The (in)visible hand: do workers discriminate against employers?*

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Abstract

Do employees discriminate against potential employers? We implement a randomized experiment in an online labor market that is pro-typical for a set-up where employees have the opportunity to discriminate against employers to answer this under-explored research question. In our experiment, workers make labor-supply decisions after we randomly expose them to signals about the race and sex of the potential employer. Investigating average effects for the full sample of workers, we find evidence of discrimination on the quantity and quality of work (conditional on working), but not on the general willingness to work in our labor task. Upon completing the experiment, workers fill out a survey in which they report whether they discerned the signals regarding race and sex of the employer. The survey responses reveal an interesting pattern of heterogeneity: subjects who discerned the treatment signals were more likely to work for the minority employer, while those who did not discern the signal were less likely to work for the minority employer. The latter result suggests that discriminating employees try to conceal their behavior ex-post. An additional survey with randomized components among online workers suggests that our results are *not* driven by statistical discrimination.

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1 Introduction

The existing literature on discrimination in labor markets tends to focus on discrimination among employers against employees and among employees against other employees. Studies on employer-to-employee discrimination cover cases where employers make job-related decisions (e.g., hiring decisions, promotion, and salary increases) based on workers' characteristics such as sex and race rather than workers' productivity. Numerous audit and experimental studies have confirmed discrimination with respect to race and gender in this branch of the literature (see the surveys by Bertrand and Duflo 2017 and Neumark 2018). Studies on employee-to-employee discrimination focus on cases where employees discriminate against each other. This could arise among employees of similar rank (e.g., Hedegaard and Tyran 2018) or employees of different ranks (e.g., Glover et al. 2017; Abel 2019). While both branches have highlighted the desperate treatment of minority groups in the labor market, another dimension of discrimination in labor markets remains largely understudied: discrimination by employees against (potential) employers.

The relevance of employee-to-employer discrimination has increased significantly given the dramatic increase in gig-economy markets where workers are able to choose among many employers. Employee-to-employer discrimination is also relevant in traditional labor markets during times of low labor-market thickness and in contexts where individuals with specialized skills are able to select their employer. Finally, the inability of employers to perfectly monitor employees' labor effort implies that employee-to-employer discrimination can manifest in all kinds of labor markets in the form of shirking.

Conceptually, employee-to-employer discrimination is different than employer-to-employee discrimination, regardless of the motivation for discrimination. For example, consider taste-based discrimination: paying money to someone I do not like possibly elicits a different psychological response than receiving money from someone I do not like. A similar difference might arise when we think about exerting effort for someone rather than having someone exert effort for us. Statistical discrimination may also be different across these dimensions of discrimination: while employees (especially those in a gig-market environment) are primarily interested in being paid by their employer, employers seeking an employee (gig worker) attach importance to a wider set of attributes. For example, an employer is interested in an employee's effort, diligence, productivity, reliability and punctuality.

Employee-to-employer discrimination is also conceptually different from employee-to-employee discrimination (even against employees of higher ranks). First, group-identity possibly affects how workers view their co-workers versus their employer. Second, workers generally have greater interaction with their co-workers (incl. those of higher rank) than with the employer. Third, with some exceptions, shirking by the employee will harm the employer but not the co-workers of higher rank. Fourth, in many smaller operations like

the one we study, hiring/firing, wage setting, evaluation, and promotion decisions are handled by the employer and not co-workers of higher rank. As such, it is not clear that a worker's discriminatory response toward co-workers will mimic her response toward her employer.

The rise of work settings in which employees can freely choose their employer as well as conceptual differences between employee-to-employer and the dimensions of discrimination that have been studied in the literature constitute the motivation for our paper. In particular, we contribute to the literature on discrimination in labor markets by asking the following question: do workers discriminate against potential employers on the basis of the employer's race or sex? We answer our research question using data generated in a randomized experiment with 2174 subjects recruited from Amazon Mechanical Turk (mTurk). mTurk is an established online labor-market platform and a big player in the gig-economy. The subjects are invited to complete a survey about a topic unrelated to our discrimination study. Upon completion of this survey, subjects are randomly assigned to one of five groups and offered an opportunity to complete an additional bonus task where they are paid a piece rate to transcribe information from gasoline receipts. The announcement of the additional bonus task features a photograph of a hand holding a gasoline receipt.

We signal the employer's race and sex by randomly varying the presence and characteristics (gender and race) of the hand in the photograph across groups (as in Doleac and Stein 2013).[1] After being exposed to the treatment photograph, subjects are asked if they wish to complete the bonus task. Subjects who respond *yes* are allowed to transcribe up to 40 gasoline receipts. Signaling sex and race through the hand in a photograph (and not through, say, pictures of a face) is an important design feature that allows us to study discrimination in the absence of confounding factors such as a sympathy or trustworthiness.

The experimental design also allows us to observe subjects who are treated but decide not to complete the bonus task. As a result, we are able to study discrimination against employers on the two margins we identified to be important above: an *extensive margin* – the decision to work for the employer in the bonus task or not[2] – and the *intensive margin* – the amount and quality of work conditional on accepting the task.

Subjects who decide to transcribe pictures and subjects who opt out of the bonus task complete a post-experimental survey (either after they are done transcribing re-

[1]The photograph used in the control group does not feature a hand while the photograph used in the treatment groups features either a black or white hand (to signal race) with or without nail polish (to signal gender).

[2]We use the term 'extensive margin' when we examine the decision to work in our bonus task or not after the exposure to the randomized treatments. It is of course possible that subjects who decline our bonus task work on another HIT on mTurk. In this regard, our usage of the term 'extensive margin' should not be understood as describing the decision to work at all or not.

ceipts or after they declined the bonus task). Among other questions, this survey asks subjects about the gender and race of the person holding the receipt in the treatment photograph. These questions allow us to define the self-reported saliency of the treatment. We first define subjects' perceived treatment as their self-reported response to our post-experimental survey questions and then classify a subject as *treatment-salient* if her perceived treatment matches her actual treatment. For example, a subject is sex-salient if her perceived sex-treatment matches her actual sex-treatment. We classify a subject *sex-nonsalient* if her perceived sex-treatment does not match her actual sex-treatment. The saliency measure for race is defined similarly.

We study two dimensions of discrimination (race and sex) along three margins of labor supply: the extensive-margin response (see above and footnote 2) and two intensive margin responses (number of receipts transcribed, and transcription quality (accuracy)). For each of these outcome variables, we test whether the mean outcome for the minority employer (black or female) is different from the mean outcome for the majority employer (white or male). We always start off estimating intent-to-treat effects, which focus on the direct effect of the randomly assigned treatment. We further implement two complementary analyses to account for the possibility that the treatment signal was not salient to all subjects.

First, we estimate the effect of *perceived* treatment on our three margins of labor supply using a two-stage least squares IV model where the randomly assigned treatment serves as an instrument for the perceived treatment.[3] Second, we split the sample by self-reported treatment saliency and estimate treatment effects separately for treatment-salient and treatment-nonsalient workers. In order to hide their discriminating behavior, subjects who discriminated against a minority employer might indicate in the (self-reported) post-experimental survey that they did not discern the sex or race of the employer. Non-zero treatment effects among nonsalient workers might be an indication for such behavior. This sample-split analysis thus sheds light on the question of whether discriminating subjects try to conceal their behavior ex-post. We further test for within-group biases by estimating treatment effects separately for male/female and non-white/white workers.

We find the following patterns of discrimination in our data. First, on average across all workers, we find no evidence of discrimination against employers for neither sex nor race on the extensive-margin decision to work in the bonus task. This finding holds for both the intent-to-treat and the IV approaches. Second, we find evidence of discrimination on the intensive margin across the full sample of workers who decide to

[3]Because subjects self-report their perception after making their labor supply decisions, it is possible that perception is influenced by the the labor supply decision. This IV strategy exploits the variation in treatment saliency that is driven by the randomized actual treatment and identifies a treatment-on-the treated effect.

work in the bonus task. In particular, we find that workers are less accurate and transcribe fewer receipts for black employers, relative to white employers (though the latter is not significant). Interestingly, workers are significantly more accurate and tend to transcribe more receipts for female employers, relative to male employers.

These average effects mask some interesting sources of heterogeneity. When we split the sample by treatment salience, we find that sex-salient workers are significantly more likely to work for female employers, and sex-nonsalient workers are significantly less likely to work for females (both relative to male employers).[4] Similarly, we find that race-salient workers are significantly more likely to work for black employers and race-nonsalient workers are less likely (though not significant) to work for a black employer (both relative to white employer).[5] These opposing effects for treatment-salient and treatment-nonsalient workers mask the average zero effect on the extensive margin that we find across the entire sample of workers. The intensive-margin results for accuracy are somewhat larger for treatment-salient workers than for the overall sample, but the average effects do not mask any opposing effects of salient and nonsalient workers.

Overall, one would have expected to see zero effects among nonsalient subjects because these subjects seemingly did not discern the race and gender of their potential employer and therefore should not exhibit any discriminatory behavior. However, our finding of discrimination against minority employers among nonsalient workers suggests that these workers misreported their responses to questions about race and gender of the employer in the post-experimental survey, either deliberately or unconsciously.[6]

We also split the sample by white and non-white workers to study within-group bias in the effect of the black-hand treatment. While we do not see any heterogeneity on the extensive margin, we do find that white workers discriminate against black employers on the intensive margin while non-white workers do not discriminate against black employers. In fact, the average discrimination against black employers that we reported above for the intensive margin is almost entirely driven by white workers. We also study the discrimination behavior separately for female and male employees. This exercise shows that males transcribe more receipts and do so more accurately when exposed to a female hand (both relative to male hand) and females work more accurately for other females.

Overall, our results suggest that workers consider potential employers' race and sex

[4]In other words (to recall the definition of treatment-saliency), those who reported to have discerned the race of the employer were more likely to work for female employers, while those who reported to have not discerned the gender of the employer were less likely to work for a female employer.

[5]In other words, we find that workers who reported to have discerned the race of the employer were significantly more likely to work for black employers and those workers who reported to have not discerned the race were less likely to work for a black employer (both relative to white employer).

[6]We conducted a pilot study to test the salience of the photos used in the experiment. Results from our pilot study further supports the suggestive evidence of misreporting among subjects in the real-effort experiment. Subjects had no trouble identifying the race or sex of the hands used in the experiment. See Section 2.1 for details on the pilot study.

when making labor supply decisions. But, do workers discriminate against employers because of taste or statistics? The primary channel through which statistical discrimination could arise in our setting is through workers' beliefs about the likelihood that employers of a certain type would honor the labor contract. For example, a worker might be less likely to work for black or female employers if they believe that these employers are less likely to approve and pay the bonus. Results from a survey of mTurkers who did not participate on the real-effort experiment (described in the paragraph below) reveal that non-payment is quite frequent; more than 50% of mTurkers have experienced an employer's refusal to pay for work already completed.

We check for the source of discrimination by surveying a sample of mTurkers (N=955) who did not participate in our main study; referred to as 'follow-up survey' throughout. The primary goal of the survey is to collect information about workers' interactions with employers including workers' perceptions about employers of different types and non-payments. The survey includes several questions and also features randomized components in which we expose survey respondents to the same treatment pictures as in our main experiment. Results from this survey suggest that the sex and race gaps we estimate are *not* driven by statistical discrimination. For example, we find that mTurkers believe male and female employers are equally likely to pay a bonus that was specified in a HIT. The evidence against statistical discrimination is even stronger in the case of the race gap. On the one hand, results from our real-effort labor task study show that race-salient workers were more likely to work for a black employer. On the other hand, race-salient mTurkers in our follow-up survey generally believe that black employers are less likely to pay a bonus. These two findings are inconsistent with statistical discrimination since they suggest that workers are more likely to work for employers who are less likely to pay for the task.

Our paper contributes to the literature on labor-market discrimination by exploring the extent to which employees discriminate against employers. There have been numerous empirical studies of discrimination in labor markets (see Riach and Rich 2002; Bertrand and Duflo 2017; Neumark 2018 for reviews). While some studies focus on employees discriminating against their bosses or other employees (Glover et al. 2017; Hedegaard and Tyran 2018; Abel 2019; Benson et al. 2019), the existing literature mostly studies discrimination by employers toward workers. Such strong emphasis on discrimination by employers against employees is not surprising considering that traditional labor markets are usually characterized by contexts that provide employers the flexibility to discriminate against employees. In particular, workers are generally competing with many other workers for a limited number of job openings. Additionally, in many cases the firm is a neutral entity with respect to race and sex because the owner of the firm is not a single person, but rather many shareholders of varying types including other firms. As a result,

the worker might neither be willing nor able to discriminate against the employer.[7]

However, even traditional labor markets include many arrangements where workers are able to discriminate against employers. For example, consider small family-owned firms (where the identity of the owner is known) or settings where workers are more accurately described as short-term independent contractors. Workers in these latter settings work directly for a specific person (the employer) and are therefore able to consider the employer's characteristics when deciding whether to take on a given task and the effort to exert conditional on accepting the task. These types of employment arrangements have increased significantly in the last decade with the rise of the 'gig' economy (e.g., see Farrell and Greig 2016, Farrell et al. 2018, and Katz and Krueger 2019). Therefore, it is important to understand the extent of discrimination in these settings. Our results suggest that discrimination in these contexts might be of a different character than in the previous literature.

We are aware of one paper that also considers discrimination among workers against employers. Asad et al. (2020) study whether white workers exert greater labor effort for white employers compared to black employers in an online labor market.[8] Our study goes beyond this paper in several ways. First, we explore discrimination based on both sex and race, while Asad et al. (2020) only explore race. Second, our sample of workers includes whites, non-whites, female, and males. In contrast to Asad et al. (2020), we are therefore able to study in-group and out-group discrimination.

Third, our sample design allows us to study intensive-margin *and* extensive-margin responses. Asad et al. (2020), who only consider labor-effort responses, do not study the decision to work for a particular employer or not (our extensive margin). However, we believe that this extensive margin is particularly relevant for understanding discrimination against employers. Additionally, the extensive-margin decision ties to the existing discrimination literature which typically studies the analogous extensive-margin decision to hire/call back an applicant or not. We are able to study this extensive margin due to our innovative experimental design with an initial unrelated survey, which allows us to obtain data even for workers who, after exposure to treatment, decide *not* work on our task. Fourth, our post-experimental survey allows us to distinguish workers by treatment saliency. As a result, we are able to detect an interesting source of heterogeneity and move beyond intent-to-treat effects. Finally, our analysis includes results from an additional survey that allows us to disentangle statistical and taste-based discrimination. Asad et al. (2020) do not explicitly investigate the source of discrimination.

[7]Of course a worker might still express a preference for working for a supervisor of the particular race and sex (Glover et al. 2017). However, this dimension of discrimination is conceptually different than discrimination against emplyoers; see further above in the Introduction.

[8]Our paper was developed independently of Asad et al. (2020). They ran their experiment between August and October 2019 and we became aware of a first version of the paper in December 2019. We ran our initial experiments on mTurk already in December 2017.

We also add to an extensive literature that examines discrimination in platform and online markets. These include markets such as housing rental on AirBnB (Edelman et al. 2017), ride-share (Ge et al. 2016; Cook et al. 2018), and consumer markets (Pope and Sydnor 2011; Nunley et al. 2011; Doleac and Stein 2013; Zussman 2013; Ayres et al. 2015). Our experimental context is an online labor market where workers complete micro tasks for pay on a contractual basis. Unlike many of the other market platforms that have been studied so far, race and gender are not particularly salient in our setting. With the exception of name, participants in the mTurk labor market know very little about each other. Additionally, there is no face-to-face interaction, and communication is primarily by email when needed. This is a particularly interesting case to study because it allows us to comment on the likely effects of increasing the saliency of employer characteristics. Interestingly, we find that minority groups might benefit on some margins but not others. This is unlike the existing literature, which finds almost unanimous evidence that minority groups face discrimination when race and sex are salient. These results are suggestive of the possibility that discrimination by employees toward employers might be different than the traditional setting where employers discriminate against employees (see above for the conceptual differences between these dimensions of discriminationn).

One implication of our findings is that crowd-source labor markets that are designed with strong contract-based arrangements like mTurk can minimize their employer's exposure to discrimination on the basis of race and sex by reducing the saliency of these characteristics. While mTurk maintains a strong sense of anonymity, this is not true of all similar labor markets. For example, some of these labor markets require both employers and workers to establish user-profiles with names and pictures.

The remainder of the study proceeds as follows. We describe the experimental design in section 2. This is followed by a description of the data in section 3, empirical strategy and results in section 4, and discussion of results and mechanisms in section 5. We conclude the paper in section 6.

2 Design and implementation

Our objective is to determine whether workers consider employers' race or sex when making labor supply decisions. We isolate the effect of employer's race and sex on workers' labor supply via a randomized experiment on a crowd-sourcing labor platform. The remainder of this section provides a detailed description of the experimental design.

2.1 Design

Recruitment. We recruit subjects based in the U.S. from Amazon's Mechanical Turk (mTurk) using a HIT that invites subjects to complete a road mileage user-fee survey

for a flat fee of \$0.65.[9] All subjects who decide to complete this survey are directed to the external website of a survey provider (Qualtrics). Upon completion of the survey, subjects complete a brief demographic-questionnaire and are then randomly assigned to one of five treatment groups (between-subjects design) that differ only in the race/sex signal of the employer that we send to subjects. Figure 1 provides an illustrative diagram of the flow of the experiment. The interaction between employers and employees in our experiment is a one-time event. This design feature removes any reputational motives among employers and employees.

Treatment. Once the software has assigned subjects to treatment groups, we thank them for completing the mileage user-fee survey, and inform them that there is an opportunity to earn additional income by completing a transcription task for the same employer (called requester in mTurk language). The additional task and its description to subjects are identical across treatment groups; subjects are asked to transcribe gas station name, date of purchase, gallons of gasoline purchased, price per gallon, and total sale value from gasoline receipts hoarded by one of the authors. We also tell them the approximate time it will take to transcribe the information from one receipt (approximately 30 seconds) and the wage per receipt (\$0.06). See Figure 2 for a screen shot of the details shown to subjects at the time they receive treatment.

When we inform subjects about the additional working opportunity, we show them an example of the gasoline receipts which are to be transcribed in the additional task (see Figure 3). Subjects perceive these pictures to be an illustration of the transcription task. As indicated above, the picture that we show subjects at this stage is identical across treatment groups except for the signal of race and sex (see below). We start with a stock of gasoline receipts that one of the authors collected over a four year period for tax reasons. From this stock of receipts, we selected approximately 100 receipts that were in good condition; all of the information we wanted subjects to transcribe was visible.

