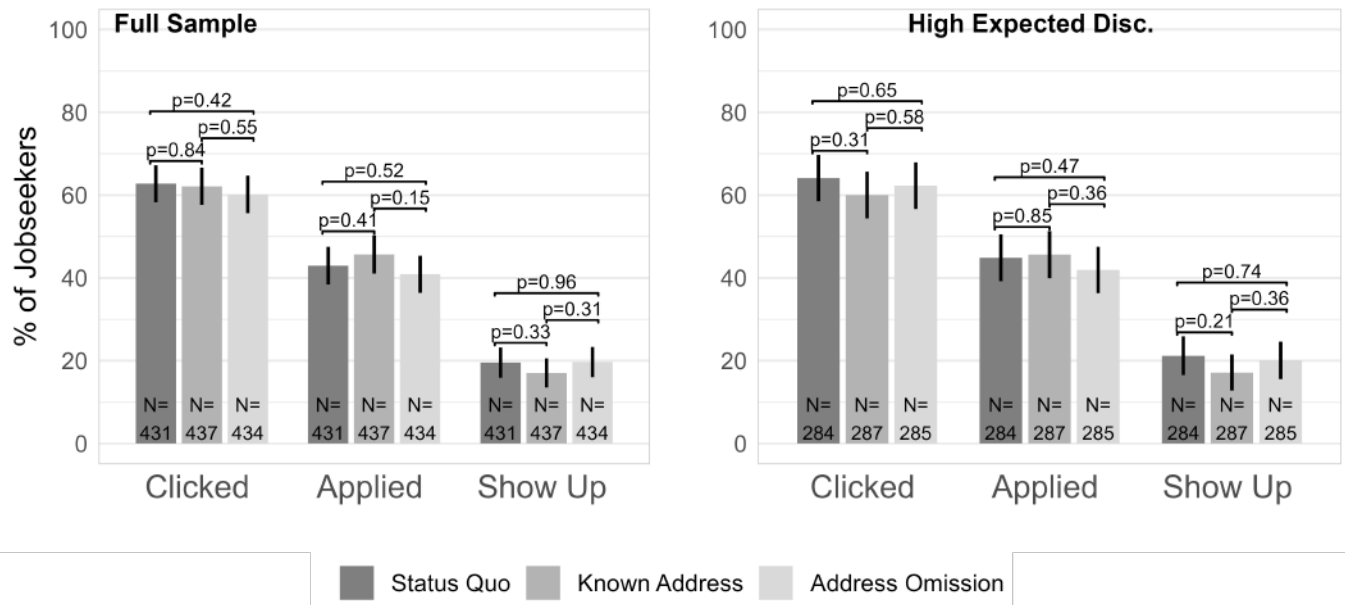
Figure A.2: Figure 5 with Lasso-selected Controls
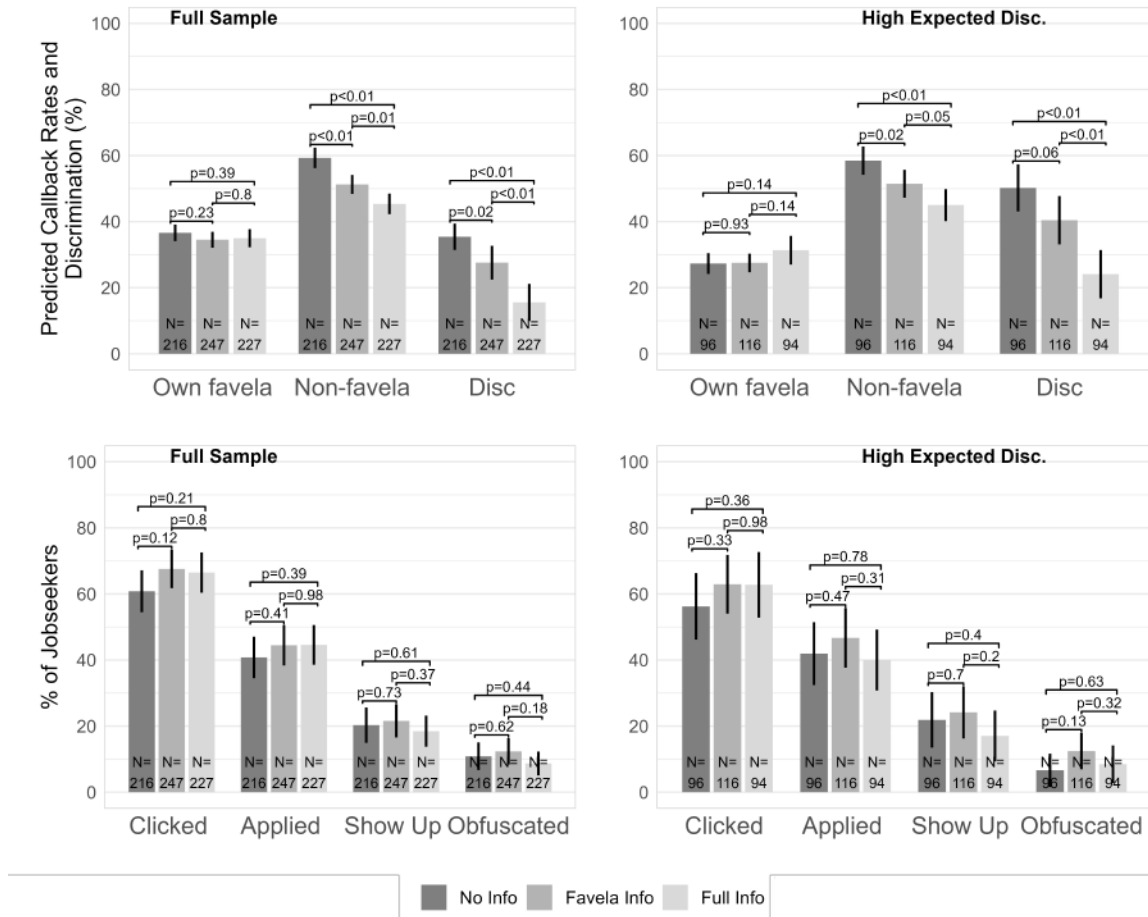
Figure A.3: Effects of Information Treatments on Beliefs and Applications by Whether Job-seekers Initially Under- or Overestimated the Favela Callback Rate
