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Essays on Information and Discrimination

Darya Korlyakova

Dissertation

Prague, February 9, 2023

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Dissertation Committee

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Contents

Abstract

In the first chapter, we study experimentally whether public beliefs about ethnic discrimination, an emotionally loaded issue, are shifted more by information from experts or from ordinary people. We also examine whether people are inclined to choose the most influential sources. For this purpose, we combine, in a novel design, the random provision of information from different sources with endogenous information acquisition from the same sources. We find that individuals update their beliefs most in response to information from experts, namely researchers studying ethnic minorities and human resource managers. Exogenous adjustments in beliefs do not induce changes in attitudes to ethnic minorities. Consistent with the strength of belief updating, more individuals choose information from experts over information from ordinary people. This result suggests that, in the aggregate, people behave rationally as they favor a source that is perceived to be relatively accurate. The findings have implications for information-dissemination policies.

In the second chapter, we shift the focus from the general public to racial minorities and study the effects of information provision on minorities' beliefs and behavior. There is a long-standing concern that expected discrimination discourages minorities from making efforts to succeed. Effort withdrawal could contribute to confirming negative stereotypes about minorities' productivity and enduring disparities. This chapter extends the findings of correlational research by exogenously manipulating individuals' beliefs about discrimination against their group and exploring a causal link between perceived discrimination and individuals' labor market behavior. For this purpose, we conduct an online experiment in the US with a diverse sample of 2,000 African Americans. We randomly assign individuals to two groups and inform one group about the frequency of discrimination against African Americans in a previous survey. To study the information effects on effort, we subsequently measure participants' results on a math task. We document that most individuals initially overestimate discrimination against African Americans. The overestimation decreases strongly and significantly as a result of information provision. At the same time, treated individuals, males in particular, attempt and correctly solve *fewer* math problems than untreated individuals. Hence, our findings do not support the common concern that minorities' inflated expectations about discrimination induce them to underperform.

In the third chapter (joint work with Sona Badalyan and Rastislav Rehák), we focus on communication among hiring team members and document the existence of discrim-

ination in the disclosure of information about candidates. In particular, we conduct an online experiment with a nationally representative sample of Czech individuals who act as human resource assistants and hiring managers in our online labor market. The main novel feature of our experiment is the monitoring of information flow between human resource assistants and hiring managers. We exogenously manipulate candidates' names to explore the causal effects of their gender on information that assistants select for managers. Our findings reveal that assistants disclose more information about family and less information about work for female candidates than for male candidates. An in-depth analysis of types of information disclosed suggests that gender stereotypes play an important role in this disclosure discrimination.

Abstrakt

V první kapitole pomocí experimentu zkoumáme velmi citlivou problematiku etnické diskriminace. Zkoumáme, zda se veřejné mínění ohledně diskriminace změní více, když subjektům poskytneme informace od expertů nebo od obyčejných lidí. Dále se zabýváme tím, zda si lidé vybírají (spíše) nejvlivnější zdroje. Námi vytyčené otázky zodpovídáme pomocí experimentálního designu, v němž nově kombinujeme dvě možnosti výběru informačních zdrojů, a to zcela náhodný výběr informačního zdroje a endogenní volbu informačního zdroje. Docházíme ke zjištění, že lidé nejvíce pozměňují svá apriorní mínění v reakci na informace od expertů, konkrétně od vědeckých pracovníků zabývajících se tématikou etnických minorit a HR manažerů. V souladu s tímto zjištěním se také ukazuje, že si lidé často jako zdroj svých informací vybírají spíše odborníky. Exogenní manipulace apriorního mínění nijak nepozměňuje postoje k etnickým menšinám. Obecně se dá tedy říci, že se lidé chovají racionálně, jelikož si vybírají informace z relativně přesnějších zdrojů. Výsledky této studie se dají využít při utváření politik informovanosti.

Ve druhé kapitole přesouváme pozornost z široké veřejnosti na rasové menšiny a studujeme dopady poskytování informací na mínění a chování menšin. Otázka, zda očekávaná diskriminace odrazuje menšiny od vynakládaní vyššího úsilí k dosažení úspěchu, je dlouhou dobu diskutována. Nedostatek vynaloženého úsilí může přispívat k přetrvávajícím rozdílům a potvrzování negativních stereotypů o produktivitě menšin. Tato kapitola přispívá ke zjištění existujících korelačních studií exogenní manipulací přesvědčení jednotlivců o diskriminaci jejich menšiny a zkoumáním kauzálního vztahu mezi vnímanou diskriminací a chováním jednotlivce na trhu práce. Za tímto cílem provádíme v USA online experiment s bohatým vzorkem 2000 Afroameričanů. Náhodně přiřazujeme jednotlivce do dvou skupin, kde jednu skupinu informujeme o zaznamenání diskriminace proti Afroameričanům v předcházejícím průzkumu. Následně sledujeme výsledky účastníků studie při řešení matematické úlohy a zkoumáme vliv informace o diskriminaci na vynaložené úsilí. Zjišťujeme, že většina původně nadhodnocuje diskriminaci vůči Afroameričanům. Poskytnutí informace vede k ekonomicky i statisticky významnému snížení nadhodnocení diskriminace. Jednotlivci vystaveni informaci o diskriminaci, obzvláště mužští účastníci, se pokusili vyřešit a úspěšně vyřešili méně matematických úloh ve srovnání s jednotlivci, kteří informaci o diskriminaci neměli. Naše výsledky proto nepodporují běžnou domněnku, že zvýšené očekávání diskriminace menšin vede k jejich nižšímu výkonu.

Ve třetí kapitole (společná práce se Sonou Badalyan a Rastislavem Rehákem) se zaměřujeme na komunikaci mezi členy náborového týmu a dokumentujeme existenci

diskriminace při zveřejňování informací o kandidátech. Konkrétně provádíme online experiment s celostátně reprezentativním vzorkem českých jedinců, kteří na našem online trhu práce působí jako asistenti lidských zdrojů a náboroví manažeři. Hlavním přínosem našeho experimentu je sledování toku informace mezi asistenty lidských zdrojů a náborovými manažery. Exogenně manipulujeme se jmény kandidátů, abychom prozkoumali kauzální účinky jejich pohlaví na informace, které asistenti vybírají pro manažery. Naše zjištění ukazují, že asistenti prozrazují více informací o rodině a méně informací o práci u kandidátek než u kandidátů. Detailní analýza typů zveřejňovaných informací naznačuje, že v této diskriminaci při zveřejňování hrají důležitou roli genderové stereotypy.

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Introduction

This dissertation contributes to experimental literature on the causes and consequences of discrimination. Discriminatory behavior against certain social groups could lead to adverse outcomes for them through several channels. First, discrimination could result in a reduced set of opportunities, a higher bar to exit poverty or to prove oneself suitable for a certain job, limited access to key services, and poorer psychological wellbeing. Second, unequal treatment may discourage the discriminated groups from investing into their human capital and exerting effort in the workplace, contributing to the vicious circle of inequalities. Although discrimination has received considerable attention from researchers and policymakers for a long time, it remains pervasive (Bertrand and Duflo, 2017; Quillian et al., 2017; Lippens, Vermeiren, and Baert, 2023). Combating discrimination requires deeper knowledge about its roots and forms (Gneezy, List, and Price, 2012), and remedial measures. However, empirical knowledge is limited so far. In this regard, we design a series of experiments with controlled provision of information to explore the role of information for discriminatory behavior and for the behavior of discriminated groups.

Specifically, the first chapter tests the conjecture that inaccurate public beliefs about the extent of local discrimination could be a mechanism that contributes to unequal treatment. As misperceptions are found to be ubiquitous (Bursztyn and Yang, 2022), we considered an intervention with randomized provision of information to be promising. The second chapter examines whether correcting minorities‘ beliefs about the prevalence of discrimination against them in local society could affect their effort. Similar to the first chapter, this chapter adds to the literature on information provision experiments

(see Haaland, Roth, and Wohlfart (forthcoming) and Fuster and Zafar (2022) for recent reviews). In the third chapter, we explore whether differential provision of information about job applicants - which is a key input that firms use when making hiring decisions - is one of the channels through which discrimination could manifest itself. To this end, we perform a hiring experiment (e.g. Bohren et al., 2019; Bohren, Hull, and Imas, 2022). Understanding whether hiring team members disclose candidate's information to others differently depending on the candidate's gender is crucial for firms that are concerned with de-biasing their selection process.

Methodologically, this dissertation relates to the literature on online experiments[1]. The choice of this method enables us (i) to make information (treatment) in the first two experiments salient and (ii) to closely monitor information transmission during a hiring process in the third experiment, which would be difficult to carry out in field settings. Although subjects are aware of their participation in the survey and thus may intend to please the experimenter, we make deviations from stating true preferences costly by collecting behavioral measures (e.g. donations to a real charity) in addition to self-reported outcomes. Recent studies that use online experiments have offered other creative ways to mitigate the experimenter-demand effects. The first chapter of this dissertation discusses the results of an obfuscated follow-up survey[2], which is presented and structured in a way that aims to hide a link between the treatment and outcome questions. In addition, we conduct the survey with a lag of one week from the main experiment and do not remind subjects of their previous responses. In contrast to conventional laboratory experiments, online experiments open the door to recruiting diverse subject pools and, thanks to the small cost per participant, interventions can be implemented on a large scale. This flexibility in terms of sample size and composition enables us to study questions that the first two chapters pose on large suitable populations.

We can now move on to a more detailed discussion of each chapter. The first chapter explores what information sources - experts or ordinary people - individuals tend to choose to learn about a controversial issue, in this case ethnic discrimination. Whereas traditional economic theory suggests that the choice will be guided by source accuracy, behavioral research has uncovered other motives, including preference for like-minded information sources (e.g. Martin and Yurukoglu, 2017; Gentzkow and Shapiro, 2010;

[1]For discussion of the benefits from online and survey experiments and their challenges, see Horton, Rand, and Zeckhauser (2011), Arechar, Gächter, and Molleman (2018), Stantcheva (2022) or Fréchette, Sarnoff, and Yariv (2022).

[2]Obfuscated surveys were introduced by Haaland and Roth (2021) and Haaland and Roth (2020).

Mullainathan and Shleifer, 2005). A particularly interesting question, which existing literature leaves unanswered, is whether individuals *rationally* select the most influential information. The first chapter addresses this question by investigating the choices of information sources and the causal effects of information from these sources on belief updating in the same setting. While a handful of studies vary information sources, they tend to provide different messages from different sources (e.g. Cavallo, Cruces, and Perez-Truglia, 2017; Coibion, Gorodnichenko, and Kumar, 2018). In contrast, we fix the information content to identify the causal role of an information source alone.

More specifically, in a large-scale survey experiment with a representative sample of Czech individuals, we provide information about the extent of local labor-market discrimination against Asians from ordinary people, human resource (HR) managers or researchers. This information is truthful and is based on the results of our supplementary survey. Subjects were able to see information from one randomly chosen information source. Additional groups of participants were either exposed to information unrelated to Asians (the control group) or had an opportunity to choose a favorite information source and subsequently acquire information from it. We find that each source shifts individuals' beliefs about the prevalence of discrimination against Asians and that experts, i.e. researchers and HR managers, are more influential than ordinary people. The differences across information sources, albeit smaller in magnitude, are also observed one week later. Belief updating is not accompanied by changes in subjects' attitudes to Asians; a result which is confirmed by both self-reported and behavioral measures. Consistent with the strength of belief updating, more individuals prefer to acquire information from experts over information from ordinary people. As the experimental participants perceive the experts' opinions on discrimination to be much more accurate than those of ordinary people, our results seem to align with economic theory that highlights rationality in information acquisition and belief updating.

In the second chapter, we look at the importance of beliefs about discrimination from a minority's perspective. Existing literature has identified that perceived discrimination is negatively correlated with job satisfaction and organizational commitment, and positively correlated with self-reported absenteeism in the workplace and intent to leave the firm (e.g. Ensher, Grant-Vallone, and Donaldson, 2001; Jones, Ni, and Wilson, 2009; Foley, Kidder, and Powell, 2002). However, little is known about the causal effects of expected discriminatory treatment by employers on minorities' behavior. Whereas previous studies document that women and minorities alter their behavior in response to a potentially

unfavorable environment, e.g. by hiding or misrepresenting their gender or ethnicity (Charness et al., 2020; Alston, 2019; Kudashvili and Lergetporer, 2019), the exact role of beliefs about discrimination against one's own group remains unclear. The second chapter aims to fill this gap. Another value added of our study is that we use data from a large sample of racial minorities to investigate how minorities respond to information on a controversial issue. In contrast, previous information provision (online) experiments (e.g. Haaland and Roth, 2021; Grigorieff, Roth, and Ubfal, 2020; Alesina, Miano, and Stantcheva, 2018) usually recruit members of a majority or samples representative of the general population.

In an online experiment with 2,000 African Americans, we assign subjects to two groups and provide information to the treatment group about the true frequency of discrimination against African Americans in a previous survey. The control group does not receive any information. Subsequently we measure subjects' performance on a real-effort task, re-elicit their beliefs about discrimination in an incentivized manner and collect a few additional behavioral outcomes. We find that upward-biased beliefs about discrimination against one's racial group are widespread among our experimental participants and that information is effective in reducing these misperceptions. Treated subjects, men in particular, *decrease* their performance in response to optimistic news about discrimination. Therefore, our findings do not support the concern that minority groups reduce their effort in response to expected discrimination.

In the third chapter (a joint work with Sona Badalyan and Rastislav Rehák), we consider a novel type of discrimination - differential disclosure of information about job applicants depending on applicants' gender. This discrimination may arise when HR assistants communicate information about prospective workers collected at the earlier stages of the recruitment process to hiring managers. Our design differs from previous experiments on discrimination (He, Li, and Han, forthcoming; Kübler, Schmid, and Stüber, 2018; Kessler, Low, and Sullivan, 2019) because we incorporate the involvement of multiple decision-makers in hiring. The exchange of information in the hiring team could be a channel through which discrimination propagates and unfavorable stereotypes emerge.

To observe the information selection process of HR assistants, we perform an online experiment with a nationally representative sample of Czech individuals. The subjects select information from multiple workers' profiles, which contain details about the workers' demographics, education, professional experience, qualifications, and personal

qualities. To vary gender exogenously, we randomly assign names to the profiles. We make assistants' decisions incentive compatible by recruiting a complementary sample of respondents (hiring managers) who could reward assistants for the quality of information selected (based on the managers' subjective judgments). We find that assistants are more likely to disclose information about the number of children and marital status of female workers relative to male workers. Such selection may make women's family responsibilities salient. Furthermore, we observe that, although assistants disclose less work-related information about female workers overall, they are more likely to provide information about women's job positions if they are employed in female dominated occupations. Taken together, these findings suggest that the discrimination uncovered in disclosure may be connected to gender stereotypes.

Chapter 1

Learning about Ethnic Discrimination from Different Information Sources

Abstract

Darya Korlyakova[1]

We study experimentally whether public beliefs about ethnic discrimination, an emotionally loaded issue, are shifted more by information from experts or from ordinary people. We also examine whether people are inclined to choose the most influential sources. For this purpose, we combine, in a novel design, the random provision of information from different sources with endogenous information acquisition from the same sources. We find that individuals update their beliefs most in response to information from experts, namely researchers studying ethnic minorities and human resource managers. Exogenous adjustments in beliefs do not induce changes in attitudes to ethnic minorities. Consistent with the strength of belief updating, more individuals choose information from experts over information from ordinary people. This result suggests that, in the aggregate, people

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behave rationally as they favor a source that is perceived to be relatively accurate. The findings have implications for information-dissemination policies.

Key words: ethnic discrimination, beliefs, information sources, experts
JEL Classification: C90, D83, J71

1.1 Introduction

Individuals often have to choose among multiple information sources and they may apply different criteria to identify their preferred source. Consistent with the conventional economic view, people select sources whose information provides them with an accurate representation of reality. However, in the context of controversial social issues, truthful information about ingroup members' unethical behavior can induce an unpleasant emotional response and thus the truth may be avoided or distorted. This idea is in line with a growing body of behavioral research which argues that people have a taste for *like-minded* information sources (e.g. Martin and Yurukoglu, 2017; Gentzkow and Shapiro, 2010; Mullainathan and Shleifer, 2005). Competing theoretical predictions raise the following questions: What information sources are the most influential? Will people choose these sources to acquire information about an emotionally loaded issue?

This paper aims to identify the most influential source(s) by investigating the causal responses of beliefs to information about ethnic discrimination from three distinct sources. Specifically, we gauge willingness to learn from the most influential source by simultaneously studying information acquisition from the same sources, namely ordinary people, human resource managers, and researchers studying ethnic minorities. We chose ordinary people and experts because each group is likely to possess one positive characteristic - low social distance or high accuracy - but never both of them. Therefore, subjects will face a trade-off between listening to potentially uninformed individuals similar to them and learning from HR managers or researchers who have relevant expertise and knowledge but engage less frequently in social interactions with subjects.

More broadly, our choice of information sources helps to add to the ongoing discussion on whether the public distrust experts and science. On one side of the spectrum, studies find that people learn less effectively from more knowledgeable peers (Ambuehl et al.,

2018), take less advice from experts than from peers (Läpple and Barham, 2019), and exhibit a *modest* level of trust in experts (Johnston and Ballard, 2016). However, recent results of surveys with representative samples from the US (Pew Research Center, 2019, 2020) and the Czech Republic (Public Opinion Research Center, 2019a) as well as the analysis of EU and UK survey data by Dommett and Pearce (2019) do not reveal growing anti-expert sentiment. Furthermore, US and Czech survey data, along with Ipsos global poll results (2019), illustrate a generally high level of confidence in the scientific profession. This evidence suggests that widely discussed negative perception of experts may be exaggerated, and hence further scientific investigation is warranted.

Studying differences in belief updating among individuals who *selected* different sources has a number of problems. We would not be able to reliably conclude that a source with the largest belief responses is the most influential, because it may simply be chosen most frequently by people with a high propensity to update. To rule out selection-driven interpretation of results, we conduct a pre-registered survey experiment with a large representative sample of Czech individuals. We consider the Czech Republic to be particularly suitable for our information intervention because Czech society is presumably unaware of *widespread* local ethnic discrimination. According to Special Eurobarometer 437 (2015), 44 percent of Czech people believe that ethnic discrimination is a rare phenomenon locally, even though research and survey evidence documents prevalent unfavorable attitudes to Roma people, Asians, and other ethnic and national minorities in the Czech Republic (e.g. Bartoš et al., 2016; Public Opinion Research Center, (2019b); Bartoš et al., 2021). East Asians, whose discrimination our information intervention targets, are one of the two largest ethnic minority groups in the Czech Republic.

In the experiment, we randomly assign subjects to five groups. After eliciting prior beliefs about discrimination, we inform three groups of participants about how many applications a job seeker with an Asian-sounding name has to send to receive one interview invitation. This information is provided from a specific source; either it is a group of 9 ordinary people, 9 HR managers, or 9 researchers who mainly study issues that ethnic minorities face in the Czech Republic.

It would be problematic to isolate the role of an information source if we simultaneously vary the information content. To mitigate this confound, we oversample experts and ordinary people in a supplementary survey and randomly select from these groups the subsets of 9 individuals who, on average, agree on the extent of discrimination. As a

result, across three treatments, experimental participants see the *same* number of applications that an Asian has to send, regardless of the source that provides the information. In the Control group, subjects receive neutral information unrelated to Asians. To investigate whether participants select sources that induce the strongest shifts of beliefs, we allow the last group to choose a preferred source and subsequently acquire information from it. After the information stage, we collect data on posterior beliefs, self-reported attitudes to Asians, and donations to a pro-Vietnamese[2] charity. In a follow-up study one week later, we examine the persistence of treatment effects on these three outcomes.

A complementary goal of this paper is to present a theory-based interpretation of the experimental findings. Traditional theory would predict that signal precision underlies people's decisions related to information acquisition and belief updating. To test whether individuals update more in response to more accurate sources and choose these sources more frequently, we elicit subjects' perceptions regarding the accuracy of three information sources. The second alternative, Akerlof's theory of social distance (1997), highlights the dependence of individual utility on the actions and opinions of others. In accordance with this theory, subjects are expected to learn most effectively from their social network members. To verify this hypothesis, we compare patterns in belief updating and information choices to differences in a self-reported likelihood of befriending an ordinary person, researcher, and HR manager. Finally, we examine a link between our results and the predictions of the confirmation-bias theory (Rabin and Schrag, 1999). In our setting, confirmation bias would manifest itself as a tendency to select a source whose opinion subjects expect to be the closest to their prior belief[3]. We measure subjects' expectation about a source opinion by asking them to predict the estimate of ethnic discrimination for each of the three sources.

We find that individuals' beliefs respond to information from every source, but the strongest response is to the message from experts, i.e. researchers and HR managers. These differences in belief responses persist over a one-week period, although they become

[2]Vietnamese people constitute the largest Asian minority in the Czech Republic.

[3]In the existing literature, there is no unique commonly used test aimed at identifying the presence of the confirmation bias. Charness and Dave (2017) specify a regression that allows them to estimate an extra weight that one places on confirming information. Charness, Oprea, and Yuksel (2021) define confirmatory-seeking types as those who, under different conditions, choose an information structure biased in the direction of their prior beliefs. Jones and Sugden (2001) study several forms of the confirmation bias. First, they explore in an incentive-compatible design whether subjects tend to choose an uninformative but potentially confirming card. Second, they examine whether subjects become more confident in the truth of the hypothesis if they receive a statement confirming it.

smaller in magnitude. Despite revising their beliefs about discrimination, subjects do not change their attitudes to Asians and donate at similar rates to a local pro-Vietnamese charity. Consistent with the strength of belief updating, more individuals prefer to acquire information from experts over information from ordinary people. The auxiliary analysis suggests that revealed patterns in belief updating and preferences for sources can be explained by the relatively high (perceived) accuracy of experts. At the aggregate level, our results accord with economic theory highlighting rationality in information acquisition and belief updating. The counterbalancing effects of social distance do exist, but they are not strong enough to reverse the gap between information sources.

Existing literature studies exogenous information provision separately from endogenous information acquisition, which does not inform policymakers about whether providing the most influential information is effective, measured in terms of people's willingness to acquire it. From an ex-ante perspective, it is conceivable that individuals will listen most to those whose opinions they perceive to be relatively accurate. However, they may *choose* to learn primarily from like-minded people, thereby leaving the dissemination of information from experts unattended. By investigating, in the same setting, the choices of information sources and the effects of exogenous information from these sources on beliefs, our study uncovers a general consistency between information preferences and causal updating.

Random assignment of information relates our paper to numerous studies that explore information treatment effects on individuals' beliefs, policy preferences (Alesina, Stantcheva, and Teso, 2018; Lergetporer et al., 2018; Kuziemko et al., 2015; Cruces, Perez-Truglia, and Tetaz, 2013) and behavior (Haaland and Roth, 2020; Alesina, Miano, and Stantcheva, 2018; Grigorieff, Roth, and Ubfal, 2020). The closest work to ours is the study by Haaland and Roth (2021), who investigate whether exposure to research evidence about the extent of racial discrimination in the US labor market reduces political polarization in preferences for pro-black policies. In contrast to the above studies, we additionally create exogenous variation in a source that disseminates information. Several studies have already randomly manipulated an information source (Coibion, Gorodnichenko, and Kumar, 2018; Jacobsen, 2019; Cavallo, Cruces, and Perez-Truglia, 2017; Alt, Marshall, and Lassen, 2016). However, we differ from these by fixing the information content across treatments to cleanly identify the effects of a source alone and by varying a source expertise rather than its independence.

Simultaneously, our findings inform the literature on information acquisition (Bartoš et al., 2016; Hoffman, 2016; Hoopes, Reck, and Slemrod, 2015; Charness, Oprea, and Yuksel, 2021). Similar to our work, Fuster et al. (2022) give survey respondents an opportunity to choose among several pieces of information that differ in their informativeness. However, the authors primarily examine what information about *home price changes* subjects acquire and whether they incorporate favorite information into their forecasts, which is very different from the focus of our study. In addition, the authors concentrate on the ex-ante predictive power of a source as the criterion that respondents use while ranking information sources. In contrast, we differentiate between perceived source accuracy, social distance and the potential to confirm one's initial belief.

The rest of the paper is organized as follows. Section 1.2 describes the experimental design, while Section 1.3 discusses our sample and follow-up attrition. Section 1.4 presents the experimental results, and Section 1.5 links our findings to theories. Section 1.6 concludes.

1.2 Experimental Design

Figure 2.A.1 outlines the main elements of our two-wave experiment, which randomly assigns participants to five different groups. In the first wave (main experiment), we elicited beliefs about the prevalence of local labor market discrimination against Asians. Subsequently, three subsets of subjects received exogenous information about discrimination from three different sources. The Control group was exposed to neutral irrelevant information, while the last subset of subjects was offered to choose from a list of three information sources and no-information option. Next, we measured subjects' posterior beliefs, attitudes to Asians, and donation behavior. In the second wave (obfuscated follow-up), which took place a week after the main experiment[4], we tested whether potential treatment effects on the participants' beliefs and attitudes persist. In both waves, we asked several questions on potential explanations for (i) belief responsiveness to information from specific sources and (ii) information choices.

[4]The minimum (maximum) number of days between the two waves was 5.43 (19.17), while the same number for an average participant was 6.95 days.

1.2.1 First Wave

Prior beliefs about the prevalence of discrimination against Asians

To put subsequent belief elicitation into the context, we first provided all subjects with a brief description of the labor market experiment from the correspondence study by Bartoš et al. (2016). In particular, participants were told that researchers from CERGE-EI studied the extent of discrimination against Asians in the Czech labor market by sending job applications that were identical except for a job seeker's name (i.e. a Czech- vs. Asian-sounding name) to signal ethnicity.

Next, we informed all subjects that, according to the researchers, a job seeker with a Czech-sounding name has to send on average 7.5 applications in order to receive one interview invitation. Subsequently, we elicited participants' beliefs about the prevalence of local discrimination against Asians[5] by asking them to estimate the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation. We incentivized correct answers (i.e. estimates that were the same as the CERGE-EI researchers' finding) with a 22-cent bonus[6] paid in addition to the participation fee. Research evidence indicates that small bonuses could serve as effective incentives (e.g. DellaVigna and Pope, 2018; Bullock et al., 2015). In addition, large rewards for accuracy could have motivated our subjects to search for Bartoš et al.'s findings (2016), which are easily accessible online, and state the number they found instead of a true belief. Furthermore, a small pilot prior to the main experiment indicated that individuals who were randomly provided with a 22-cent bonus incentive (in the follow-up) took the posterior elicitation task more seriously. Specifically, these subjects spent more time on a page with the posterior-belief question and were more likely to update their beliefs (see Table 1.B.1).

Information provision or acquisition

After we measured subjects' confidence in their prior beliefs about the extent of local discrimination against Asians, participants moved to an information provision or information acquisition stage. Three subsets of participants (hereafter Exogenous-Info groups)

[5] In both waves, we asked subjects to consider among Asians mainly Vietnamese, Chinese and Japanese people.

[6] The bonus was paid in Czech crowns. Before giving consent to participate, potential subjects were told that they would receive information about the bonus at the end of November 2018 (instead of learning about it immediately after their completion of the survey). We chose to postpone this information to avoid its influence on subjects' posterior beliefs that were repeatedly collected in the follow-up.

obtained the *same* piece of information about discrimination, namely the estimated number of applications that a job seeker with an Asian-sounding name has to send to receive one interview invitation. However, the source whose estimate participants saw was different across three treatment arms. Subjects in Laymen-Info group obtained the average estimate of 9 ordinary people. Participants in Practitioners-Info group received the average estimate of 9 HR managers, while participants in Researchers-Info group received the average estimate of 9 researchers who primarily study issues that ethnic minorities face in the Czech Republic. In the context of our study, we consider HR managers and researchers to be experts because the former are likely to have practical experience relevant for estimating the prevalence of discrimination and the latter can back up their perception of discrimination by theoretical knowledge and research findings.

To avoid deceiving subjects by presenting *fictional* individuals' estimates, we truthfully surveyed passers-by (whom we refer to as "ordinary people"), HR managers, and researchers in June-July 2018, before running the experiment. To elicit beliefs of passers-by, research assistants approached people in Prague parks and squares and asked whether they were willing to take part in a brief survey[7]. If the passer-by agreed, research assistants briefly described the labor market experiment by Bartoš et al. (2016) and stated the number of applications a job seeker with a Czech sounding name has to send to receive one interview invitation. Next, they asked the passer-by to estimate the number of applications that a job seeker with an Asian-sounding name has to send to obtain an invitation for one interview[8].

HR managers and researchers were contacted via an email that explained that a survey is a part of a PhD student's dissertation, mentioned the topic and length of the survey, and included a link to the online questionnaire. After clicking the link (which differed between the two groups), HR managers and researchers saw the survey consent page[9] followed by the same text and questions that passers-by had obtained. We retrieved the emails of HR managers working in various parts of the Czech Republic by mainly using the Czech Labor office database. In the case of researchers, we contacted individuals

[7]Research assistants also asked whether a person is over 18 years old when it was not obvious.

[8]We also elicited the corresponding beliefs of passers-by, HR managers, and researchers about an applicant with a Roma-sounding name. However, later on we did not find a match, i.e. an average estimate that was the same across three groups, and thus we did not use information about a Roma job seeker in the experiment.

[9]We decided to collect email addresses (which were deleted within 24 hours) in an attempt to avoid multiple responses. In addition, this allowed us to send reminders without disturbing individuals who had already filled in the questionnaire.

from different Czech universities and research institutes (mostly from Charles University and Masaryk University) who hold a PhD degree and whose research is focused on inter-ethnic relations, integration of foreigners, migration, attitudes to ethnic minorities, etc.

Overall, we collected 53 responses from passers-by, 36 responses from HR managers, and 20 responses from researchers[10]. Subsequently, we randomly divided each group into the subsets of 9 people, searched for an estimate that was the same across three subsets (belonging to different groups) after being rounded off to the nearest integer, and found that 14 applications was the match. This number is different from the actual result of the research study by Bartoš et al. (2016), 20 applications. In the experiment, we truthfully informed participants in Exogenous-Info groups that the average estimate of a group of 9 individuals was 14 applications, but we did not mention that the group whose estimate they were given was random. If this information had been provided, it would likely have led to subjects' questions[11] and confusion, and eventually might have decreased their trust in our questionnaire. Some other studies (see, for instance, Falk and Zimmermann, 2018) withheld information from their experimental participants in a similar fashion.

We did not use the averages of the whole groups for the following reasons. First, the averages of all surveyed passers-by, HR specialists and researchers were equal to 20, 14, and 13 applications[12], respectively. Thus, if we had provided different numbers from different sources, we would not have been able to disentangle the source effect from differences in the message content on belief updating.

The supplementary survey results were communicated to subjects in Exogenous-Info groups in the following manner:

> *We asked 9 passers-by/HR managers/researchers who primarily study ... to estimate the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation. The average estimate of 9 passers-by/HR managers/researchers was 14 applications.*

Participants assigned to Exogenous-Info treatments also saw a bar chart comparing their

[10]The response rate for HR managers and researchers was about 6.5 percent and 37 percent, respectively.

[11]For example, participants might have started wondering why we did not provide the estimates of remaining individuals we had surveyed.

[12]We mention the values after rounding off. After excluding two passers-by with extreme beliefs (which we define in Section 1.4 as estimates larger than 50 or smaller than 1), the average for this group drops to 16 applications.

prior belief to 14 applications, i.e. the average estimate of the respective source. In Researchers-Info group, we additionally informed participants that 9 researchers whose estimate they saw are not related in any way to the authors of the earlier discussed study. This information is truthful, and it was conveyed to prevent subjects from listening to researchers only because the latter are assumed to be familiar with the study results.

The Control group saw a placebo message that, in order to account for anchoring (Tversky and Kahneman, 1974), contained the same number (14) that Exogenous-Info groups saw. Specifically, untreated participants were exposed to the following text:

> *We compared the prices of granulated sugar in 9 Czech regions in August 2018. The average price of sugar in these 9 regions was 14 crowns/kg.*

We chose to provide a neutral message (rather than no information) to the Control group to mitigate potential experimental demand effects in the subsequent (main-experiment) collection of posterior beliefs. Participants could think that it is better to state a different estimate of discrimination if they were asked to reconsider their belief immediately after it had been elicited. In addition to the above text, we showed to untreated subjects a graph that compares August 2018 prices of sugar across 9 Czech regions. The average price across regions (14) was highlighted to draw subjects' attention to the number of interest.

The last subset of participants (Info-Choice group) was presented with a list of three groups of 9 individuals to receive discrimination-related information from, and an alternative of no information. Subjects in this group were told that they have a chance to obtain the average estimate of one source and they were asked to rank the options according to their preference[13]. Subsequently, the most preferred alternative was implemented. If a participant chose to obtain a source estimate, he or she saw the following message (depending on the first-ranked source):

[13]We did not mention that information provided by a source could be useful for a subject's subsequent estimation of discrimination. Other studies (e.g. Haaland and Roth, 2021; Chopra, Haaland, and Roth, 2019) also do not emphasize the instrumental value of information that participants could acquire. It is probable that participants anticipated that we would ask them again to estimate the extent of discrimination against Asians. Subjects knew that they had completed a small part of the survey and thus they were likely to expect further related questions. Even if subjects were not sophisticated enough to anticipate the repeated estimation of discrimination, they could value information beyond the survey context. Consistent with this idea, Fuster et al. (2022) find that individuals are willing to pay for their favorite information much more than they will gain if their subsequent forecast of house prices is perfectly accurate.