Following Doleac and Stein (2013), we signal race and sex of the potential employer by showing subjects a picture of a hand holding a receipt. For this purpose, we selected four hand models; one black and one white female, and one black and one white male. In order to make race and sex salient, we selected black hand models with dark skin, and asked the females to wear nail polish. We then conducted a photo-shoot where we took a picture of each person holding each of the receipts; approximately 100 pictures per person. The pictures included only the receipt and the models' hands. Finally, we selected the most clear pictures for each hand model. From this list, we identified the set of clear receipts that were common across models. This left us with 40 receipts, which were used in the experiment.

[9]The results of this survey are used in an unrelated paper about road mileage user fees.

Our treatment groups differ with respect to the picture that subjects are shown when we illustrate the additional working opportunity to them. We have five treatment groups:

i) **Black-Female** (BF): Subjects see receipts held by a female hand with black skin and nail polish.

ii) **Black-Male** (BM): Subjects see receipts held by a male hand with black skin (no nail polish).

iii) **White-Female** (WF): Subjects see receipts held by a female hand with white skin and nail polish.

iv) **White-Male** (WM): Subjects see receipts held by a male hand with white skin (no nail polish).

v) **Control**: Subjects see receipts that do not include a hand.

Note, again, that the receipts available for transcription are identical across treatments. Receipts are presented in the same order across subjects and groups.

After we expose subject to these receipts, we ask them if they would like to work in the additional task and transcribe the gasoline receipts (see Figure 4). Because the transcription was not included in the initial recruitment HIT, we make it clear to the subjects that the transcription task is optional and that there is no penalty for opting out. Subjects who respond *yes*, transcribe receipts sequentially and are allowed to exit the task after each receipt (see Figure 5 for details).

Outcome Variables. The experimental design allows us to measure three outcomes; one extensive-margin outcome and two intensive-margin outcomes. First, we measure an extensive-margin response based on a subject's decision to accept the additional labor task or not.[10] Second, among all subjects who say *yes* and proceed to transcribing pictures (i.e., conditional on deciding to work on the additional task), we measure the number of receipts transcribed. Recall that subjects could exit after each transcribed picture and that the maximum number of receipts that could be transcribed was 40. The majority of workers did not transcribe 40 pictures and we observe sufficient variation in this variable. Third, we measure transcription accuracy among transcribers by comparing each subject's transcriptions with the actual information on each receipt.[11]

[10]Extensive-margin in our context refers solely to the decision to transcribe gasoline receipts or not; see footnote 2 above in which we acknowledge this point.

[11]Accuracy was calculated as follows: Each receipt had seven items for subjects to transcribe. Let n_i be the number of receipts transcribed by subject i. Then the total number of items transcribed by subject i is $T_i = 7 * n_i$. If we define c_i as the number of correct items for subject i, then the accuracy rate

Post-treatment survey and treatment saliency. One of the features of our experimental design is that we run a post-experimental survey in which we collect data on whether subjects discern the race and gender of the person in the pic that was randomly exposed to them. For this purpose, we ask the following two post-experiment survey questions of all subjects, including those who decided not to transcribe any of the receipts:

1. What is the race of the person holding the receipt in the picture?

2. What is the sex of the person holding the receipt in the picture?

Possible responses are black/female, white/male, 'I don't know', and 'The picture did not include a person'. We randomized the order of the questions and the answer possibilities to control for any order effects. We use responses to these questions to create a measure of (self-reported) treatment-salience. The salience measure indicates if subjects' perceived treatment is equal to their actual treatment, where perceived treatment is based on the subjects' responses to the questions above. For example, a subject's perceived race-treatment is black if her response to question 1 is black. Additionally, she is labeled race-salient if her actual treatment is black; i.e., if her perceived race-treatment matches her actual race-treatment.

These measures are used for two purposes. First, we estimate treatment-on-the treated effects where we instrument perceived-treatment status with the actual (randomized) treatment. Second, we use the treatment-salient measures to study whether treatment effects of the randomized pictures are different between subjects who responded correctly to these questions (salience subjects) and subjects whose responses were incorrect (nonsalient subjects). If, for example, we see non-zero effects for nonsalient subjects, this might be an indication for strategic misreporting to conceal discriminating behavior.

Motivation for the flow of the experiment. We choose a design where subjects recruited via a mTurk HIT flow through the experiment as follows (see Figure 1 for a graphical illustration): complete an unrelated survey, be provided the opportunity to

for subject i is $a_i = c_i/T_i$. The fist step in creating this variable is to transcribe the receipts ourselves. Second, we compare our transcription of each item to the corresponding transcription for each subject and adopt two separate rules to identify correct entries. The first rule is strong in the sense that an entry is correct if it is an exact match to the corresponding entry on the receipt. This decision rule does not allow for rounding of dollar figures. However, the receipts report dollar figures to three decimal places and we do observe that some subjects round these entries. Further, there is a lot of variation across subjects in the rounding rule; some round to two decimals place, others to one decimal place and so on. Because we did not include any instructions about rounding in the experiment, we adopt a weaker definition of accuracy, where an entry is labeled accurate if it matches the corresponding entry on the receipt or any of its possible rounded representations. So, if the receipt lists price at \$2.476, then \$2.476, \$2.48, \$2.5, and \$2 would all be coded as accurate under weak accuracy, while only \$2.476 would be coded as accurate under strong accuracy. Following this procedure, we calculate $a_i = c_i/T_i$ for each subject.

work on an additional labor task and get exposed to randomized sex and race signals, decide to accept or reject additional labor task, complete labor task if accepted additional task task, complete post experiment survey. The key advantage of this design flow is that we are able to collect information on all subjects who are exposed to treatment whether or not they decide to work in the additional labor task. As a result, we are able to study the effect of the treatment on the decision to say *yes* or *no* to our labor task HIT (i.e., what we label the extensive margin). Studying the extensive margin is plausibly the most relevant response margin and therefore very important. An alternative design flow would have been to signal race and gender directly when the HIT is advertised on the mTurk platform. However, this approach would not have allowed us to study the extensive margin since we would only be able to observe people who eventually accept the HIT. In addition, we are able to relate better to the usual type of discrimination papers which study the effect of randomly provided signals of applicants (e.g., via fake CVs) on what is comparable to our extensive-margin response, the decision to be hired, called back or invited for an interview. Our design also allows us to measure labor effort conditional on accepting the the task; we use number and accuracy of transcription to measure effort.

Salience of employers's race and sex. There are three sources of saliency to consider. First, it is important that subjects do not select the initial (survey) mTurk HIT based on their perception of the requester's sex or race because we only have data for workers who accept the HIT. When subjects see a HIT on mTurk and decide on whether to accept the HIT, they do not receive any information about the requester except the requester's name. In order to ensure that selection of the inital HIT is not based on the sex or race of the requester, we select a requester-name 'Alex Wright', which is mostly neutral with respect to race and sex. This approach should minimize the likelihood that subjects select the HIT based on the requesters race or sex. We can confirm that the demographic characteristics of our sample is very comparable to that of other samples that the authors and other researchers have recruited from mTurk in the past.[12]

Second, we want to make sure subjects receive the signal we intended to send via the hands in the pictures. The saliency of the race and sex signals were tested in a pilot-experiment. Subjects were recruited on mTurk (N=120) to view a picture and answer questions about the picture. Subjects were randomly assigned to one of four groups and each subject in each group was shown one picture with a sex/race mix: black female, white female, black male, white male.

We found that the majority of subjects correctly identified the race and sex of the

[12]On average, our sample is 78% white, 56% with B.Sc. or higher, 37 years old, and 48% female (see Table 1). This is comparable to the US sample in Bohren et al. (2019) and the samples in Duncan and Li (2018) and Kuziemko et al. (2015).

hands. Specifically, 83% of subjects correctly identified the race and sex of the black-female hand; 62% and 75% correctly identified the race and sex of the black-male hand, respectively; and 90% of subjects correctly identified the race and sex of the white-female hand. The race of white-male hand used in the pilot was correctly identified 86% of the time, but the sex was only correctly identified 25% of the time. In response to the latter pilot result, we changed the white-male hand model for the actual experiment, but did not run another pilot. However, the race and sex of the white-male hand used in the actual experiment was correctly identified by 79% and 69% of the subjects, respectively. The treatment is presented in a way that should maximize the salience of the hands. After subjects complete the mileage survey and click submit, they are taken to a new page that has the picture of the receipt at the top of the page. Depending on the size of the subject's screen, the picture with the receipt and the hand will be the only thing the subject sees before scrolling down the page.

We follow-up this design feature with a survey at the end of the experiment to capture subjects perception of their treatment status. Subjects are asked about the race and sex of the person in the picture they saw. We also ask subjects about the United States president in order to check if subjects were paying attention. Responses to this post-treatment survey are used to determine whether subjects correctly perceived their treatment status (as will be important, note that the post-experimental survey is self-reported and subjects might strategically lie in the post-experimental survey).

Finally, we want to make sure subjects make the connection between the hand in the picture and the requester. We attempt to increase the connection between the hand and the requester by writing the mTurk HIT and the treatment in first-person singular 'I'. For example, the mTurk HIT includes language like "I would like your opinion about the move toward the mileage tax." Similarly, the instructions subjects see when they receive treatment is written with the intent of connecting the hand to the person making the job request; see Figure 2. For example, we tell subjects *" **I** want to know how much **I** would pay in mileage tax compared to what **I** now pay for gasoline tax"*, *" **I** would like you to transcribe information from **my** gasoline receipts"* , and *" **I** have included a sample of one of **my** receipts above"*.

2.2 Implementation

The experiment was conducted on Qualtrics using subjects recruited from mTurk. We first create a human intelligence task (HIT) that is advertised on mTurk. The HIT includes a description of the initial survey and compensation. We deliberately exclude any mention of the transcription task in the HIT. Instead, we recruit a large sample of subjects to complete a survey and then introduce the treatment. In this way we are able to collect data on all of subjects that are randomly assigned to one of our treatments,

even if they subsequently refuse to transcribe the receipts. Note that we present the transcription task as an additional working task to subjects and that we give subjects the deliberate and explicit opportunity to quit after the initial survey. This ensures that they do not feel confused when they are presented the additional task which was not initially mentioned in the advertisement of the HIT on the mTurk website.

Subjects are told to accept the HIT and click on the weblink if they are interested in completing the survey. Subjects who click on the link are taken to our Qualtrics site where they complete the survey before being assigned to a treatment group to transcribe images. We selected the mileage user-fee survey and gasoline receipt transcription task because it allowed us to present the whole experiment as one event being implemented by a private citizen who is concerned about her state potentially adopting a road mileage user-fee (thus the first survey part) and who is a frequent driver (thus the gasoline receipts). We view it as advantageous that both parts of our study – the initial survey on road mileage user-fees and the subsequent experiment with gasoline-receipt transcription – are in the context of car driving; this makes it appear like an integrated set-up with related components. This reduces the likelihood that subjects view the HIT as part of an academic study thus preserving the reliability of their decisions and responses. Transcribing text from a scanned or photographed receipt is a common type of task on mTurk. This further reduces the chances that subjects realize they are participating in an experiment.

We chose to run the experiment on mTurk for several reasons. First, mTurk is one of the largest online labor markets where job offers are posted and workers choose jobs for payment. According to Amazon, there are over 500,000 workers from 190 countries in the mTurk labor market: `https://requester.mturk.com/tour`. Therefore, mTurk has a special place in the digitally-mediated labor markets that have come on the scene in the last decade. Second, experimenter effects are avoided because subjects do not know that they participate in an experiment (Paolacci, Chandler, and Ipeirotis 2010; Horton, Rand, and Zeckhauser 2011; Buhrmester, Kwang, and Gosling 2011; Mason and Suri 2011). Importantly for us, we are able to identify the effect of race and sex in a naturally occurring labor market. In general, experiments on Amazon's Mechanical Turk therefore combine internal and external validity since it is a "real" labor market with actual workers where randomized trials can be conducted (Horton et al. 2011).[13]

Payment. The experiment ends for each subject when she decides to stop or when she transcribes 40 pictures, whichever comes first. In either case, each subject is instructed to copy her personal ID number and paste it in the entry box on the mTurk website. This process is necessary for us to match subjects to their mTurk worker ID and thus process their payments. Subjects receive a participation reward of $0.65, which is paid as long as a

[13] Kuziemko et al. (2015) and DellaVigna and Pope (2018) are recent examples of economics papers using Amazon's Mechanical Turk.

subject accepts the HIT and completes the survey. Additionally, subjects are paid a piece rate of $0.06 for each transcribed receipt. Given the payment restrictions imposed by the mTurk platform, we frame the piece rate as a bonus in all communications to the subjects. Overall, we paid a total of $2419 for 2500 subjects who took an average of 7.8 minutes to complete the study; this translates to an hourly effective wage of approximately $7.4, which is above the Federal minimum wage ($7.25 per hour since 2009).

3 Data Summary

3.1 Data Cleaning

We fielded the experiment in two waves collecting 1250 responses each time for a total of 2500 subjects; approximately 500 observations per treatment group.[14] We cleaned the data in the following ways before performing our empirical analysis. First, we calculate the total time taken to complete the experiment in minutes and trimmed the top 5% and bottom 5% of the sample. This removed subjects who took fewer than 2 minutes or more than 38 minutes; 267 subjects uniformly distributed across our treatment groups. Second, we drop 66 subjects who stated in check questions that the president of the US is Michael Jordan since this is an indication that subjects were simply clicking through the study. Finally, we drop 68 cases where subjects had the same ipaddress because this might be an indication that the same subject is taking the experiment multiple times or it could be that turkers from other countries are taking the experiment when they should not. These adjustments leaves us with 2174 total observations; a bit over 430 subjects per treatment. Importantly, these adjustments were equally distributed across treatment groups.

3.2 Demographic Characteristics

Because we are interested in race and sex discrimination and the groups are very similar to each other on observables (see appendix Table 7), we combine the treatment groups in the following ways for our analyses: black, white, control, male, female. Summary statistics for these race and sex combinations are presented in Table 1. Overall, our sample is typical of other mTurk samples; average age of 37, 78% white, 48% female, 51% urban, and highly educated with approximately two-thirds of subjects having at least a two-year college degree. Data from the 2018 American Community Survey suggests that our sample is fairly comparable to the U.S. population on age, race and sex. However, the

[14] As indicated above, the HIT included two parts: a mileage user-fee survey and a transcription task. The current paper analyses the data from the transcription task. The mileage user-fee data are used to write a separate paper on public opinion of mileage userfees.

mTurk sample is less urban and more highly educated than the U.S. population.

For ease of comparing demographic characteristics across groups, we take the difference in means between treatment and control groups for each demographic variable and present these results in Table 2 along with pvalues from a ranksum test of the null hypothesis that the means are the same.[15] Of particular interest is the difference between race and sex groups. Except for education, there is no statistically significant difference between the female and male treatments. We find that, relative to the male treatment, the female treatment has 4 percentage points fewer subjects with a B.Sc. degree and 4 percentage points more subjects with a Graduate degree; $p - value = 0.089$ & 0.011, respectively. Similarly, subjects' race is the only statistically significant difference between the black and white treatment groups; 4 percentage points more white subjects in the black treatment relative to the white treatment. We control for these variables in the empirical analysis and find that they do not change our results.

3.3 Salience of Treatment

Figure 6 shows the share of subjects whose perceived treatment matches their actual treatment, for race and sex (i.e., share of subjects who are treatment-salient). Note, again, that the numbers in this figure are based on the self-reported post-experiment survey, where subjects potentially lie for strategic reasons (for example, in order to conceal their preceding discrimination behavior). We find that just under 40% of subjects in the black treatment correctly perceived their treatment compared to over 80% of subjects in the control and white treatment groups. Similarly, subjects in the male treatment were more likely to correctly perceive their treatment status than subjects in the female treatment; 65% for males versus 47% for females.[16] The summary statistics in Table 3 show that the demographic profile of subjects is mostly similar across race and sex salience. Subjects for whom race was salient tended to be modestly younger and from urban areas, while sex-salient subjects tended to be modestly younger.

[15]The majority of the differences between treatment and control groups are statistically indistinguishable from zero. Notable exceptions are race, sex, and education where we observe small differences between the treatment and control groups in some cases.

[16]Results presented in Figures 28 to 33 show subjects' responses across the possible responses on the post-experiment race and sex questions. We find that just under 40% of subjects in the black treatment correctly perceived their treatment, while 30% reported being in the white treatment, 7% reported being in the control group, and 24% did know the race of the hand in the picture. On the other hand, over 80% of subjects in the control and white treatment groups correctly perceived their treatment status with the remaining subjects mostly saying they don't know the race of the hand. The findings are somewhat similar when we look at the salience of the sex treatments. Subjects in the women treatments are more likely to misperceive their true treatment status.

4 Empirical Strategy and Results

This section describes our empirical strategy and results. We present extensive-margin results followed by results for number of receipts transcribed and accuracy. In each case we present intent-to-treat and treatment-on-treated effects.

4.1 Empirical Strategy

We estimate Equation 1 to determine if subjects consider requesters' race and sex when making their labor supply decision in the transcription task.

$$y_i = \alpha + \beta Treatment_i + \delta X_i + \epsilon_i, \tag{1}$$

where y_i is one of three outcome variables of subject i; bonus-task acceptance, number of receipts transcribed, and accuracy. Bonus-task acceptance is an indicator variable that takes a value of 1 if the subject accepted the transcription task and zero otherwise. Number of receipts transcribed simply is the number of receipts that a subject transcribes. Accuracy is measured by the share of accurate entries (see footnote 11). The latter two outcome variables are only observed for subjects who decide to accept the transcription task. X is a vector of subject-level covariates including age, sex, race, education and urban, and ϵ_i is a standard error term.

$Treatment$ is specified in two ways. First, we estimate equation 1 using a subject's randomly determined treatment assignment. This approach identifies an intent-to-treat effect. When we estimate discrimination with respect to race, $Treatment$ is equal to 1 if the subject was assigned to a black-hand treatment and zero if the subject was assigned to a white-hand treatment. When we study gender-based discrimination, $Treatment$ is equal to 1 if the subject was assigned to the female-hand treatment and zero if the subject was assigned to a male-hand treatment.[17] Therefore, β is the estimated race or sex gap depending on the specification; positive values indicate that discrimination benefits the minority group (black or female employers). In these specifications, we estimate equation 1 using three different sample definitions: the full sample, treatment-salient, and treatment-nonsalient samples. We also explore inter-group responses by estimating the model separately for white and non-white workers, and female and male workers.