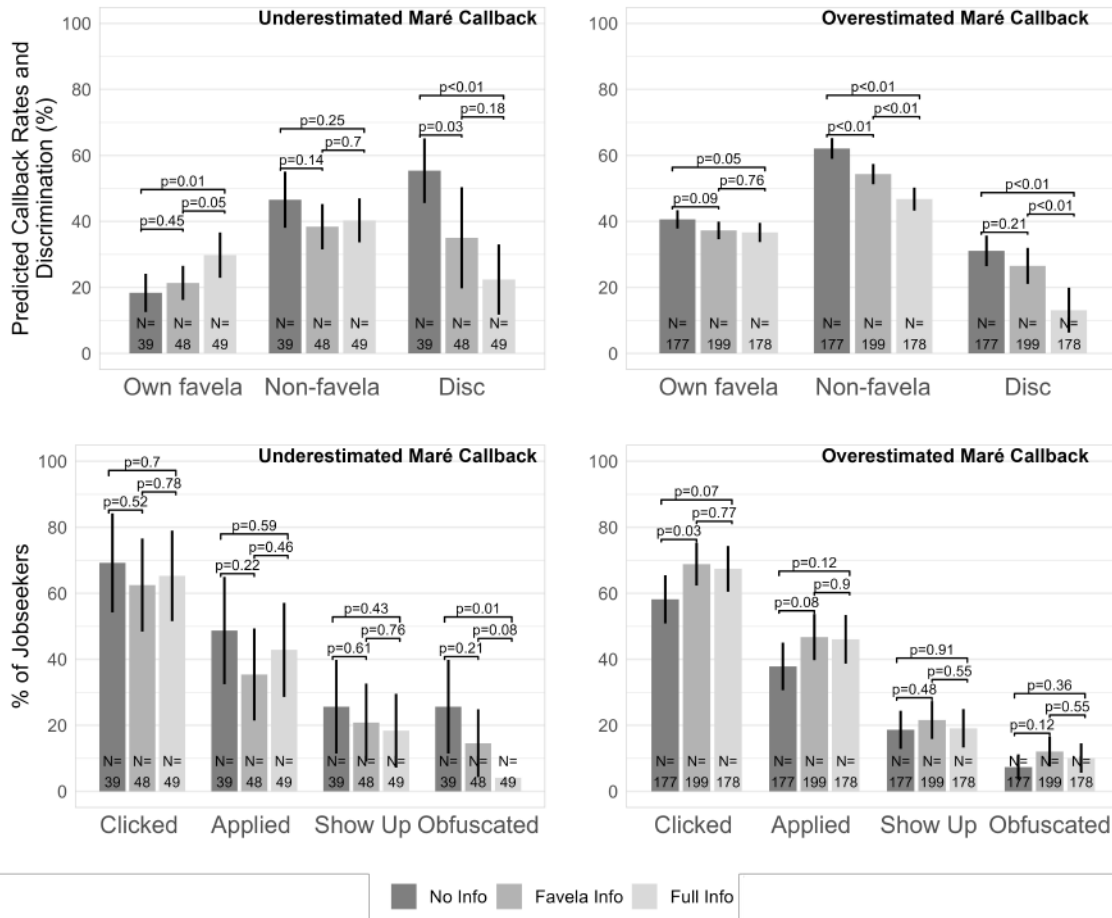
Figure A.4: Heterogeneous Effects in the Address Omission Experiment – No Controls
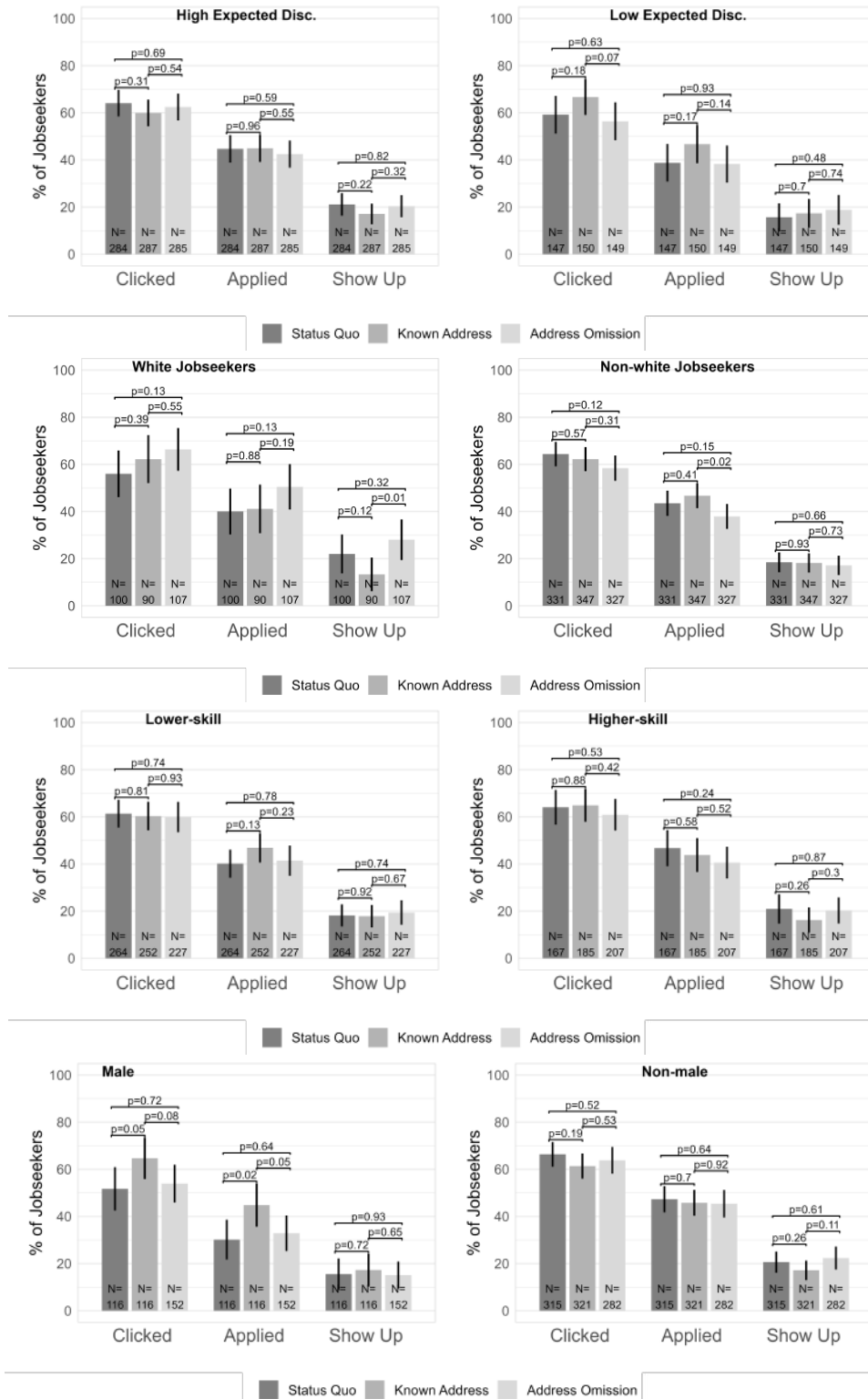
Figure A.5: Heterogeneous Effects in the Address Omission Experiment – Double-lasso Controls
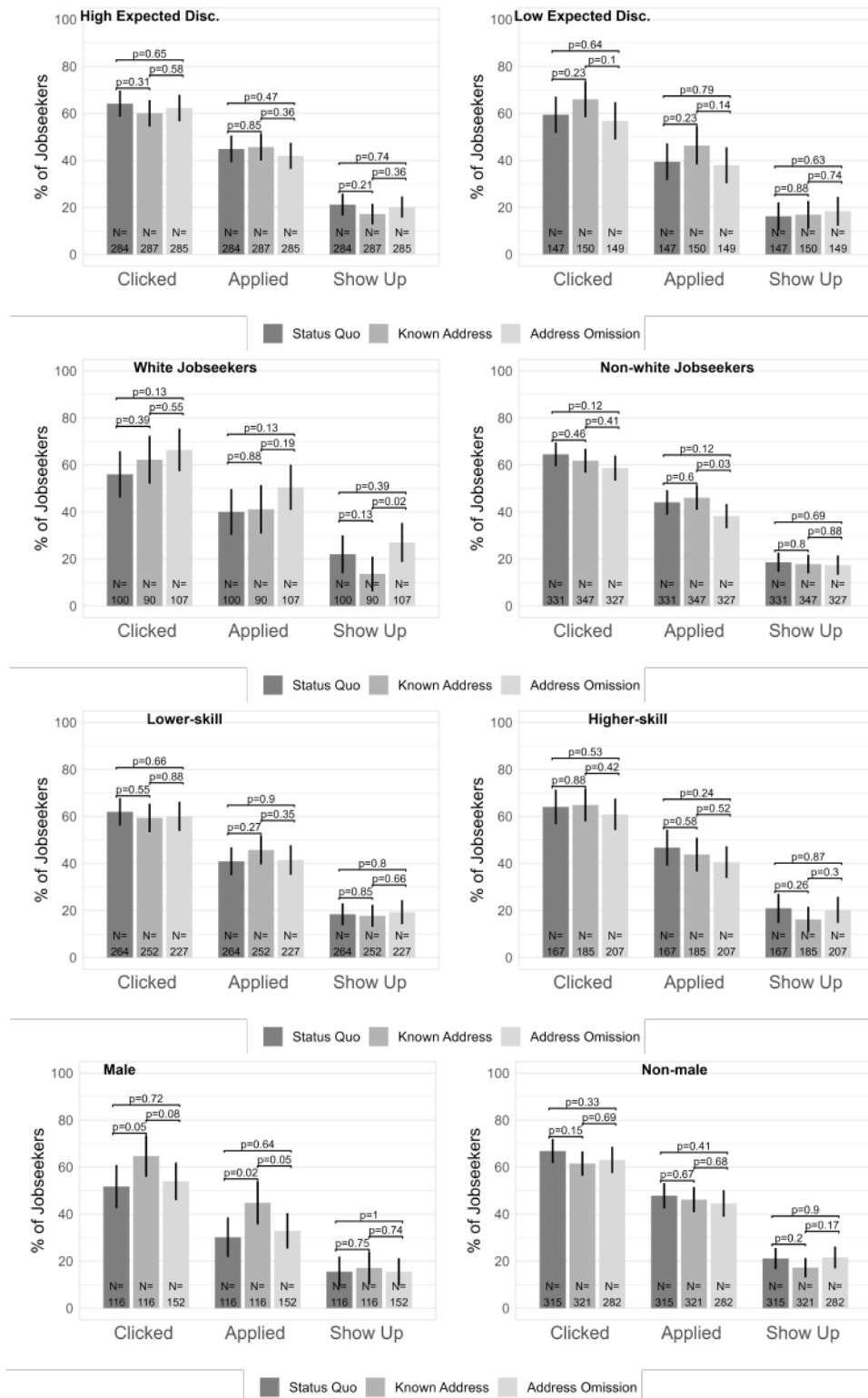