The average estimate of 9 passers-by/9 HR managers/9 researchers who primarily study ... was 14 applications.

Similar to Exogenous-Info groups, subjects who selected one of the three sources additionally saw a graphical comparison of their prior belief and the average estimate of the respective source. If a participant preferred to see no information over all other options, the information-provision stage was omitted for him or her.

Collection of posterior beliefs, attitudes, and donations

Next, we asked all subjects whether they would like to revise their initial estimate of the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation. Answers that coincided with the CERGE-EI researchers' finding were again incentivized with a 22-cent bonus.

We hypothesized that, if people are broadly unaware of existing discrimination against Asians by employers, exogenously shifting their beliefs may lead individuals to feel sympathetic toward Asians. Hence, we asked subjects (in a randomized order) whether they agree or disagree that Asian workers (i) take Czech people's jobs and (ii) produce more disadvantages than advantages for the Czech labor market. To test whether revised beliefs shift *overall* attitudes to the minority of interest, we included a question on how comfortable or uncomfortable a person would feel if his/her neighbor was Asian.

Since self-reported outcomes are subject to social desirability bias, we additionally employed a measure that makes concealing true attitudes to Asians costly. At the very end of the first wave, we offered participants three alternatives in relation to their reward: (i) sending the reward to their personal bank account; (ii) donating the reward to a specific pro-Vietnamese charity[14]; (iii) declining the reward. A decision regarding one's reward is a common part of MEDIAN's[15] surveys, but we modified the donation option by including solely a pro-Vietnamese charity.

Predicted estimates of three information sources

Before facing the choice of an information source, a random half of Info-Choice group was asked to predict for each of the three groups (ordinary people, HR managers, and

[14]We did not find a charity that helps various Asian minorities in the Czech Republic; therefore, we selected a non-profit organization that supports the integration of Vietnamese people into Czech society.

[15]MEDIAN is the company we cooperated with on data collection (see section 1.3).

researchers) how many applications, according to a group, a job seeker with an Asian-sounding name has to send to receive one interview invitation[16]. The inclusion of this task enables us to assess the presence of *confirmation bias* which could imply that individuals tend to select information sources whose predicted beliefs are close to the individuals' prior beliefs.

1.2.2 Second Wave

Hiding the connection between two waves

Following Haaland and Roth (2021, 2020), we performed an obfuscated follow-up survey. Specifically, to mitigate potential experimental demand effects, we did not tell subjects that the two waves were connected. Moreover, the topics of both surveys were somewhat different[17] as was the text that participants saw before agreeing to participate in the surveys. In addition, at the beginning of the follow-up, we asked subjects several demographic questions to present the follow-up as an independent survey. Next, subjects answered a series of questions that concerned the Czech labor market but were not related to ethnic discrimination in order to further obfuscate the connection with the first wave.

Posterior beliefs, attitudes, donations and willingness to share information

After the obfuscation part, subjects received a series of attitudinal questions that were mostly reformulated[18]. We additionally measured participants' attitudes toward Ukrainians, a large national minority in the Czech Republic, to make the relationship between two waves less evident. Simultaneously, we aimed to explore whether there are spillovers from shifted attitudes to Asians (as a result of the information treatment) to the perception of other local minorities.

Later, subjects saw the text about Bartoš et al.'s experiment (2016) from the first wave and faced familiar incentivized elicitation of beliefs about the number of applications a

[16]These predictions were not incentivized. Several patterns indicate that subjects took this belief elicitation task seriously. We find that an average (median) person spent about 74 (56) seconds on a page with the respective questions. Moreover, no subject wrote down the same number for three groups, and no more than 8 percent of subjects stated the same prediction for two groups. Finally, no more than 5 percent of individuals recorded an "extreme" belief for a source, i.e. higher than 50 applications that an Asian has to send for one interview invitation. For comparison, about 4 percent of subjects in the Info-Choice group stated an "extreme" prior belief which was elicited using an incentivized procedure.

[17]While the topic of the main experiment was "Attitudes toward social issues", the topic of the follow-up was "Economic and social issues".

[18]We used the same formulation of the neighbor-related question in both waves as MEDIAN uses it in other surveys and thus connection to the first wave should not be evident.

job seeker with an Asian-sounding name has to send to receive one interview invitation. Participants were not reminded of their prior beliefs collected in the first wave.

At the end of the second wave, subjects were again offered to donate their experimental earnings to a pro-Vietnamese charity. This time, we used a different non-profit organization and, in both waves, we explicitly informed subjects that the charity is not related to the client who ordered the survey.

In addition to repeatedly collecting the above outcomes, we intended to access the value that individuals attach to information they had received the week before. To this end, we measured in Exogenous-Info groups the willingness to share with one's friends the source estimate of the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation[19].

Perceived accuracy of information sources and social distance from them

While deciding how much weight to attach to a signal relative to the prior belief and what signal to choose, subjects may be guided by the signal precision. To test this hypothesis, we asked (for each of the three sources) how accurate, in one's opinion, an average source estimate of ethnic discrimination would be[20]. Alternatively, individuals may want to form the same opinion on the prevalence of local ethnic discrimination as their social network has. In this regard, we asked how likely it is that a subject would become friends with an ordinary person, HR manager, and a researcher who primarily studies issues that ethnic minorities face in the Czech Republic[21]. The order of the information sources within each series of questions was randomly determined.

1.3 Sample Characteristics and Follow-up Attrition

We recruited 3,216 subjects in cooperation with MEDIAN, which is a leading survey agency in the Czech Republic. Of these 3,216 participants, 2,233 subjects completed the follow-up survey, which took place, on average, a week after the main experiment. Subjects randomly assigned to the Info-Choice group were not invited to the follow-up

[19]This question came *after* the posterior belief elicitation. The type of information source that was mentioned depended on one's initial treatment assignment.

[20]We exposed a random half of follow-up participants to the questions about accuracy and social distance to prevent some participants from thinking too thoroughly about source characteristics before their posterior beliefs were collected.

[21]Both accuracy and social distance were measured using a 5-point Likert scale.

due to financial constraints. Taking this into account, the response rate in the follow-up made up about 87 percent.

Table 1.B.2 presents summary statistics for our sample. In Table 1.B.3, we compare the demographics of our subjects (that we pre-specified to target) to the corresponding characteristics of the Czech population (mostly using data from the Czech Statistical Office). In both the main experiment and follow-up, our sample is fairly representative of the Czech population in terms of gender, age, education, and geography. Tables 1.B.4 and 1.B.5 illustrate that most covariates are balanced across five (four) treatment arms in the main experiment (follow-up survey).

As Table 1.B.6 shows, overall attrition is unrelated to the majority of observables, although it is not entirely random[22]. Crucially, subjects with different prior beliefs about the extent of discrimination against Asians are equally likely to attrit from the follow-up [23]. A key concern, however, is that subjects in Practitioners-Info group are 4 percentage points less likely to complete the follow-up compared to untreated participants ($p<0.05$). Despite this, attrition does not differ significantly among three treatment arms with different information sources ($p>0.10$ in all cases) which is important for our comparison of belief updating across Exogenous-Info groups. In addition, we do not observe more covariate imbalances in the follow-up relative to the main experiment (Tables 1.B.4 and 1.B.5). Furthermore, while focusing on individuals who would appreciate particularly a practitioner's advice on an important issue (N = 1,146), we find that subjects from the Practitioners-Info group are 6.3 percentage points *less* likely to participate in the follow-up relative to untreated participants ($p < 0.05$) [24]. The somewhat higher attrition rate for this subsample would, if anything, attenuate the effects of information from HR managers on subjects' beliefs collected in the follow-up. Finally, only 9.8 percent of non-participants (in the follow-up) quit the survey *after* opening our questionnaire, including those who quit before opening the page with the first (obfuscation) question, and this percentage

[22]While several covariates predict a likelihood of participation in the follow-up (or its completion), this does not significantly affect the sample representativeness in the second wave.

[23]We ran two alternative regression specifications in which, instead of the underestimator dummy, we included (i) the continuous measure of pre-treatment beliefs or (ii) the dummy for above-median initial misperceptions regarding discrimination. Both predictors are insignificant ($p=0.85$ and $p=0.54$, respectively).

[24]The other categories of potential advisors included: a person like me, academician, family member, and colleague. Although we asked subjects the respective question at the end of the first wave, after some of them were treated, the sample is balanced on the fraction of participants who would value practitioners' advice most.

does not differ significantly across treatment arms (p>0.10). Note that individuals who did not respond to the invitation to participate in a new survey were unlikely to know that it was related to the first questionnaire (as the topics of both surveys differed). While looking at the potential reasons for non-participation, we observe that untreated subjects were *more* likely to receive a reminder to fill in a questionnaire compared to their treated counterparts (p<0.05 when controls are included). The difference is (insignificantly) higher in Practitioners-Info group relative to the two other Exogenous-Info groups, which could at least partially account for the higher attrition rate in this group[25].

1.4 Experimental Results

We present four sets of results. First, we discuss the distribution and correlates of prior beliefs about the extent of labor market discrimination against Asians. Second, we analyze how individuals' beliefs respond to the same information about discrimination from different sources. Third, we discuss whether causal shifts in beliefs are accompanied by changes in self-reported attitudes to Asians and donation behavior. Finally, we analyze which information sources individuals select to acquire information about discrimination against Asians.

1.4.1 Prior beliefs about the extent of discrimination against Asians

As Table 1.B.2 indicates, an average subject from the main experiment believes that a person with an Asian-sounding name has to send 15.75 applications to receive one interview invitation[26]. Hence, it is reasonable to expect that the source estimate (14 applications) will not significantly shock the mean prior belief. Nevertheless, the provided information may be surprising if there is variation in pre-existing misperceptions at the *individual* level, which the average belief is not informative about. Figure 1.A.1 plots the cumulative distribution function of prior beliefs about discrimination against Asians. Compared to the finding of Bartoš et al. (2016), which indicates that a job seeker with an Asian-sounding name has to send on average 20 applications to receive one interview invitation, almost 70.5 percent of participants underestimate ethnic discrimination, while

[25]Regressions supporting the above findings about subjects who quit the follow-up survey or chose not to participate in it are available upon request.

[26]The respective number for a median subject is 13 applications.

10.2 percent are correct about it. This finding contrasts markedly with the result by Haaland and Roth (2021), who compare their American subjects' priors against Bertrand and Mullainathan's finding (2004) and find that only 35 percent of individuals underestimate racial discrimination in the U.S. labor market.

For our further analysis, it is important to compare people's beliefs to the source estimate because this is the number that our subjects subsequently see. From now on, we refer to subjects as ex-ante underestimators (overestimators) if their initial belief is lower (higher) than *14* applications. We find that 51.2 percent of individuals underestimate discrimination against Asians, while 46.8 percent of individuals overestimate it. A median underestimator and overestimator believe that an Asian applicant has to send 10 and 20 applications, respectively.

Our classification based on a prior belief does not set apart respondents who may believe in more favorable or equal treatment of Asians in the Czech labor market. It may be more difficult to persuade such individuals because they are not wrong about the extent of discrimination but about the existence of discrimination per se. We find that about 15.6 percent of participants believe that an Asian has to send *fewer* applications than a Czech person with the same qualifications, whereas 2.9 percent believe that Czech and Asian job seekers are treated equally[27]. Low-educated individuals are more likely to believe that employers treat Asians more positively or at least equally, with 22.7 percent stating a number below or equal to 7.5 applications. This fraction constitutes a 49.3 percent increase relative to the fraction of higher-educated individuals with similar beliefs.

In addition to discussing the overall heterogeneity of prior beliefs, it is worth mentioning how prior beliefs differ depending on subjects' background characteristics. Figure 1.A.2 illustrates that, compared to male subjects, females believe that a job seeker with an Asian sounding name has to an additional 1.5 applications (on average) in order to be invited for one interview ($p<0.01$). Participants from Prague and those with a university degree also believe in more discrimination against local Asians ($p<0.10$). Relative to their counterparts, employed subjects[28] and participants with above-median income believe that an Asian person has to send 1.20 and 1.25 *additional* applications on average for one interview invitation ($p<0.05$ and $p<0.01$, respectively). In contrast to Haaland

[27]The number of applications (7.5) that a job seeker with a Czech-sounding name has to send on average for one interview invitation was taken from the study by Bartoš et al. (2016).

[28]The comparison group is composed of the unemployed, retired, subjects on parental leave or engaged in housework, students and others.

and Roth (2021), we do not find differences in beliefs about discrimination based on political orientation, which may suggest lower political polarization in Czech people's views on ethnic/racial inequalities compared to Americans. Multiple research evidence (e.g. Draca and Schwarz, 2021; Boxell, Gentzkow, and Shapiro, 2020) indicates a more pronounced political divide in the US relative to other countries. Finally, we do not find that subjects who had a direct contact with Asians in the past perceive the extent of discrimination differently than those with no previous exposure (p=0.45).

Result 1: *An average person is unlikely to be surprised by provided information because his or her prior belief about the extent of discrimination against Asians is fairly close to the source estimate. However, the average belief masks a substantial amount of heterogeneity. Compared to the source estimate, about half of people underestimate to different degrees the extent of local labor market discrimination against Asians.*

1.4.2 Treatment effects on beliefs

We start by presenting graphical evidence indicative of existing information treatment effects and, more importantly, of differences in updating depending on a source whose estimate subjects see. Figure 1.2 shows the kernel densities of posterior beliefs collected in the main experiment. The modes of densities for Exogenous-Info treatments shift[29] markedly in the direction of the number that participants saw. The spikes around 14 applications and reduction in belief uncertainty are most noticeable in groups that were given information from experts [30]. Figure 1.A.4 illustrates persistent but smaller differences across information sources in posterior beliefs elicited one week later.

[Figure 1.2 here]

To establish the information source effects more formally, we present the results in a regression framework, where, in the even-numbered columns of Table 1.1, we include the pre-specified covariates[31]. Panel A shows that mean posterior beliefs are not in general

[29]For comparison, Figure 1.A.3 illustrates the densities of prior beliefs that look very similar across treatment arms due to the virtue of randomization.

[30]Differences *between* densities for Experts-Info groups are less apparent (but significant from Laymen-Info group) if we limit the analysis to individuals who participated in both parts of the experiment. The results are available upon request.

[31]Eventually, we found the inclusion of income dummies to be more reasonable instead of coding household income as the log of the interval chosen by a respondent (which was pre-specified). We also slightly deviated from the pre-analysis plan by recoding beliefs lower than 1 application. However, the fraction of subjects with such estimates never exceeds 1 percent, and we describe below in the robustness checks the exclusion of participants with seemingly unreasonable beliefs.

statistically distinguishable between the control and treatment groups. As was anticipated, an average person whose prior belief closely resembled the source estimate was not greatly surprised by information from a source.

Splitting the sample into underestimators and overestimators[32] based on comparison with the *provided* information (Panel B of Table 1.1) reveals significant adjustments in treated participants' beliefs in expected directions[33]. The effect of exogenous information about the extent of ethnic discrimination exists in all treatments, which is in line with Haaland and Roth (2021). Our novel result is that a source that provides information matters for the strength of the information effect. Specifically, in the main experiment, underestimators who receive information from ordinary people raise their estimates on average by 0.8 applications, which represents a 9 percent increase compared to the control mean ($p<0.05$). At the same time, underestimators who learn from HR managers (researchers) increase their beliefs by 2 (1.8) applications or equivalently by 22 (20) percent compared to the control mean ($p<0.01$). Both changes are significantly different from the Laymen-Info group increase. Overestimators who obtain information from ordinary people reduce their estimates on average by 2 applications or by 8.3 percent compared to the control mean ($p<0.05$). Overestimators from Practitioners- and Researchers-Info group decrease their beliefs by 3.5 and 2.7 applications or by 14.5 and 11 percent relative to the control mean ($p<0.01$). The former change is marginally significantly different from the Laymen-Info group decrease.

In the follow-up, we continue to observe highly significant information treatment effects on both underestimators and overestimators. Differences in belief responses to information from experts relative to information from ordinary people become less pronounced and fall short of statistical significance. Nevertheless, it is likely that in the case of overestimators, the results are influenced by the presence of outliers. In Table 1.B.10, we exclude these subjects instead of top- and bottom-coding their beliefs. Focusing on this subsample (N=2136) reveals a 6.7 and 5.9 percentage point reduction in beliefs of overestimators who learn from HR managers and researchers *in addition to* a downward adjustment that subjects in the Laymen-Info group make ($p<0.05$ and $p<0.10$, respectively).

[32]This category includes both overestimators, i.e. subjects with prior belief above 14 applications, and subjects whose prior coincided with a source estimate. However, the latter subgroup makes up only about 2 percent of our sample, and the results remain virtually the same if we exclude these subjects.

[33]Heterogeneity analysis by a prior belief was a part of the pre-analysis plan. See Tables 1.B.7-1.B.9 for the results of exploring differences in belief updating along other pre-specified dimensions, such as confidence in a prior belief, previous exposure to Asians, and political affiliation.

[Table 1.1 here]

Next, we investigate individual-level patterns in belief updating depending on a source that provides information. For this exercise, we exploit our experimental design that allows us to measure posterior beliefs both instantly and with a delay. Although the outcomes we analyze were not pre-specified, they are closely connected to the hypotheses stated in our pre-analysis plan. Furthermore, studying *which* individuals are affected more by experts' opinions on discrimination relative to ordinary people's views may provide useful insights for policymakers.

In this respect, Table 1.2 classifies subjects from the Exogenous-Info groups into four categories. We omit comparisons to the Control group from the subsequent analysis because our current focus is on differences between individual information sources. First, we consider participants who update their beliefs immediately and retain information, i.e. state the same posterior belief or partly move to their prior belief in the follow-up. The message from HR managers and researchers increases the share of such subjects by 7 and 10 percentage points, respectively, compared to the group that saw the message from ordinary people ($p < 0.01$, Laymen-Info group mean = 10%). Second, we focus on the other extreme - participants who stick to their prior belief both in the main experiment and in the follow-up survey. Subjects from Laymen-Info group never update their beliefs in 21 percent of cases. The percentage of non-updaters decreases to 13% and 14% in Practitioners- and Researchers-Info treatment arm, respectively ($p < 0.01$). The remaining categories include subjects (i) who update beliefs with a lag or (ii) who update beliefs initially but forget information with time, i.e. return to their preconceptions in the follow-up. We do not find systematic differences in the shares of such subjects across Exogenous-Info treatment arms[34]. Similarly, we do not find that subjects randomly assigned to different sources are more or less likely to be unclassified, e.g. to shift their beliefs away from the signal value both in the main experiment and follow-up[35].

[34] An exception is a *lower* fraction of subjects with delayed updating in the Researchers-Info group relative to the Laymen-Info group ($p<0.10$).

[35] The percentages of immediate updaters and non-updaters in the Control group are 4% and 27%, respectively. About 43% (2%) of untreated participants shifted their beliefs with a lag (updated initially but returned to their priors later). One fourth of the Control Group are unclassified, which is significantly higher than in any of the Exogenous-Info treatments. Untreated respondents may have changed their beliefs for several reasons, e.g. eventually understood the elicitation task better or forgot their prior belief recorded one week earlier. Fuster et al. (2022) discuss further possibilities of belief revision when no signal is sent. Note, however, that the information treatment effects we observe are *above* those that may arise from pure forgetting, anchoring or lack of initial comprehension. This could be seen from the Laymen-Info group means presented above.

[Table 1.2 here]

Result 2: *Individuals update their beliefs about ethnic discrimination more strongly when they obtain an expert's estimate relative to ordinary people's estimate. Larger responses of people's beliefs to information from HR managers and researchers persist over a one-week period.*

In the remainder of this subsection, we discuss the robustness of the above result. First, note that the effects of the information sources on updating outcomes remain virtually the same if we include the pre-specified covariates in the regressions (Tables 1.1 and 1.2). Furthermore, our findings are robust to excluding (i) subjects with extreme beliefs (i.e. those who state prior and/or posterior beliefs below 1 or above 50), (ii) subjects who report searching for an answer after learning about the CERGE-EI researchers' study[36], and (iii) the top and bottom 2 percent of the survey time distribution (see Appendix Tables 1.B.10, 1.B.11 and 1.B.12). The results are qualitatively similar if we solely focus on attentive subjects (Table 1.B.13), but we do not use this specification as the preferred one for the following reasons[37]. First, Table 1.B.14 shows that passing the attention check is correlated with a number of observables. Thus, excluding inattentive respondents would make our sample less representative. More importantly, selecting subjects on the basis of a post-treatment attention check could affect the balance across groups[38]. In addition to the above robustness checks, we run probit regressions (where appropriate) instead of OLS and find no qualitative differences (the results are available upon request).

1.4.3 Treatment effects on attitudes, donations and willingness to share information

Table 1.3 shows the effects of information from different sources on self-reported attitudes to Asians collected in the main experiment (Columns 1-4) and in the follow-up survey (Columns 5-8). All outcomes are standardized and recoded such that higher values imply better attitudes to Asians. Overall, the attitudes do not respond to information about

[36] Only 4.39 percent of follow-up participants indicate that they were looking for the study results.

[37] 29 (27.9) percent of our main-experiment (follow-up) sample did not pass the attention check. The corresponding percentages documented by other researchers who use diverse national samples are below 30 percent, e.g. 19.9 percent and 27.7 percent in Berinsky, Margolis, and Sances (2014), and 22.4 percent in Haaland and Roth (2020).

[38] We chose to administer it after most of our main outcomes were collected to keep participants continuously focused. Subjects were informed at the very beginning of the main experiment that their attention would be checked at some point of the survey.

discrimination regardless of the source that provides it[39]. This result is consistent with some other studies that find no or ambiguous effects of information on self-reported measures (Barrera et al., 2020; Hopkins, Sides, and Citrin, 2019; Haaland and Roth, 2021; Lergetporer, Piopiunik, and Simon, 2021; Barnes et al., 2018; Kuziemko et al., 2015).

[Table 1.3 here]

A plausible explanation for no treatment effects on mean attitudes could be that ex-ante underestimators and overestimators shift attitudes to Asians in opposite directions consistent with their changes in beliefs. However, we do not observe counteracting information effects when the sample is split by a prior belief (Panel B of Table 1.3). Another reason for insignificant effects could be that shifts in attitudes require *additionally* correcting people's misperceptions about the level of support that Asians receive from the Czech government, an Asian-specific criminality rate, the share of Asians who could speak Czech, etc. Alternatively, it is possible that people's attitudes to minorities are formed by emotional experiences and are largely independent of beliefs about minorities' characteristics and their treatment.

Columns 1 and 2 of Table 1.4 show the treatment effects on charity donations (in the main experiment and follow-up survey, respectively). We do not observe that information from any source significantly affects the share of subjects (22.5% in the main experiment and 20% in the follow-up) who decide to donate their experimental earnings to a pro-Vietnamese charity[40]. Column 3 of Table 1.4 illustrates whether individuals' willingness to share information with friends that they obtained in the first wave differs across the three Exogenous-Information groups. Coefficients on Experts-Info treatment indicators are positive but small in magnitude and insignificant.

[Table 1.4 here]

[39] Although subjects who receive information from ordinary people become more likely to disagree that Asians bring more disadvantages than advantages to the local labor market ($p<0.01$), we do not focus on this finding for two reasons. First, other attitudinal questions do not support this conclusion, and second, the positive effect drops in magnitude and becomes insignificant in the case of the reformulated question in the follow-up survey (Column 6 of Table 1.3).

[40] In contrast to previous studies where participants were asked to (partially) donate a windfall income (Haaland and Roth, 2021; Alesina, Miano, and Stantcheva, 2018; Grigorieff, Roth, and Ubfal, 2020), we asked subjects to donate their *earnings* from the experiment. Although some research (e.g. Bekkers, 2007) has found that most individuals are reluctant to fully sacrifice their earnings, we still decided in favor of this donation measure due to its higher external validity.

Result 3: *Self-reported attitudes to Asians and donations of one's own experimental earnings to a pro-Vietnamese charity are generally unresponsive to information about the extent of local labor market discrimination against Asians. Willingness to share information with friends is not significantly affected by a source that provides this information.*

1.4.4 Information choices

Figure 1.3 illustrates the distribution of information options that were ranked first by experimental participants[41]. About 38 (32) percent of subjects favored the HR managers' (researchers') average estimate of the number of applications an Asian job seeker has to send to receive one interview invitation. Almost 23 percent of participants ranked the corresponding estimate of ordinary people as the top alternative. The frequencies of choosing three information sources significantly differ at the 1% level from frequencies that would be observed if subjects were randomizing uniformly across three options. Apparent preference for the experts' opinion suggests consistency, at least at the aggregate level, between people's information choices and their updating behavior when an information source is randomly manipulated. We present further evidence reinforcing this finding later in the subsection.

[Figure 1.3 here]

It is worth mentioning that only a *small* fraction of individuals (7 percent) do not want to see any estimate of discrimination against local Asians. This observation could be interesting given that information available for choice concerns an emotionally-charged issue[42]. Table 1.B.15 presents the relationships between subjects' observable characteristics and choosing no information. Table 1.5 shows the determinants of preferences for

[41] Recall that a random half of the Info-Choice group had to predict the beliefs of three sources about the extent of local discrimination against Asians *before* they selected a preferred piece of information. Figure 1.A.5 illustrates that this additional belief elicitation task does not significantly affect the distribution of subjects' information choices relative to the subgroup for whom this task was omitted (p=0.6). Hence, we use data from the whole Info-Choice group in our analysis. During the experiment, some participants experienced difficulties with ranking information choices that was presented as a drag-and-drop task. Therefore, we had to change the question format, which seems to influence significantly the distribution of information choices (Figure 1.A.5, p<0.01). However, the differences between distributions may have resulted from a programming error that was fixed *after* the info-choice question was modified. To check this intuition, we examine data from the Control group in which no programming error occurred. A random half of this group was asked to rank the same four information options but was not given an opportunity to see any information. Restricting attention to this subsample, we do not find significant differences between distributions of the information options before and after the respective question change (Figure 1.A.5, p=0.13).

[42] In paper by Fuster et al. (2022), 4.3 percent of subjects preferred no information about *home prices*.

information sources[43]. Similar to Fuster et al. (2022), we find that only a handful of observable characteristics predict information choices. Intuitively, less-educated subjects are more likely to favor ordinary people's estimate of discrimination against Asians over experts' estimate ($p<0.10$ and $p<0.05$ relative to participants with middle and high education, respectively). Specifically, these individuals decide to learn from ordinary people in 29 percent of cases, while higher-educated individuals choose similarly in 20 percent of cases. Subjects with low income are more likely to decide in favor of acquiring information from a source other than HR managers. Finally, focusing on subjects who prefer one type of experts over the other, we find that older respondents, subjects with right-wing orientation and those with more confidence in their prior belief are *less* likely to choose researchers ($p<0.10$)[44].

[Table 1.5 here]

Education was identified as the only observable that determines the strength of subjects' preference for information from experts. Table 1.B.16 provides complementary evidence by further exploiting participants' ranking of information options. Conditional on choosing either type of experts (i.e. researchers or HR managers) as the preferred information option, low-educated subjects are 10 percentage points *more* likely to rank ordinary people - but not the other type of experts - as the second-best alternative (mean = 33%, $p < 0.05$). The coefficient of interest barely changes if multiple covariates are included.

The main result of this subsection is that people tend to choose information sources that will shift beliefs about discrimination most. Consistent with this finding, low-educated individuals who exhibit a stronger preference for information from ordinary people should also update more in response to a lay opinion on discrimination compared to higher-educated individuals. Table 1.6 splits the sample (excluding the Info-Choice group) by education. In the main experiment, low-educated subjects who see ordinary people's estimate move their beliefs on average by 1.69 applications, while higher-educated subjects shift their beliefs by 0.95 applications ($p < 0.05$ from a raw comparison of means and $p < 0.10$ from a model with the interaction term). This difference arises not because

[43]This table uses data from a random half of Info-Choice group to include a gap between one's prior and a predicted belief of a source. We return to this variable in Section 1.5. The regression was pre-specified.

[44]Higher age and right-wing orientation do not significantly predict the choice of researchers if we run a multinomial logit instead of linear probability models. Other correlations established in Table 1.5 seem to be robust.

low-educated individuals are more easily swayed by others' opinions. In fact, updating[45] by subjects who see either experts' estimate does not differ significantly depending on their education.

Choosing ordinary people's estimate at a higher rate implies, in our context, that low-educated participants will differentiate less among the three sources relative to higher-educated participants. The follow-up patterns in updating (Columns 4-6 of Table 1.6) support this intuition. One week later, significant differences in belief shifts across information sources persist only for higher-educated participants. In contrast, low-educated individuals update similarly regardless of a randomly assigned information source.

[Table 1.6 here]

Result 4: *Individuals predominantly choose to consult experts, i.e. the more influential information source, while learning about the prevalence of local discrimination. Only a few observable characteristics, particularly education, are correlated with information choices.*

1.5 Connection to theories

We now discuss the agreement of our experimental results with theoretical predictions outlined below. The experiment was designed to allow differentiation between three theories that highlight the role of source accuracy, social distance, and confirmation bias, mostly with the help of descriptive and correlational evidence.

1.5.1 Perceived accuracy

One of our hypotheses was that source accuracy considerations underlie the strength of causal updating and information preferences. In light of our findings, this should imply that individuals perceive experts' opinion on discrimination to be more accurate relative to ordinary people's opinion. Figure 1.A.6 compares subjects' judgements regarding each source accuracy. The average estimates of researchers and HR managers are perceived to

[45] Even though this analysis is exploratory, it naturally follows our finding on the role of education in determining people's information preferences. Updating is defined as an absolute difference between a person's posterior belief and his/her prior estimate of discrimination. 93.7 (87.3) percent of main-experiment (follow-up) participants who shifted their beliefs in response to exogenous information from a source updated in a logical direction.

be significantly more accurate than the average estimate of ordinary people ($p<0.01$)[46,47].

Another way to explore the role of accuracy in explaining our results is to look at the reduction of uncertainty in subjects' beliefs about discrimination that should follow from seeing a source opinion. After eliciting prior beliefs in the main experiment and posterior beliefs in the follow-up, we asked subjects to state on a 5-point Likert scale how confident they are in their estimate. Although this task provided us with a less granular measure of belief uncertainty compared to asking subjects to assign percent chances to different alternatives (i.e. ranges of estimates), we preferred the Likert-scale question due to its simplicity and efficiency. Based on our data, we construct *several* measures of uncertainty reduction to validate the conclusions of this exploratory analysis.

In line with earlier evidence on experts' higher perceived accuracy, individuals who receive information from experts become (insignificantly) more sure about their estimate of local discrimination relative to those who receive information from ordinary people (Table 1.7, $p=0.12$)[48]. The effect is somewhat larger and marginally significant for the Researchers-Info group, in which subjects' posterior uncertainty decreases by almost 0.10 of a standard deviation ($p=0.10$). We also observe that a fraction of subjects who become more confident in their belief about discrimination grows by 4.4 percentage points after subjects see information from experts (Laymen-Info group mean = 29%, $p = 0.07$).

The final piece of evidence in favor of the accuracy explanation concerns posterior uncertainty among different educational groups. Recall that low-educated subjects do not differentiate much between experts' estimate of discrimination and ordinary people's es-

[46]It should be noted here that additional randomization (which divided follow-up participants into those who answered questions on accuracy and social distance and those for whom these questions were omitted) was not successful. The omission was needed to ensure that *subsequent* posterior belief elicitation was not influenced by subjects' judgements in relation to source characteristics. Table 1.B.17 shows more covariate imbalances across two large subsets relative to main randomization. Nevertheless, data on the perceived accuracy of three sources collected in the pilot (where *all* follow-up subjects answered the question of interest) confirm the finding about higher accuracy of experts (see Figure 1.A.9).

[47]In the pre-analysis plan, we specified to test whether subjects who perceive an information source to be very accurate or accurate (very inaccurate or inaccurate) respond more strongly (weakly) to information from this source. However, we do not perform the pre-specified heterogeneity analysis because information treatment seems to affect the perception of the source accuracy in some cases (see Table 1.B.18 for details). We refrained from collecting the data on accuracy and social distance in the main experiment to avoid priming subjects to think thoroughly about the information sources *before* they were treated. We planned to use information from the Control group only if the treatment contaminated perceptions of accuracy and social distance. However, in that case, we were able to investigate only general patterns (such as Figure 1.A.6 presents) but not heterogeneity.

[48]Table 1.B.5 demonstrates that confidence in a prior belief is balanced across treatment arms.

timate, which is noticeable in their belief updating and information choices. Conversely, higher-educated subjects seem to place higher importance on experts' opinion and thus this source should reduce their posterior uncertainty more strongly. We do not find that a *gap* between average confidence of subjects who receive ordinary people's estimate and those who receive experts' estimate is smaller if we restrict the analysis to the low-educated subsample (Table 1.B.20). However, we do find that the fraction of low-educated subjects who become more confident than initially is almost the same across the Exogenous-Info groups (Laymen-Info group mean = 32%, p = 0.98). In contrast, the fraction of higher-educated subjects who report belief uncertainty reduction is larger by a significant 7.8 percentage points if they were exposed to information from experts one week earlier (Laymen-Info group mean = 27%, p = 0.01).

[Table 1.7 here]

Overall, perceived source accuracy seems to be a plausible explanation for our findings[49]. Individuals tend to consider experts who estimate the extent of ethnic discrimination on the Czech labor market to be more accurate relative to ordinary people. The complementary result is that posterior uncertainty decreases more if individuals receive experts' estimate. This decrease is driven by the higher-educated subsample whose beliefs respond more to a message from experts and who choose this source more frequently.