Second, we estimate equation 1 using a subject's self-reported 'perceived' treatment as the right-hand-side variable of interest (that is, variable $Treatment$ now indicates subjects' self-reported perceived treatment status). Perceived treatment is self-reported, likely influenced by the transcription decision, and obviously not randomly assigned to subjects. To ensure causal identification, we use the randomly assigned actual treatment

[17] We exclude the control group in these specifications. However, the result we obtain is the same as if we estimated $transcribed = \alpha + \beta_b black + \beta_w white + \epsilon$ and then calculate $\beta = \beta_b - \beta_w$.

status as an instrument for the perceived treatment status. This approach then identifies a treatment-on-the-treated effect. We use two definitions of the perceived-treatment variable (and we will refer to these two variants as models 'IV1' and 'IV2'). When we study race discrimination, the first definition is restricted to subjects whose perceived treatment is black or white. Consequently, the variable takes a value of 1 if perceived treatment is black and 0 if white. The second definition includes all subjects. Consequently, the variable takes a value of 1 if perceived treatment is black and 0 otherwise.[18] The perceived sex treatment is defined similarly.

4.2 Results

4.2.1 Extensive Margin

Mean acceptance rate by treatment group. Figure 7 reports the overall acceptance rate across race and sex groups. The figure shows that the mean acceptance rate was approximately 36% across treatment groups.[19] Importantly, there does not appear to be much difference in subjects' willingness to transcribe receipts across employer characteristics.

Intent-to-Treat Effects. We estimate equation 1 to check whether workers decision to transcribe receipts was influenced by the employers race or sex. We present the results graphically by plotting the estimated β coefficients in Figure 8. The left panel (named 'Full') of the Figure presents the estimates for race (black dot) and sex (red dot) along with 95% confidence intervals for the full sample of workers. These estimates are based on the intent-to-treat regressions in which we use the actual treament status as the explanatory variable of interest. The estimated intent-to-treat coefficients are practically zero for both race and sex, which indicates that, on average, subjects were equally likely to work for black/female employers as white/male employers.

Instrumental Variables. The second and third panels of Figure 8 report the second-stage effects effects of the IV models in which the perceived-treatment status is the explanatory variable of interest. The two IV models differ in the definition of the perceived-treatment status (as described above). We exploit the random variation in actual treatment as an instrument for the perceived-treatment. First-stage statistics show that there is a significant and positive relationship between the instrument – actual treatment – and

[18]Recall that possible responses to the perception question are black, white, no hand, and I don't know.

[19]Figure 37 shows that including a hand did not affect the extensive margin decision to transcribe receipts. Receipts with a hand had an acceptance rate of approximately 37% compared to 35% for the control group. See Figure 38 for detailed results across treatment groups.

the variable of interest – perceived treatment (first stage coefficients range from 0.39 to 0.69, with p-values: $< .01$, across all specifications). These first stage results apply for all of the IV results discussed throughout the paper.

The IV results are consistent with the intent-to-treat effects; discrimination coefficients are indistinguishable from zero. The race coefficients in both models and the gender estimate in the second IV model are very small and statistically insignificant. The gender-discrimination estimate for the first IV model is estimated to be 5 percentage points, but the estimate is imprecisely estimated.

Heterogeneity with respect to treatment salience. We split the sample by treatment saliency and plot the treatment coefficients in Figure 9. The left panel of this Figure ('Full') shows the intent-to-treat coefficients for the full sample which we described before; we add them to the Figure to have a benchmark. The middle panel ('Salient') shows treatment effects for the sample of workers who reported to have correctly identified the treatment, while the right panel ('Non-Salient) depicts treatment effects among those workers who reported the 'wrong' treatment.

Interestingly, we find that the null results in Figure 8 hide some important heterogeneity. We find large and statistically significant effects when we split the sample based on the salience of the treatment. Subjects for whom the race treatment was salient were 11 percentage points more likely to work for a black employer than a white employer. Similarly, sex-salient subjects were 14 percentage points more likely to work for a female employer than a male employer. Both estimates are significantly different from zero.

Our null result in the full sample combined with large treatment effects among the 'salient' subjects suggests large negative treatment effects in the non-salient sub-samples. This is precisely what we find. As shown in the right panel of Figure 9, subjects who "misperceived" their treatment were less likely to work for black or female employers. The estimated gap among the non-salient sub-sample is -9 percentage points (p-value=0.007) for sex and -4.3 percentage points (p-value=0.312) for race. This finding is puzzling since we would have expected to find a null result among subjects who paid no attention to the race or sex of the employer. We explore this finding further in Section 5.

Heterogeneity with respect to worker's race and gender. We explore with-in group dynamics by cutting the sample by the race of workers when estimating the race gap and by the sex of workers when estimating the sex-gap. We classify workers as either white or non-white based on their responses to the the survey. The non-white group is a fairly small share of the total sample (only 22%). Therefore, we are careful when interpreting the race estimates, especially for the subsamples on race salience.

Even so, we find a similar patter of results when we split the sample by the race of the

workers. Figure 10 shows that while there is no evidence of a racial gap in the full sample, both white and non-white workers for whom the employer's race was salient exhibited a preference for working for the black employer. This result also shows that the preference for working for a black employer is stronger among non-white workers. Interestingly, we find no statistically significant evidence of a racial gap among workers for whom the employer's race was not salient. However, there is an economically substantive gap among non-white workers for whom race was not salient; the gap among white workers is both economically small and statistically indistinguishable from zero.

Figure 11 reports similar within-group results for the sex-gap. In particular, we find no evidence of a gap in the full sample for neither female nor male workers, but a large positive and statistically significant gap among both male and female workers for whom the employer's sex was salient. Interestingly, we find a strong negative sex-gap among male workers for whom the employers sex was not salient. The gap is similarly negative among female workers in this subsample, but the estimate is smaller and cannot be distinguished from zero.

Overall, our results suggest that women generally prefer working for women. While some men prefer working for women, other men prefer working for men. The group-dynamics for race are also interesting. There appears to be out-group bias among white workers who are more likely to work for black employers relative to white employers. The evidence is more mixed for non-white workers. Nonwhite workers for whom race is salient prefer working for black employers while non-white workers for whom race was not salient prefer to work for white employers. Again, the small number of non-white subjects makes us cautious in interpreting the race group-dynamics.

4.2.2 Intensive Margin I: Number of receipts transcribed

Mean number of transcriptions by treatment group. Approximately 36% (or 753) of subjects transcribed at least 1 receipt, and subjects transcribed an average of 7.6 receipts. However, the distribution is highly skewed; the median number of transcribed receipts is 3, 75% of subjects transcribed fewer than 9 receipts, 90% transcribed fewer than 21 and only 37 subjects transcribed all 40 receipts. Figure 12 shows how the mean number of transcribed receipts varies across treatment groups. Subjects in the control group transcribed 10 receipts on average. While subjects in the treatment groups transcribed fewer receipts than those in the control group, the reduction appears to be uniform across the race treatments but slightly larger for male employers compared to female employers.

Intent-to-Treat Effects. We estimate intensive margin effects using equation 1 with 'number of transcribed receipts' as the outcome variable. The results in the first panel

of Figure 13 show the intent-to-treat effect of actual treatment on number of receipts transcribed for the full sample of workers. We find that workers transcribed more pictures for female employers relative to male employers ($p-value = 0.059$) and fewer receipts for black employers relative to white employers ($p - value = 0.2$). The estimated effects are 1.5 additional picture for females and 1 fewer picture for blacks, which is approximately 15% and 10% of the standard deviation of number of pictures transcribed for sex and race, respectively.

Instrumental Variables. The last two panels of Figure 13 show that the qualitative results are largely the same when we estimate the effect of perceived treatment on number of transcribed receipts using the instrumental variables approach.

The magnitude of the estimated gaps on the second-stage increase slightly when we use the IV approach, but we lose precision. Together, the results in Figure 13 suggest that workers transcribed more receipts for women than men, and fewer receipts for blacks compared to whites.

Heterogeneity with respect to treatment salience. We also estimate equation 1 separately for the salient and non-salient samples. The results presented in Figure 14 show that transcriptions among the non-salient group are in line with expectations; the estimated gap is practically zero for both race and sex. However, we do find evidence that workers for whom the gender-treatment was salient transcribed more pictures for female employers compared to male employers. The comparable gap for race is practically zero.

Heterogeneity with respect to worker's race and gender. There is suggestive evidence of within-group bias for the race-gap and out-group bias for the sex-gap; see Figure 15. In particular, we find that non-white workers complete more transcriptions for black employers than white employers, while white workers complete more pictures for white employers than black employers. On the sex margin, Figure 16 shows strong evidence that male workers complete more transcriptions for female employers than male employers. However, female workers expressed no preference for the gender of the employer when deciding how many receipts to transcribe. We find no evidence of a sex or race gap among non-salient workers, which is what we would expect if subjects did not perceived the treatment signal.

4.2.3 Intensive Margin II: Accuracy

This section describes the accuracy of the transcriptions. We define accuracy as the share of correct entries across all transcribed receipts for each subject and test whether accuracy differs depending on employer's race and sex (see footnote 11 for a more detailed

description of the measurement of accuracy).

Mean accuracy rate by treatment group. Figure 17 summarizes the accuracy of transcription across race groups. We find that subjects in the control group got about 87% of their entries correct. The corresponding rate for the treatment groups is 76% and 80% for black and white groups, respectively, and 80% and 75% for female and male employers, respectively.

Intent-to-Treat Effects. Figure 18 shows that the differences in means reflect significant treatment effects. On average, workers are more accurate for female employers and less accurate for black employers. The estimate for discrimination in favor of women is 4.4 percentage points and discrimination against black employers is -6.3 percentage points; both estimates are precisely estimated.

Instrumental Variables. Considering the two right panels of Figure 18, we see that the intent-to-treat effects are confirmed in our IV regressions, in which the perceived treatment status is the explanatory variable of interest. We again find significant evidence that workers are more accurate for female employers and less accurate for black employers. The IV coefficients tend to be a little bit larger than the intent-to-treat effects. The corresponding first-stage statistics are assuring as we find a first-stage relationship between actual random treatment and perceived treatment that has the expected direction and is estimated to be statistically significant from zero.

Heterogeneity with respect to treatment salience. The above results are driven by workers to whom the treatment was salient. As shown in Figure 19, we find particular evidence among treatment-salient workers that they are significantly more accurate for female employers and less accurate for black employers. The discrimination gaps point in the same direction among non-salient workers, but the coefficients are smaller and not statistically different from zero. As with the other intensive-margin response (transcribed picture), we thus find no evidence of treatment effects among treatment-nonsalient workers.

Heterogeneity with respect to worker's race and gender. Figure 20 shows evidence of within-group bias among white workers who were more accurate when working for white employers compared to black employers. There is no evidence that non-white subjects' accuracy level varied with the race of the employer. The results in Figure 21 show that both male and female workers were more accurate when transcribing for female employers compared to male employers.

5 Discussion

Our results suggest that mturk workers consider an employer's race and sex when making labor supply decisions. We also find some evidence that there are two types of subjects; those who are biased toward minority groups and those who are biased against minority groups. This section of the paper explores these findings. We first discuss the case for the heterogenous preference toward minority groups. Next we explore the extent to which the sex and race gaps reflect an underlying preference for certain types of employers versus statistical discrimination.

5.1 Salient vs nonsalient subjects

We find that the extensive-margin response to the treatment differs between salient and non-salient subjects. This suggests that our salience variable identifies two types of subjects. On the extensive margin, the treatment-salient workers are biased toward the minority employers (black and female), whereas the treatment-nonsalient workers are biased against the minority workers (recall Figure 9). The fact that we find significant evidence of discrimination among subjects in the non-salient group on this margin is particularly interesting and open to multiple explanations. We explore this pattern in the data and discuss possible explanations in this subsection.

Figure 3 shows the race and sex signals that we sent to subjects (actual treatment) and Figure 6 shows the *self-reported* accuracy with which those signals were perceived. Recall that the perceptions are surveyed after the experiment and that they represent self-reported measures which are potentially subject to intentionally false answers. Approximately 80% of subjects in white treatments correctly perceived treatment status compared to only 37% in the black treatments. Interestingly, 31% of subjects in the black treatments reported that the employer is white, while 25% stated they did not know the race of the employer. Only 0.71% of subjects in the white treatments stated that the employer was black and 11.6% responded that they did not know the race of the employer. A similar pattern is observed for sex; 65% of subjects in the male treatments correctly identified the sex of the employer compared to only 43% in the female treatments. Additionally, 17% of subjects in the female treatments stated that the hand belonged to a male while only 1.8% of subjects in the male treatments said the hand belonged to a woman.

Considering that the signals in Figure 3 are clear based on results from our pilot study, then there are at least three possible explanations for the observed difference in subjects' perception of the employer's race and sex. First, it could be that subjects paid attention to the pictures and correctly identified the race and sex, but ex-post misreported the race and sex in an effort to conceal their biases. Second, it could be that

subjects received the race/sex signal and responded subconsciously in a manner that favors majority groups even if they did not pay careful attention to the treatment. This type of channel is referred to as implicit bias in the literature (Bertrand, Chugh, and Mullainathan 2005). Finally, subjects could have ignored the treatment signal and thus failed to respond to the signal, and then guessed at treatment in the post-experiment survey.

The pattern of results presented in Figure 9 is consistent with the first and second explanations. In particular, if non-salient subjects were inattentive to the treatment and simply guessed a response to the race and sex questions, then there should be no significant treatment effect in the non-salient samples. This is especially true since attentive and inattentive samples have very similar demographic profiles. Our results are not consistent with this predicted null-treatment effect.

Subjects for whom race was not salient were approximately 4 percentage points more likely to transcribe for white than black employers (although not statistically different from zero). The effect is even more dramatic if we exclude subjects who responded 'I don't know'; in this case, non-salient subjects are 13 percentage points more likely to work for white employers than black employers ($p - value = 0.03$).[20]

We also find that non-salient subjects are almost 10 percentage points more likely to transcribe for male employers than female employers (statistically significant). Unlike the race treatments, we also find a large difference among subjects who responded 'I don't know'; 25% ($N = 270$) transcribed in the female treatment compared to 34% ($N = 214$) in the male treatment. Furthermore, of the 842 subjects assigned to the female treatments, 147 reported that the hand belonged to a male employer and only 14% of these subjects agreed to transcribe images. Only 15 of the 849 subjects in the male treatment reported that the employer is a female. Overall, these patterns in the data are highly suggestive of strategic misreporting in the post-experiment survey among subjects who discriminate against minority groups on the extensive margin.

Additional evidence suggestive of intentional misreporting among non-salient groups on the extensive margin is the fact that we do not find any statistically significant effects among the non-salient sample on the intensive margin. One possible explanation for this null intensive-margin result among nonsalient subjects is the following. Assume our subject pool includes subjects who notice our treatment signal and those who did not. Those who did not notice treatment can guess their treatment status correctly or incorrectly and are likely equally distributed in the salient and nonsalient groups. For

[20]Subjects who responded 'I don't know' transcribe at the same rate for both white and black employers; approximately 27% ($N = 213$ in the black treatment and $N = 98$ in the white treatment). Subjects in the black treatment who responded that the employer was white or that there was no hand in the picture transcribe at rates of 26% (N=262) and 32% ($N = 56$) for white and 'no hand', respectively. Subjects in the white treatment who responded that the employer was black or that there was no hand in the picture transcribe at rates of 33% ($N = 6$) and 43% ($N = 54$) for black and 'no hand', respectively.

those who noticed our treatment signal, the ones who are honest are in the salient group, while those who "misreported" for any reason but mostly due to the guilt of discrimination are in the non-salient group. Notice that subjects who misreported treatment status to cover up discriminatory reasons for their extensive margin response are not part of the sample used to study the intensive margin responses. Therefore, it is possible that the nonsalient sample used to study the intensive-margin responses is predominantly comprised of subjects who were inattentive to the treatment signal. This would explain the null intensive-margin results among nonsalient subjects.

Overall, these results are very suggestive of a split sample. Some subjects are biased toward minority groups and are not afraid to expose their bias. Based on the evidence that the non-salient workers are more likely to work for the male or white employer, we observe that other subjects are biased against minority groups (either consciously or sub-consciously). Those who discriminated against minority groups conciously then presumably try to hide their discrimination behavior in that they report ex-post that they did not notice the race or gender of the person holding the receipt. We argue that this explanation is supported by the fact that subjects identified the race and sex on the hands with high levels of accuracy in our pilot studies.

5.2 Taste-based or Statistical Discrimination?

In this section, we report the results of an additional survey that we run on mTurk to disentangle taste-based and statistical discrimination as an explanation of the race and sex gaps. In general, workers are interested in their working environment broadly defined to include wages being paid on time and in full, health risks, collegiality, and other amenities. However, the mturk labor market is largely anonymous in that workers never meet employers. Additionally, workers complete their tasks in their own environment and on their own schedule. This implies that mTurkers generally do not have to worry about the working environment provided by the employer except for payment. Therefore, the likely source of statistical discrimination in our context is a worker's expectation of being paid by the employer.[21] Taste-based discrimination is another possible explanation for our findings. This mechanism of course requires that workers are able to identify the employer's race or sex. Although most HITs are accompanied by the employer's name, which may signal the employer's race and sex, it is not clear if workers generally pay attention to the requester's name when selecting HITs.

We designed a survey of mTurkers to obtain information on workers' past experi-

[21] Although requesters (i.e., employers) are required to hold funds in an Amazon account prior to publishing a HIT, a worker is only paid after the requester approves that worker's work. Additionally, approval of a task does not guarantee that a bonus will be paid because employers are not required to hold bonus payments in an Amazon account prior to posting a job. In other words, the requester must first approve the work and then process each worker's bonus separately.

ences with employers and to shed additional light on the role of employer sex and race for mTurkers. The goal is to determine if mTurkers' perception of likely non-payment is correlated with the race or sex of the employer. We are also interested in identifying the extent to which workers contact employers, pay attention to employer's name, and whether workers consider employer's characteristics when selecting a HIT.

5.2.1 Survey Design

The survey has four sections. First, we ask subjects to report their age, sex, race, and education. Second, we ask about their usage of mTurk; year they joined mTurk, whether mTurk is their primary job, and number of HITs completed per month. The third section asks about their experiences as an mTurker. Here we ask about frequency with which work is accepted by requesters, experience with requesters' refusal to pay for work completed, communication with requesters, attentiveness to requesters' name, and whether knowledge of a requester's characteristics would affect likelihood of accepting a HIT. We also ask subjects about the likelihood of a requester paying a bonus for a completed task.

For the final section, we randomly assign subjects to one of five groups that correspond to the five treatment groups of the original experiment. That is, we randomly show survey respondents the same treatment pictures that we showed to workers in the original experiment and then asked them a set of questions. Recall that these pictures differed with respect to the race (black or white) or gender (male or female) of the person whose hand holds a gasoline-station receipt. The treatment is presented as a hypothetical scenario. Specifically, the subjects saw the following text: *"In the next set of questions, I am going to ask you about your perception of what Turkers like you are likely to do when faced with a transcription task."*

Subjects are then asked three questions; i) what percent of mTurkers would accept the HIT?, ii) would you accept the HIT?, iii) how likely is it that the requester would pay the bonus accompanying the HIT?. We also ask subjects to identify the race and sex of the person represented by the hand in the picture. The full set of questions is available upon request.