Figure A.6: Heterogeneous Effects of Information Treatments – No Controls
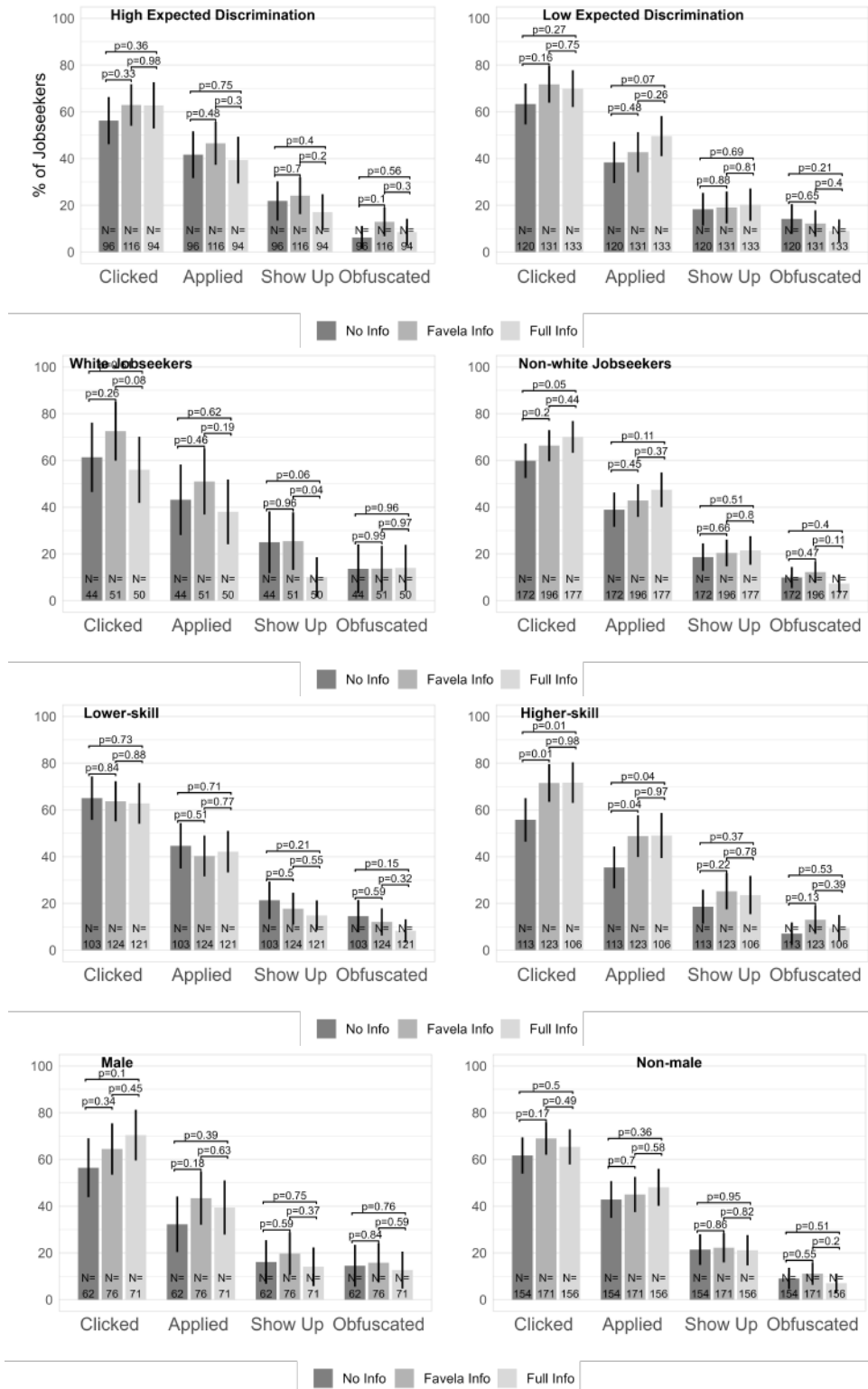
Figure A.7: Heterogeneous Effects of Information Treatments – Double-lasso Controls
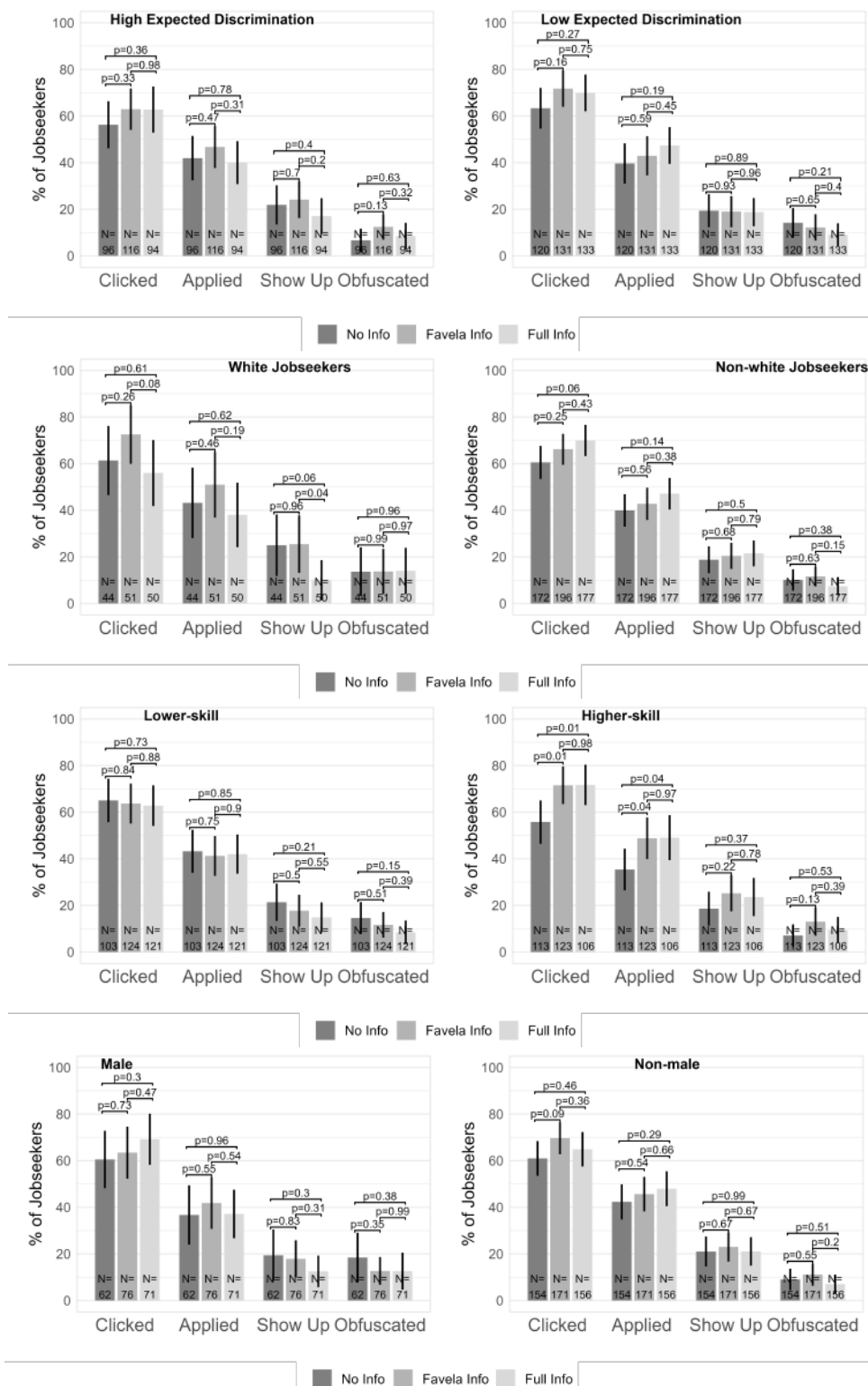
Figure A.8: Heterogeneous Effects of *Name-Only*
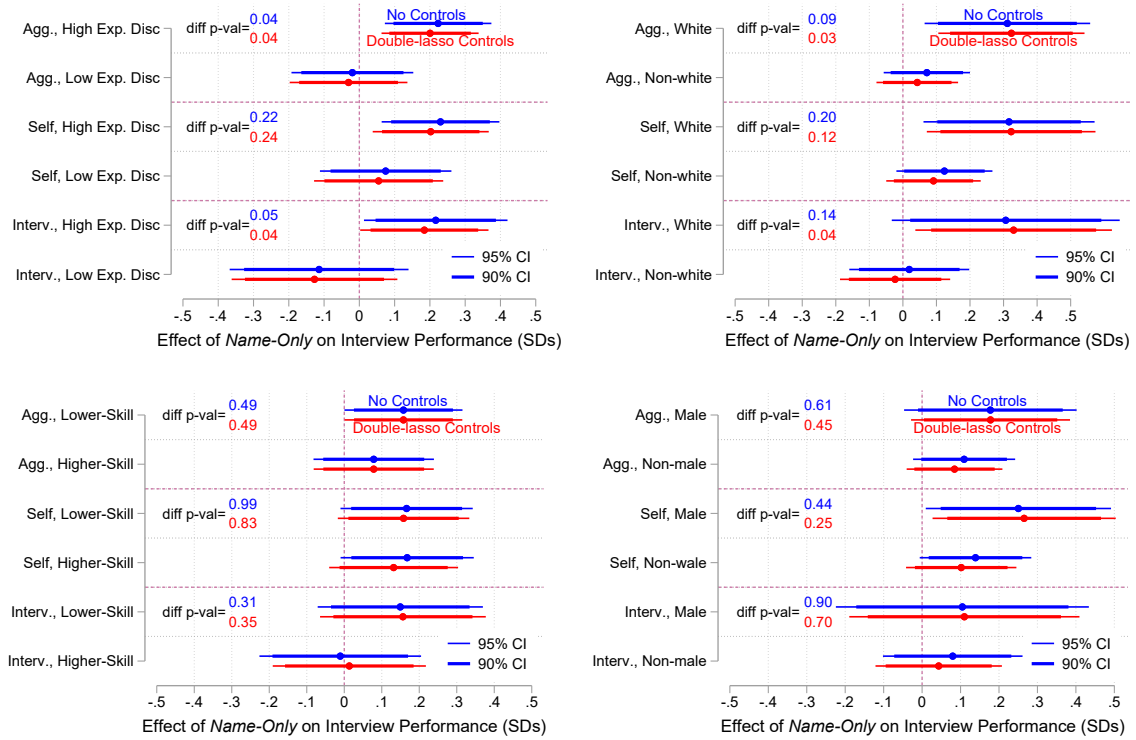