1.5.2 Social distance

Our second hypothesis was that people's beliefs will be moved most by a source with the lowest social distance, which will also be their modal information choice. In light of our findings, this should imply that individuals perceive experts to be socially closer relative to ordinary people. Figure 1.A.7 compares self-reported likelihood of friendship with an ordinary person to likelihood of friendship with an HR manager or researcher who primarily studies issues that ethnic minorities face in the Czech Republic. The graphical comparison clearly indicates that subjects consider themselves to be much more socially *distant* from experts than from ordinary people ($p < 0.01$ from the raw comparison of means).

Next, we examine the relevance of social distance theory for explaining our findings using

[49] Appendix 1.C discusses why a non-trivial portion of individuals may have chosen information from ordinary people, the source with the lowest *average* accuracy.

a regression in which we interact a social distance dummy with each treatment indicator[50]. We exclude the Practitioners-Info group from the interpretation of results because this treatment arm seems especially affected by imperfect *additional* randomization (see footnote 46) implemented in the follow-up. Hence, patterns in belief updating of subjects who were assigned to this group and did not see the questions on accuracy and social distance do not replicate findings discussed earlier. Table 1.B.21 illustrates that subjects who report above-median social distance from researchers are less likely to shift their beliefs in the main experiment in response to information from this source (p=0.06). A coefficient on the interaction term is similar in magnitude but more noisy in the follow-up (p=0.28). At the same time, social distance does not seem to significantly influence the updating behavior of subjects in the Laymen-Info group.

The regression results suggest that social distance, if anything, *reduces* the gap between belief responses to information from researchers compared to information from ordinary people. At the same time, social distance theory does not provide insight into the existence of the gap per se. Our primary finding does not confirm this theory prediction: information from experts, a *more* socially distant group relative to ordinary people, causes stronger belief responses and is chosen more frequently.

1.5.3 Confirmation bias

Finally, we hypothesized that people will acquire information from a source that is most likely confirm their original perception of local discrimination. In the light of our findings, this should imply that individuals expect both experts' estimates of discrimination to be closer to their prior belief compared to ordinary people's estimate[51]. Figure 1.A.8 depicts prior beliefs about the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation along with the corresponding *predicted* beliefs of the three information sources[52]. According to subjects' predictions, experts

[50]Table 1.B.19 shows that information treatment does not affect social distance from any source. We pre-specified the heterogeneity analysis by social distance. We planned to use a posterior belief as an outcome, but eventually we used updating to increase statistical power by *not* dividing the sample into over- and underestimators. We also planned to use three levels of social distance, which leads to almost no variation in a dummy variable that is equal to 1 if a subject is unlikely or very unlikely to befriend an ordinary person. Hence, we split the sample by *median* social distance.

[51]Confirmation bias may also manifest itself in a decision to acquire no information to avoid challenging one's prior belief. Recall that a *handful of* our experimental participants go for the no-information option.

[52]The comparison is based on the data from a random half of the Info-Choice group. We also asked untreated participants in the follow-up (N = 570) to predict the beliefs about discrimination against Asians for each of three sources. The comparison of prior beliefs and predicted source estimates looks

disagree on the extent of local labor market discrimination against Asians. In particular, an average subject expects HR managers to state 14.06 applications, while the corresponding number for researchers is 15.98 applications ($p<0.01$). Furthermore, an average participant believes that his or her prior belief is the closest to *researchers'* respective belief ($p=0.74$) and is the furthest from *HR managers'* belief ($p<0.01$)[53].

When further examining the presence of confirmation bias in the regression framework, we do not find that lower (absolute) distance between a subject's prior belief and a source predicted belief is associated with stronger preference for the source (see Table 1.5). The results are similar if we additionally include a gap between a subject's prior belief and predicted belief of the *alternative* source which may also play a role when a person decides between two sources (see Table 1.B.22 that additionally excludes individuals with extreme beliefs). In an alternative specification (Table 1.B.23), we explore the role of a *relative* gap between one's prior belief and predicted belief of a source. We find that subjects whose prior belief is closer to the researchers' predicted estimate of discrimination relative to the predicted estimate of another source are 10 percentage points more likely to favor information from researchers. However, the respective coefficients fall short of statistical significance ($p=0.15$ and 0.13 when ordinary people and HR managers, respectively, serve as a comparison group)[54].

In general, confirmation bias does not appear to be a leading explanation for our findings. We find, at best, weak evidence indicating the presence of this bias in the choices of participants who decided to learn from researchers. In addition, this theory does not explain why subjects frequently select the HR managers' estimate of discrimination if they do *not* expect HR managers' beliefs to be consonant with their prior.

1.6 Conclusion

Recent discussions on anti-intellectualism provoked by Michael Gove's famous quote[55] have given rise to a commonly held belief that "the death of expertise" is real. However,

very similar for this group, and can be provided upon request.

[53]The difference between the mean prior belief and the mean predicted belief of ordinary people, 14.57 applications, is significant at the 5 percent level.

[54]We find some significant correlations between the distance of researchers' predicted estimate and the choice of that information source when we use a multinomial logistic regression. There is no evidence, though, in favor of confirmation bias in the case of other sources. The results are available upon request.

[55]During his interview with Faisal Islam at Sky News (June 3, 2016), Michael Gove said in relation to Britain's exit from the European Union that "people in this country have had enough of experts".

there seems to be no clear evidence from economic experiments, at least with representative samples, that the public is receptive to information from ordinary people rather than from intellectual elites. In this paper, we cast doubt on whether experts are no longer influential. Our design distinguishes itself from previous research by capitalizing on synergy between exogenous information provision from distinct sources and endogenous information acquisition. This enables us to address an important policy relevant question: Will individuals become *voluntarily* exposed to information from a source whose influence is the strongest when the message is unavoidable? Answering this question is important to prevent unnoticed information dissemination and thus wasteful governmental spending targeted at raising societal awareness on sensitive topics.

Our findings give reasons for both optimism and pessimism. The good news is that most individuals choose experts as their preferred source of information about ethnic discrimination, a topic that often evokes strong emotions. Moreover, experts, whose information individuals perceive to be relatively accurate, are more influential relative to ordinary people. Hence, there is consistency, at least at the aggregate level, between individuals' information choices and their causal belief updating. In this regard, our findings support the standard theory predictions. At the same time, information from experts, as well as information from ordinary people, does not affect individuals' self-reported attitudes to ethnic minorities and their donations to an ethnic minority charity. Null effects of information on self-reported measures corroborate some earlier research results (e.g. Haaland and Roth, 2021; Kuziemko et al., 2015).

This paper opens several potentially interesting avenues for future research. We study responses to expert and non-expert information and its acquisition in the context of ethnic discrimination. It is an open question as to how our results would extend to settings in which individuals are exposed to other sensitive issues, such as climate change, gun control or vaccination. Another possible extension is to focus messages on an ethnic minority whom society perceives *very* negatively. We have shown that, when there are merely unfavorable attitudes to a minority, individuals are generally willing to correct their initial misperceptions regarding the treatment that this minority faces. Clearly, this finding does not guarantee that information will not cause "backfiring effects", i.e. an increase in original misperceptions, in the case of a minority that is a common target of societal hatred and violence. Finally, it would be useful to vary the nature of provided information. It is plausible that the public learns from experts when they describe facts

but not when they give advice, due to the dislike of being preached to.

Our findings have important implications for information dissemination policies. If the goal of an information campaign is solely to raise awareness about the prevalence of local ethnic discrimination, sharing the opinion of practitioners or academic experts, who are perceived to be socially distant but relatively accurate, could contribute to higher effectiveness of the campaign. If a campaign aims instead to improve attitudes to ethnic minorities, correcting people's beliefs about the extent of discrimination against these minorities could be insufficient or even irrelevant.

Main figures

Figure 1.1: Main elements of the experimental design

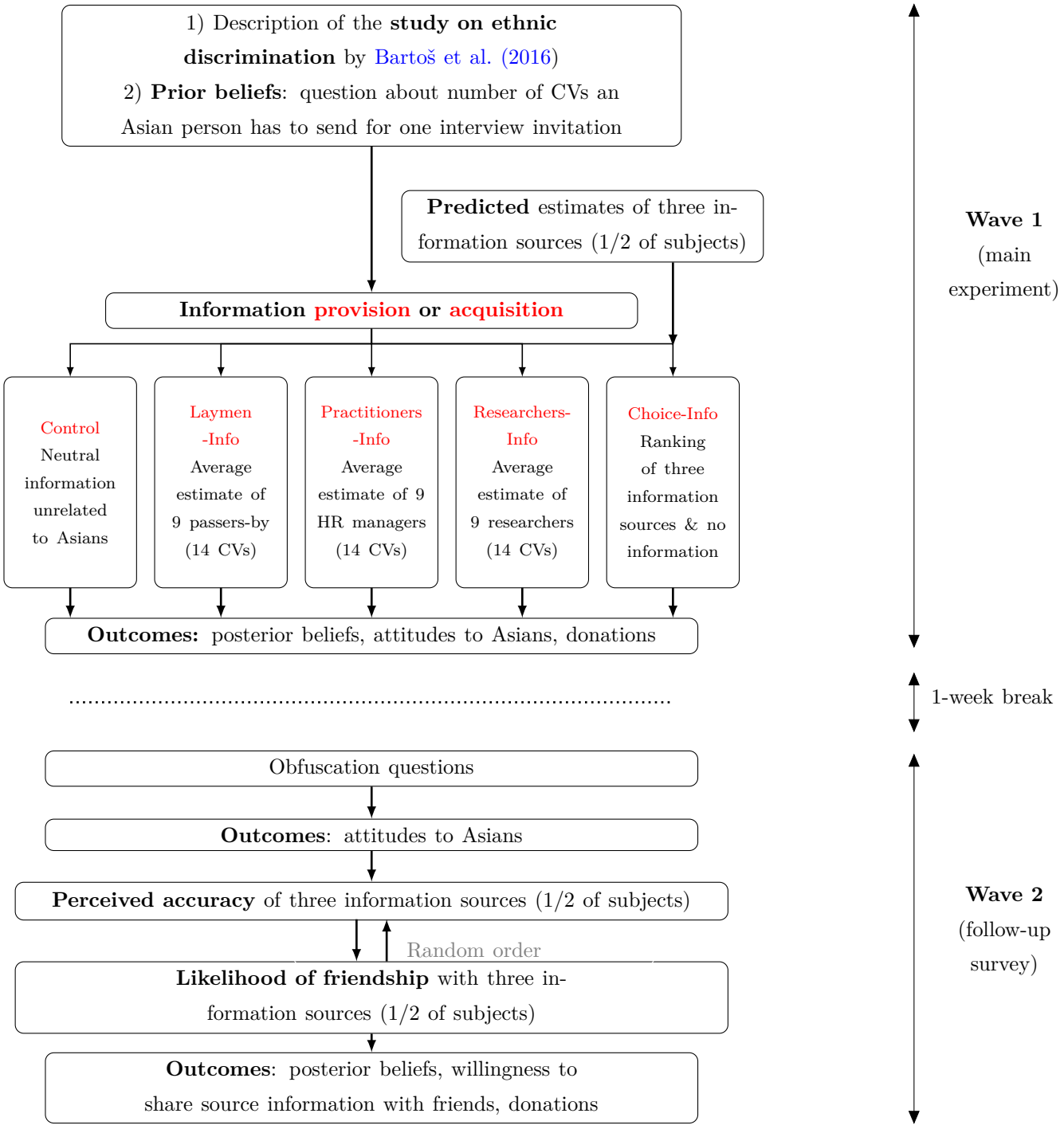
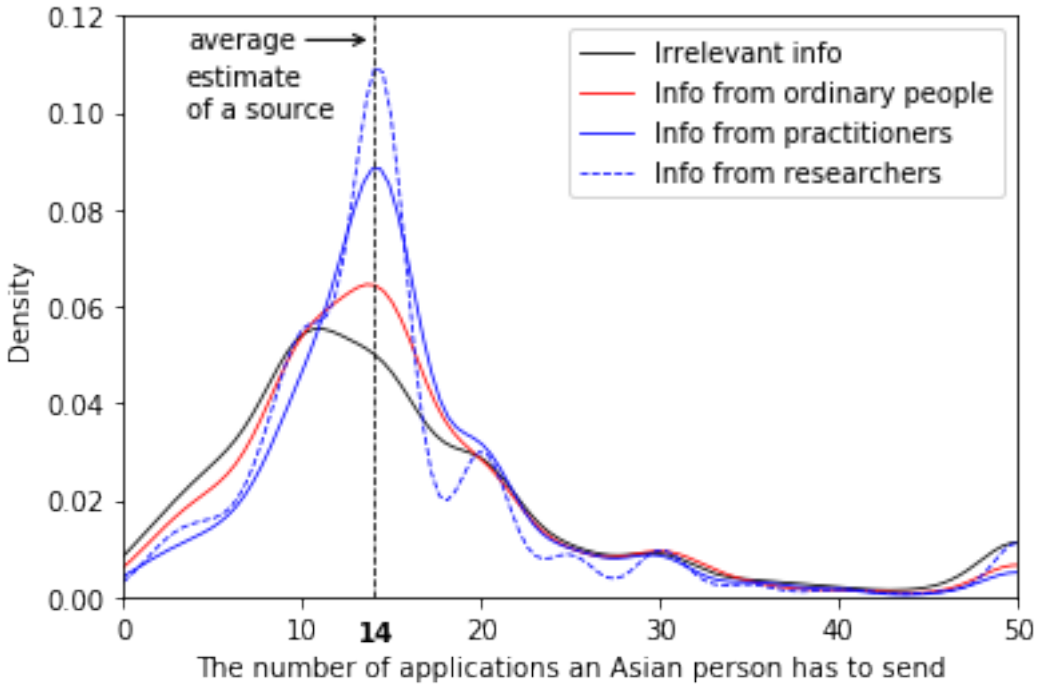
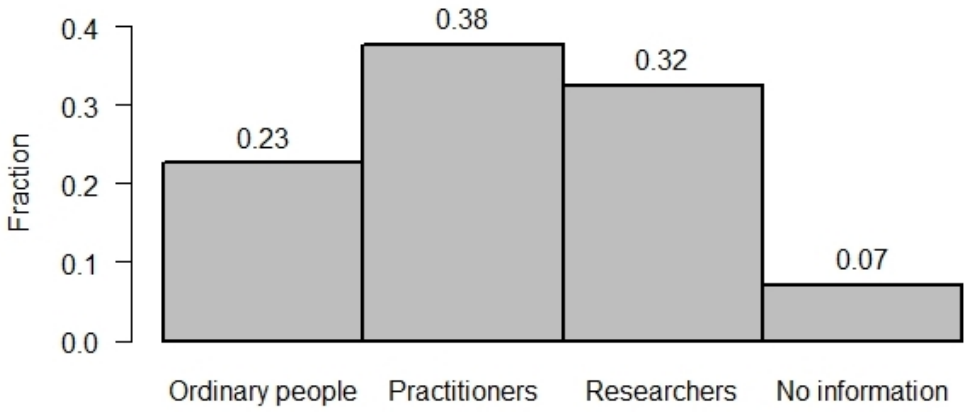


Figure 1.2: Posterior beliefs about discrimination against Asians (main experiment)



Notes: The figure plots the kernel densities of posterior beliefs using main-experiment data from the Control group and Exogenous-Info groups (N = 2,571). The dotted vertical line indicates the number of applications estimated by a source. Extreme beliefs (i.e. estimates higher than 50 and lower than 1) were re-coded accordingly.

Figure 1.3: Chosen information options



Notes: The figure shows the distribution of alternatives that participants in the Info-Choice group (N = 645) ranked as the most preferred. We often use interchangeably "practitioners" and "HR managers" in the figures.

Main tables

Table 1.1: Posterior beliefs about discrimination: main experiment and follow-up survey

	Posterior: main		Posterior: follow-up	
	(1)	(2)	(3)	(4)
Panel A: Main specification				
Laymen-Info	-0.68	-0.60	-0.45	-0.40
	(0.60)	(0.46)	(0.58)	(0.52)
Practitioners-Info	-0.51	-0.82*	-1.00*	-1.13**
	(0.57)	(0.46)	(0.56)	(0.52)
Researchers-Info	-0.71	-0.42	-0.77	-0.82
	(0.60)	(0.48)	(0.58)	(0.53)
Observations	2,571	2,571	2,233	2,233
Control mean	16.44	16.44	15.92	15.92
Covariates	No	Yes	No	Yes
Panel B: Prior heterogeneity				
Laymen-Info (a)	-2.08**	-2.03**	-3.04***	-3.15***
	(0.85)	(0.85)	(0.88)	(0.88)
Practitioners-Info (b)	-3.47***	-3.52***	-4.26***	-4.39***
	(0.81)	(0.80)	(0.88)	(0.88)
Researchers-Info (c)	-2.74***	-2.66***	-4.13***	-4.30***
	(0.89)	(0.89)	(0.89)	(0.88)
Underestimator	-15.08***	-14.91***	-10.91***	-10.84***
	(0.71)	(0.71)	(0.79)	(0.80)
Underestimator × Laymen-Info (d)	2.92***	2.83***	5.33***	5.42***
	(0.94)	(0.95)	(1.04)	(1.04)
Underestimator × Practitioners-Info (e)	5.44***	5.42***	6.35***	6.54***
	(0.90)	(0.91)	(1.01)	(1.02)
Underestimator × Researchers-Info (f)	4.51***	4.35***	6.76***	6.86***
	(0.97)	(0.97)	(1.05)	(1.05)
Observations	2,571	2,571	2,233	2,233
Intercept	24.11		21.43	
Covariates	No	Yes	No	Yes
p-value: $a = b$	0.06	0.05	0.10	0.10
p-value: $a = c$	0.42	0.45	0.15	0.13
p-value: $b = c$	0.34	0.27	0.86	0.91
p-value: $a + d = b + e$	0.00	0.01	0.70	0.83
p-value: $a + d = c + f$	0.02	0.02	0.56	0.62
p-value: $b + e = c + f$	0.62	0.59	0.31	0.46
p-value: $a + d = 0$	0.04	0.05	0.00	0.00
p-value: $b + e = 0$	0.00	0.00	0.00	0.00
p-value: $c + f = 0$	0.00	0.00	0.00	0.00

Notes: Underestimator equals to 1 if the value of an individual's prior belief is lower than 14. In columns (2) and (4), the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, confidence in a prior belief, municipality size, employment status, exposure to Asians, and political orientation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.2: Fractions of subjects with different updating patterns

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share of those who									
	updated immediately & retained information		updated beliefs with delay		updated immediately but forgot later		never updated beliefs		are unclassified	
Practitioners-Info	0.08***	0.07***	-0.02	-0.01	0.00	0.00	-0.08***	-0.08***	0.02	0.02
	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Researchers-Info	0.10***	0.10***	-0.06**	-0.05*	0.00	0.00	-0.07***	-0.07***	0.04*	0.03
	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	1,663	1,663	1,663	1,663	1,663	1,663	1,663	1,663	1,663	1,663
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Laymen-Info group mean	0.10	0.10	0.54	0.54	0.02	0.02	0.21	0.21	0.13	0.13

Notes: The regression uses data from three Exogenous-Info groups. Individuals who participated in both waves are included. We consider among those who *updated immediately & retained information* subjects who shifted their beliefs and (i) kept them at the same level over one-week period or (ii) moved to some extent back to their prior belief. Subjects who *updated their beliefs with delay* include those who initially kept their prior or did not update fully but moved their beliefs (more) in the follow-up. Subjects who *updated immediately but forgot later* returned to their prior belief over one-week period. Participants are considered *unclassified* if they update in a non-standard manner, for example move further from the signal in the main experiment but choose the opposite direction in the follow-up. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.3: Self-reported attitudes to Asians

	Main experiment				Follow-up			
	(1) Asians take jobs	(2) Asians bring disadvantages	(3) Asian neighbor	(4) Pro-Asian index	(5) harder to find job due to Asians	(6) Asians bring advantages	(7) Asian neighbor	(8) Pro-Asian index
Panel A: Main specification								
Laymen-Info	0.055	0.153***	0.021	0.077*	0.060	0.038	-0.029	0.023
	(0.055)	(0.055)	(0.052)	(0.042)	(0.056)	(0.057)	(0.057)	(0.041)
Practitioners-Info	-0.018	0.032	-0.031	-0.006	0.033	-0.048	-0.036	-0.017
	(0.056)	(0.056)	(0.054)	(0.044)	(0.058)	(0.058)	(0.058)	(0.042)
Researchers-Info	0.034	0.067	-0.051	0.017	0.025	-0.013	-0.025	-0.004
	(0.054)	(0.054)	(0.053)	(0.042)	(0.057)	(0.058)	(0.056)	(0.040)
Observations	2,571	2,571	2,571	2,571	2,233	2,233	2,233	2,233
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Prior heterogeneity								
Laymen-Info	0.098	0.144*	0.025	0.089	-0.005	0.021	-0.062	-0.015
	(0.080)	(0.080)	(0.073)	(0.060)	(0.075)	(0.080)	(0.081)	(0.055)
Practitioners-Info	-0.019	-0.008	-0.050	-0.026	0.042	-0.009	-0.095	-0.021
	(0.081)	(0.083)	(0.075)	(0.063)	(0.079)	(0.080)	(0.082)	(0.058)
Researchers-Info	0.122	0.058	-0.023	0.052	-0.069	-0.011	0.013	-0.023
	(0.078)	(0.083)	(0.076)	(0.061)	(0.078)	(0.082)	(0.081)	(0.057)
Underestimator	-0.023	-0.032	0.011	-0.015	-0.203**	0.082	-0.078	-0.066
	(0.076)	(0.076)	(0.075)	(0.058)	(0.081)	(0.082)	(0.080)	(0.057)
Underestimator × Laymen-Info	-0.084	0.017	-0.008	-0.025	0.127	0.031	0.063	0.074
	(0.110)	(0.110)	(0.105)	(0.085)	(0.112)	(0.114)	(0.114)	(0.081)
Underestimator × Practitioners-Info	0.005	0.079	0.038	0.041	-0.019	-0.079	0.118	0.007
	(0.111)	(0.112)	(0.106)	(0.087)	(0.116)	(0.116)	(0.115)	(0.084)
Underestimator × Researchers-Info	-0.168	0.018	-0.053	-0.068	0.185	-0.003	-0.072	0.037
	(0.108)	(0.109)	(0.106)	(0.084)	(0.113)	(0.115)	(0.112)	(0.080)
Observations	2,571	2,571	2,571	2,571	2,233	2,233	2,233	2,233
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS in all columns in both Panels. The outcomes mentioned in columns (1)-(3) and (5)-(7) were measured on a scale from 1: "Strongly agree" to 5: "Strongly disagree", and *Asians bring advantages* was re-coded so that higher values mean more positive attitudes to Asians. These outcomes are z-scored using respective means and standard deviations in the Control group. *Pro-Asian index* is an unweighted average of the outcomes mentioned in the previous three columns. Both indices and covariates included in all regressions were pre-specified. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.4: A decision to donate earnings and willingness to share information with friends

	Main experiment	Follow-up	
	(1) Donation	(2) Donation	(3) Share with friends
Panel A: Main specification			
Laymen-Info	-0.005	-0.007	
	(0.022)	(0.023)	
Practitioners-Info	-0.003	-0.006	0.027
	(0.022)	(0.023)	(0.061)
Researchers-Info	-0.034	-0.019	0.057
	(0.021)	(0.022)	(0.060)
Observations	2,571	2,233	1,663
Control mean	0.225	0.202	
Covariates	Yes	Yes	Yes
Panel B: Prior heterogeneity			
Laymen-Info	-0.002	-0.020	
	(0.032)	(0.031)	
Practitioners-Info	0.029	0.003	0.010
	(0.033)	(0.032)	(0.084)
Researchers-Info	-0.021	-0.035	0.022
	(0.032)	(0.030)	(0.086)
Underestimator	0.029	-0.024	-0.106
	(0.031)	(0.032)	(0.086)
Underestimator × Laymen-Info	-0.008	0.029	
	(0.044)	(0.049)	
Underestimator × Practitioners-Info	-0.057	-0.020	0.033
	(0.038)	(0.043)	(0.120)
Underestimator × Researchers-Info	-0.026	0.035	0.066
	(0.042)	(0.05)	(0.121)
Observations	2,571	2,233	1,663
Covariates	Yes	Yes	Yes

Notes: In columns (1) and (2) probit, marginal effects, standard errors in parentheses. The outcome is a binary variable indicating whether a respondent decided to donate his/her earnings from the experiment to a pro-Vietnamese charity. In column (3) OLS, robust standard errors in parentheses. The outcome was measured on a scale from 1: "Very willing" to 5: "Very unwilling" and re-coded so that higher values mean higher willingness to share information with friends. This outcome is z-scored using the mean and standard deviation in the Laymen-Info group. All regressions include pre-specified controls. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.5: Correlates of preferences for information sources

	Equals to 1 if chose ...			
	(1) ordinary people over experts	(2) HR managers over ordinary people	(3) researchers over ordinary people	(4) researchers over HR managers
Ordinary-prior belief gap	0.001			
	(0.003)			
HR-prior belief gap		0.002		
		(0.005)		
Research-prior belief gap			-0.001	-0.002
			(0.005)	(0.004)
Male	0.034	-0.048	-0.059	-0.034
	(0.054)	(0.076)	(0.081)	(0.072)
Age	-0.002	0.004*	0.000	-0.004*
	(0.002)	(0.002)	(0.003)	(0.002)
Middle education	-0.098*	0.114	0.121	0.023
	(0.059)	(0.081)	(0.085)	(0.077)
High education	-0.153**	0.201*	0.190*	0.058
	(0.073)	(0.104)	(0.106)	(0.098)
Prague	-0.005	-0.041	0.019	0.133
	(0.107)	(0.160)	(0.144)	(0.146)
2nd income quartile	-0.075	0.227*	-0.058	-0.265**
	(0.100)	(0.135)	(0.136)	(0.125)
3rd income quartile	-0.150*	0.305***	0.028	-0.314***
	(0.089)	(0.114)	(0.130)	(0.101)
4th income quartile	-0.110	0.290**	-0.069	-0.407***
	(0.097)	(0.128)	(0.137)	(0.119)
Income missing	0.075	0.026	-0.198	-0.249
	(0.130)	(0.161)	(0.169)	(0.165)
Employed	-0.012	0.005	0.033	0.023
	(0.056)	(0.077)	(0.087)	(0.072)
Right-wing oriented	0.036	0.021	-0.133	-0.139*
	(0.063)	(0.077)	(0.096)	(0.080)
Household size	-0.012	-0.006	0.049	0.049
	(0.024)	(0.034)	(0.037)	(0.031)
Above-median municipality size	0.051	-0.104	0.036	0.085
	(0.086)	(0.128)	(0.116)	(0.121)
Underestimates discrimination	0.037	-0.033	-0.043	-0.013
	(0.056)	(0.072)	(0.081)	(0.069)
Sure about a prior belief	-0.005	0.094	-0.195	-0.209*
	(0.095)	(0.105)	(0.181)	(0.107)
Unsure about a prior belief	0.009	-0.048	0.043	0.060
	(0.056)	(0.078)	(0.082)	(0.074)
Exposure to Asians	-0.011	0.058	-0.068	-0.081
	(0.052)	(0.070)	(0.086)	(0.069)
Mean	0.26	0.61	0.56	0.45
Observations	303	202	179	225

Note: This table uses data from a random half of the whole Info-choice group. *Source-prior* belief gap is defined as the absolute difference between a predicted belief of a source (top-coded at 50 and bottom-coded at 1) and the prior belief. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.6: Causal shifts of beliefs by education

	(1)	(2)	(3)	(4)	(5)	(6)
	Updating: main			**Updating: follow-up**		
	Low educated	Higher educated	All	Low educated	Higher educated	All
Practitioners-info	1.12***	1.24***	1.24***	0.90	1.28***	1.15**
	(0.43)	(0.29)	(0.29)	(0.60)	(0.50)	(0.50)
Researchers-Info	1.30***	0.93***	0.95***	0.84	1.32***	1.22**
	(0.41)	(0.26)	(0.27)	(0.62)	(0.51)	(0.51)
Low educated			0.61*			0.67
			(0.32)			(0.55)
Practitioners-Info × Low educ			-0.08			-0.28
			(0.53)			(0.78)
Researchers-Info × Low educ			0.38			-0.48
			(0.49)			(0.80)
Intercept	1.69***	0.95***	1.66*	5.11***	4.40***	2.33
	(0.29)	(0.13)	(0.95)	(0.45)	(0.29)	(1.62)
Observations	819	1,042	1,861	688	904	1,592
Covariates	No	No	Yes	No	No	Yes

Notes: The regression uses data from three Exogenous-Info groups. Individuals with extreme beliefs, i.e. outliers, are excluded. *Updating* is defined as the absolute difference between a person's posterior belief and his/her prior estimate of discrimination. In Columns 3 and 5 pre-specified covariates are included. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

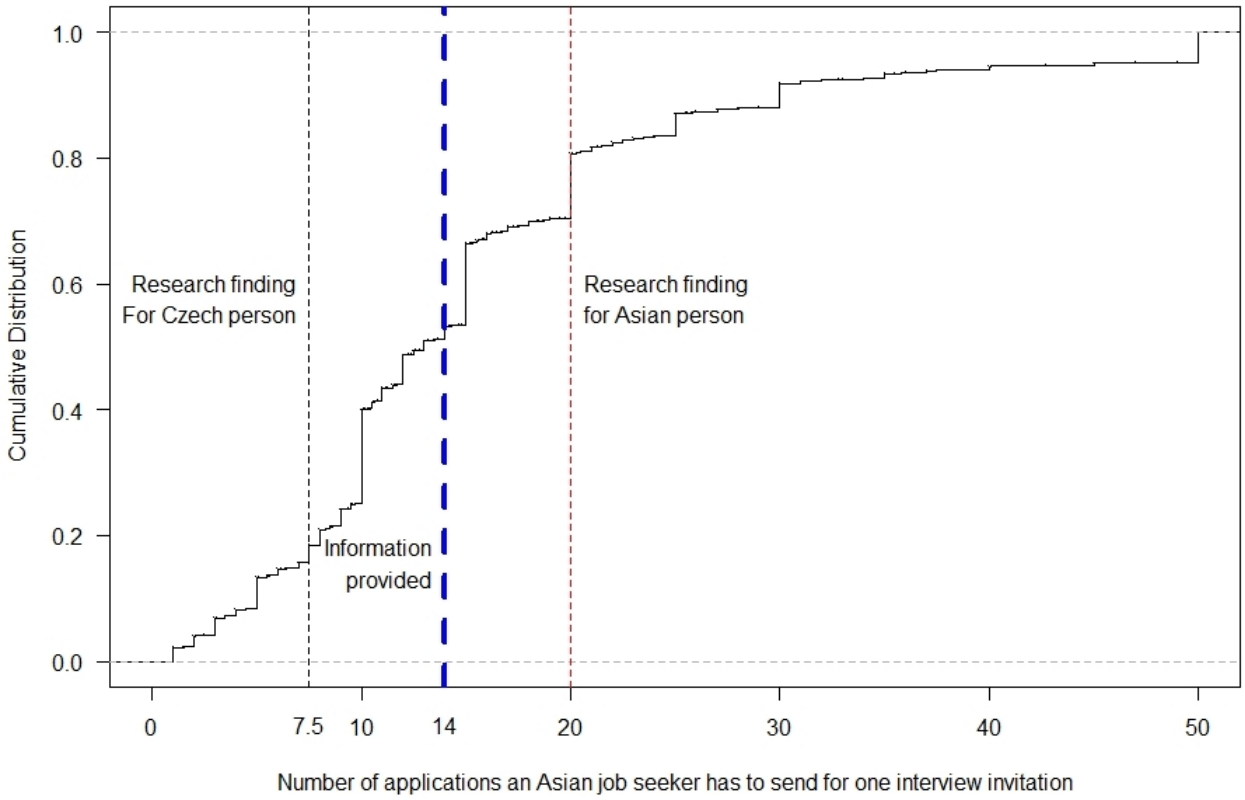
Table 1.7: Reduction of uncertainty in beliefs about the extent of discrimination

	Confidence in posterior		Share of more confident subjects than initially	
	(1)	(2)	(3)	(4)
Laymen-Info (a)	0.142**	0.142**	0.050*	0.050*
	(0.054)	(0.054)	(0.027)	(0.027)
Practitioners-Info (b)	0.200***		0.085***	
	(0.055)		(0.027)	
Researchers-Info (c)	0.239***		0.102***	
	(0.056)		(0.027)	
Experts-Info (d)		0.220***		0.094***
		(0.047)		(0.023)
Observations	2,233	2,233	2,233	2,233
Control mean			0.24	0.24
Covariates	Yes	Yes	Yes	Yes
p-value: $a = d$		0.12		0.07
p-value: $a = b$	0.32		0.21	
p-value: $a = c$	0.10		0.06	
p-value: $b = c$	0.51		0.53	

Notes: *Confidence in posterior* was measured on a scale from 1: "Very sure" to 5: "Very unsure", and was re-coded so that higher values mean higher confidence in a posterior belief (collected in the follow-up). This outcome is z-scored using the mean and standard deviation in the Control group. *Share of more confident subjects* is a binary variable indicating whether a respondent reported higher confidence in his/her own posterior belief about discrimination compared to his/her confidence in the respective prior belief. Controls included in all regressions were pre-specified. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

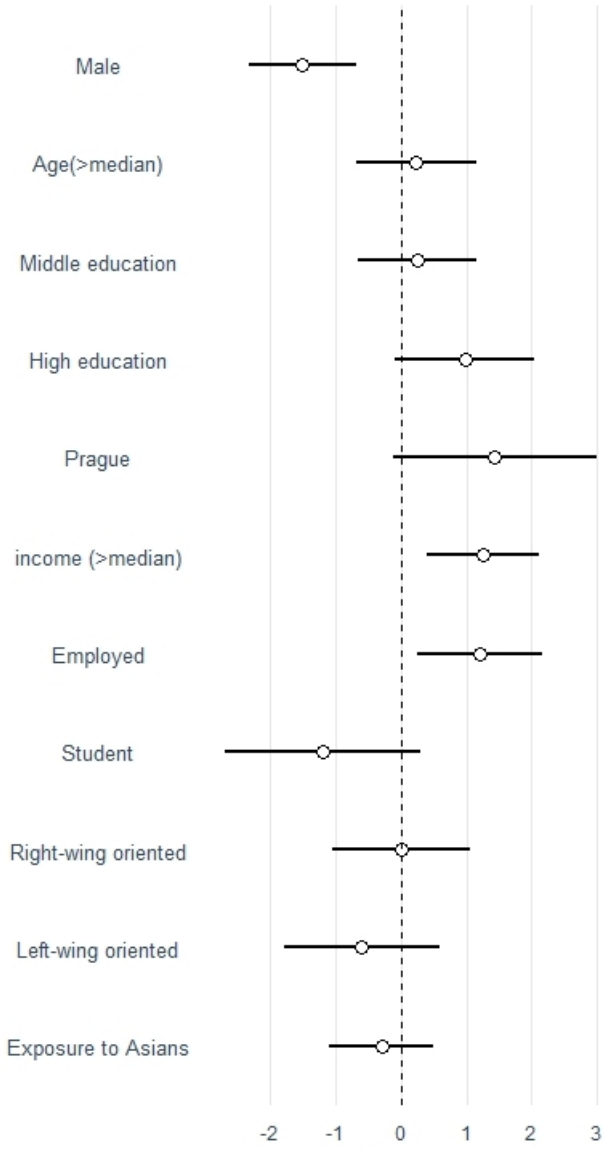
1.A Appendix figures

Figure 1.A.1: Prior beliefs about labor market discrimination against Asians



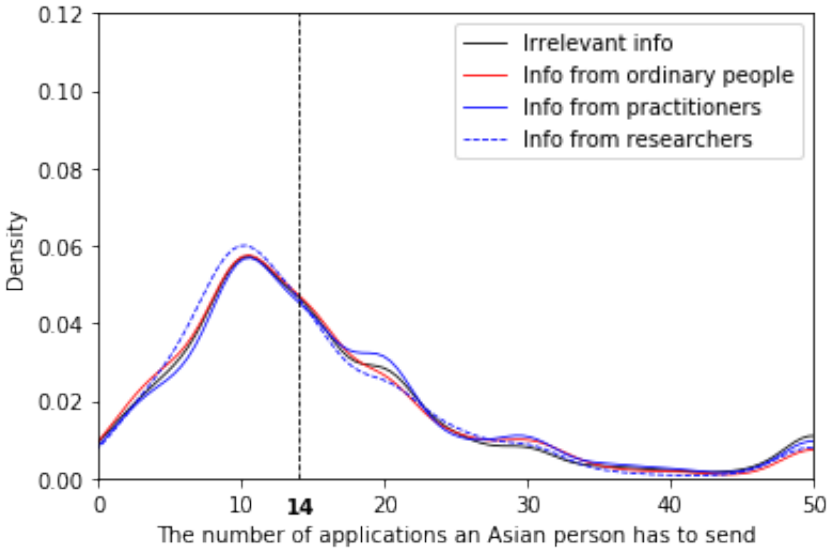
Notes: The figure uses all main-experiment participants' prior beliefs about the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation (N = 3,216). The values smaller than 1 and larger than 50 were recorded to 1 and 50, respectively. The red (black) short-dashed line denotes the number from the research study by Bartoš et al. (2016) for Asian (Czech) applicants. The blue long-dashed line indicates the source estimate, i.e. the number that subjects saw.