Sample. The survey was fielded to 1012 mTurkers who did not participate in the original study. The data are cleaned as follows; we drop all duplicated ipaddresses ($N = 41$) and everyone who identified Michael Jordan as president of the US ($N = 16$). This leaves us with a sample of 955 subjects. Subjects took an average of 4.3 minutes to complete the survey and were paid a flat fee of $1.

Balance across survey-experiment groups. We find no meaningful nor statistical difference in the observable characteristics between the race treatment groups.[22] There is a statistically significant difference in the two youngest age groups between the control and male treatments, but these differences are small. We also find that the survey sample is similar to the original real-effort sample in age, sex, race, and education. Importantly, the salience of treatment is identical between the experiment and survey samples (see Table 6).

Prior mTurk experience. Approximately 25% of the subjects report joining mTurk as a worker before 2016, and 13%, 21% and 41% report joining mTurk in 2016, 2017 and 2018, respectively. Subjects report completing an average of 474 HITs per month in the full sample. Panel A of Figure 39 shows that there is heterogeneity in HITs completed across subjects' race but not sex; white subjects complete 106 more HITs per month than non-white subjects (Ranksum $p - value = 0.001$), while female subjects complete only 14 more HITs than male subjects (Ranksum $p - value = 0.29$). The mean monthly completed HITs is 488 in the control group, 479 and 462 for black-hand and white-hand treatments, respectively, and 500 and 440 for female-hand and male-hand treatments, respectively. These differences are not statistically distinguishable from zero (see Panel B of 39).

5.2.2 Experience with Employers/Requesters and Non-payments.

Figure 22 presents the results of subjects responses about their experiences as mTurkers. Approximately 25% of subjects report that mTurk is their primary source of employment. The remaining summary information in Figure 22 describes subjects' experiences with requesters and are suggestive of both statistical discrimination and taste-based discrimination.

First, 45% and 53% of subjects report that their HITs are accepted 'all the time' and 'most of the time', respectively. Approximately 54% report being in a situation where the requester refused to pay for a completed HIT, and approximately 80% have contacted a requester in the past. Additionally, subjects reported being 67% confident that a requester who offers a bonus task on an external website, such as was the case in our real-effort task, would pay the bonus upon completion of the task. This suggests that there is a significant amount of doubt about payment in the subjects minds as they make their HIT selection decisions.

[22] Results for the observable demographic variables are available upon request; age, sex, race, education and time spent on the survey.

Role of employer characteristics for decision to accept a HIT. To the extent that concerns about payment is correlated with perceived race or sex, subjects could use this prior experience to form expectations about the honesty of the requester. Therefore, rather than selecting on the basis of taste, subjects could instead be selecting HITs on the basis of expected payment by the requester.

72% report that that they check the names of requester, and 71% of subjects report that they would consider a requester's characteristics when making HIT selection decisions on mTurk. These responses could support both taste-based as well as statistical discrimination. It could be that subjects use requesters' name and characteristics in order to identify probabilistically-honest requesters. Alternatively, subjects could possibly use this information to identify groups of requesters they have a deep-seated bias against.

Interestingly, we do not find any meaningful differences in these self-reported experiences across subjects' race or sex. This suggests that differential experiences across race and sex is not a strong explanation for the group-dynamics we observe in our real-effort experiment.

Randomized survey experiment to disentangle taste-based and statistical discrimination. We explore the possibility of separating taste-based from statistical discrimination by presenting subjects with the same treatments as in the original real-effort experiment and then asking about hypothetical acceptance and perceived likelihood of being paid. The question about likelihood of being paid allows us to identify the effect of a requester's race or sex on workers' perception of the requester's honesty, which further allows us to comment on the source of the bias uncovered in our real-effort experiment.

Subjects' responses to the three post-treatment questions across all treatment groups are summarized in Figure 23. Subjects reported that approximately 46% of other mTurkers would accept the HIT, but only 34% of subjects reported that they themselves would accept the HIT. So, while subjects thought the acceptance rate among other mTurkers is about 10 percentage points higher than what mTurkers actually did, the subjects' personal acceptance rate is identical to the acceptance rate observed in the experiment.

Importantly for our analysis, subjects reported a 69% likelihood that the requester would pay the bonus. Again, this suggests that subjects have some amount of uncertainty about being paid at the time they make their HIT-acceptance decisions and this is suggestive of statistical discrimination as a possible explanation for our results. However, Figure 24 shows that the sex of the requester has no bearing on workers' uncertainty about payment; estimates are both small and statistically indistinguishable from zero across all samples (full sample, treatment-salient sample and treatment non-salient sample). There is no statistical or economic evidence that the race of the requester affects the uncertainty of being paid in the full sample. However, this null result masks variation

within the salience sub-samples. On the one hand, race-salient subjects reported that they thought black requesters were about 5 (p-value 0.13) percentage points less likely to pay than white requesters. On the other hand, subjects for whom race was not salient reported a gap of only 2 (p-value 0.54) percentage points.

The fact that subjects thought both male and female employers are equally likely to pay suggests that the sex-gaps observed in our experiment are not driven by statistical discrimination. Sex-Salient subjects in the real-effort experiment where more likely to work for females while sex-nonsalient subjects were more likely to work for males despite the fact that both sexes are equally likely to pay for work. The results for race are similarly suggestive of an even stronger rejection of statistical discrimination. Notice that subjects in our salient-samples are more likely to work for the black employer despite the general impression among mTurkers that black employers are less likely to pay. Results for the non-salient sample are also suggestive of a taste-based effect. Subjects in this group were less likely to work for the black employer although the general impression among mTurkers is that there is no difference in likelihood of being paid.

We extend the analysis to account for the possibility of within-group bias by estimating the treatment effect separately for sex and race of the survey respondents, and reporting the results in Figures 25 and Figures 26 for race and sex, respectively. While there does not appear to be any statistical evidence for a difference in perceived likelihood of the employer paying among non-white subjects, we find that white subjects in the salient sample tend to believe that black employers are less likely to pay than white employers. These results further support the case for a strong taste-based source of bias; white workers in the salient sample preferred working for black employers despite the perception among their peers that black employers are less likely to pay. Similarly, non-white workers prefer black employers despite no apparent difference in perception of being paid.

We find no evidence of within-group bias in the gender treatments. Estimates are small and statistically indistinguishable from zero except in the non-salient sample where estimates are fairly large, but imprecisely estimated. Among subjects in the non-salient sample, female subjects report that female employers are about 6 percentage points less likely to pay than male employers, while male workers report that female employers are about 4 percentage points more likely to pay. These results are also inconsistent with statistical discrimination; males in the non-salient sample of the real-effort experiment were less likely to work for female employers despite the fact that their peers in the follow-up survey are of the view that female employers are more likely to pay than male employers.

Overall, the results from our mTurk user-survey are strongly suggestive that the sex and race gaps identified in our real-effort experiment are not driven by statistical

discrimination.

6 Conclusions

We estimate the effect of employers' race and sex on the willingness of workers to persist on a labor task using data generated on Amazon's Mechanical Turk. We find no evidence of discrimination against employers for neither sex nor race on the extensive-margin. However, these null results mask important heterogeneity by salience of treatment, which is suggestive of misreporting by those who do discriminate against minorities. Our results also point to discrimination on the intensive margin. First, workers were less accurate and transcribed fewer receipts for black employers, relative to white employers. Second, workers were significantly more accurate and tended to transcribe more receipts (though the latter is not significant) for female employers.

The fairly strong preference for female employers in our study is consistent with the general trend toward a preference for female bosses in Gallup polls on Work and Workplace.[23] Only 5% of participants expressed a preference for a female boss compared to 66% preference for a male boss in the 1953 Gallop Poll. By 2017, the Gallup Poll results showed that the share of participants who preferred a female boss increased to 21% while the share for male bosses fell to 23%. The findings are also in line with Elsesser and Lever (2011) who find that when rating one's own boss, respondents who have female managers do not rate them lower than respondents who have male managers. We acknowledge that working for a female boss is not the same as working for a female employer. However, these results do convey some information about changing attitudes toward female employers.

Results from a mTurk-user survey suggest that the biases we detect are not driven by statistical discrimination. Although subjects express some uncertainty about the likelihood of being paid for mTurk HITs, this uncertainty is not caused by the sex of the employer. Furthermore, the effect of race on the likelihood of being paid is not consistent with statistical discrimination. On the one hand, we find that subjects who prefer to work for black employers believe black employers are less likely to pay than white employers. On the other hand, those who prefer to work for white employers perceive no difference in likelihood of paying between black and white employers. Therefore, to the extent that the likelihood of being paid is the primary channel through which statistical discrimination would manifest itself in our setting, this finding suggest that the biases we estimate are not driven by statistical discrimination.

[23]The survey results can be found online at: https://news.gallup.com/poll/1720/work-work-place.aspx.

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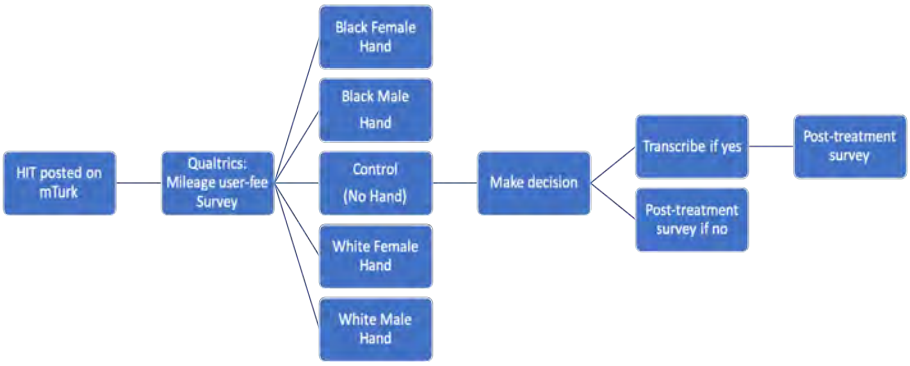
7 Tables and Figures

Figure 1: Experimental Design



Notes: Reported is the flow of the experiment. Subjects are recruited on Amazon's Mechanical Turk (mTurk) to complete a mileage userfee survey on Qualtrics. Subjects are randomly assigned to a treatment group where they are shown a picture of a hand holding a receipt and asked whether they would like to complete a transcription task. Subjects who respond yes transcribe images and then complete a post-experiment survey. Subjects who respond no complete the post-experiment survey.

Figure 2: Treatment Instructions

Thank you! You have earned $0.65 for completing my survey.

****below is an optional bonus opportunity****

I want to know how much I would pay in mileage tax compared to what I pay now for gasoline tax. To help me, I would like you to transcribe information from my gasoline receipts; this will allow me to estimate my annual gasoline taxes.

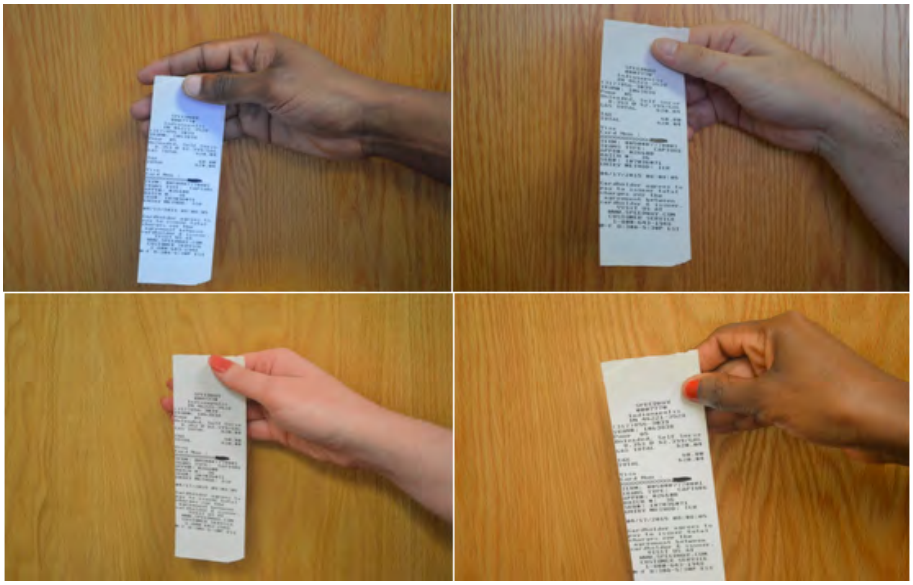
Each receipt should take approximately 30 seconds to transcribe, and I will pay you a bonus of $0.06 for every receipt that you transcribe. You can stop at anytime.

I have included a sample of one of my receipts above. I would like you to transcribe the following information:

1. *Name of the gas station*
2. *Date of the purchase*
3. *Gallons of gasoline purchased*
4. *Price per gallon*
5. *Total sale price*

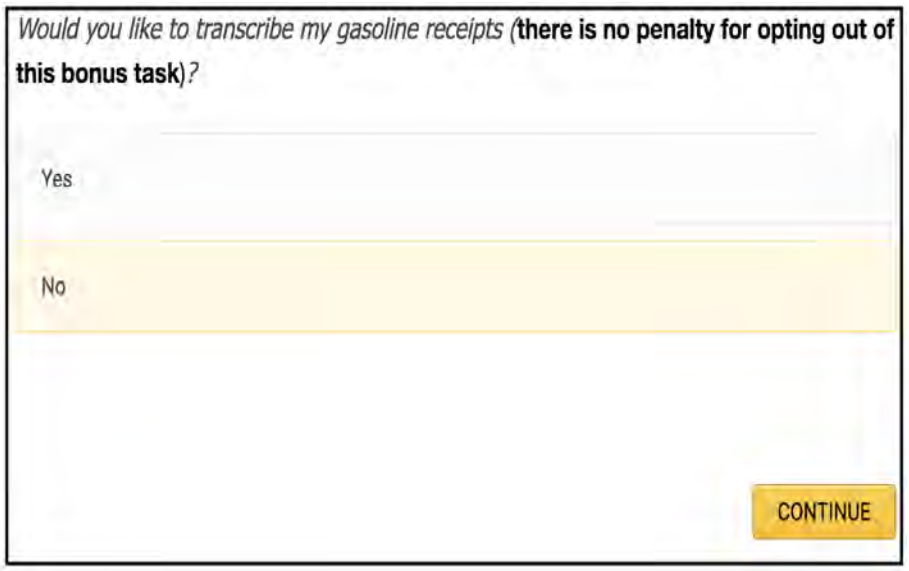
Notes: Reported are the instructions for the bonus transcription task.

Figure 3: Treatment Pictures



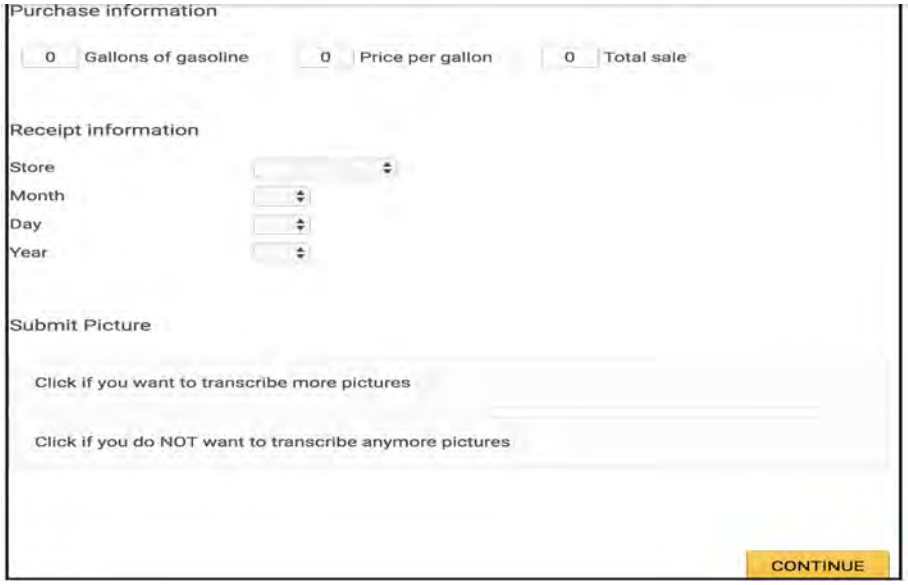
Notes: Reported are the pictures used in the treatment stage of the experiment. The pictures have been compressed significantly to fit side-by-side on one page.

Figure 4: Treatment Question



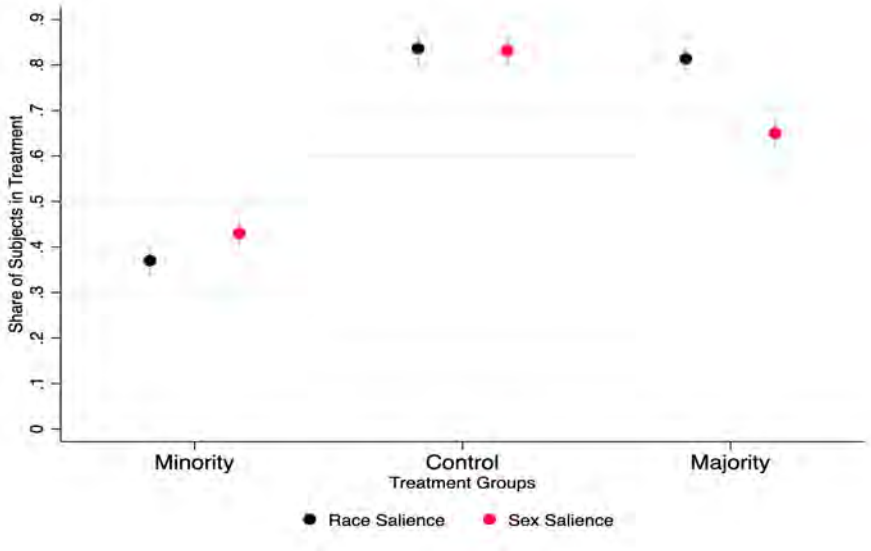
Notes: Reported is the question that subjects saw after the treatment instructions.