Figure A.9: Predicted *vs.* Actual Discrimination Rates
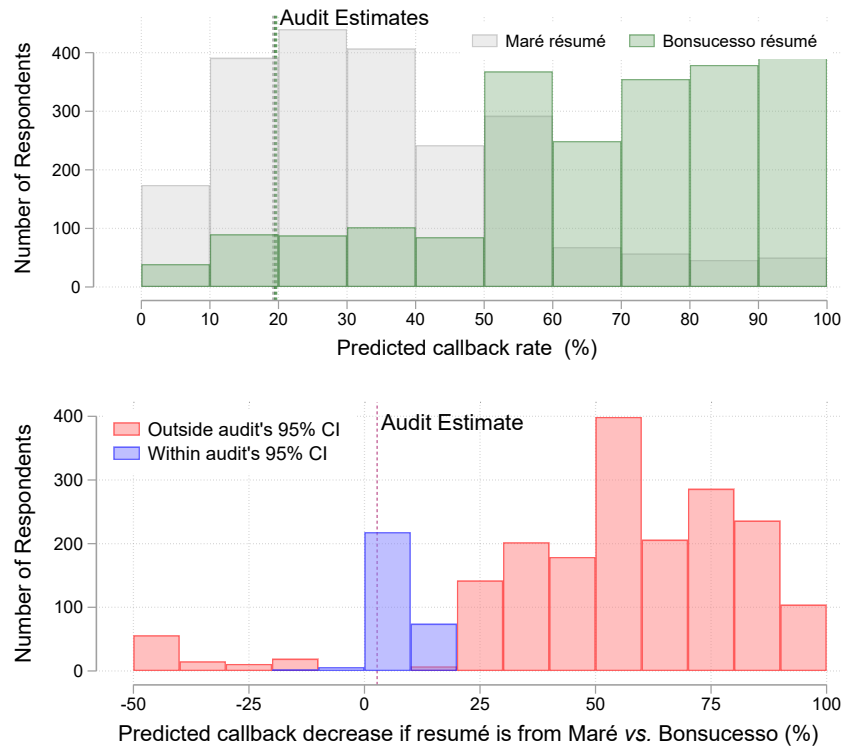
Figure A.10: Predicted Audit Study Discrimination Correlates with Other Measures of Expected Discrimination
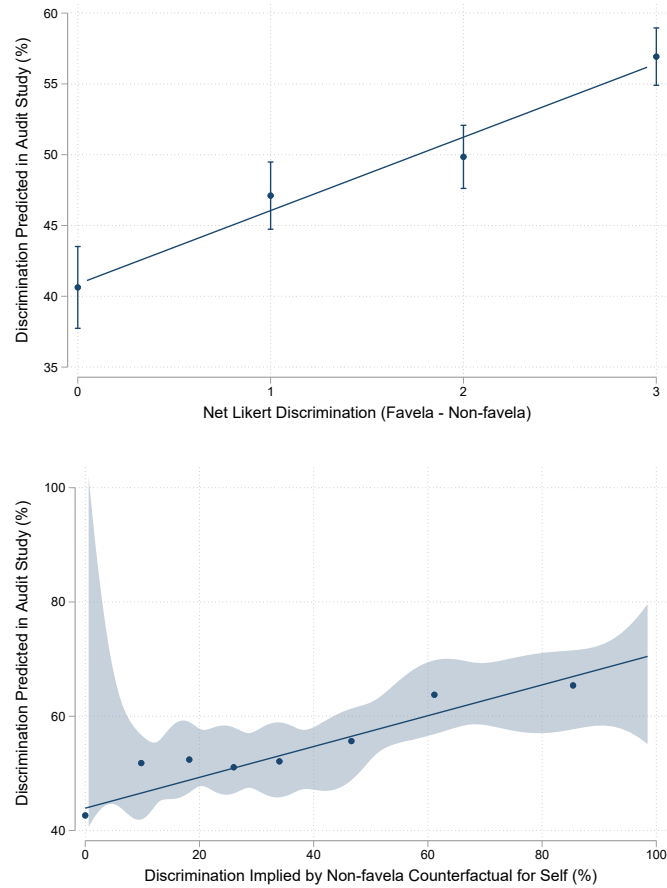
Note: Negative values of discrimination are pooled with zero discrimination – since there are few observation with negative discrimination, which make estimates noisy. We construct the Likert discrimination measure by taking the Liker-scale answers of how much employers discriminate against individuals in each neighborhood (from no discrimination to a lot), converting them into ordered integers, and and taking the difference. We calculate the discrimination for the counterfactual self by comparing the beliefs about one's job-finding probability over the next six months to "somebody just like you, but from [adjacent non-favela]".