Figure 1.A.2: Correlates of prior beliefs about labor market discrimination against Asians



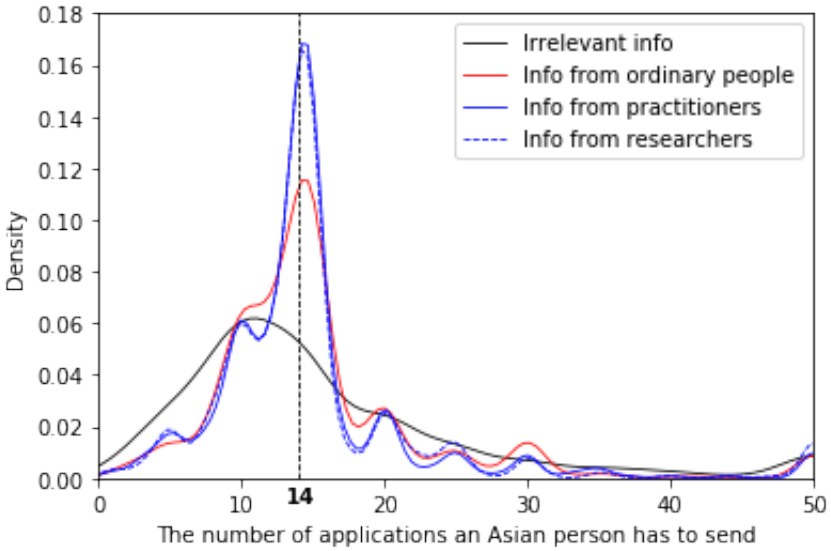
Notes: The figure uses data from the main experiment. The dependent variable is a prior belief about the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation. The dots depict the mean estimated values of multivariate regression coefficients, while the lines show the 95% confidence intervals. To save space, we do not present the estimated coefficients on two municipality dummies, indicators for missing income and political orientation which were also included in the regression and are not significantly correlated with a prior belief.

Figure 1.A.3: Prior beliefs about discrimination against Asians (main experiment)



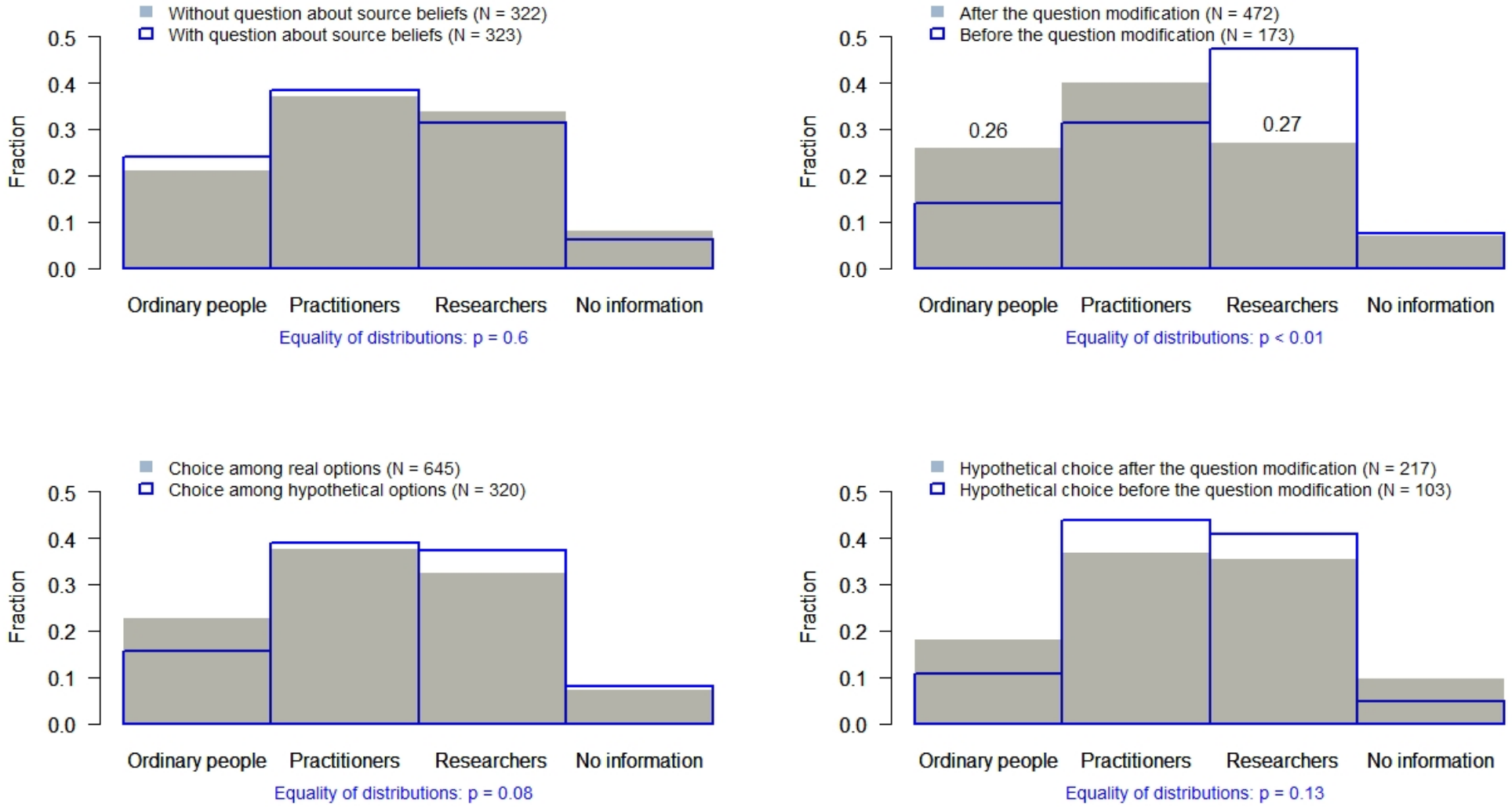
Notes: The figure plots the kernel densities of prior beliefs using main-experiment data from the Control group and Exogenous-Info groups (N = 2,571).

Figure 1.A.4: Posterior beliefs about discrimination against Asians (follow-up)



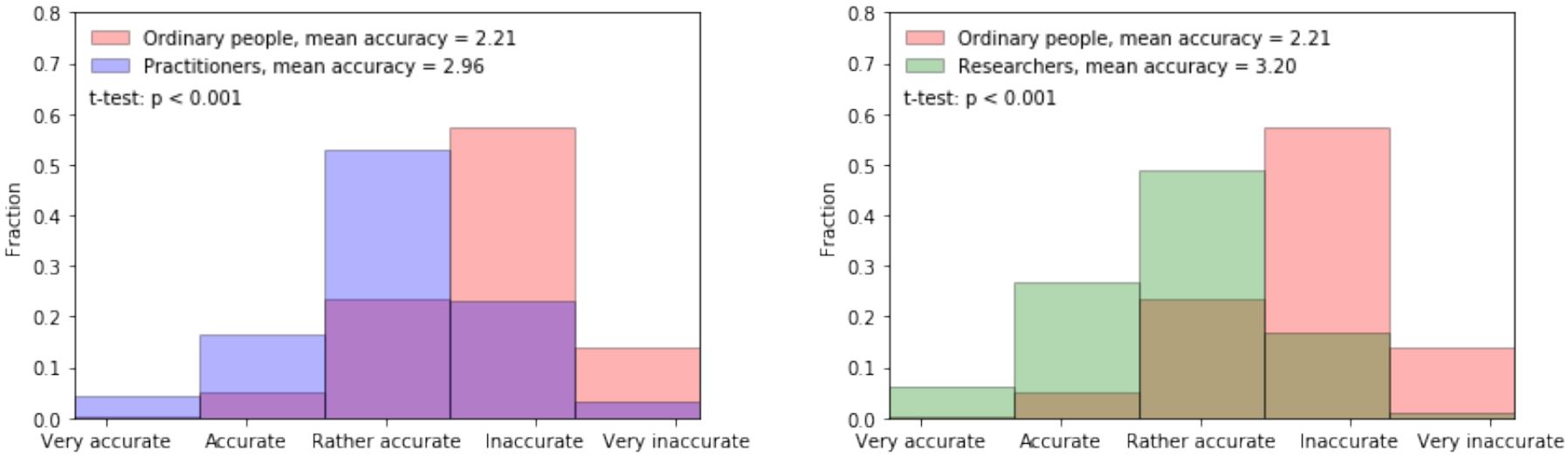
Notes: The figure plots the kernel densities of posterior beliefs using follow-up survey data from the Control group and Exogenous-Info groups (N = 2,233). The dotted vertical line indicates the number of applications estimated by a source. Extreme beliefs (i.e. estimates higher than 50 and lower than 1) were re-coded accordingly.

Figure 1.A.5: Information choices across different contexts



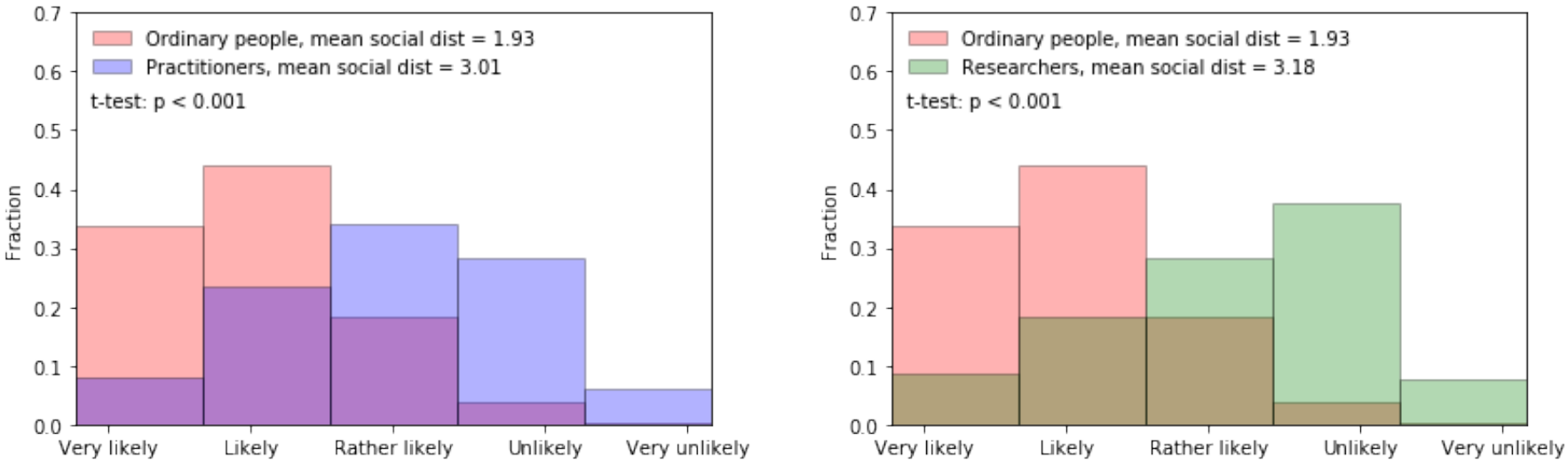
Notes: The top left (top right) panel compares distributions of the most-preferred alternatives between a random half of Info-Choice group that was asked to predict the estimates of three sources before ranking the information options and the other half that did not predict the source beliefs (a group that dragged and dropped information options according to their preferences and a group that ranked the alternatives with the help of a multiple choice task.). The bottom left panel compares distributions of the most-preferred alternatives between Info-Choice group and a random half of Control group that was asked to rank the information options without expecting the implementation of a favorite option later. Finally, the bottom right panel illustrates distributions before and after the question modification (similar to the top right panel) using the data from the half of Control group.

Figure 1.A.6: Perceived accuracy of three information sources



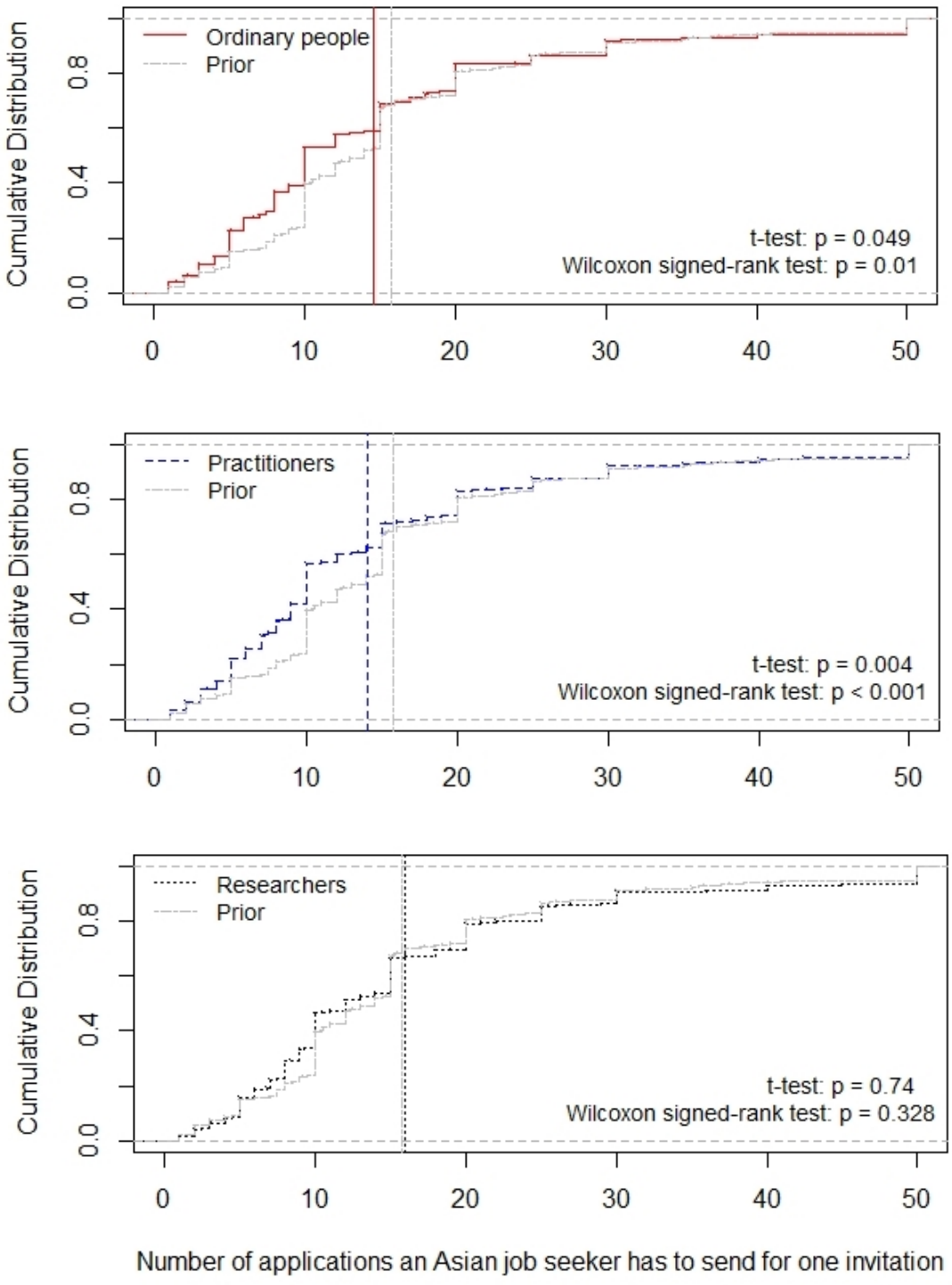
Notes: The figure, based on the data from untreated subjects who answered the questions on accuracy and social distance in the follow-up (N = 259), compares the distribution of perceived accuracy of ordinary people who estimate the prevalence of local ethnic discrimination to the corresponding distribution for each type of experts.

Figure 1.A.7: Likelihood of friendship with information providers (proxy for social distance)



Notes: The figure, based on the data from untreated subjects who answered the questions on accuracy and social distance in the follow-up (N = 259), compares the distribution of the likelihood of friendship with an ordinary person to the corresponding distribution for each type of experts.

Figure 1.A.8: Subjects' prior beliefs and predicted estimates of information sources



Notes: The figure uses data from the random half of Info-Choice group (N = 323) and compares for each of three sources the cumulative distribution of prior beliefs about the extent of discrimination against Asians to the distribution of corresponding *predicted* beliefs of a source. Vertical lines indicate means of distributions whose equality was tested (t-test p).

Figure 1.A.9: Perceived accuracy of three information sources (pilot, follow-up)

Notes: The figure, which uses pilot follow-up data (N = 72), plots the mean perceived accuracies of three groups of individuals (information sources) who estimated the degree of ethnic discrimination in the Czech labor market.

Figure 1.A.10: Social distance from three information sources (pilot, follow-up)

Notes: The figure, which uses pilot follow-up data (N = 72), plots mean social distance from the members of three groups (information providers). Social distance equals to 1(5) if it is very likely (very unlikely) that a subject would befriend a group member.

1.B Appendix tables

Table 1.B.1: The bonus effects on belief-related outcomes and time (pilot, follow-up)

	(1)	(2)	(3)	(4)	(5)	(6)
	Share of updaters:		Share of posterior = 14:		Time spent on posterior belief	
Bonus incentive	0.21**	0.23**	0.23**	0.26**	18.00**	17.75**
	(0.10)	(0.11)	(0.10)	(0.10)	(8.74)	(8.68)
Observations	54	54	54	54	54	54
Covariates	No	Yes	No	Yes	No	Yes

Notes: This table uses data from the Exogenous-Info groups in the pilot follow-up. OLS in Columns 1-4 (robust standard errors in parentheses); median regression in Columns 5 and 6. *Time spent on posterior belief* is defined as seconds spent on a screen with text about the CERGE-EI study and a question about the number of applications an Asian person has to send. Bonus incentive equals to 1 if the accuracy of a subject's posterior belief was incentivized with a 22-cent bonus. Bonus incentive is equal to 0 if a subject saw instead the following accuracy appeal from Prior et al. (2015): *In order for your answers to be most helpful for us, it is really important that you answer the questions as accurately as you can.* We included age, gender, indicator for the initial underestimation of discrimination, and high-education dummy among the covariates. Regressions with other combinations of controls give similar results. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.2: Summary statistics

	Mean	SD	Median	Min.	Max.	Obs.
Male	0.49	0.50	0.00	0.00	1.00	3,216
Age	48.12	16.50	47.00	18.00	92.00	3,216
Low education	0.44	0.50	0.00	0.00	1.00	3,216
Middle education	0.35	0.48	0.00	0.00	1.00	3,216
High education	0.20	0.40	0.00	0.00	1.00	3,216
Prague	0.12	0.33	0.00	0.00	1.00	3,216
Central Bohemia	0.13	0.33	0.00	0.00	1.00	3,216
Southwest	0.11	0.32	0.00	0.00	1.00	3,216
Northwest	0.10	0.31	0.00	0.00	1.00	3,216
Northeast	0.14	0.35	0.00	0.00	1.00	3,216
Southeast	0.16	0.37	0.00	0.00	1.00	3,216
Central Moravia	0.12	0.32	0.00	0.00	1.00	3,216
Moravian-Silesian	0.11	0.32	0.00	0.00	1.00	3,216
Household income (categories)	11.43	3.12	12.00	1.00	18.00	3,216
Full-time or part-time employee	0.51	0.50	1.00	0.00	1.00	3,216
Self-employed or small business owner	0.07	0.26	0.00	0.00	1.00	3,216
Student	0.06	0.24	0.00	0.00	1.00	3,216
Unemployed	0.02	0.13	0.00	0.00	1.00	3,216
Right-wing oriented	0.20	0.40	0.00	0.00	1.00	3,216
Left-wing oriented	0.14	0.34	0.00	0.00	1.00	3,216
Center-oriented	0.39	0.49	0.00	0.00	1.00	3,216
Household size	2.68	1.24	2.00	1.00	14.00	3,216
Very small municipality	0.13	0.34	0.00	0.00	1.00	3,216
Very large municipality	0.25	0.43	0.00	0.00	1.00	3,216
Prior belief about discrimination against Asians	15.75	11.08	13.00	1.00	50.00	3,216
Underestimates discrimination against Asians	0.51	0.50	1.00	0.00	1.00	3,216
Confidence in a prior belief	3.48	0.81	4.00	1.00	5.00	3,216
Exposure to Asians	0.45	0.50	0.00	0.00	1.00	3,216

Notes: This table presents the summary statistics for the main experiment. Low (middle) education implies complete or incomplete primary school or secondary school without school-leaving exam (secondary school with a school-leaving examination or higher professional school). High education is equivalent to university degree.

Table 1.B.3: Demographic composition of our sample compared to the Czech population

	Mean: main experiment	Mean: follow-up	Mean: CZSO/VŠPS
Gender			
Male	0.49	0.496	0.487
Female	0.51	0.504	0.513
Age group			
18 to 24 years	0.083	0.08	0.082
25 to 34 years	0.163	0.158	0.162
35 to 44 years	0.205	0.203	0.202
45 to 54 years	0.169	0.165	0.166
55 to 64 years	0.154	0.154	0.152
65 years and over	0.226	0.24	0.236
Education			
Primary			
Secondary without maturita	0.443	0.433	0.451
Secondary with maturita			
Higher professional	0.354	0.361	0.351
University degree	0.203	0.206	0.198
Region (NUTS 2)			
Prague	0.123	0.124	0.123
Central Bohemia	0.125	0.124	0.125
Southwest	0.114	0.108	0.115
Northwest	0.105	0.104	0.105
Northeast	0.142	0.149	0.142
Southeast	0.161	0.161	0.16
Central Moravia	0.115	0.119	0.115
Moravian-Silesian	0.115	0.11	0.114

Notes: This table compares the shares of selected socio-demographic groups in our main experiment and follow-up survey to their counterparts received mainly from the Czech Statistical Office (CZSO). The only exception is information about education, which was obtained from Výběrové šetření pracovních sil (VŠPS), a national analogue of the EU Labor Force Survey. Maturita means secondary school-leaving examination. Both CZSO and VŠPS contain 2017 data.

Table 1.B.4: Randomization check: Main experiment

	Control	Laymen-Info	Practitioners-Info	Researchers-Info	Info-choice	F-stat (p-value)	Obs.
Male	0.51	0.49	0.46	0.49	0.50	0.35	3,216
Age	48.04	48.05	47.40	48.96	48.17	0.59	3,216
Low education	0.46	0.41	0.45	0.45	0.44	0.53	3,216
Middle education	0.34	0.38	0.36	0.34	0.35	0.60	3,216
High education	0.20	0.21	0.19	0.21	0.21	0.95	3,216
Prague	0.13	0.13	0.12	0.10	0.12	0.48	3,216
Central Bohemia	0.14	0.10	0.12	0.13	0.13	0.27	3,216
Southwest	0.11	0.11	0.11	0.11	0.13	0.89	3,216
Northwest	0.10	0.08	0.13	0.11	0.10	0.13	3,216
Southeast	0.15	0.17	0.15	0.17	0.17	0.51	3,216
Northeast	0.15	0.18	0.14	0.14	0.11	0.03	3,216
Central Moravia	0.12	0.11	0.12	0.12	0.11	0.85	3,216
Moravian-Silesian	0.10	0.11	0.11	0.11	0.13	0.62	3,216
Household income (categories)	11.49	11.41	11.41	11.24	11.58	0.42	3,216
Full-time or part-time employee	0.48	0.52	0.52	0.49	0.55	0.12	3,216
Self-employed or small business owner	0.09	0.07	0.06	0.07	0.06	0.54	3,216
Student	0.06	0.06	0.06	0.06	0.06	0.98	3,216
Unemployed	0.02	0.02	0.02	0.02	0.01	0.38	3,216
Right-wing oriented	0.21	0.20	0.20	0.18	0.21	0.71	3,216
Left-wing oriented	0.13	0.14	0.13	0.14	0.14	0.99	3,216
Center-oriented	0.41	0.41	0.36	0.38	0.40	0.34	3,216
Household size	2.66	2.64	2.70	2.64	2.75	0.45	3,216
Very small municipality	0.15	0.12	0.13	0.12	0.13	0.64	3,216
Very large municipality	0.24	0.27	0.23	0.26	0.22	0.37	3,216
Prior belief about discrimination	16.20	15.42	16.46	15.35	15.32	0.20	3,216
Underestimates discrimination	0.51	0.52	0.49	0.53	0.51	0.59	3,216
Confidence in a prior belief	3.50	3.49	3.50	3.51	3.42	0.38	3,216
Exposure to Asians	0.48	0.42	0.49	0.42	0.46	0.02	3,216

Notes: Means. Column 7 reports p-values for an F-test testing the null hypothesis that the means are equal across five treatment arms.

Table 1.B.5: Randomization check: Follow-up

	Control	Laymen-Info	Practitioners-Info	Researchers-Info	F-stat (p-value)	Obs.
Male	0.52	0.50	0.46	0.50	0.28	2,233
Age	48.52	48.38	48.03	49.57	0.47	2,233
Low education	0.44	0.40	0.45	0.44	0.32	2,233
Middle education	0.36	0.39	0.36	0.34	0.40	2,233
High education	0.20	0.21	0.19	0.22	0.76	2,233
Prague	0.13	0.14	0.12	0.11	0.55	2,233
Central Bohemia	0.15	0.10	0.12	0.13	0.12	2,233
Southwest	0.11	0.11	0.11	0.11	1.00	2,233
Northwest	0.11	0.08	0.13	0.10	0.11	2,233
Southeast	0.15	0.17	0.14	0.18	0.29	2,233
Northeast	0.13	0.18	0.13	0.15	0.07	2,233
Central Moravia	0.12	0.11	0.12	0.11	0.94	2,233
Moravian-Silesian	0.10	0.10	0.12	0.11	0.72	2,233
Household income (categories)	11.46	11.41	11.36	11.30	0.84	2,233
Full-time or part-time employee	0.49	0.51	0.51	0.48	0.69	2,233
Self-employed or small business owner	0.08	0.07	0.06	0.07	0.47	2,233
Student	0.06	0.06	0.06	0.06	0.98	2,233
Unemployed	0.02	0.02	0.02	0.02	0.87	2,233
Right-wing oriented	0.21	0.20	0.20	0.18	0.68	2,233
Left-wing oriented	0.13	0.15	0.14	0.14	0.84	2,233
Center-oriented	0.43	0.41	0.36	0.39	0.11	2,233
Household size	2.64	2.60	2.66	2.62	0.87	2,233
Very small municipality	0.15	0.13	0.12	0.13	0.47	2,233
Very large municipality	0.24	0.27	0.24	0.26	0.51	2,233
Prior belief about discrimination	16.37	15.22	16.24	15.48	0.22	2,233
Underestimates discrimination	0.51	0.52	0.49	0.52	0.72	2,233
Confidence in a prior belief	3.49	3.51	3.50	3.51	0.98	2,233
Exposure to Asians	0.47	0.42	0.49	0.42	0.04	2,233

Notes: Means. Column 6 reports p-values for an F-test testing the null hypothesis that the means are equal across four treatment arms.