Figure 5: Treatment Task



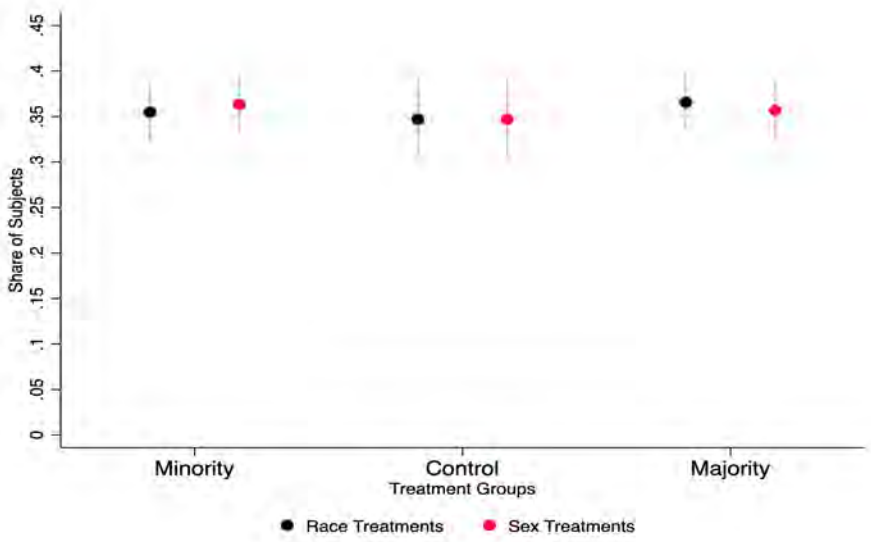
Notes: Reported is the data entry screen that subjects used wen transcribing data from the gasoline receipts.

Figure 6: Salience of race and sex by Treatment Groups



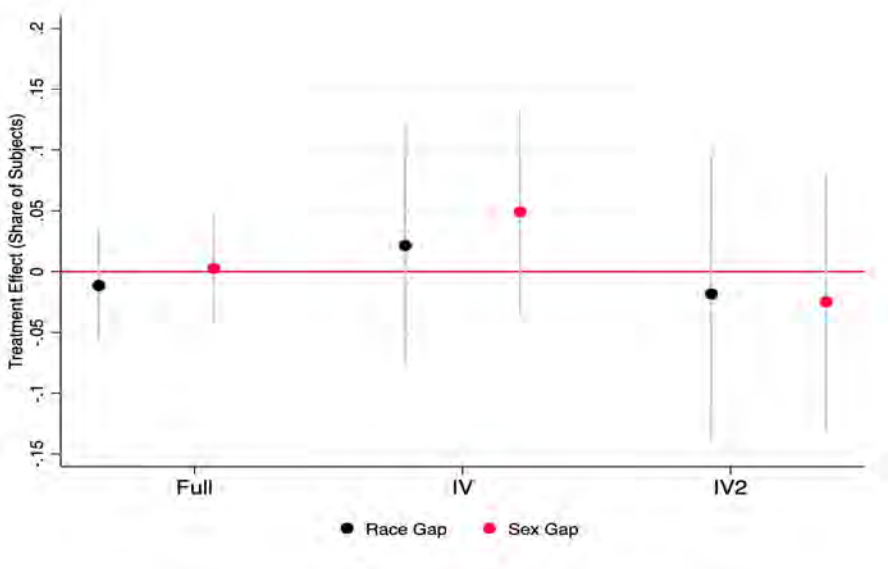
Notes: Reported is the share of subjects whose self-reported perceived treatment matches the actual treatment they are assigned to for race and sex, respectively, along with 95% confidence intervals. Minority refers to Black-hand and female-hand treatments, while majority refers to white-hand and male-hand treatments.

Figure 7: Acceptance Share: by Treatment Group



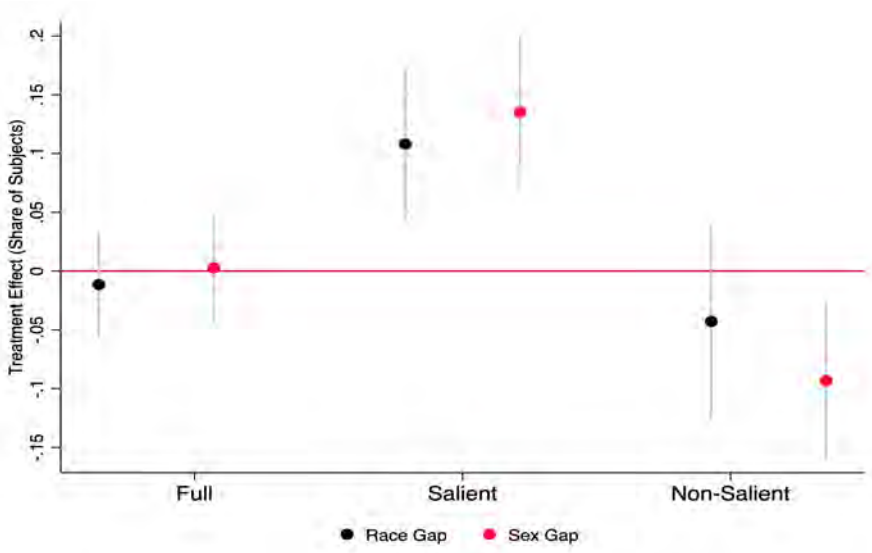
Notes: Reported is the acceptance share by treatment group for race and sex, along with 95% confidence intervals. Acceptance share refers to the share of subjects who agreed to transcribe receipts. Minority refers to Black-hand and female-hand treatments, while majority refers to white-hand and male-hand treatments.

Figure 8: Treatment effect of race and sex on acceptance share



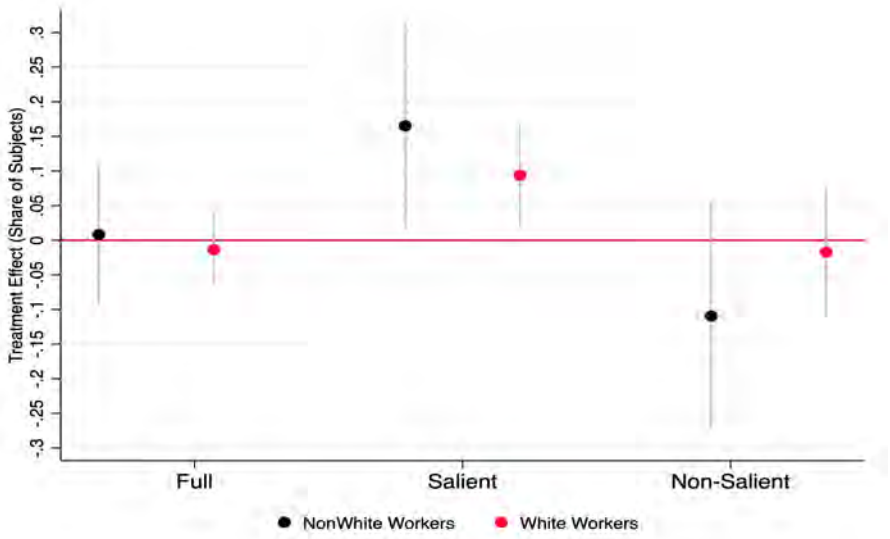
Notes: Reported is the race-gap and sex-gap (with 95% confidence intervals) in the share of subjects who agreed to transcribe pictures. 'Full' shows results from a linear probability model that estimates the effect of actual treatment on the decision to transcribe. IV and IV1 are 2-stage least squares estimates to the effect of perceived treatment on the decision to transcribe, where perceived treatment is instrumented by actual treatment. Perceived treatment is based on subjects' responses to post-treatment questions about the hand in the picture. IV defines perceived treatment as 1 if subjects respond that they saw a hand from a minority group (black/female) and 0 if majority group (white/male). IV1 defines perceived treatment as 1 if subjects respond that they saw a hand from a minority group (black/female) and 0 otherwise (white/male, I don't know). The racial-gap is defined as the difference in acceptance share between the black-hand treatment and the white-hand treatment. The sex-gap is defined as difference in acceptance share between the female-hand treatment and the male-hand treatment.

Figure 9: Treatment effect of race and sex on acceptance share, by Treatment-Salience



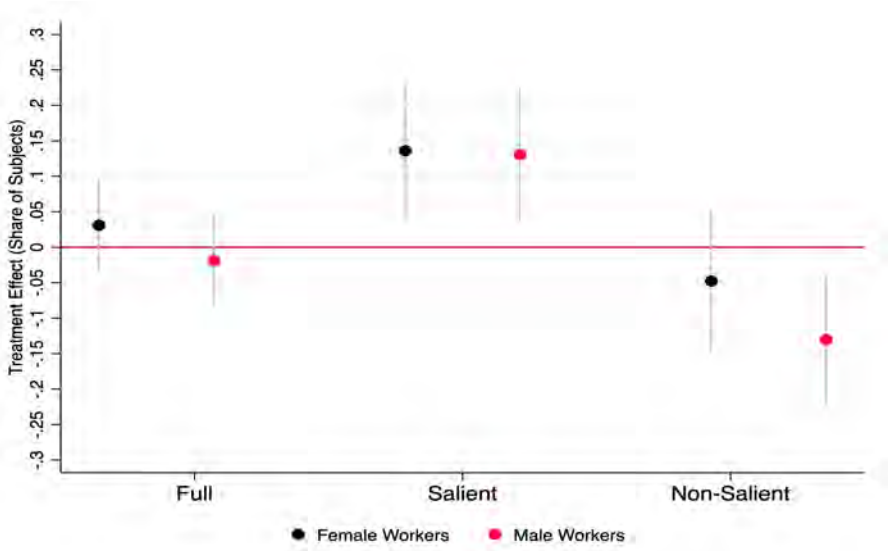
Notes: Reported is the race-gap and sex-gap (with 95% confidence intervals) in the share of subjects who agreed to transcribe pictures among the full sample (Full) and two salience samples. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The racial-gap is defined as difference in acceptance share between the black-hand treatment and the white-hand treatment. The sex-gap is defined as difference in acceptance share between the female-hand treatment and the male-hand treatment.

Figure 10: Treatment effect of race on acceptance share: within-group racial-gap



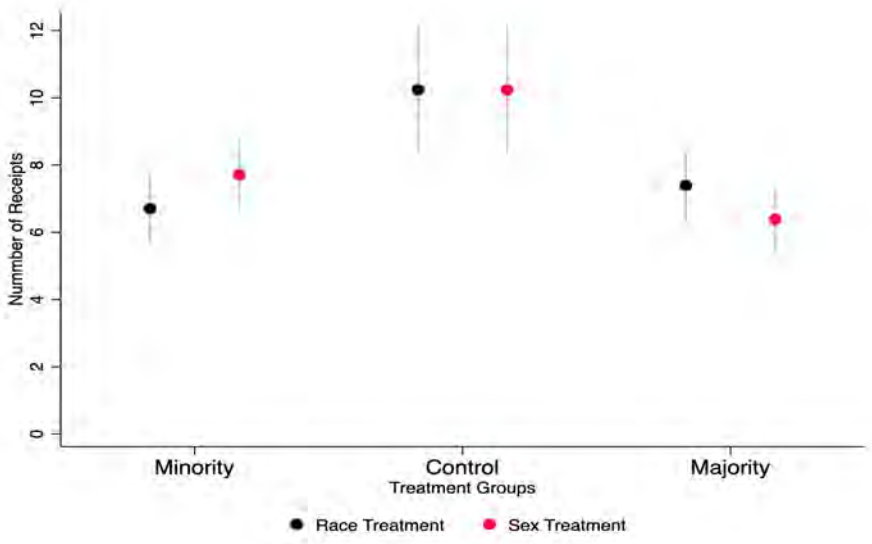
Notes: Reported is the within-group racial-gap (with 95% confidence intervals) in the share of subjects who agreed to transcribe pictures among the full sample (Full) and two salience samples. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The racial-gap is defined as difference in acceptance share between the black-hand treatment and the white-hand treatment.

Figure 11: Treatment effect of sex on acceptance share: within-group sex-gap



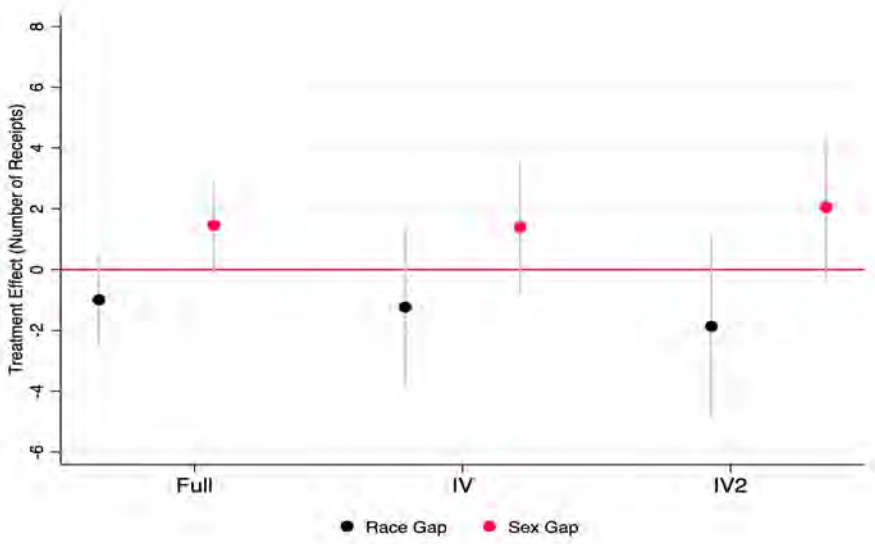
Notes: Reported is the within-group sex-gap (with 95% confidence intervals) in the share of subjects who agreed to transcribe pictures among the full sample (Full) and two salience samples. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The sex-gap is defined as difference in acceptance share between the female-hand treatment and the male-hand treatment.

Figure 12: Mean number of transcribed receipts: by Treatment Group



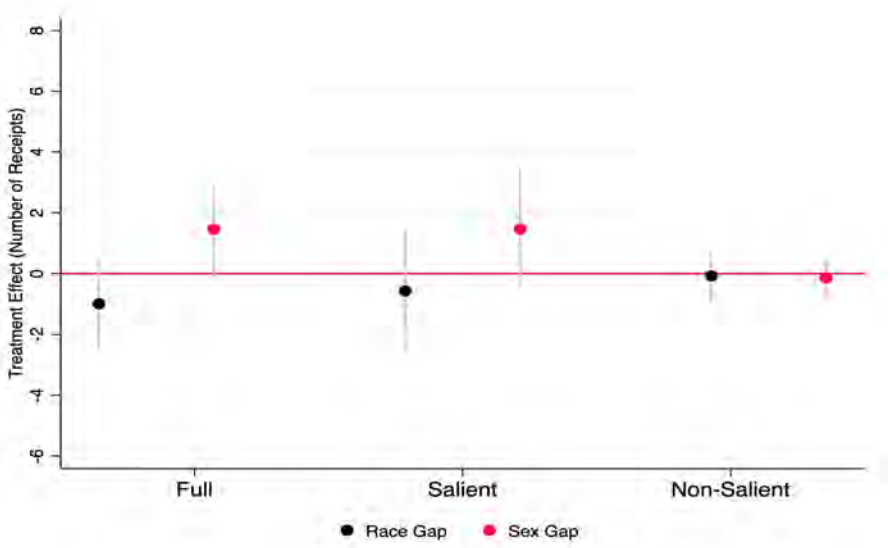
Notes: Reported is the mean number of receipts transcribed by treatment group for race and sex, along with 95% confidence intervals. Minority refers to Black-hand and female-hand treatments, while majority refers to white-hand and male-hand treatments.

Figure 13: Treatment effect of race and sex on number of transcribed receipts



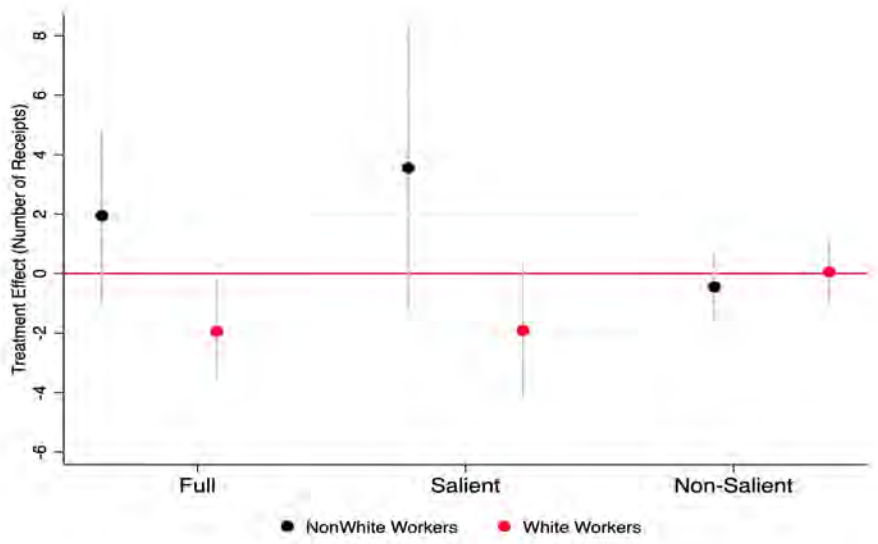
Notes: Reported is the race-gap and sex-gap (with 95% confidence intervals) in the mean number of receipts transcribed by subjects. 'Full' shows results from a linear probability model that estimates the effect of actual treatment on the mean number of receipts transcribed by subjects. IV and IV1 are 2-stage least squares estimates to the effect of perceived treatment on the mean number of receipts transcribed by subjects, where perceived treatment is instrumented by actual treatment. Perceived treatment is based on subjects' responses to post-treatment questions about the hand in the picture. IV defines perceived treatment as 1 if subjects respond that they saw a hand from a minority group (black/female) and 0 if majority group (white/male). IV1 defines perceived treatment as 1 if subjects respond that they saw a hand from a minority group (black/female) and 0 otherwise (white/male, I don't know). The racial-gap is defined as difference in acceptance share between the black-hand treatment and the white-hand treatment. The sex-gap is defined as difference in acceptance share between the female-hand treatment and the male-hand treatment.

Figure 14: Treatment effect of race and sex on number of transcribed receipts, by Treatment-Salience



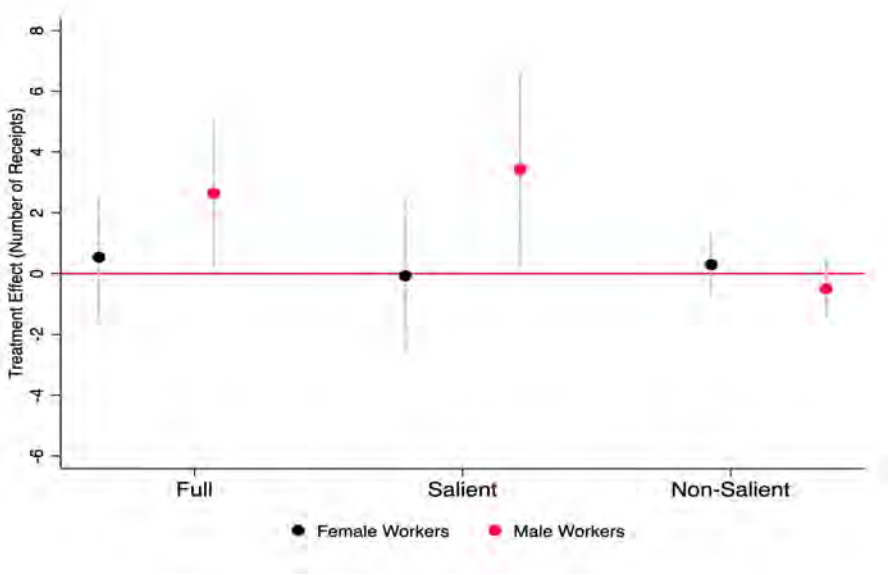
Notes: Reported is the race-gap and sex-gap (with 95% confidence intervals) in the mean number of receipts transcribed by subjects in the full sample (Full) and two salience samples who transcribed at least one receipt. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The racial-gap is defined as difference in acceptance share between the black-hand treatment and the white-hand treatment. The sex-gap is defined as difference in acceptance share between the female-hand treatment and the male-hand treatment.