Figure A.11: Belief Update in Information Experiment Occurs for Maré and Non-Maré Residents
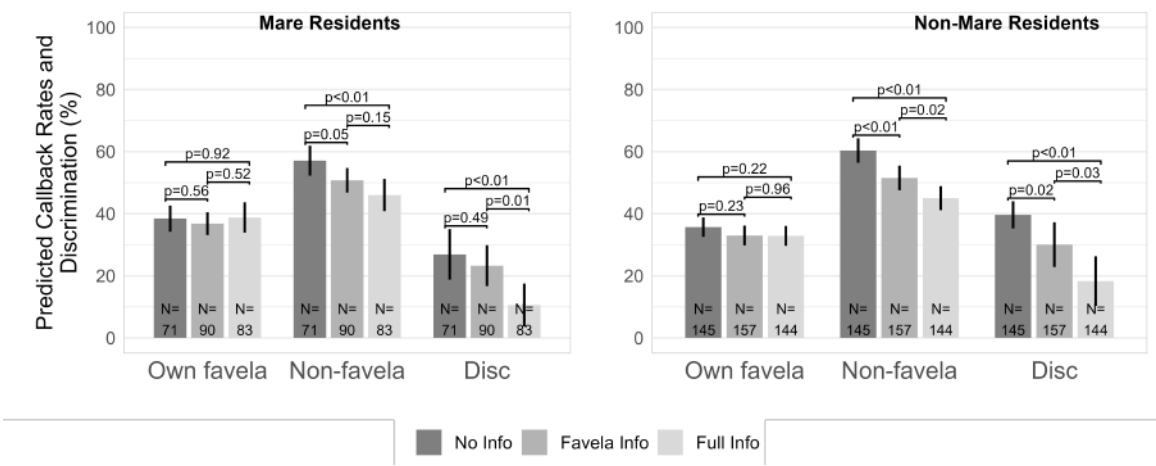
B Deviations from the Pre-Analysis Plan