Table 1.B.6: Correlates of attrition in the follow-up

	Follow-up completion	
	(1)	(2)
Laymen-Info (a)	-0.023	-0.025
	(0.018)	(0.018)
Practitioners-Info (b)	-0.041**	-0.037**
	(0.019)	(0.019)
Researchers-Info (c)	-0.019	-0.020
	(0.018)	(0.018)
Male		0.022
		(0.014)
Age		0.001*
		(0.001)
Middle education		0.023
		(0.015)
High education		0.019
		(0.018)
Above-median income		-0.006
		(0.017)
Income missing		0.033
		(0.024)
Employee		0.006
		(0.020)
Self-employed		-0.036
		(0.031)
Student		0.023
		(0.041)
Unemployed		0.026
		(0.049)
Right-wing oriented		-0.019
		(0.018)
Left-wing oriented		0.002
		(0.019)
Does not know political orientation		-0.041**
		(0.019)
Household size		-0.012*
		(0.007)
Underestimates discrimination (prior)		-0.004
		(0.013)
Sure about a prior belief		-0.038
		(0.027)
Unsure about a prior belief		-0.012
		(0.014)
Exposure to Asians		-0.013
		(0.014)
Response rate in the Control group	0.889	0.889
Observations	2,571	2,571
Prob > F	0.187	0.000
p-value: $a = b$	0.363	0.520
p-value: $a = c$	0.830	0.775
p-value: $b = c$	0.261	0.356

Notes: Follow-up completion (the dependent variable) equals to 1 if a respondent completed the follow-up survey. Info-choice group was not invited to participate in the follow-up. To save space, we do not include regional dummies and municipality size dummies and create only three income groups. The results of the extended regression, which are very similar, can be provided upon request. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.7: Heterogeneity in belief updating by confidence in a prior belief

	(1)	(2)
	Updating:	
	main	follow-up
Laymen-Info	1.01***	0.99
	(0.33)	(0.71)
Practitioners-Info	2.45***	2.06***
	(0.50)	(0.75)
Researchers-Info	2.33***	1.74**
	(0.40)	(0.74)
Sure about prior	1.14	3.61**
	(0.90)	(1.60)
Unsure about prior	0.35	1.32**
	(0.27)	(0.65)
Sure × Laymen-Info	-1.07	-0.27
	(1.11)	(2.36)
Sure × Practitioners-Info	-0.83	-3.35
	(1.54)	(2.22)
Sure × Researchers-Info	-2.43**	-3.31
	(1.11)	(2.16)
Unsure × Laymen-Info	-0.42	-1.82*
	(0.49)	(0.94)
Unsure × Practitioners-Info	-0.36	-1.07
	(0.65)	(0.99)
Unsure × Researchers-Info	-0.77	-0.01
	(0.54)	(1.02)
Observations	2,571	2,233
Covariates	Yes	Yes

Notes: *Sure (unsure)* is a dummy variable that takes value 1 if a person is very sure or sure (very unsure or unsure) about his/her initial estimate and 0 otherwise. In both columns, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, municipality size, employment status, exposure to Asians, and political orientation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.8: Heterogeneity in belief updating by previous exposure to Asians

	(1)	(2)
	Updating:	
	main	follow-up
Laymen-Info	0.76**	-0.91
	(0.34)	(0.66)
Practitioners-Info	1.75***	-0.35
	(0.44)	(0.71)
Researchers-Info	1.77***	0.93
	(0.37)	(0.74)
Exposure to Asians	-0.08	-1.67**
	(0.34)	(0.66)
Exposure × Laymen-Info	-0.20	1.90**
	(0.52)	(0.94)
Exposure × Practitioners-Info	0.86	3.24***
	(0.64)	(0.97)
Exposure × Researchers-Info	-0.21	1.01
	(0.56)	(1.01)
Observations	2,571	2,233
Covariates	Yes	Yes

Notes: *Exposure to Asians* is a dummy variable that takes value 1 if a person has an Asian among their family members, close friends, colleagues, acquaintances or neighbors. In both columns, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, municipality size, employment status, confidence in a prior belief, and political orientation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.9: Heterogeneity in belief updating by political affiliation

	(1)	(2)
	Updating:	
	main	follow-up
Laymen-Info	0.44	0.41
	(0.42)	(0.71)
Practitioners-Info	1.53***	1.36*
	(0.51)	(0.78)
Researchers-Info	1.43***	1.44*
	(0.45)	(0.77)
Right-wing oriented	0.25	-0.90
	(0.51)	(0.76)
Left-wing oriented	-0.62	1.43
	(0.42)	(1.10)
Does not know political affiliation	-0.79**	1.17
	(0.40)	(0.99)
Left × Laymen-Info	0.48	-1.62
	(0.70)	(1.56)
Left × Practitioners-Info	1.01	-1.68
	(0.91)	(1.57)
Left × Researchers-Info	0.54	0.48
	(0.86)	(1.82)
Right × Laymen-Info	-0.30	0.62
	(0.71)	(1.15)
Right × Practitioners-Info	0.86	1.06
	(0.97)	(1.30)
Right × Researchers-Info	0.39	1.05
	(0.84)	(1.30)
Does not know × Laymen-Info	0.95	-1.47
	(0.66)	(1.27)
Does not know × Practitioners-Info	1.23	-0.70
	(0.75)	(1.30)
Does not know × Researchers-Info	0.49	-1.19
	(0.60)	(1.31)
Observations	2,571	2,233
Covariates	Yes	Yes

Notes: *Left-wing (right-wing) oriented* is a dummy variable that takes value 1 if a person considers his/her political views to be far-left or left (far-right or right) and 0 otherwise. In both columns, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, municipality size, employment status, exposure to Asians, and confidence in a prior belief. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.10: Posterior beliefs (excluding subjects with extreme beliefs)

	(1)	(2)
	Posterior:	
	main	follow-up
Panel A: Main specification		
Laymen-Info	-0.48	-0.29
	(0.38)	(0.45)
Practitioners-Info	-0.25	-0.89**
	(0.38)	(0.45)
Researchers-Info	-0.14	-0.70
	(0.40)	(0.46)
Observations	2,476	2,136
Control mean	15.25	15.08
Covariates	Yes	Yes
Panel B: Prior heterogeneity		
Laymen-Info (a)	-1.70**	-2.50***
	(0.70)	(0.77)
Practitioners-Info (b)	-2.39***	-3.84***
	(0.69)	(0.77)
Researchers-Info (c)	-2.17***	-3.68***
	(0.76)	(0.77)
Underestimator	-13.06***	-9.47***
	(0.62)	(0.73)
Underestimator × Laymen-Info (d)	2.33***	4.21***
	(0.78)	(0.91)
Underestimator × Practitioners-Info (e)	4.14***	5.68***
	(0.78)	(0.90)
Underestimator × Researchers-Info (f)	3.81***	5.66***
	(0.84)	(0.92)
Observations	2,476	2,136
Covariates	Yes	Yes
p-value: $a = b$	0.25	0.03
p-value: $a = c$	0.49	0.06
p-value: $b = c$	0.74	0.81
p-value: $a + d = b + e$	0.00	0.76
p-value: $a + d = c + f$	0.00	0.56
p-value: $b + e = c + f$	0.75	0.75
p-value: $a + d = 0$	0.07	0.00
p-value: $b + e = 0$	0.00	0.00
p-value: $c + f = 0$	0.00	0.00

Notes: Extreme beliefs are defined as estimates higher than 50 applications or lower than 1 application. Underestimator equals to 1 if the value of an individual's prior belief is lower than 14. In both columns, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, confidence in a prior belief, municipality size, employment status, exposure to Asians, and political orientation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.11: Posterior beliefs (excluding subjects who reported searching for an answer)

	(1)	(2)
	Posterior:	
	main	follow-up
Panel A: Main specification		
Laymen-Info	-0.69	-0.58
	(0.50)	(0.52)
Practitioners-Info	-0.94*	-1.09**
	(0.50)	(0.53)
Researchers-Info	-0.69	-0.78
	(0.52)	(0.54)
Observations	2,135	2,135
Control mean	16.66	16.05
Covariates	Yes	Yes
Panel B: Prior heterogeneity		
Laymen-Info (a)	-2.34***	-3.11***
	(0.91)	(0.89)
Practitioners-Info (b)	-3.76***	-4.17***
	(0.88)	(0.89)
Researchers-Info (c)	-3.06***	-4.23***
	(0.93)	(0.88)
Underestimator	-15.13***	-10.73***
	(0.75)	(0.81)
Underestimator × Laymen-Info (d)	3.31***	5.07***
	(1.01)	(1.04)
Underestimator × Practitioners-Info (e)	5.68***	6.25***
	(0.98)	(1.04)
Underestimator × Researchers-Info (f)	4.73***	6.87***
	(1.03)	(1.06)
Observations	2,135	2,135
Covariates	Yes	Yes
p-value: $a = b$	0.08	0.17
p-value: $a = c$	0.41	0.15
p-value: $b = c$	0.40	0.95
p-value: $a + d = b + e$	0.04	0.83
p-value: $a + d = c + f$	0.12	0.24
p-value: $b + e = c + f$	0.57	0.32
p-value: $a + d = 0$	0.02	0.00
p-value: $b + e = 0$	0.00	0.00
p-value: $c + f = 0$	0.00	0.00

Notes: This table uses data from subjects who participated in both waves and reported (in the follow-up) that they did not look for CERGE-EI researchers' findings after they had learnt about the researchers' study in the previous survey. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.12: Posterior beliefs (excluding subjects with too short/long survey completion)

	(1)	(2)
	Posterior:	
	main	follow-up
Panel A: Main specification		
Laymen-Info	-0.55	-0.42
	(0.47)	(0.53)
Practitioners-Info	-0.82*	-1.24**
	(0.46)	(0.53)
Researchers-Info	-0.43	-0.82
	(0.48)	(0.54)
Observations	2,470	2,146
Control mean	16.53	15.94
Covariates	Yes	Yes
Panel B: Prior heterogeneity		
Laymen-Info (a)	-1.84**	-3.08***
	(0.86)	(0.90)
Practitioners-Info (b)	-3.39***	-4.48***
	(0.82)	(0.89)
Researchers-Info (c)	-2.39***	-4.26***
	(0.90)	(0.89)
Underestimator	-14.69***	-10.77***
	(0.73)	(0.81)
Underestimator × Laymen-Info (d)	2.57***	5.27***
	(0.96)	(1.06)
Underestimator × Practitioners-Info (d)	5.17***	6.52***
	(0.93)	(1.03)
Underestimator × Researchers-Info (d)	3.85***	6.80***
	(0.99)	(1.06)
Observations	2,470	2,146
Covariates	Yes	Yes
p-value: $a = b$	0.04	0.07
p-value: $a = c$	0.51	0.13
p-value: $b = c$	0.21	0.77
p-value: $a + d = b + e$	0.01	0.77
p-value: $a + d = c + f$	0.06	0.55
p-value: $b + e = c + f$	0.41	0.37
p-value: $a + d = 0$	0.09	0.00
p-value: $b + e = 0$	0.00	0.00
p-value: $c + f = 0$	0.00	0.00

Notes: This table excludes data from the top 2 percent and bottom 2 percent of the survey time distribution. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.13: Posterior beliefs (excluding inattentive subjects)

	(1)	(2)
	Posterior:	
	main	follow-up
Panel A: Main specification		
Laymen-Info	-0.27	-0.53
	(0.53)	(0.57)
Practitioners-Info	-0.41	-1.19**
	(0.50)	(0.58)
Researchers-Info	-0.23	-1.25**
	(0.53)	(0.57)
Observations	1,821	1,609
Control mean	16.31	15.97
Covariates	Yes	Yes
Panel B: Prior heterogeneity		
Laymen-Info (a)	-1.47	-2.56***
	(0.96)	(0.96)
Practitioners-Info (b)	-2.41***	-3.69***
	(0.90)	(0.96)
Researchers-Info (c)	-1.87*	-4.28***
	(1.04)	(0.97)
Underestimator	-14.13***	-9.84***
	(0.76)	(0.87)
Underestimator × Laymen-Info (d)	2.40**	4.07***
	(1.07)	(1.13)
Underestimator × Practitioners-Info (e)	4.17***	5.17***
	(0.98)	(1.12)
Underestimator × Researchers-Info (f)	3.23***	5.92***
	(1.11)	(1.15)
Observations	1,821	1,609
Covariates	Yes	Yes
p-value: $a = b$	0.27	0.18
p-value: $a = c$	0.69	0.05
p-value: $b = c$	0.57	0.49
p-value: $a + d = b + e$	0.06	0.97
p-value: $a + d = c + f$	0.32	0.81
p-value: $b + e = c + f$	0.28	0.77
p-value: $a + d = 0$	0.03	0.01
p-value: $b + e = 0$	0.00	0.01
p-value: $c + f = 0$	0.00	0.01

Notes: This table uses data from subjects who passed the attention check administered at the end of the main questionnaire in the first wave. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.14: Correlates of passing the attention check

	(1) Successful attention check
Laymen-Info	0.009
	(0.024)
Practitioners-Info	0.004
	(0.024)
Researchers-Info	-0.033
	(0.024)
Info-choice	-0.022
	(0.024)
Male	0.060***
	(0.017)
Age	-0.007***
	(0.001)
Middle education	-0.011
	(0.018)
High education	0.011
	(0.021)
Above-median income	0.093***
	(0.018)
Income missing	0.051*
	(0.029)
Employee	0.007
	(0.022)
Self-employed	0.023
	(0.034)
Student	0.017
	(0.036)
Unemployed	0.051
	(0.059)
Right-wing oriented	-0.048**
	(0.021)
Left-wing oriented	-0.050**
	(0.025)
Does not know political orientation	-0.052**
	(0.021)
Household size	-0.002
	(0.007)
Underestimates discrimination (prior)	-0.048***
	(0.015)
Sure about a prior belief	-0.038
	(0.029)
Unsure about a prior belief	0.013
	(0.016)
Exposure to Asians	0.007
	(0.016)
Attention rate in the Control group	0.718
Observations	3,216

Notes: This table uses data from all main-experiment participants. The dependent binary variable *Successful attention check* is equal to one if a person passed the attention check administered at the end of the main questionnaire in the first wave. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.15: Correlates of no-information choice

	No source
	(1)
Male	0.011
	(0.022)
Age	0.001
	(0.001)
Middle education	0.001
	(0.024)
High education	-0.044**
	(0.021)
Above-median income	-0.053**
	(0.024)
Income is missing	-0.018
	(0.041)
Employee	0.038
	(0.027)
Self-employed	0.047
	(0.046)
Student	0.004
	(0.032)
Unemployed	0.061
	(0.133)
Right-wing oriented	0.010
	(0.022)
Left-wing oriented	0.044
	(0.033)
Does not know political orientation	0.110***
	(0.031)
Household size	0.004
	(0.009)
Underestimates discrimination	0.006
	(0.020)
Sure about a prior belief	0.034
	(0.035)
Unsure about a prior belief	0.024
	(0.022)
Exposure to Asians	-0.054***
	(0.020)
Mean	0.07
Observations	645

Notes: This table uses data from the whole Info-choice group. *No source* (the dependent variable) equals to 1 if a respondent favored no information over all other options. To save space, we do not include regional dummies and municipality size dummies and create only three income groups. The results of the extended regression, which are very similar, can be provided upon request. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.16: Ranking of information sources across education groups

	(1)	(2)
	Choosing ordinary people as the second-best option (conditional on ranking either type of experts first)	
Low educated	0.10**	0.11**
	(0.05)	(0.05)
Constant	0.33***	1.15***
	(0.03)	(0.26)
Observations	444	444
Covariates	No	Yes

Note: This table uses data from subjects who were assigned to the Info-choice group and chose to see the experts' estimate of discrimination. Those who chose no information in the second stage are excluded. The dependent variable equals 1 if a person ranks HR managers or researchers as the first preferred option and ordinary people as the next preferred option. Robust standard errors in parentheses. In column (2), we additionally include pre-specified covariates, such as gender, age, income and regional dummies, political affiliation, employment status, household and municipality size, previous exposure to Asians, underestimator dummy, confidence in a prior belief. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.17: Randomization check: subgroups with/without questions about accuracy and social distance (follow-up)

	1/2 of subjects (no questions)	1/2 of subjects (questions included)	t-stat (p-value)	Obs.
Male	0.48	0.51	0.16	2,233
Age	47.84	49.41	0.03	2,233
Low education	0.45	0.41	0.04	2,233
Middle education	0.35	0.37	0.28	2,233
High education	0.20	0.22	0.23	2,233
Prague	0.11	0.13	0.16	2,233
Central Bohemia	0.13	0.12	0.47	2,233
Southwest	0.11	0.10	0.57	2,233
Northwest	0.11	0.10	0.47	2,233
Southeast	0.15	0.17	0.20	2,233
Northeast	0.16	0.14	0.38	2,233
Central Moravia	0.13	0.11	0.23	2,233
Moravian-Silesian	0.10	0.12	0.21	2,233
Household income (categories)	11.41	11.36	0.70	2,233
Full-time or part-time employee	0.52	0.47	0.02	2,233
Self-employed or small business owner	0.06	0.07	0.32	2,233
Student	0.05	0.07	0.18	2,233
Unemployed	0.02	0.02	0.11	2,233
Right-wing oriented	0.20	0.20	0.88	2,233
Left-wing oriented	0.15	0.14	0.51	2,233
Center-oriented	0.39	0.41	0.57	2,233
Household size	2.62	2.63	0.86	2,233
Very small municipality	0.14	0.12	0.13	2,233
Very large municipality	0.24	0.27	0.10	2,233
Prior belief about discrimination	15.22	16.44	0.01	2,233
Underestimates discrimination	0.55	0.48	0.00	2,233
Confidence in a prior belief	3.52	3.48	0.28	2,233
Exposure to Asians	0.47	0.43	0.04	2,233

Notes: Means. Column 4 reports p-values for a t-test testing the null hypothesis that the means are equal across two groups of follow-up participants.

Table 1.B.18: Perceived accuracy of the information sources (follow-up)

	Accuracy of ...					
	(1)	(2)	(3)	(4)	(5)	(6)
	ordinary people's estimate		HR managers' estimate		researchers' estimate	
Laymen-Info	0.159*	0.165*	-0.008	-0.011	-0.089	-0.083
	(0.085)	(0.087)	(0.083)	(0.084)	(0.092)	(0.091)
Practitioners-Info	0.105	0.082	0.016	0.019	0.044	0.060
	(0.090)	(0.087)	(0.085)	(0.085)	(0.093)	(0.092)
Researchers-Info	0.099	0.111	0.009	0.011	-0.194**	-0.159*
	(0.088)	(0.087)	(0.087)	(0.087)	(0.087)	(0.088)
Observations	1,109	1,109	1,109	1,109	1,109	1,109
Covariates	No	Yes	No	Yes	No	Yes

Notes: OLS in all columns. The outcomes were measured on a scale from 1: "Very accurate" to 5: "Very inaccurate" and subsequently were re-coded so that higher values mean higher perceived accuracy of an information source that estimates the extent of ethnic discrimination in the Czech labor market. The outcomes are z-scored using respective means and standard deviations in the Control group. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.19: Social distance from information providers (follow-up)

	Likelihood of friendship with ...					
	(1)	(2)	(3)	(4)	(5)	(6)
	ordinary person		HR manager		researcher	
Laymen-Info	-0.020	-0.025	0.048	0.079	0.012	0.055
	(0.085)	(0.085)	(0.085)	(0.082)	(0.082)	(0.080)
Practitioners-Info	-0.035	-0.051	0.059	0.091	0.064	0.091
	(0.085)	(0.085)	(0.086)	(0.082)	(0.084)	(0.081)
Researchers-Info	-0.070	-0.059	0.037	0.080	-0.081	-0.032
	(0.086)	(0.085)	(0.086)	(0.083)	(0.087)	(0.086)
Observations	1,109	1,109	1,109	1,109	1,109	1,109
Covariates	No	Yes	No	Yes	No	Yes

Notes: OLS in all columns. The outcomes were measured on a scale from 1: "Very likely" to 5: "Very unlikely" and subsequently were re-coded so that higher values mean higher likelihood of friendship with a group member (ordinary person, HR manager, and researcher who primarily studies issues that ethnic minorities face in the Czech Republic). The outcomes are z-scored using respective means and standard deviations in the Control group. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.20: Reduction of uncertainty in beliefs by education

	Low educated subjects				Higher educated subjects			
	Confidence in posterior		Share of more confident subjects than initially		Confidence in posterior		Share of more confident subjects than initially	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Laymen-Info (a)	0.046	0.046	0.060	0.060	0.202***	0.201***	0.037	0.037
	(0.087)	(0.087)	(0.042)	(0.042)	(0.069)	(0.069)	(0.035)	(0.035)
Practitioners-Info (b)	0.087		0.054		0.277***		0.102***	
	(0.083)		(0.041)		(0.074)		(0.037)	
Researchers-Info (c)	0.163*		0.067*		0.300***		0.128***	
	(0.088)		(0.041)		(0.073)		(0.037)	
Experts-Info (d)		0.125*		0.061*		0.289***		0.115***
		(0.072)		(0.035)		(0.062)		(0.031)
Observations	969	969	969	969	1,264	1,264	1,264	1,264
Control mean			0.25	0.25			0.23	0.23
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value: $a = d$		0.33		0.98		0.17		0.01
p-value: $a = b$	0.65		0.97		0.32		0.07	
p-value: $a = c$	0.22		0.87		0.18		0.01	
p-value: $b = c$	0.40		0.76		0.77		0.49	

Notes: This table uses data from three Exogenous-Info groups and Control group. *Confidence in posterior* was measured on a scale from 1: "Very sure" to 5: "Very unsure", and was re-coded so that higher values mean higher confidence in a posterior belief (collected in the follow-up). This outcome is z-scored using the mean and standard deviation in the control group. *Share of more confident subjects* is a binary variable indicating whether a respondent reported higher confidence in own posterior belief about discrimination compared to his/her confidence in the respective prior belief. Controls included in all regressions were pre-specified. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.21: Belief updating by social distance

	(1)	(2)
	Updating:	
	main	follow-up
Laymen-Info	1.38***	0.99
	(0.42)	(0.79)
Practitioners-Info	2.05***	0.54
	(0.47)	(0.75)
Researchers-Info	3.54***	2.66***
	(0.65)	(1.00)
Socially distant from ordinary	-0.70*	-0.68
	(0.39)	(0.65)
Socially distant from HR managers	0.34	0.25
	(0.46)	(0.72)
Socially distant from researchers	-0.12	0.27
	(0.37)	(0.62)
Socially distant from ordinary	0.92	-0.22
× Laymen-Info	(0.79)	(1.28)
Socially distant from HR managers	0.22	0.95
× Practitioners-Info	(0.86)	(1.21)
Socially distant from researchers	-1.49*	-1.35
× Researchers-Info	(0.78)	(1.24)
Observations	1,109	1,109
Covariates	Yes	Yes

Notes: The table uses data from individuals who answered questions on perceived accuracy and social distance in the follow-up. Updating is defined as an absolute difference between one's prior and posterior belief about the extent of discrimination against Asians. *Socially distant from ...* equals to 1 if a subject reports above-median social distance from the source in question. In both columns, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, confidence in a prior belief, municipality size, employment status, exposure to Asians, and political orientation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.22: Correlations between preferences for sources and *absolute* distances between one's prior belief and source predicted beliefs (excluding subjects with extreme estimates)

	Equals to 1 if chose ...		
	(1) HR managers over ordinary people	(2) researchers over ordinary people	(3) researchers over HR managers
Ordinary-prior belief gap	-0.007	-0.001	
	(0.007)	(0.006)	
HR-prior belief gap	0.007		0.008
	(0.007)		(0.005)
Research-prior belief gap		-0.008	-0.008
		(0.008)	(0.007)
Observations	192	167	209
Covariates	Yes	Yes	Yes

Note: This table uses data from a random half of the whole Info-choice group. *Source-prior* belief gap is defined as the absolute difference between one's prior belief about the number of applications an Asian job seeker has to send and the (corresponding) predicted belief of a source. We restrict the sample to individuals who stated a prior belief between 1 application and 50 applications and predicted the source estimate to lie within the same range. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.B.23: Correlations between preferences for sources and *relative* distance between one's prior belief and a source predicted belief

	Equals to 1 if chose ...		
	(1) HR managers over ordinary people	(2) researchers over ordinary people	(3) researchers over HR managers
Smaller gap between a prior and HR managers' belief *compared to ordinary people's belief*	-0.108 (0.076)		
Smaller gap between a prior and researchers' belief *compared to ordinary people's belief*		0.101 (0.075)	
Smaller gap between a prior and researchers' belief *compared to HR managers' belief*			0.104 (0.068)
Observations	202	179	225
Covariates	Yes	Yes	Yes

Note: This table uses data from a random half of the whole Info-choice group. *Smaller gap between a prior and source belief* is defined as a shorter distance between one's prior belief about the number of applications an Asian job seeker has to send and the (corresponding) predicted belief of a source relative to the distance from the alternative source belief. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

1.C Possible explanations for choosing information from ordinary people

This section presents several possible explanations for why a non-trivial proportion of individuals select information from ordinary people, the source with the lowest *average* accuracy. Recall that 23 percent of individuals ranked the ordinary people's estimate as the most preferred information piece. For comparison, 22 percent of the Control group consider the ordinary people's estimate of discrimination *at least* as accurate as the experts' estimate. Therefore, the behavior of subjects who favor information from ordinary people is not necessarily irrational. Splitting the accuracy judgements by education, we observe a pattern which is consistent with a stronger preference of low-educated individuals for lay information relative to higher-educated participants. Specifically, 25 percent of low-educated subjects consider ordinary people to be *at least* as good at predicting the prevalence of local discrimination as experts. The corresponding share of higher-educated individuals is 20 percent.

Perceiving ordinary people as an accurate source of information about discrimination may not be the only reason for selecting their information. To test whether behavioral mechanisms may be simultaneously at play, we lack evaluations of accuracy and social distance made by subjects from the Info-choice group[56]. However, we have these evaluations in the case of untreated subjects who were asked to rank the same information options as the Info-Choice group did, *but* they did not get a chance to see any information (N=118). Moreover, these participants were asked to predict the source estimate of the number of applications an Asian person has to send, which enables us to examine the relevance of the confirmation bias explanation[57]. Naturally individuals may not take very seriously a choice among "hypothetical" information sources. We do not find support for this conjecture. The distribution of "hypothetical" information options does not differ significantly from the distribution of "real" information options described earlier (p=0.23, Fisher's exact test). In line with our main findings, we observe that the majority of individuals - 36 percent and 42 percent - choose information from researchers and HR

[56]Recall that this data was collected in the follow-up, and the Info-choice group did not take part in the second wave due to the budget constraints.

[57]One could be concerned that individuals answer questions regarding source characteristics in a way that justifies their information choices. We partially address this concern by asking untreated participants to rank information options at the end of the main experiment, while data on accuracy, social distance and expected source estimates was collected in the follow-up.

managers, respectively. About 14% of subjects rank the ordinary people's estimate first.

Table 1.C.1 presents correlations between the likelihood of choosing "hypothetical" information from ordinary people or experts and perceived characteristics of these sources. Column 1 of shows that subjects with more optimistic (i.e. above-median) judgements of the ordinary people's accuracy are 17 percentage points more likely to choose information from this source ($p<0.10$)[58]. We do not include the accuracy of alternative information sources, i.e. experts, because none of the subjects (12 percent) who assigned above-median accuracy to *both* HR managers and researchers chose the ordinary people's estimate. The correlation between ordinary people's perceived accuracy and choice of this source becomes somewhat stronger if we control for social distance ($p<0.05$). It is noteworthy that subjects who report above-median social distance from experts are very likely to opt for information from ordinary people ($p<0.01$). This coefficient is almost twice as large as the (absolute) value of the coefficient on social distance from ordinary people, which is marginally significant ($p<0.10$). The estimates of interest barely change after we include (i) demographic controls[59] and (ii) the gap between one's prior belief and the ordinary people's predicted estimate (Columns 3 and 4 of Table 1.C.1).

In summary, our results illuminate several reasons that possibly underlie a subject's decision to select the ordinary people's estimate. Individuals may choose to acquire information from ordinary people because they genuinely consider this source to be accurate. On the top of that, individuals may choose to hear the ordinary people's opinion because they do not identify with experts. Given the data limitations discussed above and correlational nature of our analysis, the results in this section should be seen more as initial evidence rather than as definitive findings.

[58]The number of observations drops to 108 after we exclude subjects who chose no information.

[59]We had to select several variables from the pre-specified list due to a relatively small number of observations. Using other combinations of covariates leads to similar results.

Table 1.C.1: Determinants of preference for "hypothetical" information from laymen

	Equals to 1 if chose ordinary people over experts			
	(1)	(2)	(3)	(4)
Accurate (ordinary people)	0.17*	0.19**	0.20**	0.20**
	(0.10)	(0.09)	(0.09)	(0.09)
Distant (ordinary people)		-0.13*	-0.14*	-0.14*
		(0.07)	(0.07)	(0.08)
Distant (experts)		0.27***	0.29***	0.29***
		(0.09)	(0.10)	(0.10)
Male			0.01	0.01
			(0.07)	(0.07)
Age			-0.00	-0.00
			(0.00)	(0.00)
Low educated			0.09	0.09
			(0.08)	(0.08)
Employed			-0.04	-0.04
			(0.08)	(0.09)
Small gap between prior and laymen's belief				-0.00
				(0.09)
Intercept	0.12***	0.08**	0.22	0.22
	(0.04)	(0.04)	(0.16)	(0.16)
Observations	108	108	108	108

Notes: The table uses data from participants who (i) ranked "hypothetical" information options and (ii) evaluated each information source with regard to accuracy, social distance, and closeness to one's own prior belief. Subjects who chose no information are excluded. *Accuracy (ordinary people)* equals to 1 for subjects who assigned above-median accuracy to the ordinary people's estimate of ethnic discrimination (and 0 otherwise). *Distant (experts)* equals to 1 for subjects with above-median social distance from both HR managers and researchers (and 0 otherwise). *Accuracy (experts)* and *Distant (ordinary people)* are defined similarly. *Small gap between prior and laymen's belief* equals to 1 if a person predicts the laymen's estimate to be closer to his/her prior belief than each expert's estimate. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

1.D Translated instructions

1.D.1 First wave

Instructions and agreement with participation[60]

Your participation in this survey is completely voluntary; therefore, if you are not interested, you do not have to participate in it. In addition, if you wish to withdraw from the survey, you can do so without penalty.

If you decide to take part in the survey, please make sure that you will have enough time for completing a questionnaire (i.e. at least 10 minutes) and that you will be able to fulfil the following conditions.

Special conditions for participating in this survey:

- You will fill in the entire questionnaire at once. There will be no chance to interrupt the survey and return to it later.
- You will read questions and answer them carefully. The survey includes a question that aims to check your attention.
- While filling in the main part of the questionnaire ("Main questionnaire" in the heading), you will be only moving forward. You will not be allowed to return to previous pages.
- During this survey, you will have a chance to receive a bonus in addition to your participation fee. Information about whether or not you received the bonus will be sent to you at the end of November 2018 on the basis of evaluating your corresponding answer. The potential bonus will be paid to you in the relevant pay period[61].

By clicking on the "Agree and continue" button, you confirm that you take into account the above conditions and agree with participation in this survey. If the conditions are violated, you may be removed from the survey.

[60] For the sake of brevity, we do not present MEDIAN's standard instructions. A slightly modified version of these standard instructions can be found in Appendix 1.D.2. Although participants did not receive an email that they could use to contact us in the case of any questions, they saw MEDIAN's email during the whole survey. Subjects also had a chance to write feedback/questions at the end of the survey. We agreed with MEDIAN that all questions and comments would be forwarded to us.

[61] Participants also received some details about the pay period, which we omit here.

Demographic questions

1. What is you gender? [Male; Female]
2. What is your age? [respondent writes a number]
3. What is the highest level of education you have completed? [Incomplete primary; Primary; Apprenticeship or secondary school without a school-leaving examination; secondary school with a school-leaving examination; Higher professional; University degree]
4. In which district do you currently live? [respondent chooses from available options]
5. In which municipality do you currently live? [respondent chooses from available options]
6. How many people are there in your household (including yourself)? [respondent chooses a number]

Elicitation of beliefs about discrimination against Asian job seekers

In this survey, please consider among **Asians** mainly Vietnamese, Chinese and Japanese people.

page break

Before answering the below question, please read CAREFULLY the whole text:

Researchers from CERGE-EI (Center for Economic Research and Graduate Education - Economics Institute) conducted a study to estimate the prevalence of discrimination against Asian people in the Czech labor market. The research team sent applications from several groups of job seekers responding to job openings on www.jobs.cz. In the first group a job seeker was Czech and in the second group he was Asian.

The job seekers differed only in their name, which signaled ethnicity. Their education and experience were exactly the same. The following names were assigned to the job seekers: Jiří Hájek (Czech-sounding name) and Phan Quyet Nguyen (Asian-sounding name).

The applications were sent out to jobs in different parts of the Czech Republic. The jobs were in the field of administration, sales, and customer service.

The researchers have found that a job seeker with a Czech-sounding name has to send

on average 7.5 applications in order to receive one interview invitation.

In your opinion, how many applications does a job seeker with an Asian-sounding name have to send to receive one interview invitation?

If your answer is the same as what CERGE-EI researchers (the authors of the study) have found, you will be rewarded a bonus of **5 Czech crowns** in addition to the participation fee.

Confidence in a prior belief

How sure are you about your estimate?

- Very sure
- Sure
- Somewhat sure
- Unsure
- Very unsure

Prediction of average estimates across 3 groups (1/2 of Info-Choice group only)

We asked 3 groups of people to estimate the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation. In your opinion, what was an average estimate of

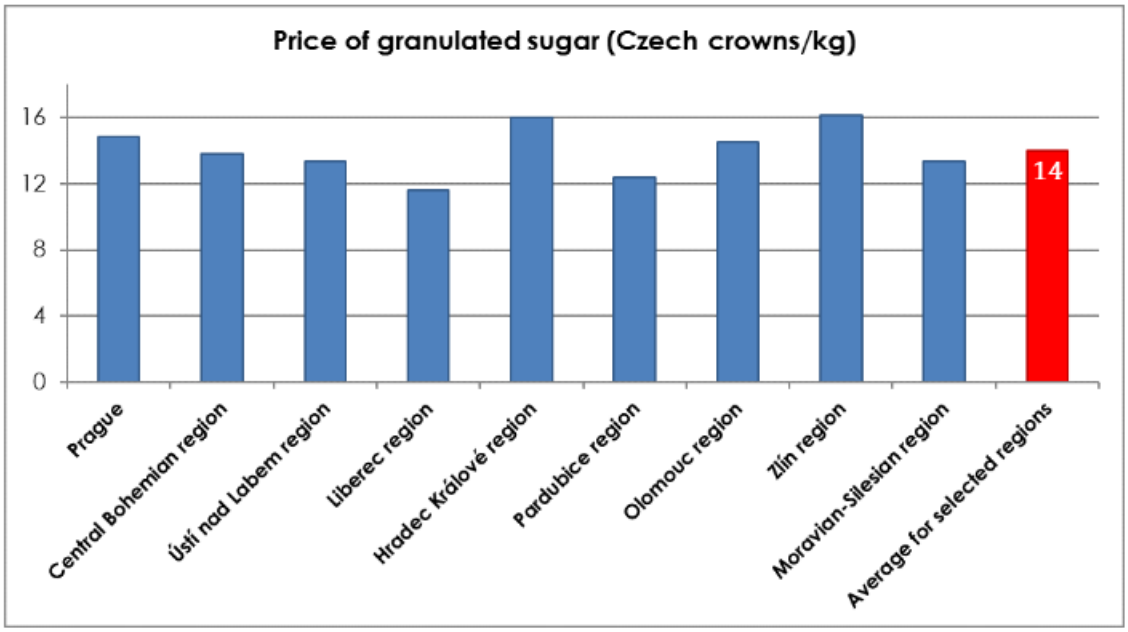
- 9 passers-by
- 9 HR managers
- 9 researchers who primarily study issues that ethnic minorities face in the Czech Republic

Three (above) alternatives appeared in a random order.

Information screen

Control group

We compared the prices of granulated sugar in 9 Czech regions in August 2018. The average price of sugar in these 9 regions was 14 Czech crowns/kg.