Figure 15: Treatment effect of race on number of transcribed receipts: within-group race-gap



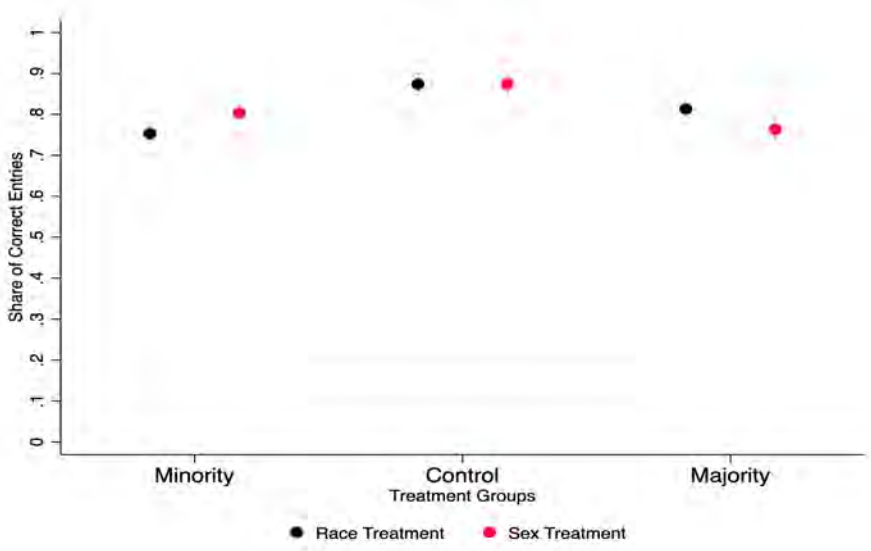
Notes: Reported is the within-group race-gap (with 95% confidence intervals) in the mean number of receipts transcribed by subjects in the full sample (Full) and two salience samples who transcribed at least one receipt. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The racial-gap is defined as difference in acceptance share between the black-hand treatment and the white-hand treatment.

Figure 16: Treatment effect of sex on number of transcribed receipts: within-group sex-gap



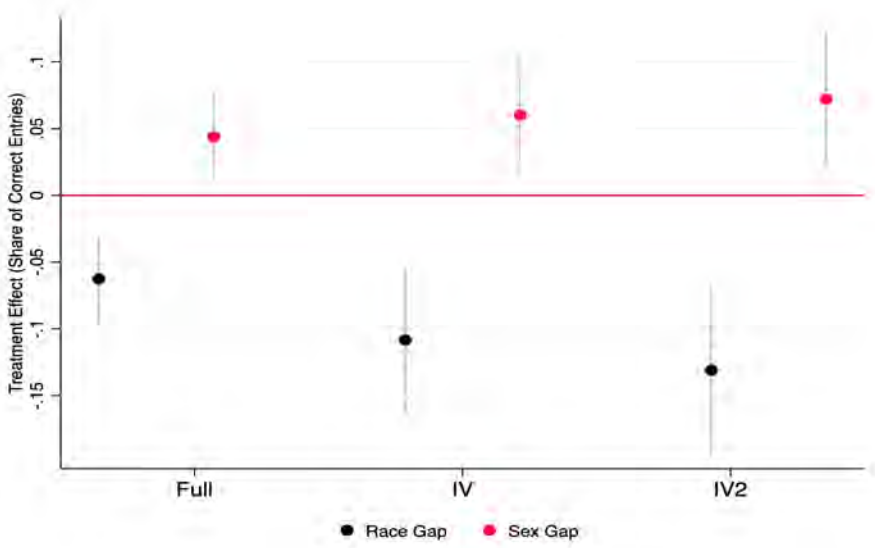
Notes: Reported is the within-group sex-gap (with 95% confidence intervals) in the mean number of receipts transcribed by subjects in the full sample (Full) and two salience samples who transcribed at least one receipt. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The sex-gap is defined as difference in acceptance share between the female-hand treatment and the male-hand treatment.

Figure 17: Accuracy rate by race and sex



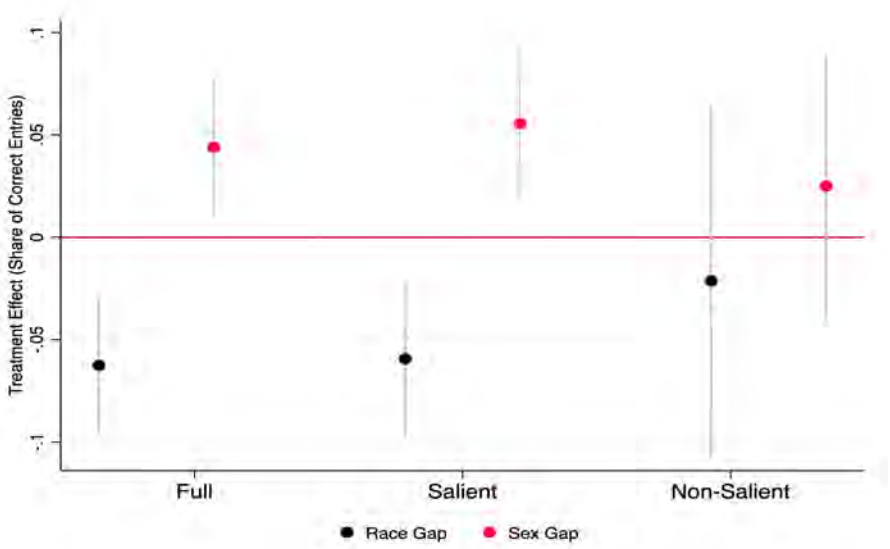
Notes: Reported is the share of accurate transcriptions across treatment groups, along with 95% confidence intervals. Minority refers to Black-hand and female-hand treatments, while majority refers to white-hand and male-hand treatments.

Figure 18: Treatment effect of race and sex on accuracy rate



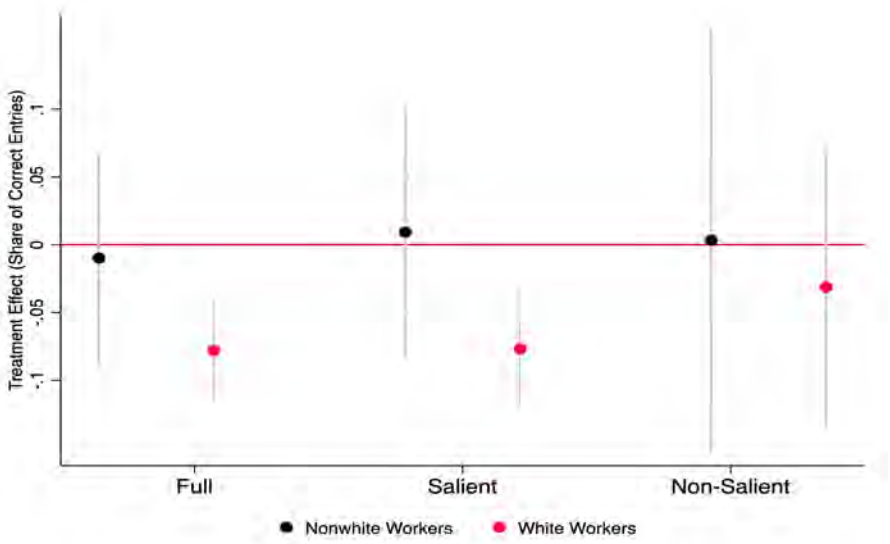
Notes: Reported is the race-gap and sex-gap (with 95% confidence intervals) in the share of correct entries by subjects. 'Full' shows results from a linear probability model that estimates the effect of actual treatment on the share of accurate entries. IV and IV1 are 2-stage least squares estimates to the effect of perceived treatment on the share of accurate entries, where perceived treatment is instrumented by actual treatment. Perceived treatment is based on subjects' responses to post-treatment questions about the hand in the picture. IV defines perceived treatment as 1 if subjects respond that they saw a hand from a minority group (black/female) and 0 if majority group (white/male). IV1 defines perceived treatment as 1 if subjects respond that they saw a hand from a minority group (black/female) and 0 otherwise (white/male, I don't know). The racial-gap is defined as the difference in share of correct entries between the black-hand treatment and the white-hand treatment. The sex-gap is defined as the difference in share of correct entries between the female-hand treatment and the male-hand treatment.

Figure 19: Treatment effect of race and sex on on accuracy rate, by Treatment-Salience



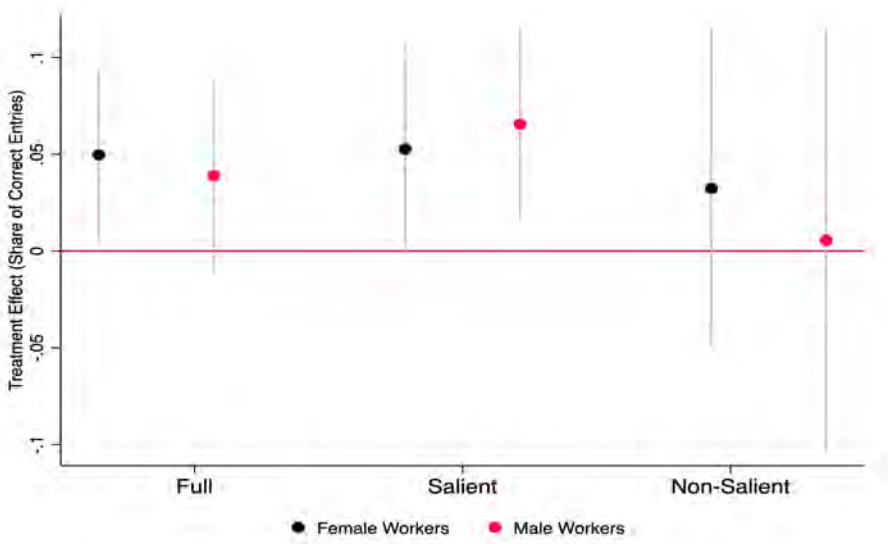
Notes: Reported is the racial-gap and sex-gap (with 95% confidence intervals) in the share of correct entries by subjects in the full sample (Full) and two salience samples who transcribed at least one receipt. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The racial-gap is defined as the difference in share of correct entries between the black-hand treatment and the white-hand treatment. The sex-gap is defined as the difference in share of correct entries between the female-hand treatment and the male-hand treatment.

Figure 20: Treatment effect of race on on accuracy rate: within-group racial-gap



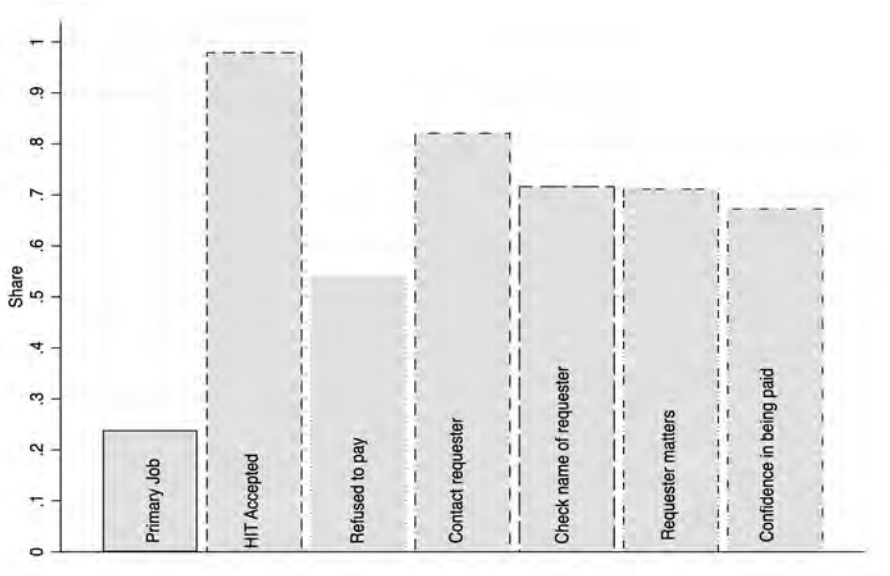
Notes: Reported is the within-group racial-gap (with 95% confidence intervals) in the share of correct entries by subjects in the full sample (Full) and two salience samples who transcribed at least one receipt. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The racial-gap is defined as the difference in share of correct entries between the black-hand treatment and the white-hand treatment.

Figure 21: Treatment effect of sex on on accuracy rate: within-group sex-gap



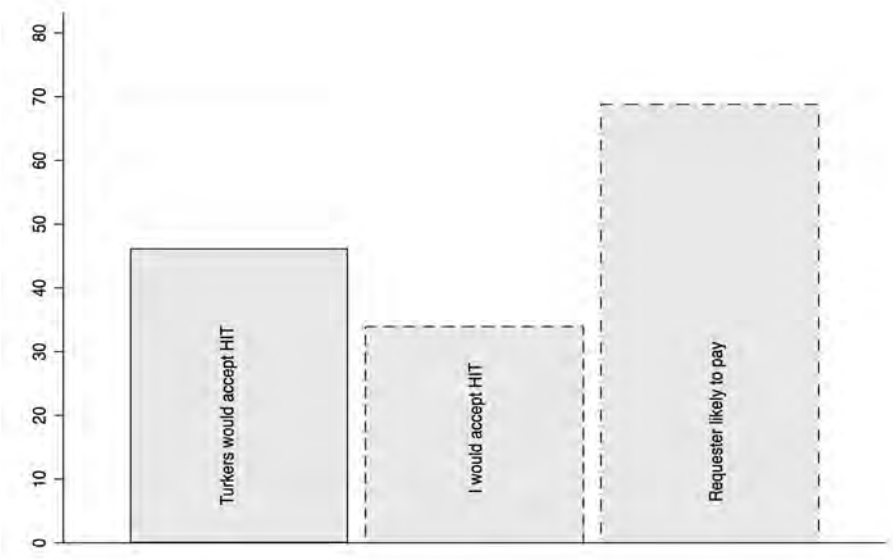
Notes: Reported is the within-group sex-gap (with 95% confidence intervals) in the share of correct entries by subjects in the full sample (Full) and two salience samples who transcribed at least one receipt. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The sex-gap is defined as the difference in share of correct entries between the female-hand treatment and the male-hand treatment.

Figure 22: mTurk Experience Survey



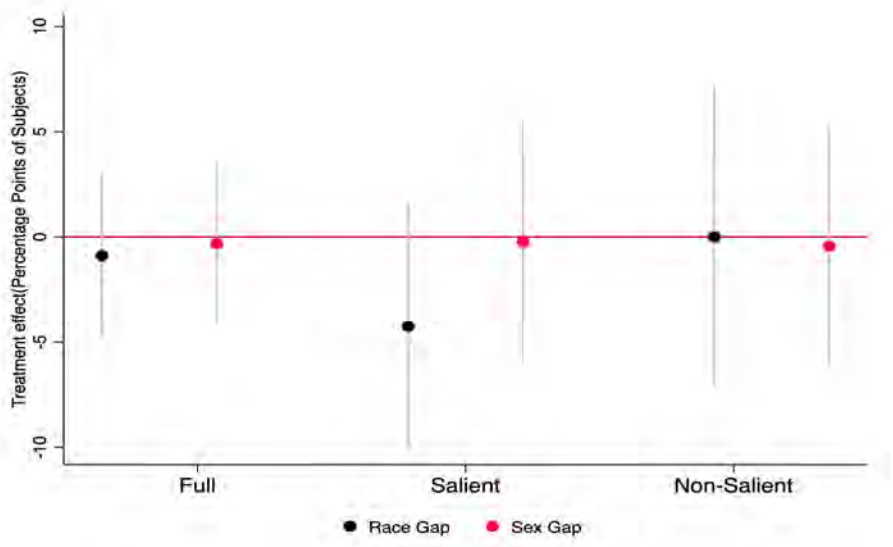
Notes: Reported is the share of mTurkers who answered "yes" to questions regarding their experiences as mTurk workers, including "*Is mTurk your primary job?*", "*Is your work often accepted?*", "*Have you experienced requesters' refusal to pay for work completed?*", "*Have you contacted requesters before?*", "*Do you often pay attention to the requester's name?*", and "*Will a requester's characteristics affect your likelihood of accepting a HIT?*". the question "*how confident are you that a requester will pay for a bonus task*" is measured a scale from 0 to 100. The variable was transformed to a 0 to 1 scale and mean confidence level is reported.

Figure 23: mTurk Experience Survey



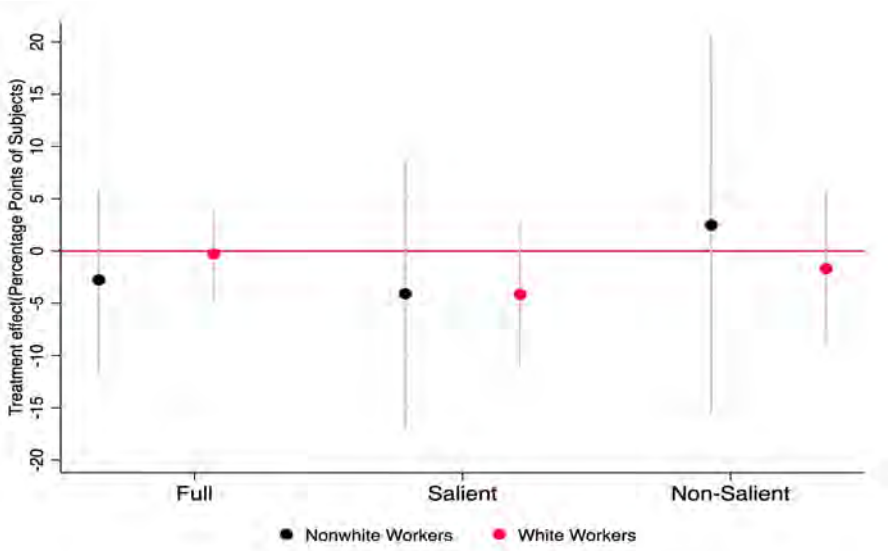
Notes: Reported is subjects' belief about the percentage of other mTurkers who would accept the hypothetical task, the percentage of subjects who would accept the task themselves, and subjects' confidence level that the requester of the task would pay the stated bonus.