- We initially planned to stratify the randomization in the Interview Experiment by predicted discrimination level *and* previous treatment assignments. On implementation we kept only stratification 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 generate a very small number of observation per strata.

- 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 and a measure of communication skills.

- 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 schedules for a period was too low for make up a single strata.

- We also updated our experimental design after completing half of our fieldwork. See https://www.socialscienceregistry.org/trials/11041 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.

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.

C.2   Résumés

Figure C.2: Example Résumé – Maré home address

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

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.

C.3 Job Postings

Figure C.4: Examples of Job Posting

Note: This is a job posting for one salesperson position in a dental clinic posted in Infojobs. It required a middle school degree and no previous work experience.

C.4 Results

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.

D   Materials Used in Experiments

Figure D.1: Door-to-Door Baseline Survey

Notes: This Figure shows surveyors interviewing research participants in Maré.

Figure D.2: Predicted Discrimination Baseline Script

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 résumés, 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: This Figure displays how we elicited prior beliefs about discrimination against favela dwellers.

Figure D.3: Partner's 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

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*

D.1 Interview Experiment Details
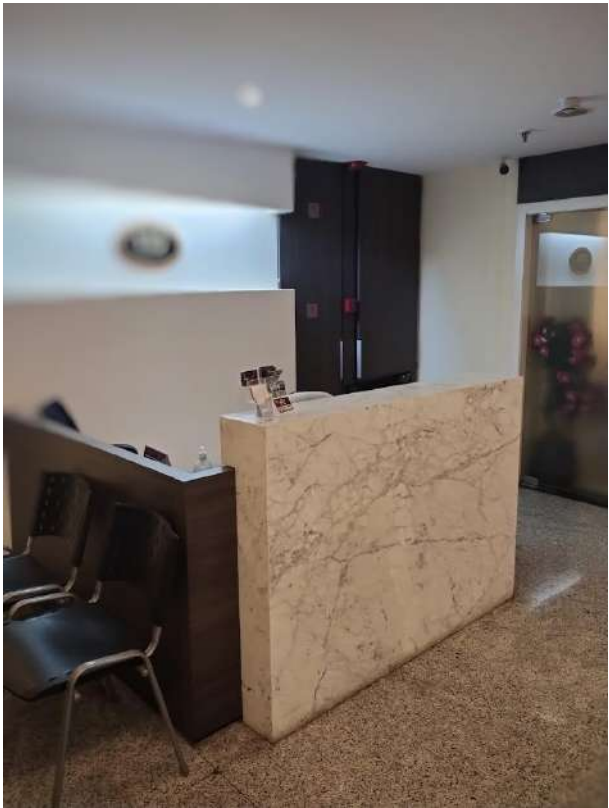
(a) Co-Working Reception (b) Interview Room
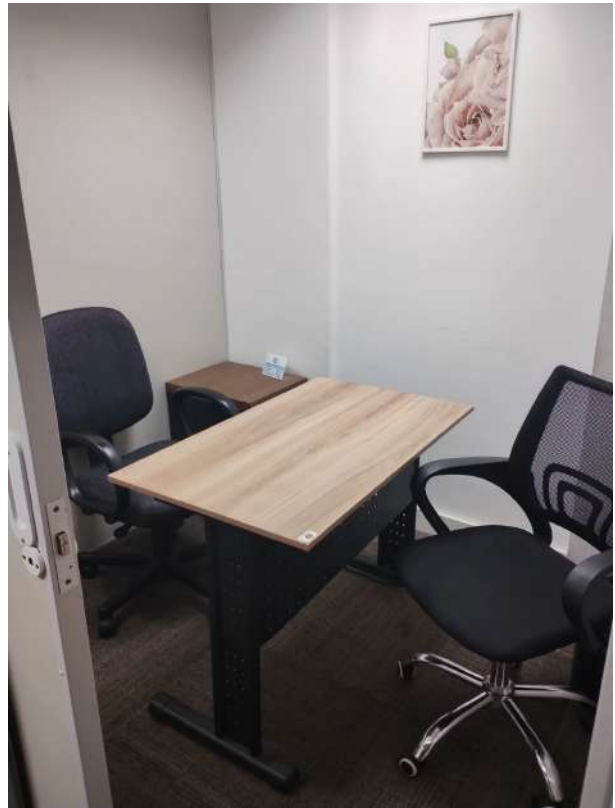
Figure D.5: Interview Co-Working Space

D.2 Interview Script

D.2.1 Introductions

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.

D.2.2 Interview's Questions

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*

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*

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*
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*
Interviewer evaluates how well the candidate did on this question, from 0 to 10, and also writes down: (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*
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*
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*

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.

D.2.3   End of the Interview and Interviewer's Assessment

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?*