Source: Czech Statistical Office, 2018

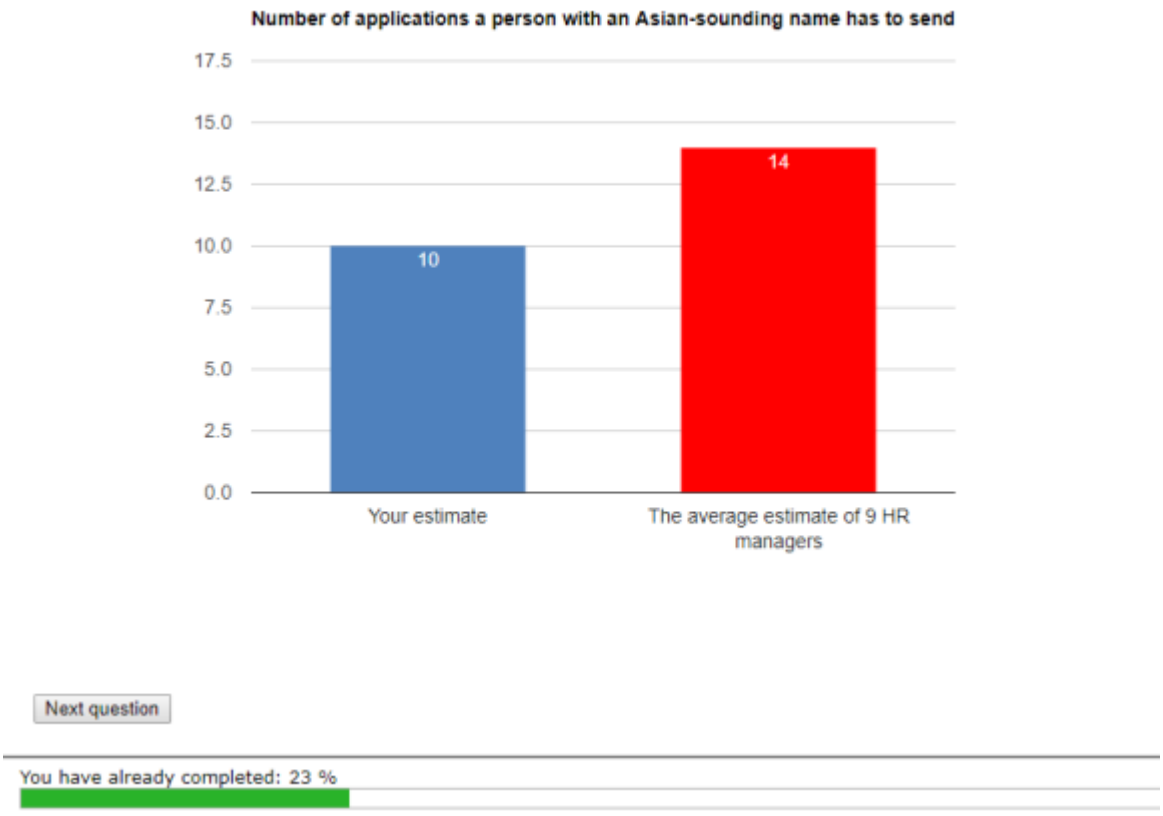
Next question

You have already completed: 23 %

Exogenous-Info groups

We asked 9 passers-by/9 HR managers/9 researchers who primarily study issues that ethnic minorities face in the Czech Republic[62] to estimate the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation. The average estimate of 9 passers-by/9 HR managers/9 researchers[63] was 14 applications.

Each Exogenous-Info group also saw a bar chart. For instance, Practitioners-Info group saw the following graph:



Information source choice (Info-choice group only)

We asked 3 groups of people to estimate the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation[64]. Now you will have

[62] A type of the information source mentioned in the message depends on the group to which subjects were randomly assigned.

[63] In Researchers-Info group, participants saw an additional paragraph: "Note: These 9 researchers are not related in any way to the authors of the study whose brief description you read earlier."

[64] For half of participants who had been asked to predict the average estimates of three groups earlier, the first sentence was omitted.

an opportunity to see an average estimate of one of the below groups. Please rank the below options on a 1 to 4 scale, where 1 is "most preferred" and 4 is "least preferred"[65].

- an average estimate of 9 passers-by
- an average estimate of 9 HR managers
- an average estimate of 9 researchers who primarily study issues that ethnic minorities face in the Czech Republic
- I would not like to see any information.

Note: 9 researchers are not related in any way to the authors of the study whose brief description you read earlier.

Four (above) alternatives appeared in a random order. After the ranking was made, participants who chose to receive information saw the following:

The average estimate of 9 passers-by/9 HR managers/9 researchers who primarily study issues that ethnic minorities face in the Czech Republic[66] was 14 applications.

Collection of posterior beliefs

Would you like to revise your estimate of the number of applications a person with an Asian-sounding name has to send to receive one interview invitation?

- Yes: [new number]
- No

If you choose "Yes", you will be rewarded the above mentioned bonus of 5 Czech crowns in addition to the participation fee if your new estimate is the same as what CERGE-EI researchers (the authors of the study) have found.

If you choose "No", you will be rewarded the above mentioned bonus of 5 Czech crowns in addition to the participation fee if your initial estimate is the same as what CERGE-EI researchers (the authors of the study) have found.

[65] Due to the difficulties with the drag-and-drop task that some subjects experienced, we later simplified the task by using three multiple-choice questions instead. First we asked participants about the most preferred alternative (a type of information source or no information), then about the second most preferred alternative, and finally about the third most preferred option. Every time the number of options decreased depending on a participant's previous choice.

[66] Subjects saw information from a group whose estimate they considered to be "the most preferred".

Attitudinal questions

Three questions appeared in a random order.

To what extent do you agree with the following statement: "Asian workers take Czech people's jobs."

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

To what extent do you agree with the following statement[67]: "Asian job seekers produce more disadvantages than advantages for the Czech labor market."

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

How comfortable or uncomfortable would you feel with having an Asian neighbor?

- Very uncomfortable
- Somewhat uncomfortable
- Neither uncomfortable nor comfortable
- Somewhat comfortable
- Very comfortable

[67] A formulation of this statement resembles the formulation used by Grigorieff, Roth, and Ubfal (2020).

Hypothetical information choice (1/2 of Control group only)

We asked 3 groups of people to estimate the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation. If you had an opportunity to see an average estimate of one of the below groups, what would you choose? Please rank the below options on a 1 to 4 scale, where 1 would be "most preferred" and 4 would be "least preferred"[68].

- an average estimate of 9 passers-by
- an average estimate of 9 HR managers
- an average estimate of 9 researchers who primarily study issues that ethnic minorities face in the Czech Republic
- I would not choose any information.

Note: 9 researchers are not related in any way to the authors of the study whose brief description you read earlier.

Four (above) alternatives appeared in a random order.

Attention check

In online surveys, sometimes there are participants who do not carefully read the questions and just quickly click through the questionnaire. To show that you read and carefully answer our questions, please choose "Extremely interested" and also "Not at all interested" as your answer in the next question[69].
How interested are you in gardening?

- Extremely interested
- Very interested
- A little bit interested
- Almost not interested
- Not at all interested

[68] Footnote 65 explains how the ranking task was simplified in the course of the experiment.
[69] Our attention check closely resembles the attention check used by Haaland and Roth (2020).

Background questions

Thank you for completing the main part of the questionnaire. Now we would like to ask you a few additional questions.

What is your current employment status? [Full-time employee, Part-time employee, Self-employed or small business owner, Unemployed and looking for work, Student, Full-time parent, Retired, Engaged in family duties, Other]

Which industry you primarily work in? If you are not currently working, please think about your last employment. [21 economic activities according to CZ-NACE and an option "I have never worked"]

Please think about total net income of your household. Net income is the total amount (after taxes) that you have at your disposal, including your wages, state support, interest, etc. Which of the below categories includes the monthly net income of your household? [Without income; Less than 4,000 Czech crowns; 4,001-6,000 Czech crowns; 6,001-8,000 Czech crowns; 8,001-10,000 Czech crowns; 10,001-12,500 Czech crowns; 12,501-15,000 Czech crowns; 15,001 - 17,500 Czech crowns; 17,501 - 20,000 Czech crowns; 20,001 - 25,000 Czech crowns; 25,001 - 30,000 Czech crowns; 30,001 - 40,000 Czech crowns; 40,001 - 50,000 Czech crowns; 50,001 - 75,000 Czech crowns; 75,001 - 100,000 Czech crowns; More than 100,001 Czech crowns; Do not know; Prefer not to answer]

How would you describe your political orientation?

- Far right
- Right
- Center-right
- Center-left
- Left
- Far left
- I do not know

Do you have Asians among your[70]

[70]In the case of each alternative, participants could choose yes, no or prefer not to answer.

- family members
- close friends
- co-workers
- acquaintances
- neighbors

If you need a piece of advice on an important issue, whom would you prefer to ask? Please choose a person whose advice you would appreciate the most.

Random order of 5 options

- a person like me
- a professional with an academic title who studies this issue
- a professional with related practical experience
- a family member
- a colleague

Donation

Participants were asked this question at the very end of the survey, after they gave feedback about the questionnaire.

What would you like to do with the reward that you could claim due to completion of the survey?

- Send to my bank account
- Donate to non-profit organization SEA-l (www.sea-l.cz), former Klub Hanoi, which among other things supports the integration of Vietnamese people into Czech society (Note: This non-profit organization is not related to the client who ordered the survey.)
- I do not want the reward, I would like to finish this questionnaire

1.D.2 Second wave

Instructions and agreement with participation[71]

Hello,
welcome to MEDIAN's questionnaire.

You were randomly selected for participation in this survey.

We would like to assure you that all your data will be used solely as a part of aggregate data and any misuse of personal information is excluded. Company MEDIAN guarantees you the absolute anonymity and confidentiality of your answers. All information that you provide during the survey will be jointly processed by computers together with the answers of hundreds of other survey participants.

Your participation in this survey is completely voluntary and you can withdraw your participation at any time without penalty.

There are no generally correct or incorrect answers to survey questions. Please answer truthfully, based on your personal opinion and knowledge, regardless of whether your views are consistent with majority views or are politically correct. For success of the survey it is important that you go through the entire questionnaire and answer each question. If you do not know an exact answer, please try to guess it as accurately as you can.

You can find information about MEDIAN, s.r.o. here. [Some words are displayed as a hyperlink]

By clicking on the below button, you confirm that you have read the above text and agree with participation in this survey.

Demographic questions

1. What is you gender? [Male; Female]

2. What is your age? [respondent writes a number]

3. What is the highest level of education you have completed? [Incomplete Primary; Primary; Apprenticeship or secondary school without a school-leaving examination;

[71] Compared to MEDIAN's standard instructions, we additionally emphasize voluntary participation and possibility of withdrawal at any time and ask explicitly for consent to participate.

secondary school with a school-leaving examination; Higher professional; University degree]

4. In which district do you currently live? [respondent chooses from available options]
5. In which municipality do you currently live? [respondent chooses from available options]

Obfuscation questions

Thank you for completing the Introduction part. Now you will receive questions related to economic and social issues. Please be attentive while filling in the questionnaire.

Do you think unemployment in the Czech Republic in comparison to other EU states is high, low or average?

- Very high
- Above average
- Average
- Below average
- Very low

Do you think that minimum wages in the Czech Republic should be increased, decreased or kept at the current level?

- Should be increased a lot
- Should be increased a little
- Should be kept at the current level
- Should be decreased a little
- Should be decreased a lot

To what extent do you agree with the following sentence: "Czech job seekers are disadvantaged by the low number of part-time jobs."

- Strongly agree

- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

Attitudinal questions

While answering further questions, please consider among Asians Vietnamese, Chinese, and Japanese people.

The order of the below questions was randomized.

To what extent do you agree with the following statements?
A. "Due to Ukrainian job seekers, Czech people have fewer opportunities to find employment."
B. "Due to Asian job seekers, Czech people have fewer opportunities to find employment."

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

To what extent do you agree with the following statements?
A. "Ukrainian job seekers produce more advantages than disadvantages for the Czech labor market."
B. "Asian job seekers produce more advantages than disadvantages for the Czech labor market."

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree

- Strongly disagree

A. How comfortable or uncomfortable would you feel with having a Ukrainian neighbor?
B. How comfortable or uncomfortable would you feel with having an Asian neighbor?

- Very uncomfortable
- Somewhat uncomfortable
- Neither uncomfortable nor comfortable
- Somewhat comfortable
- Very comfortable

Perceived social distance (1/2 of follow-up participants)

The order of groups A,B,C was randomized.

How likely is it that you would become friends with
A. an ordinary person?
B. an HR manager?
C. a researcher who primarily studies issues that ethnic minorities face in the Czech Republic?

- Very likely
- Likely
- Somewhat likely
- Unlikely
- Very unlikely

Perceived accuracy (1/2 of follow-up participants)

The order of below three questions was randomized.

Suppose that a group of 9 passers-by is asked to estimate the extent of ethnic discrimination in the Czech labor market. In your opinion, how accurate would the average estimate of this group be?

- Very accurate
- Accurate
- Somewhat accurate
- Inaccurate
- Very inaccurate

Suppose that a group of 9 HR managers is asked to estimate the extent of ethnic discrimination in the Czech labor market. In your opinion, how accurate would the average estimate of this group be?

- Very accurate
- Accurate
- Somewhat accurate
- Inaccurate
- Very inaccurate

Suppose that a group of 9 researchers who primarily study issues that ethnic minorities face in the Czech Republic is asked to estimate the extent of ethnic discrimination in the Czech labor market. In your opinion, how accurate would the average estimate of this group be?

- Very accurate
- Accurate
- Somewhat accurate
- Inaccurate
- Very inaccurate

Collection of posterior beliefs

Before answering the below question, please read CAREFULLY the whole text:

Researchers from CERGE-EI (Center for Economic Research and Graduate Education - Economics Institute) conducted a study to estimate the prevalence of discrimination against Asian people in the Czech labor market. The research team sent applications from several groups of job seekers responding to job openings on www.jobs.cz. In the first group a job seeker was Czech and in the second group he was Asian.

The job seekers differed only in their name, which signaled ethnicity. Their education and experience were exactly the same. The following names were assigned to the job seekers: Jiří Hájek (Czech-sounding name) and Phan Quyet Nguyen (Asian-sounding name).

The applications were sent out to jobs in different parts of the Czech Republic. The jobs were in the field of administration, sales, and customer service.

The researchers have found that a job seeker with a Czech-sounding name has to send on average 7.5 applications in order to receive one interview invitation.

In your opinion, how many applications does a job seeker with an Asian-sounding name have to send to receive one interview invitation?

If your answer is the same as what CERGE-EI researchers (the authors of the study) have found, you will be rewarded a bonus of 5 Czech crowns in addition to the participation fee. You will learn whether or not you will receive the bonus at the very end of the questionnaire.

Confidence in a posterior belief

How sure are you about your estimate?

- Very sure
- Sure
- Somewhat sure
- Unsure
- Very unsure

Prediction of average estimates across 3 groups (Control group only)

We asked 3 groups of people to estimate the number of applications a job seeker with an

Asian-sounding name has to send to receive one interview invitation. In your opinion, what was an average estimate of

- 9 passers-by
- 9 HR managers
- 9 researchers who primarily study issues that ethnic minorities face in the Czech Republic

Three (above) alternatives appeared in a random order.

Willingness to share information (Exogenous-Info groups only)

In a previous survey you were told that the average person in a group of 9 passers-by/9 HR managers/9 researchers who primarily study issues that ethnic minorities face in the Czech Republic[72] thinks that a job seeker with an Asian-sounding name has to send 14 applications to receive one interview invitation.
How willing are you to share this information with your friends?

- Very willing
- Willing
- Somewhat willing
- Unwilling
- Very unwilling

Searching for a number from the study by CERGE-EI researchers

Did you look for the CERGE-EI researchers' findings (specifically for the number of applications a job seeker with an Asian-sounding name has to send to receive one interview invitation) after you had learnt about their study in the previous survey?

- Yes
- No

[72] A type of the information source that participants saw depends on the group to which subjects were randomly assigned in the first wave.

Donation

Participants were asked this question at the very end of the survey, after they gave feedback about the questionnaire.

What would you like to do with the reward that you could claim due to completion of the survey?

- Send to my bank account
- Donate to non-profit organization INFO-DRÁČEK (www.info-dracek.cz), which helps Vietnamese people to integrate into Czech society (Note: This non-profit organization is not related to the client who ordered the survey.)
- I do not want the reward, I would like to finish this questionnaire

Chapter 2

Do Pessimistic Expectations About Discrimination Make Minorities Withdraw Their Effort? Causal Evidence

Abstract

Darya Korlyakova[1]

There is a long-standing concern that expected discrimination discourages minorities from making efforts to succeed. Effort withdrawal could contribute to confirming negative stereotypes about minorities' productivity and enduring disparities. This chapter extends the findings of correlational research by exogenously manipulating individuals' beliefs about discrimination against their group and exploring a causal link between perceived discrimination and individuals' labor market behavior. For this purpose, we conduct an online experiment in the US with a diverse sample of 2,000 African Americans. We randomly assign individuals to two groups and inform one group about the

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frequency of discrimination against African Americans in a previous survey. To study the information effects on effort, we subsequently measure participants' results on a math task. We document that most individuals initially overestimate discrimination against African Americans. The overestimation decreases strongly and significantly as a result of information provision. At the same time, treated individuals, males in particular, attempt and correctly solve *fewer* math problems than untreated individuals. Hence, our findings do not support the common concern that minorities' inflated expectations about discrimination induce them to underperform.

Key words: perceived discrimination, racial minorities, effort
JEL Classification: C99, D83, J15

2.1 Introduction

There is a long-standing concern that if a discriminated group reduces effort in response to expected discrimination, these expectations reinforce negative stereotypes about productivity of the group and perpetuate inequality. So far, this concern has been empirically tested by studying correlations between beliefs about discrimination against one's own group and job-related attitudes or behavior. The literature has found that individuals who perceive higher discrimination are less satisfied with their job, less committed to the organization, more likely to be absent at work, and more inclined to leave the firm (e.g. Ensher, Grant-Vallone, and Donaldson, 2001; Jones, Ni, and Wilson, 2009; Foley, Kidder, and Powell, 2002).

Despite the abundance of correlational research, causal evidence is lacking on how the prospect of facing discriminatory treatment by employers affects minorities' behavior. There are several ways in which individuals could respond to anticipated discrimination in terms of their effort. On the one hand, reducing minorities' pessimism about discrimination against them could motivate them to try harder and perform better, because now a fairer reward for their effort is expected. On the other hand, learning about lower discrimination may make minorities feel less in need of compensating their group disadvantage by impressing a majority employer with hard work. Hence, they may keep their effort unadjusted or even decrease it.

In this paper, we present evidence that minorities' pessimistic expectations about discrimination against their racial group do *not* hurt their performance. Our evidence is based on an online experiment conducted in the US with a representative sample of 2,000 African Americans. We randomly assign our participants to two groups and elicit their prior beliefs about discrimination against African Americans in a previous survey. In the treatment group, subjects are subsequently informed about the actual frequency of discrimination against African Americans. The information-provision stage is omitted for the control group. We chose an information intervention because, consistent with previous research that uncovers ubiquitous public misperceptions on a variety of topics[2], we hypothesize that minorities tend to hold inaccurate beliefs about the frequency of discriminatory treatment applied to them[3].

In the second part of the experiment, we measure treatment effects on subjects' beliefs about discrimination against their own racial group and collect a number of behavioral outcomes. Understanding whether minorities revise expectations in response to our information is important to explain changes in their subsequent behavior. Specifically, we are interested in subjects' effort on a mathematics assignment. Before proceeding with the assignment, participants are informed that the overall assignment earnings will be divided between them and a White person by another White respondent who will act as the third party. Individuals are aware that the third party may know both individuals' races and assignment performances. After the task is completed, we elicit participants' preference for a race-blind allocation of the math assignment earnings and measure subjects' willingness to exaggerate their assignment performances. A choice of the latter outcome was inspired by experimental research showing that individuals feel comfortable acting immorally or selfishly if they can justify their "questionable" behavior (e.g. Falk, Neuber, and Szech, 2020; Engel and Szech, 2020; Exley, 2020; Exley, 2016). In our study, an excuse for being dishonest would be a person's belief that he or she would eventually be treated unfairly, i.e. with a very high probability the third party would allocate money in favor of a White individual.

[2]See, for instance, Cruces, Perez-Truglia, and Tetaz (2013); Kraus et al. (2019); Bartoš et al. (2022) or Bursztyn and Yang (2022) for a recent review.

[3]This hypothesis is also somewhat aligned with supplementary findings by Haaland and Roth (2021), who have found that the majority of respondents overestimate the extent of racial discrimination in the US and that, relative to White respondents, African Americans hold similar or more extreme misperceptions regarding discrimination. However, the authors document the effects using a small sample of African Americans, and the focus of their paper is different: Haaland and Roth study a causal link between public beliefs about discrimination in local society and public support for pro-black policies.

Our main results are the following. First, we find that the vast majority of our participants (96.2%) initially overestimate discrimination against African Americans. Misperceptions, which are typical for all demographic groups, are large, with most subjects believing that 50 or fewer of White individuals (out of 100) did not discriminate against African Americans in the previous survey. In contrast, we observe that 87 White respondents did not favor a White person in the money allocation task. This finding implies that choosing information treatment as a basis for our design hinged on a correct assumption that information gaps are widespread among minorities. Second, our intervention causes substantial reduction in misperceptions, resulting in 17.8 percentage point more optimistic posterior beliefs of those who were exposed to information about actual discrimination. Third, informed individuals attempt 1.75 percent *fewer* math problems; the information effects on task performance are also negative but do not reach significance. Pre-specified heterogeneity analysis by gender indicates that favorable news about discrimination only reduces the effort of males, i.e. they attempt and solve correctly significantly fewer math problems. Finally, anticipating less discrimination is not accompanied either by lower willingness to pay for the race-blind allocation of the assignment earnings or by less frequent exaggeration of one's own assignment results.

This paper adds to growing experimental literature on information provision in the context of the online environment (e.g. Haaland and Roth, 2021; Haaland and Roth, 2020; Grigorieff, Roth, and Ubfal, 2020; Alesina, Miano, and Stantcheva, 2018; Lergetporer, Piopiunik, and Simon, 2021). A resemblance between these experiments and our work is that the former randomly expose some respondents to a credible message based on survey results, research evidence or statistics. Whereas previous research focuses on beliefs and behavior of majority members or representative samples, we study racial minorities' responses to information about a controversial issue using a large diverse sample of African Americans.

Existing literature often identifies the adverse consequences of discrimination or stereotyping on individuals' performance and their education- or career-related choices (e.g. Lavy and Sand, 2018; Carlana, 2019). Other studies document that working or studying under the guidance of negatively biased individuals depresses minorities' effort on tasks that biased individuals supervise (e.g. Glover, Pallais, and Pariente, 2017). Unlike these two strands of literature, we study minorities' effort responses to anticipated discrimination by an unfamiliar majority member. This may correspond to a real-life situation

in which individuals start a new job and have not yet had personal experience with a majority employer. Shedding light on minorities' effort shortly after hiring is important because it may contribute to the employer's first impression.

Our paper also contributes to the literature that explores how historically discriminated groups alter their behavior in response to potential discrimination, e.g. women decide not to reveal their true gender on a male-stereotypical task (Charness et al., 2020) and are even willing to pay to exclude gender from their resume-like profiles (Alston, 2019). Female economists improve the clarity of their writing to meet higher standards that are likely to be imposed on them (Hengel, 2022). In an attempt to avoid discriminatory treatment, immigrants undertake multiple assimilation steps (Fouka, 2019), racial minorities resort to "resume whitening" to conceal their race (Kang et al.,2016) and ethnic minorities engage in misrepresenting of their ethnicity (Kudashvili and Lergetporer, 2019). The key difference of our study is that earlier work does not pin down the role of beliefs about discrimination as a channel that may underlie minorities' behavioral adjustments to a potentially unfavorable environment[4].

Collecting direct causal evidence on beliefs allows us to rule out a number of alternative explanations. For instance, individuals might not change or might even decrease effort in response to optimistic news about their discrimination because we provide information on a controversial issue that could trigger strong emotions and cause major disagreement. Hence, individuals might stick to their initial misperceptions or move their beliefs even further from the truth. Such a backfire effect is not supported by our data: 80.7% of treated subjects shift their beliefs in an expected direction[5]. Furthermore, thanks to eliciting beliefs, we can exclude a possibility that asymmetric behavioral responses of males and females to our information are driven by differential belief updating.

In a related paper, Gagnon, Bosmans, and Riedl (2020) study the effects of unequal chances and their sources (e.g. gender discrimination) on labor supply. The authors inform a man and a woman paired together about a payment scheme before they perform a real-effort task. In one of the treatments, the authors aim to fix beliefs about discrimi-

[4]In a post-experimental survey, Alston (2019) asked participants to guess which worker - a man or a woman - the managers will choose. The author also elicited beliefs about gender discrimination in the real world and at Amazon MTurk in particular. These beliefs are, however, endogenous because they are subject to influences of unobservable factors that could also correlate with subjects' willingness to pay to reveal or conceal their own gender.

[5]8.2% of the treatment group state the same prior and posterior belief.

nation by mentioning to workers that their chances depend on their gender. However, it remains unclear whether subjects misperceive, i.e. overestimate, discrimination against women ex-ante. It is possible that information affects labor supply through emotions in addition to subjects' beliefs or instead of beliefs. In Study 2, the authors ask all subjects at the end of the experiment to what extent, in their opinion, gender discrimination is used to determine their wage. This non-incentivized measure provides correlational evidence on the role of beliefs. Our paper goes one step further by exploring the causal impact of beliefs about discrimination against one's own group on minorities' effort.

The rest of the paper is organized as follows. Section 2 discusses the experimental design and our sample. Section 3 presents the experimental results, and Section 4 concludes.

2.2 Experimental design and sample

2.2.1 Sample

We recruited 2,000 African American respondents[6] with the help of Prodege, which is a US market research agency. Our sample is broadly representative of the African American population residing in the US in terms of key observable characteristics. Table 3.B.1 illustrates that our sample matches the respective population with respect to gender, age, and region. Due to difficulties experienced in recruiting low-educated respondents, we slightly deviate from the sample representativeness along the education dimension. To address minor imbalances, we re-weight the data and discuss the results of this exercise in the robustness subsection. The sample size and its composition, as well as main hypotheses and empirical analysis were pre-specified: https://www.socialscienceregistry.org/trials/6734. Table 2.B.2 presents summary statistics for our sample. Table 2.B.3 demonstrates that randomization was successful, i.e. the treatment group and control group are well balanced in terms of observables.

2.2.2 Experimental design

In the beginning, we asked demographic questions and elicited respondents' prior beliefs about discrimination against their own racial group. Next, we randomly assigned one half

[6]Initially we obtained 2,003 responses, but we exclude 3 respondents from Puerto Rico from our analyses to construct regions in line with US Census Bureau classification. The results from regressions without controls remain the same if all 2,003 observations are used.

of our respondents to the information treatment. Subsequently, all subjects performed a real-effort assignment and were asked to state their posterior beliefs about discrimination. Then, we measured subjects' willingness to pay for race-blind allocation of their assignment earnings. We also asked participants to check their assignment results and to report their performance. In the very end, subjects answered additional background questions[7]. Figure 2.A.1 summarizes the main stages of our experiment.

Prior beliefs about discrimination against one's own racial group

To gain insight into the existence of original misperceptions, we first collected individuals' prior beliefs about the prevalence of discrimination against their racial group. For these purposes, we truthfully informed participants that we had conducted a survey with 100 White respondents who were asked to allocate a monetary amount (2 USD) between a White person and an African American person. Thereafter, we asked subjects to estimate (i) how many out of 100 White respondents gave more to a White person and (ii) how many of these individuals gave at least the same amount to an African American person as they gave to a White person[8]. The latter guess was incentivized in the following way. We told experimental participants that we would randomly choose 100 of them and pay these individuals 5 USD in addition to their participation fee if their estimate was equal to the actual number of White respondents who gave the same amount or more to an African American person relative to a White person.

We acknowledge that our measure of discrimination is mostly representative of contexts in which White individuals cannot disguise their preferential treatment of a White person. More ambiguous and complex settings, which enable people to excuse or misrepresent their socially inappropriate behavior, are likely to give rise to covert racism and increase the share of people who tend to discriminate against African Americans but are reluctant to express this preference openly. Nevertheless, the goal of this paper is not to identify the overall level of racial discrimination in the US society. We are interested in investigating whether minorities expect widespread unfair treatment against them even when

[7]We decided not to collect all demographics before treatment to minimize fatigue effects and to avoid priming subjects with sensitive questions.

[8]We included both questions to prevent participants from thinking that researchers find one of the scenarios - that White respondents favored a White person or that they did not discriminate *against* an African American person - more probable or more interesting. To minimize the risk of careless answers, we used a response validation that did not allow subjects to move to the next page if the numbers they had recorded did not sum up to 100.

contextual features of the decision environment do not allow the majority group to justify their discriminatory actions.

Treatment: survey evidence of discrimination against African Americans

Next, we informed a random half of our respondents (the treatment group) about the true prevalence of discrimination against their racial group in the previous survey. In particular, participants learned that, according to the survey results, 87 out of 100 White respondents had implemented an equal split or allocated more to an African American person. Subjects also saw a bar chart comparing the actual number to their own estimate.

The information-provision stage was omitted for the control group that directly proceeded to the real-effort assignment.

Outcome I: Performance on the real-effort assignment

To investigate whether information about discrimination against one's own racial group affects people's productivity, we offered all subjects to answer 20 math multiple-choice questions (problems) during a 5-minute interval. For this purpose, we borrowed some math problems from a recent paper by Bohren et al. (2019). We preferred the math assignment over a motor, mindless task or a clerical task because we expected that participants would find it engaging and cognitively demanding[9] and thus would care about their performance on the assignment. We measured both (i) the number of problems that our subjects solved correctly and (ii) the number of problems that they attempted. Participants were allowed to skip questions in this part of the experiment. We explicitly asked subjects not to use a calculator or online help but to do their best. To prevent looking up the answers, we limited the maximum time that a person could spend on each page with math problems to 60 seconds.

Before allowing participants to proceed with the math assignment, we explained to them how they would be paid for their work. Specifically, we told subjects that they would first be matched with a random White respondent (the other person) who would complete the same assignment. Later, a randomly selected third party, a White respondent[10], would

[9]While providing feedback to our survey, some respondents explicitly mentioned their engagement with the math assignment.

[10]We did not explicitly state that the third parties were selected from a different pool of respondents than 100 White individuals from the previous survey. If some of our treated subjects thought that information describes third parties' behavior, they would consider our message to be more relevant for

allocate the assignment earnings in the total amount of 2 USD between them and the other person. We explicitly mentioned that the third party may be informed about each person's race and their productivities on the assignment[11].

To ensure that subjects carefully read the instructions and understood the payment scheme, we asked them to complete a short comprehension check, which consisted of three control questions, before starting the assignment. Participants had to decide whether it is true or false that (i) they would be randomly matched with a White person; (ii) the third party, another White respondent, would allocate assignment earnings between them and the other person; and (iii) the third party may see information about races and productivities before making the allocation decision. See Appendix 2.C for precise formulations of the statements. Subjects were also provided with an opportunity to return to the instructions (before submitting their answers) by pressing the back button.

Outcome II: Posterior beliefs about discrimination against own racial group

After the assignment was completed, we collected individuals' posterior beliefs about the prevalence of discrimination against their racial group. This measure allows us to study whether the information treatment induces exogenous shifts in subjects' perceptions regarding discrimination, which may underlie subsequent changes in subjects' behavior. The posterior beliefs were elicited by asking participants to predict the behavior of the third parties who would allocate the assignment earnings between a White person and an **equally productive** African American person from their survey. The belief elicitation procedure was incentivized in a similar manner as before. Subjects were informed that they would receive an additional 5 USD (i) if they were among 100 randomly chosen individuals and (ii) if their guess about the percentage of the third parties who would decide on an equal split or would give more to an African American person equaled to the actual percentage of such third parties[12].

their situation (which does not go against our intentions).

[11]We used "may" in the instructions to avoid deception. Fiorin (2021) used a similar framing so that subjects would perceive their whistleblowing as consequential. Eventually the author did not forward any subjects' reports about their colleagues to the Ministry of Education to prevent harming employees as a result of his study. In our experiment, the third party would not necessarily see the subject's and other person's race because, at a later stage, subjects had a chance to conceal race-related information (see Outcome III). Furthermore, in 50% of cases, the third party would not see true productivities. We introduced this uncertainty to incentivize careful reporting of productivities by subjects which constituted a basis for one of our outcomes of interest. See Outcome IV for further details.

[12]We find that in 90 percent of cases third parties do not discriminate against African Americans. This number is very close to the number of White respondents (87 out of 100) who behaved in a non-

We chose not to ask an identical question while eliciting prior and posterior beliefs for several reasons. First, our goal was to measure genuine updating instead of "anchoring" to the number that we showed to treated participants[13]. Second, we aimed to detect whether information about previous discrimination induced changes in expectations about future discrimination because the latter are a channel that may underlie minorities' labor market decisions, such as the supply of effort.

Outcome III: Willingness to pay for race-blind allocation of assignment earnings

Minorities may respond to learning about the true extent of discrimination against their group not only by adjusting their effort, but also in terms of willingness to disclose sensitive information about themselves. In this regard, we elicited the subjects' marginal rate of substitution between (i) money for themselves and (ii) hiding race-related information from the assignment evaluator, i.e. the White third party. We used a multiple price list that consisted of six decision tasks presented in rows. In each row, individuals made a choice between concealing their and the other person's race from the third party and extra money for themselves. As participants moved down the list, the monetary amount was increasing in 20-cent increments from 0 USD to 1 USD. We incentivized subjects to choose in accordance with their true preferences by telling them that one of their six decisions would be randomly chosen for actual payoff.