Figure 24: Requester will pay bonus - Treatment Effects



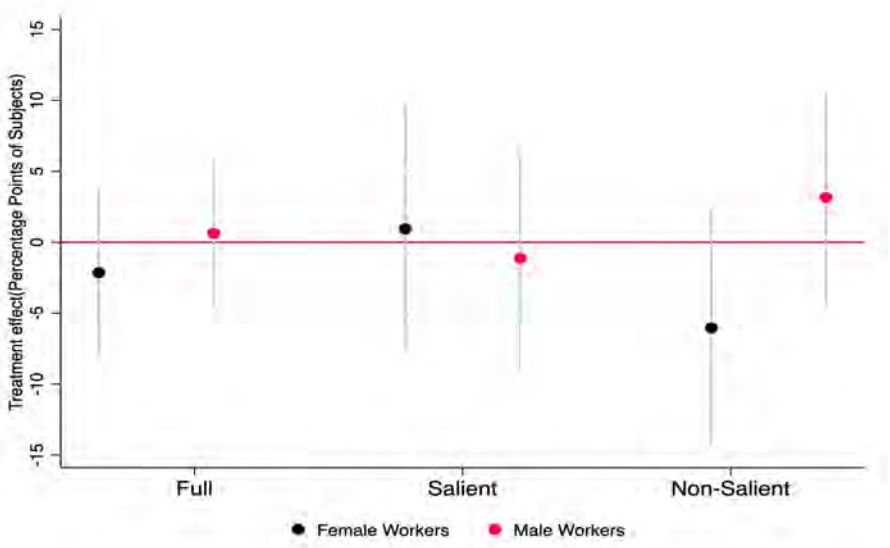
Notes: Reported is the racial-gap and sex-gap (with 95% confidence intervals) in subjects' confidence level that the requester of the task would pay the stated bonus by subjects in the full sample (Full) and two salience samples. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The racial-gap is defined as the difference in share of correct entries between the black-hand treatment and the white-hand treatment. The sex-gap is defined as the difference in share of correct entries between the female-hand treatment and the male-hand treatment.

Figure 25: Requester will pay bonus - Treatment Effects by Subjects' Race



Notes: Reported is the within-group racial-gap (with 95% confidence intervals) in subjects' confidence level that the requester of the task would pay the stated bonus by subjects in the full sample (Full) and two salience samples. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The racial-gap is defined as the difference in share of correct entries between the black-hand treatment and the white-hand treatment.

Figure 26: Requester will pay bonus - Treatment Effects by Subjects' Sex



Notes: Reported is the within-group sex-gap (with 95% confidence intervals) in subjects' confidence level that the requester of the task would pay the stated bonus by subjects in the full sample (Full) and two salience samples. Salient includes only subjects who correctly identified the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications and Non-Salient includes subjects who did not correctly identify the race of the employer for the race-gap specifications and sex of the employer for the sex-gap specifications. The sex-gap is defined as the difference in perception between the female-hand treatments and male-hand treatments.

Table 1: Summary Statistics of Covariates

Variables	Black	White	No Pic	Female	Male	Total	ACS (2018)
Age	37.46	37.49	36.64	37.80	37.15	37.31	38.2
White	0.80	0.77	0.76	0.79	0.79	0.78	0.75
Sex	0.50	0.49	0.43	0.51	0.48	0.48	0.51
Urban	0.50	0.54	0.49	0.53	0.51	0.52	0.80
Duration (mins)	7.11	7.48	7.68	7.25	7.34	7.37	
High School	0.11	0.09	0.11	0.08	0.11	0.10	0.27
Some College	0.25	0.22	0.23	0.25	0.22	0.23	0.20
2–Year College	0.11	0.12	0.10	0.12	0.12	0.11	0.09
B.Sc.	0.38	0.42	0.41	0.38	0.42	0.40	0.20
Graduate	0.16	0.15	0.16	0.18	0.13	0.16	0.13
N. Obs.	843	848	415	842	849	2106	

Notes: Reported is the mean of each variable by treatment group. We combine data by race and sex. 'No Pic' is the control group. ACS is American Community Survey. The ACS is an annual nationwide survey of approximately 3.5 million households.

Table 2: Balancedness test

	Black v Control		White vs Control		Female V Control		Male V Control		W vs B	F vs M
Demographic	Difference	Pvalue	Difference	Pvalue	Difference	Pvalue	Difference	Pvalue	Pvalue	Pvalue
Age	0.825	0.378	0.850	0.270	1.163	0.120	0.515	0.664	0.834	0.203
White	0.048	0.052	0.011	0.664	0.028	0.256	0.030	0.226	0.067	0.929
Sex	0.069	0.021	0.053	0.075	0.072	0.016	0.050	0.092	0.512	0.369
Urban	0.012	0.702	0.049	0.104	0.041	0.170	0.019	0.522	0.126	0.365
Duration (Mins)	-0.572	0.943	-0.203	0.920	-0.430	0.704	-0.344	0.685	0.780	0.340
High School	0.001	0.968	-0.019	0.282	-0.024	0.161	0.006	0.754	0.176	0.038
Some College	0.017	0.516	-0.007	0.773	0.018	0.480	-0.009	0.729	0.247	0.193
2-Year College	0.009	0.623	0.023	0.239	0.015	0.421	0.017	0.381	0.388	0.929
B.Sc.	-0.030	0.304	0.009	0.759	-0.031	0.293	0.010	0.742	0.101	0.089
Graduate	0.004	0.873	-0.006	0.792	0.022	0.341	-0.024	0.259	0.602	0.011

Notes: Reported is the difference in mean between treatment and control groups for each variable. Pvalues from a ranksum test of the differences in means between treatment groups are also reported. W vs B is white compared to black, and F vs M is female compared to male.

Table 3: Characteristics of Salient and Non-salient samples

	Race		Sex		Full
	Non-Salient	Salient	Non-Salient	Salient	sample
Age	39.16	36.28	38.52	36.50	37.31
White	0.78	0.78	0.78	0.78	0.78
Sex	0.49	0.48	0.49	0.48	0.48
Urban	0.50	0.53	0.51	0.52	0.52
Duration	6.42	7.91	6.40	8.03	7.37
High School	0.11	0.09	0.10	0.10	0.10
Some College	0.20	0.25	0.23	0.24	0.23
2-Year College	0.12	0.11	0.12	0.11	0.11
B.Sc.	0.39	0.40	0.37	0.42	0.40
Graduate	0.18	0.14	0.18	0.14	0.16

Notes: Reported is the mean of each variable for the salient and non-salient samples. A subject is in the race-salient sample if her self-reported perceived race treatment matches her assigned race treatment and the non-salient sample otherwise. A subject is in the sex-salient sample if her self-reported perceived sex treatment matches her assigned sex treatment and the non-salient sample otherwise.

Table 4: Summary statistics: mTurk worker experience survey

Variables	Black	White	No Pic	Female	Male
Age Group:					
18 to 24	0.13	0.12	0.13	0.14	0.10
25 to 34	0.45	0.42	0.44	0.40	0.48
35 to 44	0.22	0.26	0.27	0.24	0.24
45 to 54	0.13	0.12	0.10	0.13	0.13
55 to 64	0.05	0.06	0.06	0.06	0.05
>65	0.01	0.02	0.01	0.02	0.01
White	0.76	0.76	0.72	0.76	0.77
Sex	0.45	0.48	0.49	0.46	0.47
Duration (mins)	4.65	4.60	4.39	4.57	4.68
High School	0.10	0.12	0.10	0.12	0.10
Some College	0.27	0.21	0.23	0.23	0.24
2-Year College	0.12	0.13	0.11	0.12	0.13
B.Sc.	0.38	0.39	0.42	0.38	0.39
Graduate	0.13	0.16	0.14	0.15	0.14
N. Obs.	347	353	166	348	352

Notes: Reported is the mean of each variable from the mTurk worker experience survey. We combine data by race and sex. 'No Pic' is the control group.

Table 5: Balancedness test: mTurk worker experience survey

Variables	Male v Control	Female control	White vs Control	Black vs Control	Female v Male	White vs Black
18 to 24	0.411	0.598	0.807	0.993	0.095	0.753
25 to 34	0.425	0.386	0.750	0.787	0.038	0.463
35 to 44	0.426	0.513	0.748	0.251	0.862	0.308
45 to 54	0.343	0.281	0.395	0.240	0.864	0.670
55 to 64	0.569	0.896	0.973	0.697	0.390	0.661
¿65	0.762	0.228	0.313	0.553	0.197	0.542
White	0.246	0.425	0.302	0.355	0.660	0.899
Sex	0.592	0.468	0.701	0.378	0.812	0.534
Duration	0.712	0.885	0.949	0.870	0.537	0.830
High School	0.994	0.416	0.502	0.874	0.306	0.516
Some College	0.816	0.956	0.564	0.464	0.720	0.102
2-Year College	0.603	0.912	0.674	0.830	0.606	0.796
B.Sc.	0.525	0.468	0.509	0.483	0.910	0.957
Graduate	0.915	0.809	0.553	0.782	0.865	0.276

Notes: Reported is the difference in mean between treatment and control groups for each variable. Pvalues from a ranksum test of the differences in means between treatment groups are also reported. W vs B is white compared to black, and F vs M is female compared to male.

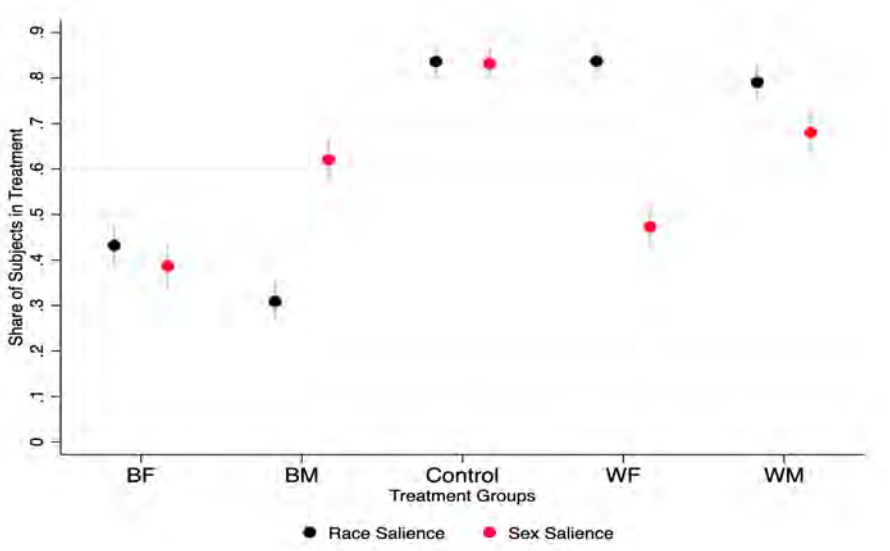
Table 6: Salience of Treatment in Experiment and Survey

	Race Salient		Sex Salient	
Treatment	Experiment	Survey	Experiment	Survey
BF	0.43	0.41	0.39	0.41
	419	185	419	185
BM	0.31	0.32	0.62	0.55
	424	192	424	192
Control	0.84	0.88	0.83	0.88
	415	190	415	190
WF	0.84	0.84	0.47	0.47
	423	200	423	200
WM	0.79	0.78	0.68	0.62
	425	188	425	188
Total	0.64	0.65	0.60	0.59
	2106	955	2106	955

Notes: Reported is the share of subjects whose self-reported perceived treatment matches the actual treatment they are assigned for race and sex, respectively, in the real-effort experiment and the mTurk user-survey. A subject is in the race-salient sample if her self-reported perceived race treatment matches her assigned race treatment and the non-salient sample otherwise. A subject is in the sex-salient sample if her self-reported perceived sex treatment matches her assigned sex treatment and the non-salient sample otherwise. BF includes subjects who were assigned to the black female hand treatment; BM includes subjects who were assigned to the black male hand treatment; Control includes subjects who were assigned to the control group; WF includes subjects who were assigned to the white female hand treatment; and WM includes subjects who were assigned to the white male hand treatment.

8 Appendix

Figure 27: Race salience by detailed treatment group



Notes: Reported is the share of race-salient and sex-salient subjects by treatment group. Race-salience includes only subjects who correctly identified the race of the employer and sex-salience includes only subjects who correctly identified the sex of the employer. BF includes subjects who were assigned to the black female hand treatment; BM includes subjects who were assigned to the black male hand treatment; Control includes subjects who were assigned to the control group; WF includes subjects who were assigned to the white female hand treatment; and WM includes subjects who were assigned to the white male hand treatment.

Table 7: Summary Statistics by Treatment Group

Variables	BF	BM	Control	WF	WM	Total
Age	38.224	36.712	36.639	37.383	37.594	37.311
White	0.809	0.800	0.757	0.761	0.774	0.780
Sex	0.516	0.491	0.434	0.496	0.478	0.483
Urban	0.536	0.470	0.491	0.530	0.551	0.516
Duration (mins)	6.793	7.429	7.684	7.712	7.252	7.374
High School	0.081	0.132	0.106	0.083	0.092	0.099
Some College	0.265	0.226	0.229	0.229	0.214	0.233
2–Year College	0.117	0.104	0.101	0.116	0.132	0.114
B.Sc.	0.346	0.408	0.407	0.407	0.426	0.399
Graduate	0.191	0.130	0.157	0.165	0.136	0.156
N. Obs.	419	424	415	423	425	2106

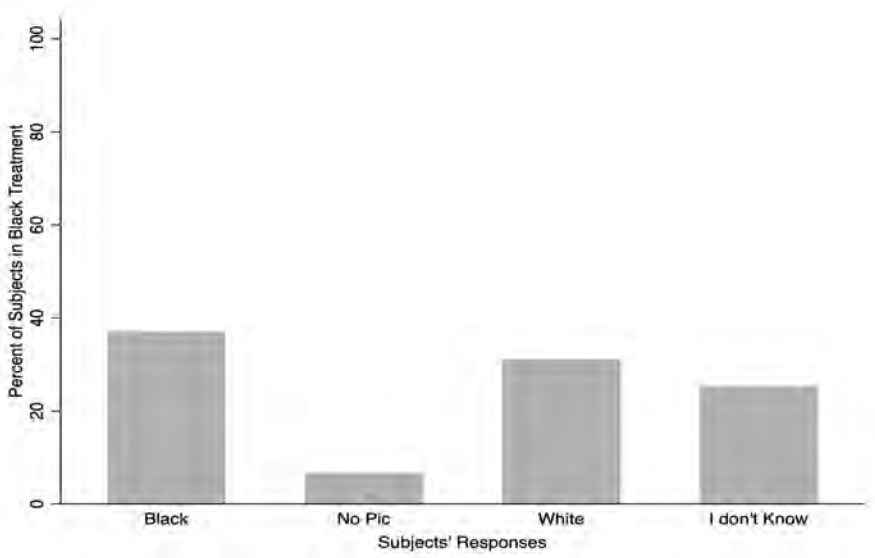
Notes: Reported is the mean of each variable by treatment group. BF is black female hand, BM is black male, WF is white female and WM is white male.

Table 8: Summary Statistics for mTurk worker experience survey

Variables	BF	BM	Control	WF	WM	Total
18 to 24	0.17	0.09	0.13	0.12	0.11	0.12
25 to 34	0.36	0.54	0.44	0.44	0.41	0.44
35 to 44	0.24	0.21	0.27	0.25	0.27	0.25
45 to 54	0.15	0.11	0.10	0.11	0.14	0.12
55 to 64	0.06	0.04	0.06	0.07	0.05	0.06
>65	0.02	0.01	0.01	0.02	0.01	0.01
White	0.75	0.77	0.72	0.76	0.77	0.76
Sex	0.41	0.49	0.49	0.51	0.44	0.47
Duration	4.78	4.53	4.39	4.38	4.83	4.58
High School	0.10	0.10	0.10	0.14	0.09	0.11
Some College	0.27	0.26	0.23	0.20	0.22	0.24
2-Year College	0.10	0.14	0.11	0.13	0.12	0.12
B.Sc.	0.41	0.35	0.42	0.35	0.42	0.39
Graduate	0.12	0.14	0.14	0.17	0.14	0.14
	169	178	166	179	174	866

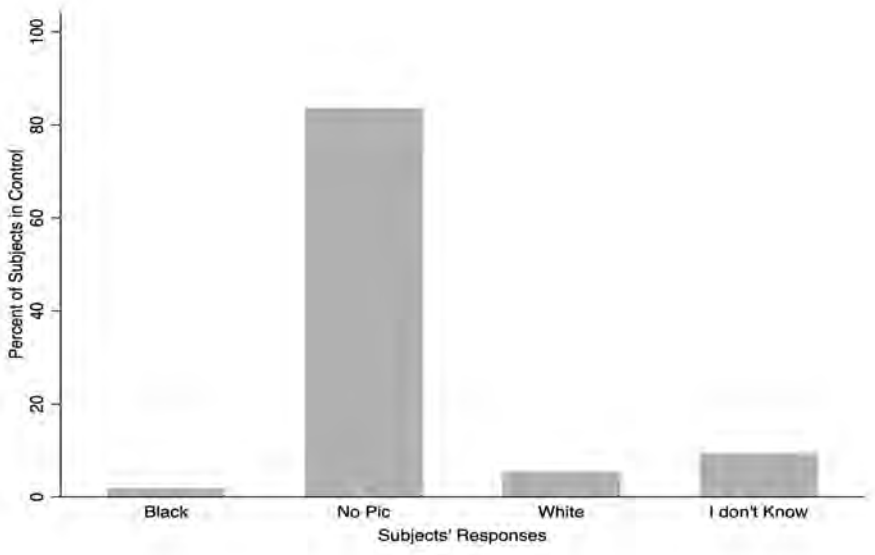
Notes: Reported is the mean of each variable by treatment group. BF is black female hand, BM is black male, WF is white female and WM is white male.

Figure 28: Race Salience in the Black Treatment Group



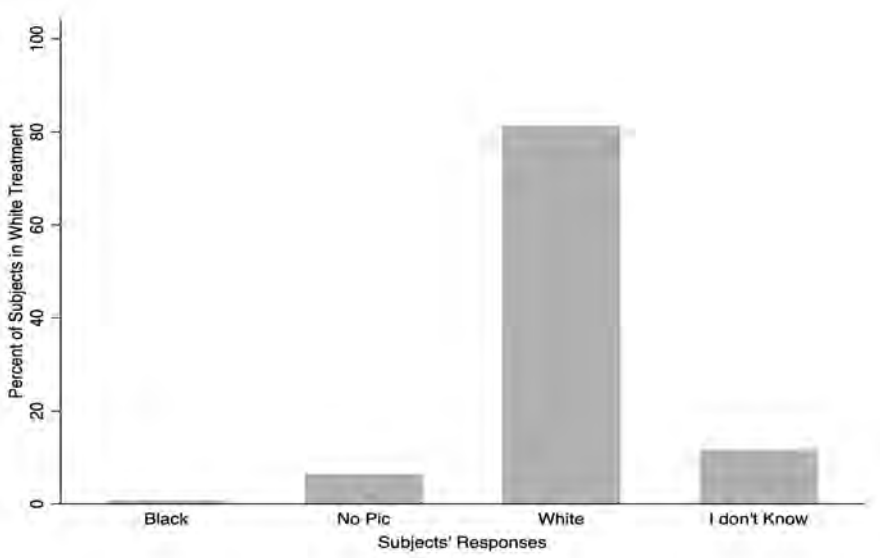
Notes: Reported is the percent of subjects in the black treatment who gave each of the possible responses to the question: "What is the race of the person holding the receipt in the picture?".