Outcome IV: Self-reported performance on the math assignment

Finally, minorities may find it less psychologically costly to engage in dishonest behavior, by exaggerating the number of problems they solved correctly[14], if they are more likely to expect that they will be treated unfairly by the third party. To probe this hypothesis, we reminded subjects of their choices by showing them their answer to each math question[15]. Next to the list of the subject's answers, we presented the answer key. To evaluate their

discriminatory way in the previous survey. Taking into account that (i) we collected original data from White respondents in cooperation with a different survey agency (Kantar) and (ii) the initial survey did not mention equal productivity of money recipients, the frequency of (explicit) discrimination against African Americans that we observe seems to be stable in the online environment.

[13]This concern would be less relevant if we additionally collected beliefs in an obfuscated follow-up survey (see, for instance, Haaland and Roth (2021)).

[14]Other experimental studies also resort to reported performance to give subjects an opportunity to cheat (e.g. Schwieren and Weichselbaumer, 2010; Mazar, Amir, and Ariely, 2008).

[15]If a participant did not make any choice, he/she saw blank space next to the corresponding question.

performance, participants had to count the number of rows in which their choices and correct answers coincided. We encouraged participants to take the self-evaluation task seriously by truthfully informing them that with a 50 percent chance the third party would see their and the other person's productivity according to their self-reports. In this case, no information about *true* productivities would be revealed to the White respondent responsible for allocation.

2.3 Results

This section presents our main results. First, we document inflated prior beliefs about discrimination against African Americans among the vast majority of our respondents. Second, we discuss the substantial reductions in subjects' misperceptions as a result of the information treatment. Third, we argue that overestimating discrimination does not have a detrimental effect on minorities' assignment performance. If anything, informing minorities about less discrimination *reduces* their subsequent effort. Fourth, we note that the information treatment does not move the remaining outcomes. We end the section by subjecting our results to a number of robustness checks.

2.3.1 Prior beliefs about discrimination against one's own racial group

To establish the rationale for our information treatment, we first explore whether our experimental participants have misperceived discrimination against African Americans. Figure 2.1 shows kernel densities of beliefs about the number of White respondents from the previous survey who allocated the same amount or more to an African American person relative to a White person. The densities have two peaks at about 25 and 50. The number highlighted in the figure (87) represents the actual number of White respondents and serves as the benchmark for categorizing subjects into underestimators and overestimators. We find that most of our participants (96.2%) initially overestimate discrimination against African Americans. A mean (median) subject believes that only 36.4 (35) White respondents allocated at least the same amount to an African American person as to a White person.

[Figure 2.1 here]

Upward biased perceptions prevail across all demographic groups and they are large in magnitude. Figure 2.A.2 illustrates unconditional mean beliefs across demographics[16]. While younger individuals on average believe that 39 White individuals (out of 100) did not discriminate against African Americans, older individuals are somewhat more pessimistic about discrimination (mean belief = 33.7). Democrats on average believe that 34.9 White individuals allocated at least the same amount to an African American person as to a White person. Compared to them, Independents and Republicans are closer to the truth (mean belief = 39.5). Females expect from White individuals more discriminatory treatment relative to males (mean belief equal to 34.8 and 38.3, respectively). Subjects with at least some college experience on average believe that 35.3 White individuals showed equal or preferential treatment toward African Americans. The magnitude of misperceptions among lower educated individuals is somewhat lower (mean belief = 38.3).

Below we summarize our first result.

Result 1*: Minorities vastly overestimate discrimination against their racial group. Original upward biased misperceptions of discrimination span across demographics.*

2.3.2 Treatment effects on beliefs about discrimination

Next, we examine the information treatment effects on subjects' beliefs about the percentage of White third parties who will allocate the same amount or more to an African American person from the current survey relative to an equally productive White person. Exploring changes in perceived discrimination is important because (i) they signal participants' attentiveness (or lack thereof) to our information and agreement with it, and (ii) they may explain patterns in subjects' post-treatment behavior that we also measure.

[Figure 2.2 here]

Both our graphical evidence and regression results indicate that information provision causes significant and economically meaningful shifts in subjects' beliefs about discrimination against African Americans. Column 1 of Panel A in Table 2.1 shows that the treatment on average raises beliefs about the percentage of White third parties who will

[16]Differences across groups (e.g. higher and lower educated individuals; Democrats and Republicans/Independents) remain directionally similar and are significant if one regresses prior beliefs on different demographics simultaneously.

not discriminate against African Americans from the current survey by 17.8 percentage points (control mean = 47.13%, p<0.01). Figure 2.2 demonstrates that the treatment moves the density of posterior beliefs about *no* discrimination against one's own racial group to the right of the control-group density[17].

[Table 2.1 here]

Our pre-specified heterogeneity analyses show that significant updating of beliefs about discrimination is a general phenomenon, rather than being driven by certain subgroups of our participants (see Column 1 in Table 2.2 and in Tables 2.B.4-2.B.5). For example, treated females and males hold posterior beliefs by 17 and 18.8 percentage points, respectively, closer to the truth compared to their untreated counterparts[18]. Nevertheless, individuals with at least some college experience tend to update their beliefs significantly more strongly compared to lower-educated counterparts.

We do not perform pre-specified heterogeneity based on prior beliefs, by splitting the sample into overestimators and underestimators, due to the lack of variation in this variable (as mentioned in Subsection 2.3.1). However, we explore whether individuals who are ex-ante more uninformed about discrimination against their racial group (i.e. hold more inaccurate prior beliefs) are more likely to revise their beliefs. Table 2.B.6 shows that this is in fact the case. Treated subjects with below-median priors, i.e. beliefs that are more distant from the true number, state a posterior belief of no discrimination against African Americans that is about 21 percentage points higher relative to a posterior belief of untreated subjects (whose prior lies in the same range). Having a prior above or equal to the median contributes to a difference of 14.4 percentage points between the treatment and control group, which is statistically different from the previous number (p<0.01).

Based on the findings in this subsection, our second result is:

***Result 2**: Information about discrimination against own racial group strongly affects minorities' beliefs.*

[17]The Appendix presents evidence that original perceptions of discrimination do not differ across two groups. Table 2.B.3 shows that the means of prior beliefs are virtually the same (p=0.97) and Figure 2.1 illustrates a strong resemblance between the control and treatment densities of prior beliefs.

[18]The regressions without controls lead to similar results.

2.3.3 Treatment effects on real-effort task performance

In this subsection, we investigate the causal effects of perceived discrimination on subjects' effort and performance on a math assignment. Given that beliefs about discrimination against one's own group are relevant for minorities' labor market behavior, two scenarios are possible. In the first case, informing individuals about less prevalent discrimination may increase a subjective probability of succeeding in the math assignment and thus will serve as a motivation for higher effort. This prediction agrees with the findings of correlational studies. We use data from the control group respondents to investigate correlations between beliefs about the percentage of third parties who would send at least the same amount to an African American person as to an equally productive White person and effort. Column 2 in Table 2.B.7 shows a positive but insignificant correlation between subjects' beliefs and math problems solved correctly. A one standard deviation increase in beliefs about *no* discrimination against African Americans is associated with a 0.05 of a standard deviation higher performance on the math assignment (p=0.12). This association is reduced to 0.04 of a standard deviation when controls are included (p=0.16).

In the alternative scenario, higher optimism regarding expected discrimination may make minorities consider their race a less negative signal and hence will reduce their effort. As the third party is expected to be less biased as a result of information provision, minorities should be less concerned about counteracting a negative stereotype applied to their racial group.

Our causal evidence contrasts the results of correlational research: minorities do *not* tend to respond to lower perceived discrimination with higher effort. Column 2 of Panel A in Table 2.1 shows that treated individuals attempt on average 0.31 fewer math problems, which represents a 1.75 percent decrease compared to the control group mean equal to 17.75 problems ($p<0.05$). The information-treatment effects on the number of math problems solved correctly (Column 3 of Panel A in Table 2.1) are also negative but do not reach statistical significance (control mean = 12.15 problems, $p = 0.20$)[19].

Pre-specified heterogeneity analysis by gender reveals that the negative information effects on effort are driven by males. Column 2 in Table 2.2 shows that treatment decreases the

[19]To demonstrate that there was room for improving results on the math assignment among treated subjects compared to the control group, we present a histogram of actual performance in the Appendix (Figure 2.A.3). Only 2.6 percent of our subjects solved all 20 math problems correctly.

number of problems attempted by males by 0.43 ($p<0.05$). This decrease constitutes a 2.4 percent change relative to the control group mean equal to 18.1 problems. Moreover, an average treated male solves 0.63 fewer math problems correctly, which represents a 4.8 percent decrease compared to an average male performance of 13 in the control group (Column 3 in Table 2.2, $p<0.05$). Both information effects are insignificant for females. Interestingly, the information treatment completely eliminates the conditional gender gap[20] in effort and performance on the math assignment which exists in the control group[21].

[Table 2.2 here]

The finding that males, but not females, exhibit significant behavioral responses to information about discrimination against their own racial group cannot be explained by patterns in posterior beliefs described earlier. Recall that both genders in our experiment state similar beliefs after receiving information about discrimination against African Americans in the previous survey (Column 2 in Table 2.2). In a similar vein, Koutout (2022) finds that *only* males' job application decisions are responsive to beliefs regarding hiring managers' beliefs about the relative productivity of females and males.

Differential effects of perceived discrimination on effort across genders could be reconciled with the conclusions of research on competitiveness. Subjects are likely to consider the environment we create as competitive because we emphasize (i) that there is a common pool of assignment earnings that will be divided between a subject and a White person and (ii) that the productivities of both individuals may be revealed to the allocator[22]. Being informed about relatively uncommon preferential treatment of Whites may make subjects perceive the other person's race as less advantageous and thus they will view the environment as less competitive compared to the control group[23]. Previous work (Niederle et al., 2011; Gneezy, Niederle, and Rustichini, 2003; Günther et al., 2010; Price, 2008; Morin, 2015) frequently shows that, in response to *more* aggressive competition, males perform better while females' performance is generally unaffected, at least in the case of male-stereotypical tasks.

[20] This gap is defined as differences between males and females after we adjust for a set of pre-specified covariates (e.g. education).

[21] An untreated man attempts on average 0.51 more problems and solves correctly 1.1 more problems relative to an untreated woman ($p<0.01$).

[22] See, for instance, a scheme in Appendix 2.C that conveys these two messages. To summarize the key features of the payment process, we showed the scheme to all participants shortly before the assignment.

[23] Future work should explicitly verify this conjecture.

Summarizing the findings in this subsection, we state our third result as follows:

***Result 3**: Pessimistic expectations regarding discrimination against one's own racial group do not make minorities underperform.*

2.3.4 Treatment effects on other outcomes

Our subsequent discussion focuses on information effects on (i) willingness-to-pay for race-blind allocation of assignment earnings and (ii) misreporting of one's own performance on the math assignment. Expecting less discrimination may make minorities less motivated to sacrifice extra money in order to hide their and the other person's race from the third party. In addition, if minorities start believing less strongly that the third party will act unfairly in relation to them, it may become harder for them to justify dishonest behavior and hence they will be less prone to exaggerate their performance[24].

Column 4 in Table 2.1 shows information effects on willingness-to-pay for race-blind allocation. In line with the pre-analysis plan, we construct the former variable by counting the number of rows (out of 6) in which subjects choose to withdraw information about their and the other person's race from the third party over extra money for themselves[25]. The mean number of rows in the control group is 2.9, with standard deviation equal to 1.99. Only 16.9% of untreated individuals never choose to conceal race-related information. Next, we standardize willingness-to-pay using the mean and standard deviation of the control group. We do not find that positive news about discrimination makes minorities less willing to pay for concealing information about races from the third parties (p=0.95).

Finally, we explore differences in performances reported by subjects at the end of the experiment. As described earlier, the mean actual performances in the control and treatment groups are 12.15 and 11.91 problems, respectively. For a comparison, an average untreated (treated) subject *reported* having solved correctly 12.68 (12.35) problems. Column 5 in Table 2.1 presents the results of a regression in which the dependent variable is the extent to which participants misreport their math results, calculated as the difference

[24]Columns 3 and 4 in Table 2.B.7 display insignificant associations between untreated subjects' beliefs about discrimination and their willingness to conceal race information from the third party and to exaggerate their performance.

[25]Haaland and Roth (2021) used a similar approach while measuring willingness to donate to a pro-black civil rights organization.

between a subject's reported and real number of correctly solved math problems. We find muted information effects on subjects' propensity to overstate their assignment results (p=0.59)[26]. However, we acknowledge that ceiling effects may have contributed to the null results: 63.6% of untreated subjects report their performance correctly and those who misreport exaggerate their performance on average only by 1.44 math problems.

Thus, our fourth result is stated as follows:

***Result 4**: Minorities' willingness-to-pay for race-blind allocation of their assignment earnings and decision to exaggerate their math assignment performance do not generally respond to favorable information about discrimination against their racial group.*

2.3.5 Robustness

To test the validity of our findings, we present the results of several robustness checks in this subsection. First, Panel B in Table 2.1 shows that the information-treatment effects stay virtually the same if a pre-specified set of covariates is included in the regressions[27]. Second, Panel A in Table 2.B.8 demonstrates that excluding subjects who did not pass the comprehension check[28] affects the significance of our estimates but the magnitudes are close to the magnitudes of the original treatment effects. It is worth noting that we informed all subjects about the correct answers to the control questions immediately after they completed the comprehension check. Third, excluding respondents in the top 2% and bottom 10% of the time spent on the entire survey (Panel B in Table 2.B.8) confirms the robustness of our findings[29]. Finally, as Panel C in Table 2.B.8 shows, re-weighting observations (to reflect a slight under-representation of African Americans with very low

[26]While splitting the sample by political views, we find that non-Democrats, who make up 34% of our subjects, respond to the information treatment in terms of their behavior somewhat more strongly compared to Democrats. Table 2.B.5 shows that, relative to untreated non-Democrats, treated non-Democrats exaggerate their performance less (p<0.10) and attempt and solve correctly fewer math problems (p<0.05 and p=0.16, respectively). Treatment effects on the same outcomes are always far from being significant for Democrats. The identified differences along political lines seem to be intuitive and suggest that Democrats are reluctant to adjust their behavior in situations that may involve discrimination against them, even if they start perceiving this discrimination as less likely than before.

[27]Due to a coding error, I omitted the category "part-time employee" when asking participants about their current employment status. Hence, it is possible that some individuals who work part-time may have chosen "full-time employee" or the "prefer not to answer" category. Our causal results are robust to including different sets of the employment-status dummies.

[28]We consider that a subject did not pass the comprehension check if he or she answered *at least* one of three control questions incorrectly. Such definition implies that 57 percent of our participants passed the comprehension check. The passing rate does not differ across the treatment and control group (p=0.94).

[29]Our lower and upper boundaries follow the choice of Alesina, Miano, and Stantcheva (2018).

education in our sample) does not significantly affect the treatment effects[30].

2.4 Conclusion

In this paper, we test the effects of an information intervention on minorities' beliefs regarding discrimination against their own racial group and labor market behavior. Two features - (i) exogenous manipulation of minorities' beliefs and (ii) subsequent measurement of minorities' causal behavioral responses to perceived discrimination - distinguish our study from previous research. We document that minorities largely misperceive discrimination and that their beliefs about racial inequality are systematically upward biased. Providing information about actual discrimination in the previous survey effectively reduces minorities' pessimism and somewhat *decreases* their effort on the assignment. Therefore, our findings do not support the common concern that minorities' inflated expectations about discrimination induce them to underperform.

Choosing an online experiment as a research method grants us a number of advantages. It creates a controlled environment in which we could causally identify the role of an important mechanism - minorities' beliefs - in prompting preventive responses to potential discrimination. At the same time, our participants are less likely to feel under observation compared to a laboratory experiment and thus their decisions are more similar to those they would make in a natural setting. In the light of the COVID-2019 pandemic, which initiated a massive transition to remote work, online experiments have started to resemble real settings even more closely. Therefore, our findings on effort have increased potential to be externally valid in the new reality of the workplace.

Our initial causal evidence informs policymakers that campaigns aimed at correcting minorities' beliefs about discrimination against them, which undoubtedly have benefits such as raising awareness, could ultimately decrease minorities' effort. This information could encourage policymakers to more rigorously test a promising policy to prevent a scenario in which unintended consequences of the policy are revealed only after it is implemented. Examples of such policies are not difficult to find. Leibbrandt and List (2018) find that equal employment opportunity statements, commonly used by companies around the world, discourage racial minorities from applying for jobs. The authors' complementary

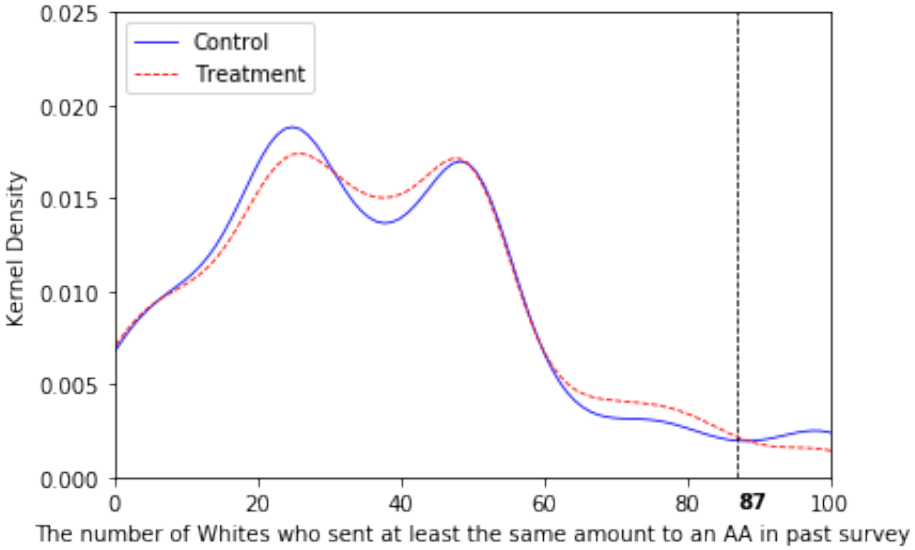
[30]When computing the weights, we used a raking procedure (DeBell and Krosnick, 2009). Although age, gender, and geography strongly resemble the respective population shares, we target them in addition to education in order to make the sample fully balanced along these dimensions.

analysis suggests that unexpected effects are driven by minorities' reluctance to be hired due to regulation rather than thanks to their own merits.

Our results provide a first step towards understanding the causal effects of perceived discrimination, thereby offering several fruitful avenues for the future research. First, it remains an open question whether the effects of information about discrimination will persist. In this study, we collect beliefs and behavioral measures immediately after exogenously providing the survey results on past discrimination. Furthermore, it is important to credibly measure whether minorities whose pessimistic expectations were re-calibrated experience less stress while performing a real-effort task. Using experimental tools for the purpose of this exploration will complement the findings of previous research (e.g. Sims et al.,2012; Pascoe and Smart Richman, 2009; Pavalko, Mossakowski, and Hamilton, 2003; Mays and Cochran, 2001), which has repeatedly documented a negative significant relationship, mostly associations, between perceived discrimination and mental and physical health. Finally, more work is needed to understand how minorities will respond to information about discrimination against their racial group in different domains, e.g. labor and housing markets, politics, education, which will be disclosed gradually. Similarly, the effects of repeated information provision are unknown and warrant further investigation.

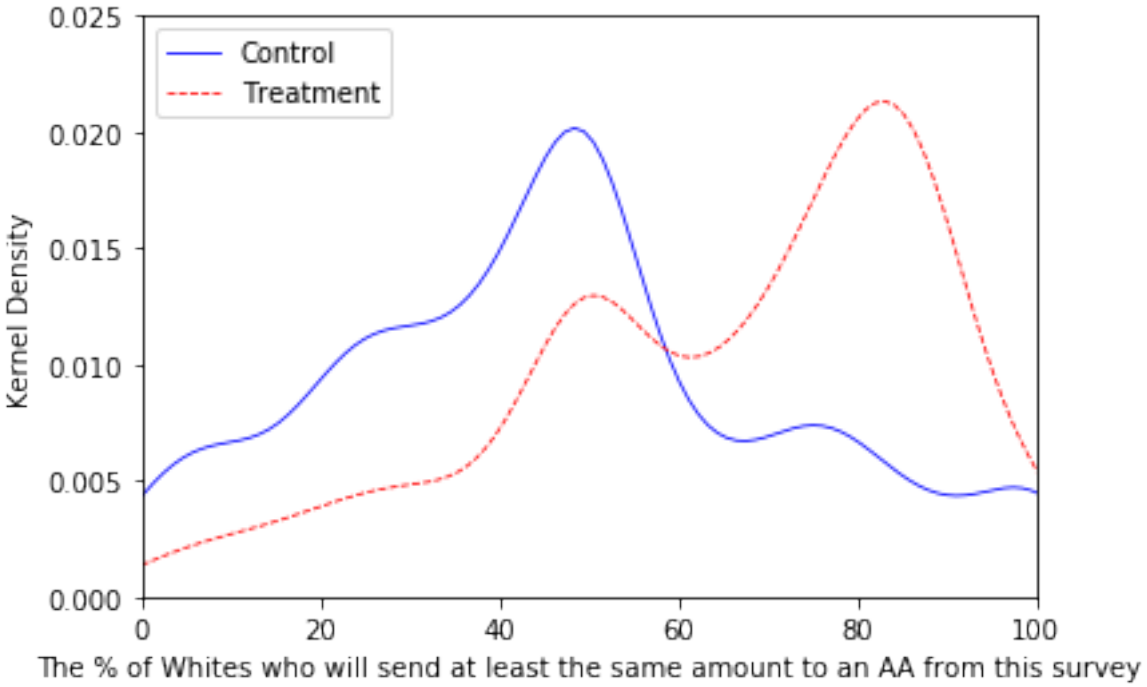
Main figures

Figure 2.1: Prior beliefs about discrimination against one's own racial group



Notes: The figure uses prior beliefs of 2,000 experimental participants about the number of White respondents (out of 100) who allocated the same or larger amount to an African American person relative to a White person. The black short-dashed line denotes the actual number of such White respondents, based on the previous survey results.

Figure 2.2: Posterior beliefs about discrimination against one's own racial group



Main tables

Table 2.1: Pre-specified outcomes (main regression specifications)

	Posterior belief	Attempted problems	Correct problems	WTP for race-blind allocation	Extent of performance exaggeration
	(1)	(2)	(3)	(4)	(5)
Panel A: Without covariates					
Treatment	17.85***	-0.31**	-0.24	0.003	-0.08
	(1.10)	(0.14)	(0.19)	(0.05)	(0.15)
Panel B: With covariates					
Treatment	17.86***	-0.30**	-0.19	-0.008	-0.13
	(1.07)	(0.13)	(0.17)	(0.05)	(0.15)
Observations	2,000	2,000	2,000	2,000	2,000
Control mean	47.13	17.75	12.15		0.53

Notes: *Posterior belief* is equal to a subject's estimate of the percentage of White third parties who would *not* discriminate against an African American person in the current survey. *WTP for race-blind allocation* equals to the number of times the subjects prefer to withdraw information about their and the other person's race from the third party over money for themselves. This variable has been standardized using the mean and standard deviation in the control group. *Extent of performance exaggeration* has been calculated as the difference between a subject's reported and real number of correctly solved math problems. In Panel B regressions, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, prior belief, employment status, and political orientation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.2: Heterogeneity analysis by gender (pre-specified)

	Posterior belief	Attempted problems	Correct problems	WTP for race-blind allocation	Extent of performance exaggeration
	(1)	(2)	(3)	(4)	(5)
Treatment (a)	17.09***	-0.19	0.18	0.007	-0.20
	(1.45)	(0.18)	(0.22)	(0.06)	(0.20)
Treatment × Male (b)	1.69	-0.25	-0.82**	-0.03	0.16
	(2.13)	(0.26)	(0.35)	(0.09)	(0.31)
Male	-1.79	0.51***	1.06***	0.04	0.01
	(1.57)	(0.18)	(0.25)	(0.06)	(0.23)
Linear combination: a + b	18.78***	-0.43**	-0.63**	-0.03	-0.04
	(1.56)	(0.18)	(0.26)	(0.06)	(0.24)
Observations	2,000	2,000	2,000	2,000	2,000
Covariates	yes	yes	yes	yes	yes
Control mean	47.33	17.46	11.45		0.55

Notes: In all regressions, the following pre-specified covariates are included: age, household size, regional, educational and income dummies, prior belief, employment status, and political orientation. Robust standard errors in parentheses. *Male* is a dummy variable that takes value 1 if a person is a male and 0 if a person is a female. *** p<0.01, ** p<0.05, * p<0.1.

2.A Appendix figures

Figure 2.A.1: Main elements of the experimental design

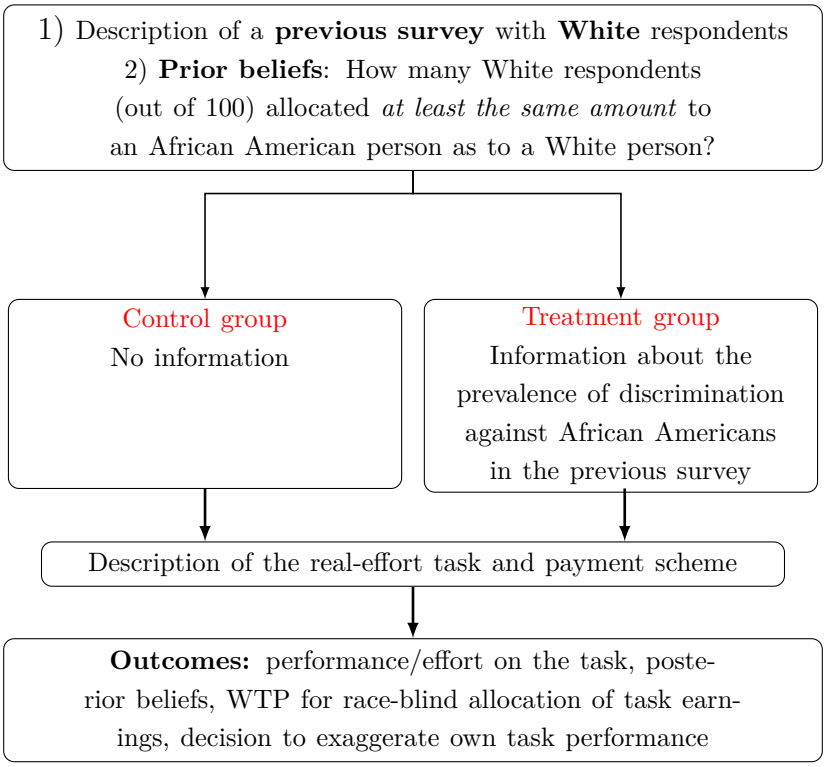
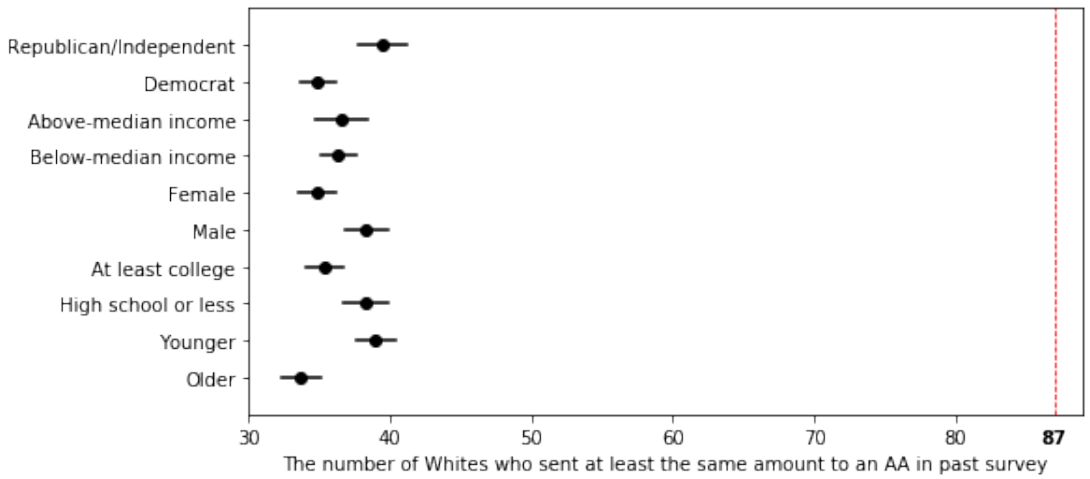
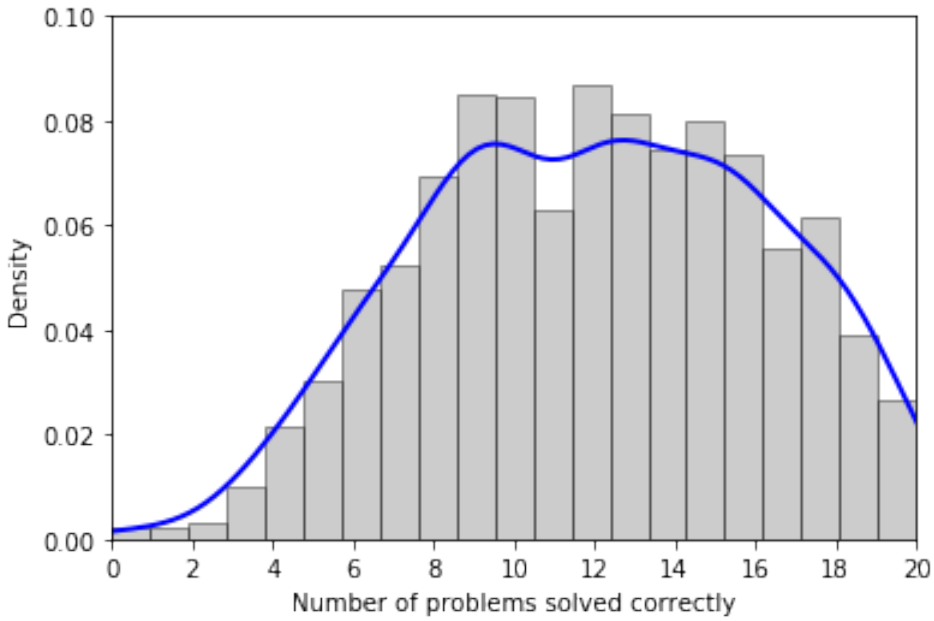


Figure 2.A.2: Prior beliefs about discrimination against one's own racial group (across demographics)



Notes: The horizontal axis depicts a prior belief about the number of White individuals (out of 100) who allocated (in the previous survey) at least the same amount or more to an African American person relative to a White person. The red short-dashed line denotes the actual number of such White respondents, based on the previous survey results. The dots illustrate the mean prior beliefs for different demographic groups, and the lines show the 95% confidence intervals. Older (younger) individuals represent those whose age is higher than (below or equal to) the median age of 42.

Figure 2.A.3: Distribution of actual performance on the math task



Notes: The figure uses data from 2,000 experimental participants and shows a distribution of the number of math problems solved correctly (out of 20). The blue curve is kernel density estimation.

2.B Appendix tables

Table 2.B.1: Demographic composition of our sample compared to the African American population residing in the US

	Mean: experiment	Mean: U.S. Census Bureau
Gender		
Male	0.45	0.46
Female	0.55	0.54
Age group		
18 to 34 years	0.361	0.345
35 to 49 years	0.264	0.255
50 to 64 years	0.234	0.241
65 years and over	0.141	0.159
Education		
Less than high-school graduate	0.053	0.128
High-school graduate	0.318	0.332
Some college, no degree	0.28	0.205
Associate's degree	0.101	0.097
Bachelor's degree	0.163	0.155
Graduate or professional degree	0.085	0.083
Region (by US Census Bureau)		
Northeast	0.156	0.173
Midwest	0.168	0.164
South	0.585	0.567
West	0.091	0.096

Notes: This table compares the shares of selected socio-demographic groups in our experiment to the corresponding shares received from the US Census Bureau. In the case of the first three categories, data from 2019 Current Population Survey are used while regional shares are based on 2019 estimates of the African American population in the United states.

Table 2.B.2: Summary statistics

	Mean	SD	Median	Min.	Max.	Obs.
Male	0.45	0.50	0.00	0.00	1.00	2,000
Age	43.23	16.59	42.00	18.00	88.00	2,000
Low education	0.37	0.48	0.00	0.00	1.00	2,000
Middle education	0.38	0.49	0.00	0.00	1.00	2,000
High education	0.25	0.43	0.00	0.00	1.00	2,000
Northeast	0.16	0.36	0.00	0.00	1.00	2,000
Midwest	0.17	0.37	0.00	0.00	1.00	2,000
South	0.59	0.49	1.00	0.00	1.00	2,000
West	0.09	0.29	0.00	0.00	1.00	2,000
Household income (categories)	3.39	2.07	3.00	1.00	9.00	2,000
Employed	0.47	0.50	0.00	0.00	1.00	2,000
Student	0.08	0.26	0.00	0.00	1.00	2,000
Unemployed	0.16	0.37	0.00	0.00	1.00	2,000
Retired	0.18	0.38	0.00	0.00	1.00	2,000
Democrat	0.66	0.47	1.00	0.00	1.00	2,000
Republican	0.06	0.23	0.00	0.00	1.00	2,000
Household size	2.79	1.69	2.00	1.00	21.00	2,000
Prior belief about discrimination against African Americans	36.42	22.90	35.00	0.00	100.00	2,000
Passed the comprehension check	0.57	0.50	1.00	0.00	1.00	2,000

Notes: This table presents the summary statistics. Low (middle) education implies **at most** secondary-school (Associate's degree) completion.