Figure 29: Race Salience in the Control Group



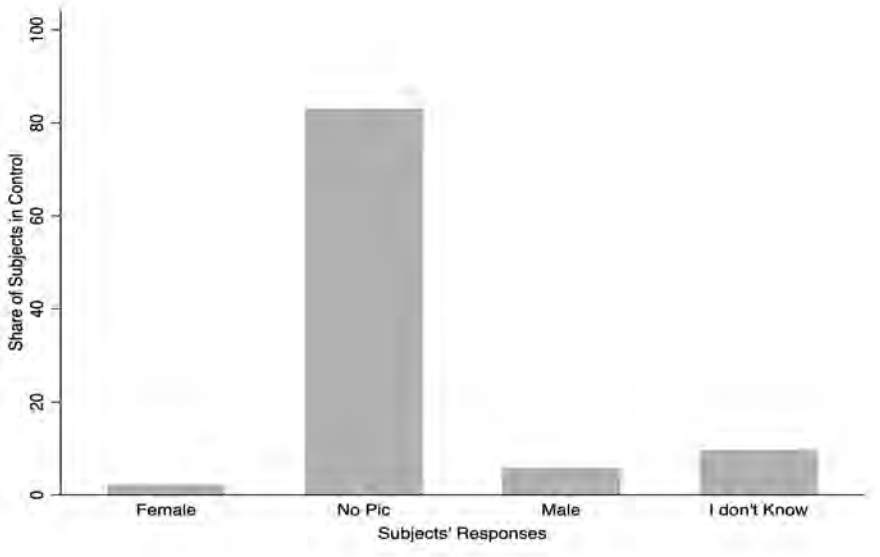
Notes: Reported is the percent of subjects in the control group who gave each of the possible responses to the question: "What is the race of the person holding the receipt in the picture?".

Figure 30: Race Salience in the White Treatment Group



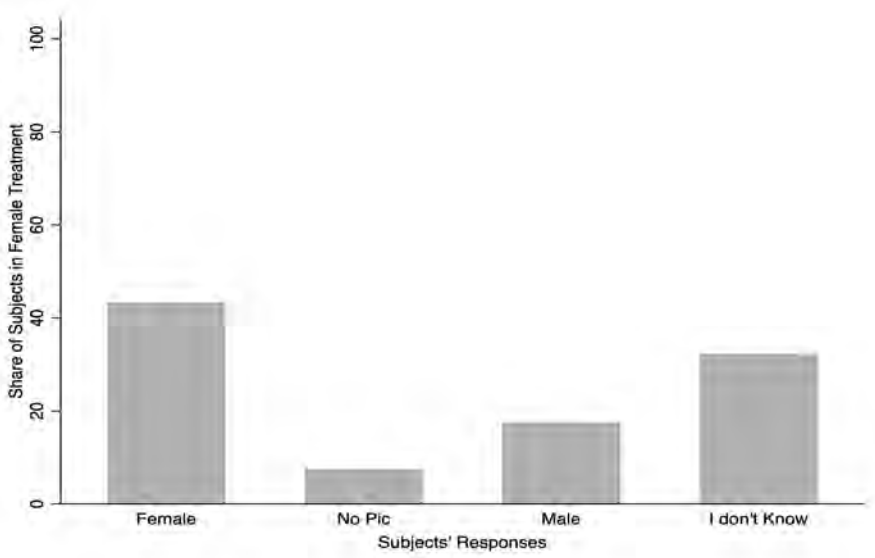
Notes: Reported is the percent of subjects in the white treatment who gave each of the possible responses to the question: "What is the race of the person holding the receipt in the picture?".

Figure 31: Sex Salience in the Control Group



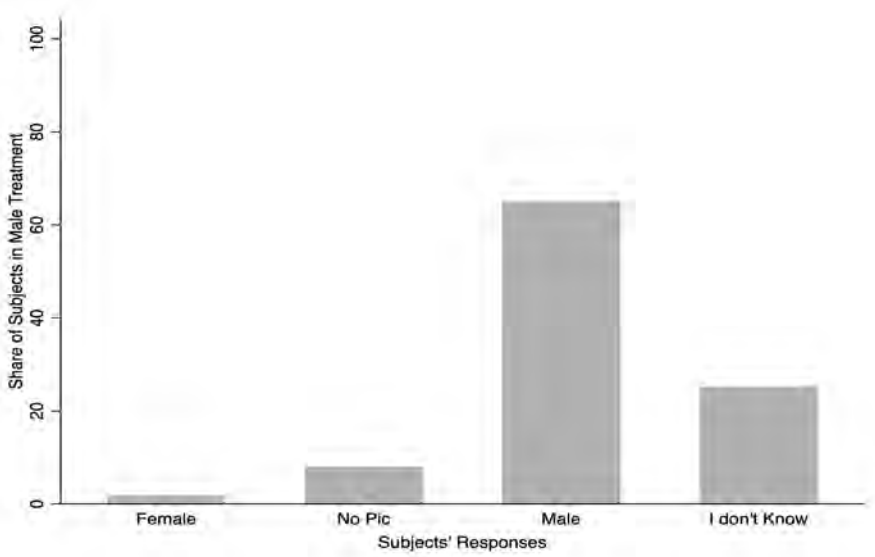
Notes: Reported is the percent of subjects in the control group who gave each of the possible responses to the question: "What is the sex of the person holding the receipt in the picture?".

Figure 32: Sex Salience in the Female Treatment Group



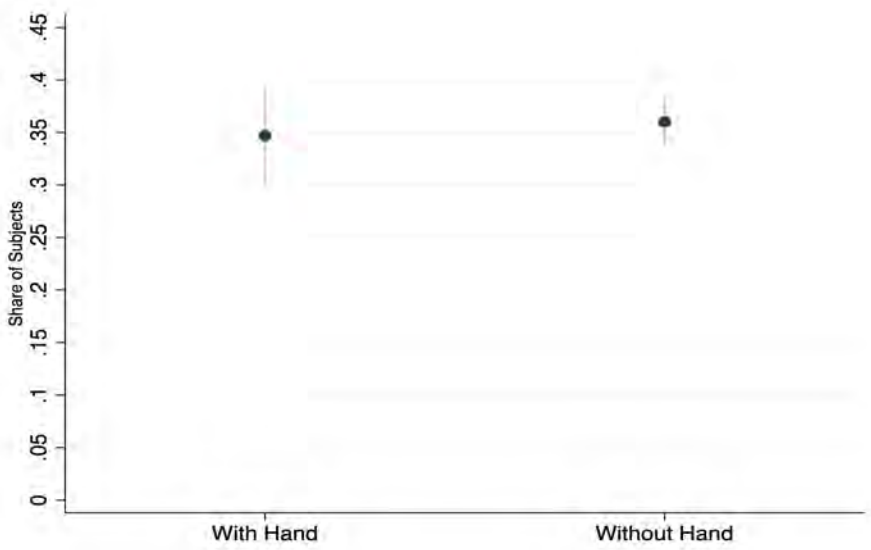
Notes: Reported is the percent of subjects in the female treatment group who gave each of the possible responses to the question: "What is the sex of the person holding the receipt in the picture?".

Figure 33: Sex Salience in the Male Treatment Group



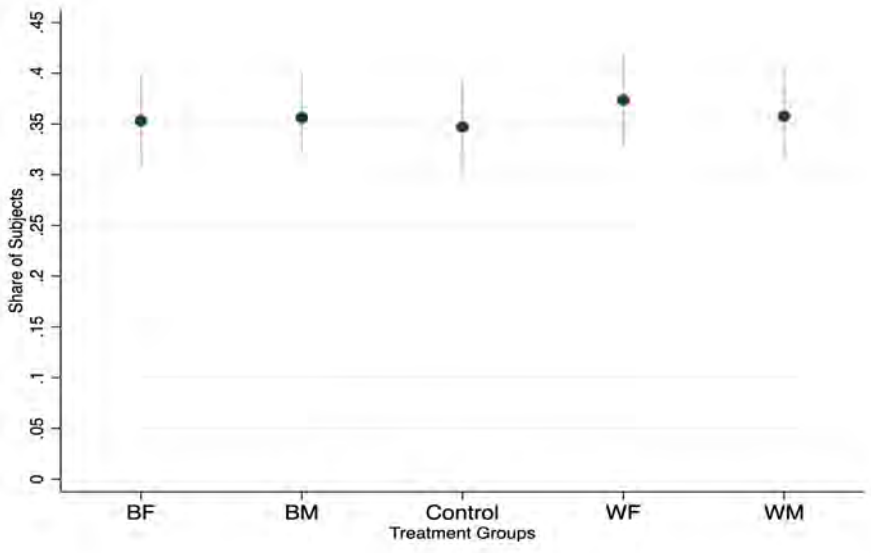
Notes: Reported is the percent of subjects in the male treatment group who gave each of the possible responses to the question: "What is the sex of the person holding the receipt in the picture?".

Figure 34: Transcription share



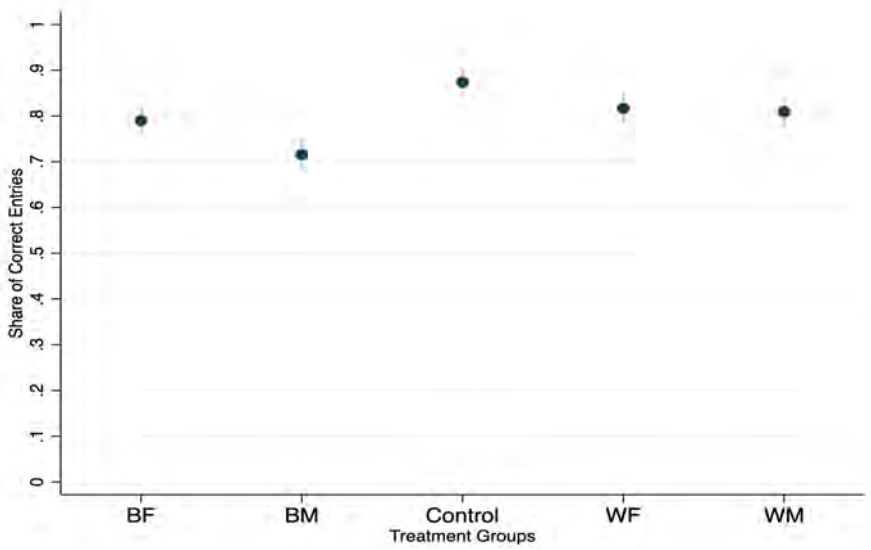
Notes: Reported is the share of subjects who agreed to transcribe receipts. 'With Hand' indicates that the image of the receipt included a hand; 'Without Hand' indicates subjects in the control group.

Figure 35: Transcription share by treatment groups



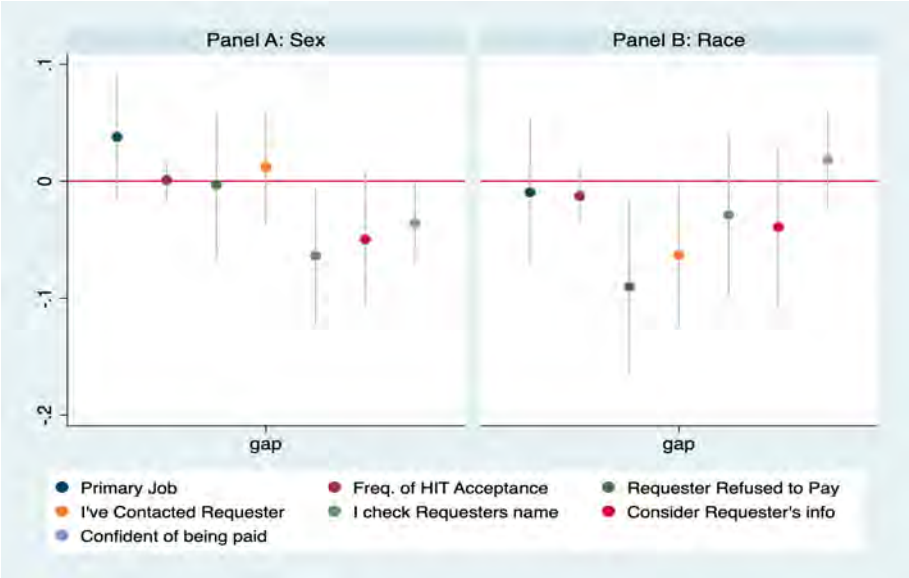
Notes: Reported is the share of subjects who agreed to transcribe receipts in each treatment group. BF includes subjects who were assigned to the black female hand treatment; BM includes subjects who were assigned to the black male hand treatment; Control includes subjects who were assigned to the control group; WF includes subjects who were assigned to the white female hand treatment; and WM includes subjects who were assigned to the white male hand treatment.

Figure 36: Accuracy Rate by Treatment Group



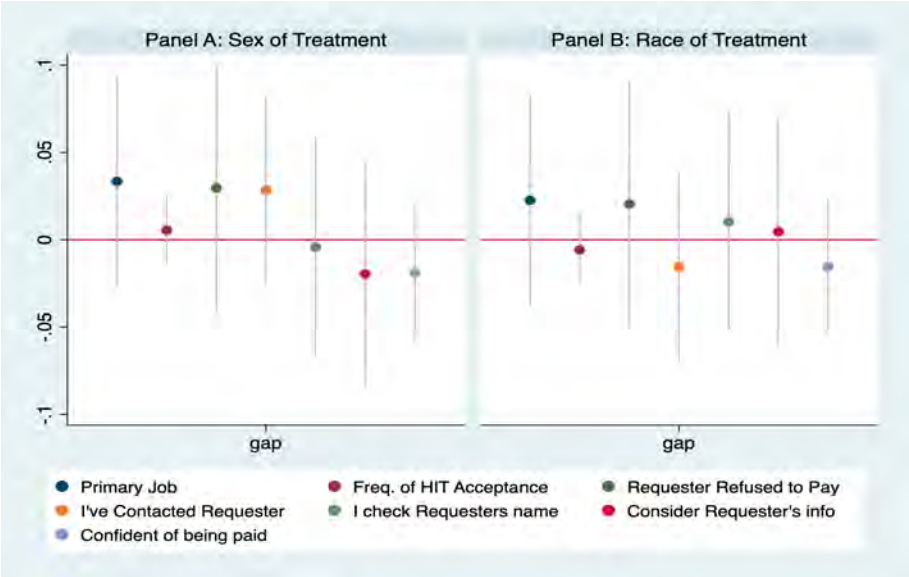
Notes: Reported is the share of correct entries in each treatment group. BF includes subjects who were assigned to the black female hand treatment; BM includes subjects who were assigned to the black male hand treatment; Control includes subjects who were assigned to the control group; WF includes subjects who were assigned to the white female hand treatment; and WM includes subjects who were assigned to the white male hand treatment.

Figure 37: mTurker Work Experience Survey



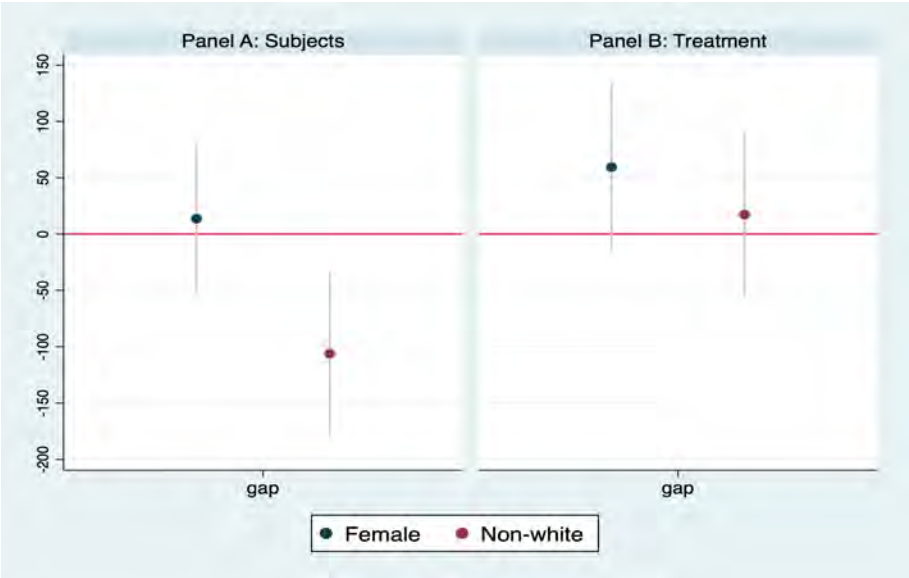
Notes: Panel A reports the difference (between female and male subjects) in the share of mTurkers who answered yes to questions regarding their experiences as mTurk workers and 95% confidence intervals. Panel B is similar to Panel A except that differences are calculated between non-white and white subjects. Questions include the following: *Is mTurk your primary job?*, "*Is your work often accepted?*", "*Have you experienced requesters' refusal to pay for work completed?*", "*Have you contacted requesters before?*", "*Do you often pay attention to the requester's name?*", and "*Will a requester's characteristics affect your likelihood of accepting a HIT?*". The question "*how confident are you that a requester will pay for a bonus task*" is measured a scale from 0 to 100. The variable was transformed to a 0 to 1 scale.

Figure 38: mTurker Work Experience Survey



Notes: Panel A reports the difference (between female and male subjects) in the share of mTurkers who answered "yes" to questions regarding their experiences as mTurk workers and 95% confidence intervals. Panel B is similar to Panel A except that differences are calculated between black and white treatments. Questions include the following: "*Is mTurk your primary job?*", "*Is your work often accepted?*", "*Have you experienced requesters' refusal to pay for work completed?*", "*Have you contacted requesters before?*", "*Do you often pay attention to the requester's name?*", and "*Will a requester's characteristics affect your likelihood of accepting a HIT?*". The question "*how confident are you that a requester will pay for a bonus task*" is measured a scale from 0 to 100. The variable was transformed to a 0 to 1 scale.

Figure 39: mTurker Work Experience Survey



Notes: Reported is the gap in the mean number of monthly HITs completed by subjects along with 95% confidence intervals. Differences are calculated between female and male subjects, and between non-white and white subjects in Panel A. Differences are calculated between female and male treatments and between black and white treatments in Panel B.