Table 2.B.3: Randomization check

	Treatment	Control	t-stat (p-value)	Obs.
Male	0.45	0.45	0.91	2,000
Age	43.82	42.63	0.11	2,000
Low education	0.37	0.37	0.77	2,000
Middle education	0.38	0.39	0.62	2,000
High education	0.25	0.25	0.82	2,000
Northeast	0.16	0.16	0.97	2,000
Midwest	0.17	0.17	0.87	2,000
South	0.58	0.59	0.76	2,000
West	0.10	0.09	0.43	2,000
Household income (categories)	3.40	3.38	0.88	2,000
Employed	0.48	0.45	0.14	2,000
Student	0.07	0.08	0.32	2,000
Unemployed	0.15	0.17	0.17	2,000
Retired	0.17	0.18	0.57	2,000
Democrat	0.67	0.66	0.73	2,000
Republican	0.06	0.06	0.97	2,000
Household size	2.75	2.83	0.28	2,000
Prior belief about discrimination	36.44	36.46	0.97	2,000
Passed comprehension check	0.57	0.57	0.94	2,000

Notes: Means. Column 6 reports p-values for a t-test testing the null hypothesis that the means are equal across two treatment arms. The p-value of an F-test for the joint significance of all covariates in predicting the treatment status is 0.7513.

Table 2.B.4: Heterogeneity analysis by education (pre-specified)

	Posterior belief	Attempted problems	Correct problems	WTP for race-blind allocation	Extent of performance exaggeration
	(1)	(2)	(3)	(4)	(5)
Treatment (a)	14.67***	-0.48**	-0.03	-0.08	-0.005
	(1.86)	(0.22)	(0.29)	(0.07)	(0.27)
Treatment × College (b)	5.07**	0.29	-0.25	0.12	-0.19
	(2.26)	(0.28)	(0.36)	(0.09)	(0.33)
College	-1.42	-0.37*	2.29***	-0.11*	-0.31
	(1.69)	(0.19)	(0.27)	(0.07)	(0.24)
Linear combination: a + b	19.73***	-0.19	-0.28	0.04	-0.20
	(1.29)	(0.16)	(0.21)	(0.06)	(0.18)
Observations	2,000	2,000	2,000	2,000	2,000
Covariates	yes	yes	yes	yes	yes
Control mean	48.47	18.02	10.72		0.63

Notes: In all regressions, the following pre-specified covariates are included: gender, age, household size, regional, income dummies, prior belief, employment status, and political orientation. Robust standard errors in parentheses. *College* is a dummy variable that takes value 1 if a person a person has at least some college experience and 0 if an individual has high-school education or less. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.B.5: Heterogeneity analysis by political affiliation (pre-specified)

	Posterior belief	Attempted problems	Correct problems	WTP for race-blind allocation	Extent of performance exaggeration
	(1)	(2)	(3)	(4)	(5)
Treatment (a)	17.55***	-0.54**	-0.43	0.08	-0.46*
	(1.81)	(0.21)	(0.31)	(0.08)	(0.26)
Treatment × Democrat (b)	0.45	0.37	0.37	-0.13	0.49
	(2.23)	(0.27)	(0.37)	(0.09)	(0.33)
Democrat	-0.62	-0.01	-0.20	0.06	-0.29
	(1.64)	(0.18)	(0.27)	(0.07)	(0.24)
Linear combination: a + b	18.01***	-0.17	-0.06	-0.05	0.04
	(1.32)	(0.16)	(0.21)	(0.05)	(0.19)
Observations	2,000	2,000	2,000	2,000	2,000
Covariates	yes	yes	yes	yes	yes
Control mean	48.49	17.95	12.47		0.68

Notes: In all regressions, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, prior belief, employment status. Robust standard errors in parentheses. *Democrat* is a dummy variable that takes value 1 if a person reports to be a Democrat and 0 if an individual identifies himself/herself with Republicans or Independents. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.B.6: Heterogeneity analysis by prior belief

	Posterior belief	Attempted problems	Correct problems	WTP for race-blind allocation	Extent of performance exaggeration
	(1)	(2)	(3)	(4)	(5)
Treatment (a)	14.44***	-0.42**	-0.11	0.08	-0.23
	(1.41)	(0.18)	(0.24)	(0.06)	(0.23)
Treatment × Distant prior (b)	6.53***	0.25	-0.13	-0.17*	0.19
	(2.16)	(0.26)	(0.34)	(0.09)	(0.31)
Distant prior	-12.56***	-0.19	0.52**	0.10	-0.44**
	(1.58)	(0.18)	(0.25)	(0.06)	(0.22)
Linear combination: a + b	20.96***	-0.17	-0.24	-0.09	-0.04
	(1.64)	(0.19)	(0.24)	(0.07)	(0.20)
Observations	2,000	2,000	2,000	2,000	2,000
Covariates	yes	yes	yes	yes	yes
Control mean	53.48	17.9	11.9	0	0.73

Notes: In all regressions, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, employment status, and political orientation. Robust standard errors in parentheses. *Distant prior* is a dummy variable that takes value 1 if a person initially stated that fewer than 35 White individuals (median belief) allocated the same amount or more to an African American person relative to a White person. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.B.7: Associations between beliefs and outcomes

	Attempted problems	Correct problems	WTP for race-blind allocation	Extent of performance exaggeration
	(1)	(2)	(3)	(4)
Panel A: Without covariates				
Belief about no discrimination against African Americans	0.001	0.009	0.001	0.005
	(0.004)	(0.006)	(0.001)	(0.005)
Panel B: With covariates				
Belief about no discrimination against African Americans	-0.000	0.007	0.001	0.005
	(0.004)	(0.005)	(0.001)	(0.005)
Observations	995	995	995	995

Notes: *Belief about no discrimination against African Americans* is equal to a subject's estimate of the percentage of White third parties who would allocate the same amount or more to an African American person relative to an equally productive White person in the current survey. In Panel B regressions, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, employment status, and political orientation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.B.8: Robustness checks (main regression specifications)

	Posterior belief	Attempted problems	Correct problems	WTP for race-blind allocation	Extent of performance exaggeration
	(1)	(2)	(3)	(4)	(5)
Panel A: Excluding subjects who did not pass comprehension check					
Treatment	20.14***	-0.26	-0.26	-0.06	-0.09
	(1.39)	(0.17)	(0.22)	(0.06)	(0.18)
Observations	1134	1134	1134	1134	1134
Covariates	yes	yes	yes	yes	yes
Control mean	47.5	17.61	12.61		0.45
Panel B: Excluding subjects with too short & long survey completion					
Treatment	18.56***	-0.25*	-0.25	-0.005	-0.07
	(1.12)	(0.13)	(0.17)	(0.05)	(0.15)
Observations	1759	1759	1759	1759	1759
Covariates	yes	yes	yes	yes	yes
Control mean	46.76	17.56	12.4		0.46
Panel C: Re-weighting observations					
Treatment	16.65***	-0.38**	-0.19	-0.04	-0.17
	(1.21)	(0.16)	(0.18)	(0.04)	(0.18)
Observations	2,000	2,000	2,000	2,000	2,000
Covariates	yes	yes	yes	yes	yes
Control mean	47.71	17.53	11.78		0.61

Notes: In *Panel A*, we limit our analysis to respondents who answered all three comprehension questions correctly. In *Panel B*, we exclude respondents in the top 2% and bottom 10% of the time spent on the survey. The chosen cutoffs are the same as Alesina, Miano, and Stantcheva (2018) used in their robustness checks. In *Panel C*, we present the results of OLS regressions that we run on the re-weighted data. Re-weighting is employed mostly to correct for undersampling of respondents with very low education. In all cases, the following pre-specified covariates are included: gender, age, household size, regional, educational and income dummies, prior belief, employment status, and political orientation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

2.C Experimental Instructions

2.C.1 Introduction and agreement with participation

The results from this study will be used in a research project. It is therefore important that you precisely follow the instructions. You will remain anonymous throughout the survey. Your participation is completely voluntary and you can withdraw at any time without penalty.

All data obtained from you will be kept in a form that does not permit your identification.

IMPORTANT NOTE: This study employs a strict non-deception policy. This means that all information you receive is truthful. You MAY be eligible for extra incentives. The total amount of these incentives will be calculated at the beginning of December and added to your fixed participation fee.

I have read and understood the above and agree to participate in this study:

- Yes
- No

2.C.2 Demographic questions

1. What is your age? [respondent writes a number]
2. Please indicate your gender. [Male; Female]
3. Which of the following best describes your race or ethnicity? [African American/Black; Asian/Asian American; Caucasian/White; Native American, Inuit or Aleut; Native Hawaiian/Pacific Islander; Other]
4. Which category best describes your highest level of education? [Eighth Grade or less; Some High School; High School degree / GED; Some College, no degree; Associate's degree, occupational; Associate's degree, academic; Bachelor's Degree; Master's Degree; Professional Degree (JD, MD, MBA); Doctoral Degree]
5. In which state do you currently reside? [List of 50 US states, DC, Puerto Rico]

2.C.3 Prior belief elicitation

Recently we have conducted a survey with 100 White respondents who were asked to allocate real 2 USD between a White person and an African American person.

What do you think? How many out of 100 White respondents...

- gave more to a White person than to an African American person
- gave at least the same amount to an African American person as they gave to a White person?

The numbers that you record should sum up to 100.

Please try to estimate the number of White respondents as close as possible. We will select by chance 100 respondents from this survey and reward the accuracy of their estimates with an additional incentive.

If you are selected and you guess correctly the true number of White respondents who gave at least the same amount to an African American person as they gave to a White person, you will receive 5 USD in addition to your participation fee.

You will be allowed to proceed to the next screen after 45 seconds.

2.C.4 Information screen (This page was skipped for the Control group)

You stated that [subject's guess] of White respondents allocated at least the same amount to an African American person as they allocated to a White person.

According to the survey results, 87 out of 100 White respondents implemented the equal split or allocated more to an African American person.

The graph below compares your estimate, i.e. the number of respondents who gave at least the same amount to an African American person as to a White person, to the corresponding actual number of White respondents (out of 100 people) from the previous survey.

[A subject additionally sees a bar chart.]

You will be allowed to proceed to the next screen after 30 seconds.

2.C.5 Assignment instructions

In this part of the survey, you have a chance to receive extra money, at most 2 USD, for answering multiple-choice math questions. We are interested in determining how many of 20 math questions you can get right without any help. So please do not use a calculator or look up the answers online, but rather just do your best.

In most cases, your answers will be submitted automatically after 60 seconds and you will auto-advance to the set of new math questions. This assignment will last for about 5 minutes.

We will now explain how you will be paid for this work.

1. You will be matched with a random White respondent (the other person) who will complete the same assignment.

2. We will ask a randomly selected third party, a White respondent, to allocate 2 USD, the reward for performing the assignment, between you and the other person.

The third party will not be able to identify you or the other person. However, he or she may be informed about (i) your and the other person's race and (ii) your and the other person's productivity on the assignment.

If you receive extra money for the assignment, it will be paid to you as an additional incentive for completing this survey.

2.C.6 Comprehension check

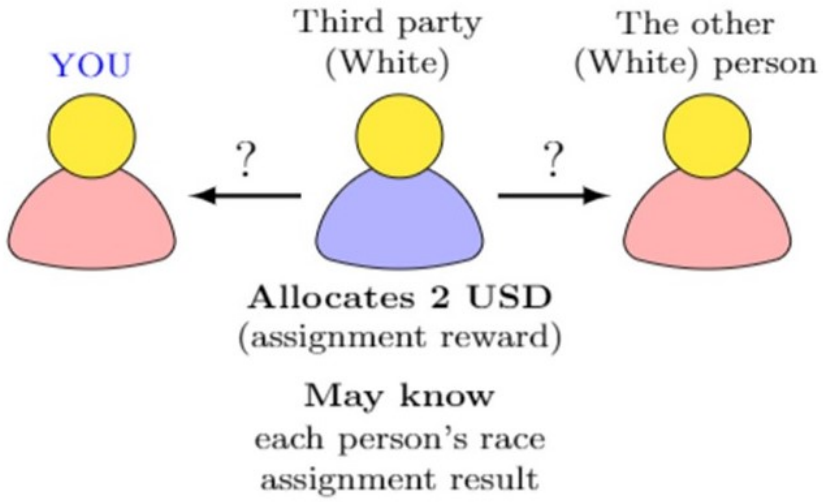
In this part, we would like to check your understanding of the assignment instructions that you read before. If you prefer to go through the instructions one more time, press the back button.

For each of the below statements, please decide whether it is true or false.

	True	False
I will be randomly matched with a White respondent who will perform the same assignment.		
A third party (a White respondent) will allocate extra 2 USD between me and the other person.		
Before allocating the money, the third party may see information about my and the other person's race and productivity on the assignment.		

You will be allowed to proceed to the next screen after 20 seconds.

All statements that you saw are TRUE.



Please press ⟶ to start the assignment.

2.C.7 Assignment

$5 + 6 + 7 + 8 + 9 + 10 =?$

- 45
- 51
- 42
- 48

$(18 + 19 + 20)/3 =?$

- 19
- 18
- 20
- 21

$56/8 =?$

- 7
- 6
- 5
- 8

$76/4 =?$

- 18
- 17
- 19
- 20

$(4 + 5)/5 =?$

- 6.25
- 1
- 1.8
- 5

$x = 5, y = 6, z = 7$, then what is $xy/(z - 4)$?

- 8
- 10

- 6
- 4

Twenty cannot be divided by which of the following?

- 5
- 2
- 3
- 4

Which of the following fractions cannot be further reduced?

- 7/35
- 46/2
- 3/5
- 3/6

$45 + 3 - 1 =?$

- 48
- 46
- 49
- 47

$10/5 + 34 - 4 =?$

- 36
- 34
- 30
- 32

$16 < x + 8 < 26$. Which of the following could x be?

- 23
- 18
- 13
- 8

$2 * (-3 - 8) =?$

- -14
- 14
- 22
- -22

Which of the following can be divided by five (without a remainder)?

- 44
- 46
- 45
- 43

$4 + 8 + 12 + 16 =?$

- 40
- 20
- 25
- 45

$24/4/3 =?$

- 4
- 1
- 2

- 3

$(4 * 2 + 7 * 8)/4 =?$

- 20
- 24
- 12
- 16

$35/7 + 1 =?$

- 4
- 7
- 6
- 5

$2 * 3 * 4 * 5 =?$

- 120
- 720
- 24
- 240

$5 * (7 + 3) + 5 - 4 =?$

- 55
- 39
- 51
- 32

Which of the following is the closest integer to 45/7?

- 6
- 5
- 7
- 8

2.C.8 Collecting posterior beliefs

Now we would like you to predict the behavior of the third parties who will allocate the assignment earnings (2 USD) between an African American person and an equally productive White person from this survey. The third parties are White respondents.

What do you think? What percentage of the third parties will give at least the same amount to an African American person as they will give to a White person?

We will select by chance 100 respondents from this survey and reward the accuracy of their predictions with an additional incentive.

If you are selected, we will compare your prediction to the actual percentage of third parties who will allocate at least the same amount to an African American person as to an equally productive White person.

If your prediction is the same as the corresponding true percentage of third parties, you will receive 5 USD in addition to your participation fee.

2.C.9 Measuring willingness-to-pay for race-blind allocation

Now we want to assess how much you would value race-blind allocation.

For each of the 6 choices below, please decide whether the third party will see no information about your race and the other person's race or whether you should get money in addition to your participation fee.

We will randomly implement your decision for one of these choices, which involve real money, so please consider each choice carefully. Each decision has the same chance of being implemented.

Your decision

The third party will not learn about our races	I will receive 0 cents
The third party will not learn about our races	I will receive 20 cents
The third party will not learn about our races	I will receive 40 cents
The third party will not learn about our races	I will receive 60 cents
The third party will not learn about our races	I will receive 80 cents

The third party will not learn about our races I will receive 1 USD

Please note that this extra money is independent of your assignment earnings.

2.C.10 Reporting one's own performance

There is a 50 percent chance that the third party you are matched with, a White respondent from a different survey, will see how productive you were on the assignment in comparison to the White person according to your own reports. Specifically, each of you will be asked to write down the number of problems that you solved correctly.

If this option is implemented, the third party will see the reported result but NOT actual performance before deciding how to allocate money between you and the other person. He or she will be made aware whether or not information about performance is based on self-reports.

Please carefully compare your choices to the test answers and write down the number of arithmetic problems that you solved correctly.

	Correct answer	Your choice
Q1	1	...
Q2	1	...
Q3	1	...
Q4	3	...
Q5	3	...
Q6	2	...
Q7	3	...
Q8	3	...
Q9	4	...
Q10	4	...
Q11	3	...
Q12	4	...
Q13	3	...
Q14	1	...
Q15	3	...
Q16	4	...
Q17	3	...
Q18	1	...
Q19	3	...
Q20	1	...

I have solved the following number of problems correctly: ...

2.C.11 Background questions

Including yourself, how many people are currently living in your household?

In politics, as of today, do you consider yourself a Republican, a Democrat or an Independent? [Republican; Democrat; Independent]

What is your current employment status? [Full-time employee; Self-employed or small business owner; Unemployed and looking for work; Student; Not currently working and not looking for work; Retiree; Prefer not to answer]

If unemployed and looking for work: How long have you been unemployed and seeking for work? [0-3 months; 4-6 months; 6-12 months; 1-2 years; more than 3 years; Prefer not to answer]

What was your TOTAL household income, before taxes, last year? [Less than $15,000; $15,000 to $24,999; $25,000 to $49,999; $50,000 to $74,999; $75,000 to $99,999; $100,000 to $149,999; $150,000 to $200,000; More than $200,000; Prefer not to answer]

2.C.12 Participants' feedback

Finally, if you have any comments or questions related to this survey, please write them down in the blank space below. Your feedback is very important to improve our research.

Chapter 3

Discrimination in Disclosing Information about Female Workers: Experimental Evidence

Abstract

Sona Badalyan, Darya Korlyakova, and Rastislav Rehák[1]

We focus on communication among hiring team members and document the existence of discrimination in the disclosure of information about candidates. In particular, we conduct an online experiment with a nationally representative sample of Czech individuals who act as human resource assistants and hiring managers in our online labor market. The main novel feature of our experiment is the monitoring of information flow between human resource assistants and hiring managers. We exogenously manipulate candidates' names to explore the causal effects of their gender on information that assistants select for managers. Our findings reveal that assistants disclose more information about family

[1]This chapter is joint work based on Badalyan, S., Korlyakova, D., Rehák, R. (forthcoming) "Disclosure Discrimination: An Experiment Focusing on Communication in the Hiring Process", CERGE-EI WP series. *Author contributions*: Badalyan, S., Korlyakova, D., Rehák, R. designed the experiment, collected and analyzed data, and wrote the paper. This project has received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (grant agreement No. 101002898). This study was supported by Charles University, GAUK project No. 333221. We thank Michal Bauer, Julie Chytilová, Filip Matějka, Andreas Menzel, Nikolas Mittag, and Jaroslav Groero for valuable discussions. We are also grateful to Data Collect and MEDIAN for excellent cooperation on data collection. The study was pre-registered in the AEA RCT Registry: AEARCTR-0008662. The experimental design was approved by the CERGE Ethical Committee.

and less information about work for female candidates than for male candidates. An in-depth analysis of types of information disclosed suggests that gender stereotypes play an important role in this disclosure discrimination.

Keywords: Information, Disclosure, Hiring, Discrimination, Foreigners, Women, Online Experiment
JEL Codes: C90, D83, J71

3.1 Introduction

Information about job applicants is a key input that firms use when making hiring decisions. It has long been acknowledged that the lack of access to individual-level information can lead to statistical discrimination against certain societal groups (Phelps, 1972). More recently, researchers have become interested in understanding mechanisms that may underlie biases in information acquired depending on the group characteristics of job applicants, which could arise even when individual-level information is available. In particular, Bartoš et al. (2016) show that employers may discriminate in attention allocation in the presence of cognitive constraints.

In this paper, we focus on *disclosure discrimination*—biases that arise due to the exchange of information among individuals in hierarchical organizations[2]. For example, in communication with a hiring manager, human resource (HR) assistants may emphasize strong features of a majority applicant and make them less salient in the case of a minority applicant. Assistants could also omit some information about some applicants to promote a candidate whom they favor.

Our primary question is whether HR assistants select different information for hiring managers depending on the applicants' gender. One reason this question is understudied in previous academic work is that monitoring communication during a hiring process is difficult, especially in field settings. Nevertheless, this topic requires attention because recent evidence is indicative of possible discrimination in information transmission in

[2]The importance of smooth communication between human resource specialists and hiring managers is a popular topic on many career-related websites. For instance, Glassdoor (2021) advises that "Recruiters and hiring managers must be in constant contact with one another to be effective and efficient - and a great way to do this is to hold post-interview debriefs via phone, Skype, or in person [...] if feedback is delayed or non-existent, [...] hiring decisions [...] can be postponed."

the hiring context. Specifically, Kline, Rose, and Walters (2022) find that firms with greater centralization of recruiting —a measure indicative of hiring responsibility being divided among fewer individuals—have smaller racial and gender callback gaps. Moreover, a meta-analysis by Quillian, Lee, and Oliver (2020) shows that discrimination at the interview stage contributes substantially to less-frequent job offers to racial minorities than to majority candidates. Although interviewers are not necessarily responsible for final hiring decisions, they may affect them by sharing and emphasizing observations with hiring managers, which is plausible according to our qualitative interviews with HR specialists. For example, Rivera (in Dobbin and Kalev, 2016) finds that unsuccessful test results of female and African American candidates are scrutinized more relative to those of White men during hiring meetings. Assistants may want to accommodate the biased preferences of the hiring team and thus manipulate the disclosure of their information about potential employees accordingly.

To address our research question, we conduct an online experiment with a large nationally-representative sample of Czech individuals (N=757) who act as HR assistants[3]. These individuals select information from eight workers' profiles, which contain details about the workers' demographics, education, professional experience, qualifications, and personal qualities. To exogenously manipulate gender, we randomly assign names to the profiles. The random assignment of names also aims to vary the workers' nationality, which we will discuss in an extended version of our paper. To gain additional insights into the mechanisms that may lead to potential discrimination in disclosure, we collect data on assistants' attention during the information-selection task. While the assistants only select information about the workers, we recruit a different sample of participants in the experiment to act as hiring managers, who will make final hiring decisions (specifically, the workers can be hired for a financial task). Importantly, before making each hiring decision, a manager sees information that an assistant has disclosed about each worker in addition to the manipulated name. Eventually, the managers can reward the assistants for selected information if they find the selection valuable. The managers want to reveal the potential of workers on the financial task, because their performance on this task affects the managers' payoffs.

[3]Women are likely to be over-represented among HR assistants. According to the International Labor Organization (2020), 61% of other clerical support workers, a category that includes HR assistants, are women. Nevertheless, we decided to over-sample men (relative to their potential share in the HR profession) to have higher power to detect gender differences.

Cleanly identifying the causal effects of gender on disclosure is empirically challenging if the content of candidates' profiles differs depending on gender. For this reason, we showed different assistants the same profiles with exogenously varied names. These profiles are real; we constructed them on the basis of information collected in a pre-experimental survey. We refer to the participants of this survey as workers because they performed real-effort tasks. We had to assign the names to the profiles exogenously because it was practically impossible to match precisely real information-rich profiles of men and women that would feature their actual names. The exogenous assignment of names is commonly used by correspondence studies (see, for instance, Bertrand and Duflo, [2017] for a review) in which researchers send the same fictitious applications with different names to real firms. Employers in these studies are not informed that the applicants are fictitious, so they behave in a realistic manner and have an incentive to study the information about those applicants. Similarly, we omit the information that the workers' names are fictitious to make the assistants take the information-selection task seriously.

Our choice of an online experiment as a suitable method to explore patterns in assistants' disclosure is inspired by recent experimental literature. Identifying the components of discrimination or mechanisms that may contribute to group-based disparities serves as a basis for successful policy responses, but is often unfeasible in natural settings. In this regard, researchers started to conduct hiring experiments on online crowdsourcing platforms in which they assign participants the roles of workers, recruiters, hiring managers or employers (Bohren, Hull, and Imas, 2022; Bohren et al., 2019). Furthermore, a growing number of studies (Cappelen, Falch, and Tungodden 2019; Almås, Cappelen, and Tungodden 2020; Cappelen et al. 2020) address questions related to distributional preferences by observing the decisions of impartial spectators with respect to workers' outcomes in online settings. These studies often recruit participants with the help of research agencies similar to those we cooperated with.

Our main findings are the following. First, if a CV has a female name, assistants select more demographic information for hiring managers, in particular information that may signal increased household responsibilities. For example, assistants are 31.4% more likely to disclose information about the number of children for female workers than for their male counterparts. The effects are driven by male assistants and are somewhat stronger for those who seem to be more supportive of traditional gender roles. Second, assistants provide less work-related information about female workers. This effect is driven by

our low-quality profiles. However, this overall negative effect hides important differences across types of information: whereas assistants disclose less information about the job responsibilities of female workers with low-quality profiles, they provide more information about their job positions.

The differential disclosure of information depending on candidates' gender seems to be connected to gender stereotypes. By providing more information on the number of children and marital status of women compared to men, the assistants emphasize (consciously or unconsciously) the importance of family for women. This information could make family obligations salient, which can reduce the chances of women finding a job (Becker, Fernandes, and Weichselbaumer, 2019; Petit, 2007). By over-providing information about job positions in low-quality profiles for female workers relative to male workers, assistants highlight women's stereotypical occupational choices, because our low-quality profiles tend to represent workers from female-dominated professional fields.

A distinctive contribution of our online experiment are the heterogeneity analyses by the characteristics of assistants. On the methodological side, such analyses are difficult to perform in standard correspondence studies (e.g. Quillian et al., 2017; Kaas and Manger, 2012; He, Li, and Han, forthcoming; Bertrand and Mullainathan, 2004) because to collect information about employers' demographics, researchers would need to ask them additional questions, which would make the employers aware of their participation in an experiment. On the theoretical side, heterogeneity analyses reveal systematic differences in disclosure patterns across assistants with different characteristics (e.g. gender). This suggests that the composition of a hiring team affects communication. Therefore, the role of an HR assistant cannot be formally reduced to acting merely as an attention system of a manager, which could be captured by a single-agent model[4].

In addition to correspondence studies, our paper adds to other types of experiments on discrimination in hiring. In vignette studies (e.g. Kübler, Schmid, and Stüber, 2018; Bertogg et al., 2020; Oesch, 2020), professionals (often human resource managers) evaluate fictitious candidates' CVs in terms of the likelihood that they would invite the candidates to an interview, the next stage of the recruitment process, or consider them for a specific job. While these experiments rely on subjects' hypothetical choices, Kessler,

[4]If there were no systematic differences in disclosure across assistants, the situation could be modeled parsimoniously as if it was directly the hiring managers directing their attention to the disclosed pieces of information (or just asking the assistants to prepare those pieces of information without the assistants' subjective involvement in the selection process). We thank Filip Matějka for this observation.

Low, and Sullivan (2019) design an incentivized resume rating. In their study, employers express interest in hiring hypothetical candidates, knowing that these choices reveal their preferences which will be used to match them (the employers) with actual candidates. Our work is different from these types of experiments because we incorporate the involvement of multiple decision-makers in hiring in order to reflect more closely the real-life processes. Communication among them could be a channel through which discrimination propagates and unfavorable stereotypes emerge.

Our results about the role of workers' gender are broadly related to recent evidence suggesting that gender discrimination often manifests itself in subtle forms (e.g. Dupas et al., 2021; Hengel, 2022). Many of these studies (Barron et al., 2022; Brock and De Haas, forthcoming) aim to detect implicit gender bias that does not materialize in simple decisions. For instance, Brock and De Haas (forthcoming) do not observe that loan officers discriminate against women directly: unconditional loan approval rates are the same for male and female applicants. However, female applicants are 30% more likely to be asked for a guarantor. We contribute to this literature by identifying a subtle form of disclosure discrimination. Our assistants do not seem to systematically provide unfavorable information about one group of workers (e.g. low education, a limited set of skills relevant for the hiring task, below-average performance on previous real-effort tasks, self-reported weaknesses). However, they tend to emphasize women's family situations and their employment in traditionally female occupations.

Our uncovered gender discrimination in disclosure also relates to the literature on the role of stereotypes in governing the decision-making of employers, recruiters and other professionals (e.g. Wu, 2018; Gallen and Wasserman, 2021). Within this literature, a few studies (González, Cortina, and Rodríguez, 2019; Van Borm and Baert, 2022) have found that gender stereotypes are triggered more strongly when female CVs explicitly mention family responsibilities and that gender bias in recruitment becomes stronger if female candidates have children. We contribute to this literature by uncovering a new domain in which gender stereotypes may influence decisions: selection of candidates' information by HR specialists for later stages of the recruitment process.

In the closest paper to ours, Eberhardt, Facchini, and Rueda (2022) investigate which attributes recommendation letter writers emphasize when describing academic job-market candidates of different genders. The authors find that women are more frequently de-

scribed using "grindstone" terms (e.g. "hard-working" or "dedicated") while also less likely praised for their ability. Our findings also suggest that individuals aim to emphasize somewhat different characteristics of female job seekers by means of differential disclosure. Our paper complements Eberhardt, Facchini, and Rueda (2022) in several directions. First, we use an experimental setting, while they use machine-learning techniques. Second, we study causal effects of candidates' gender on information selection, while Eberhardt, Facchini, and Rueda (2022) measure associations between job-market candidates' gender and language used in their reference letters. Finally, the agents who choose information in our setting are HR assistants representing the labor-demand side of the market, while the supervisors writing the reference letters in Eberhardt, Facchini, and Rueda (2022) represent the labor-supply side.

The rest of the paper is organized as follows. Section 2 describes the study design and our samples. Section 3 presents our identification strategy. Section 4 discusses our experimental results, and Section 5 concludes.

3.2 Study design

In this section, we describe the online experiment with a representative sample of Czech respondents, to whom we assign the role of HR assistants in order to test for discrimination in information disclosure. We also outline two supplementary surveys that were conducted (i) to collect information for workers' profiles and (ii) to provide assistants with real incentives.

Figure 3.A.1 in the Appendix provides an overview of the project and Figure 3.A.2 focuses on the flow of the main experiment with assistants.

3.2.1 Sample of assistants

We hired subjects for the assistant role with the help of Data Collect, a local research agency, by using their online panel. The data were collected from a sample of 757 adults during November-December 2021. The sample is representative of the Czech general population aged 18-64 years in terms of gender, age, education, and regional coverage (Table 3.B.1).

The characteristics of the assistants are summarized in Table 3.B.2. Of the assistants,

49% are women, 75% are employed, and 2% unemployed. According to the Czech Statistical Office (2021), the share of employed people in the total Czech population aged 15-64 years was 75.1% and the unemployment rate in the same age group was 2.2% in December 2021[5]. The unemployment rate in our sample, calculated by dividing the number of unemployed participants by the sum of employed and unemployed individuals, is equal to 2.7%. Of the assistants, 38% completed secondary education with a school-leaving examination and 21% hold a university degree. The net monthly household income of a median assistant lies between 40,001 and 50,000 Czech crowns,[6] which is somewhat higher than the statistics based on data from the Czech Statistical Office (37,436 Czech crowns in 2021)[7]. About 13% of the assistants report having recruitment experience.

After the main part of the experiment (the information selection task described below), we asked the assistants how much they had thought about a hiring manager during the information-selection task. The answers were coded on an 11-point scale, where 0 means "not at all" and 10 means "a lot". The average score is 8.15 (83% of assistants chose 7-10), which suggests that the manager's role in the information-selection process of our experimental subjects is high.

A number of additional measures suggest that the assistants largely took the task seriously. A median assistant spent about 11.5 minutes on selecting information from the 8 profiles. The assistants tended to disclose more than half of a worker's profile and to provide diverse information about a worker.

Before providing the details about the assistants' main task, we explain how the workers' profiles, from which the assistants selected information, were constructed and which elements they included.

3.2.2 Creating workers' profiles

To collect information for workers' profiles, we conducted a survey with 20 Czech respondents with the help of MEDIAN, a different research agency. This survey consisted of real-effort tasks and questions about demographics, education, work experience, etc. To

[5]We did not find corresponding statistics for the group aged 18-64 years, which would be the same age range as our sample of assistants covers.

[6]The dollar equivalents are approximately $1,690 and $2,110, respectively. 9.5% of the assistants did not provide information about their household income.

[7]To calculate this number, we multiplied monthly net income per capita of a median household by the average number of the median household members. The inputs were obtained from Table 2a here.

reduce workers' fatigue, we asked MEDIAN for additional information (e.g. media consumption and self-reported financial literacy) on the same respondents from the agency's previous surveys. Before asking for consent to participate in our survey, we explicitly informed respondents that we may use their data when creating questionnaires for other respondents but these data would never be linked to their names or other identifying information.

We aimed to create a diverse set of credible profiles, which would resemble real-life CVs or LinkedIn profiles (we describe the content of the profiles below). In particular, we had to ensure that the profiles did not contain suspicious information, especially when varying the names attached to them—for example, we did not want to use a profile of a construction worker because we could not credibly assign a female name to it. The goal was to make the task for the assistants realistic and engaging. In the end, we chose 8 workers whose responses and task results were used to construct the 8 profiles.

The 8 workers were being hired for an actual task with a series of financial decisions (we describe the hiring managers' task in a separate section later). The assistants were aware of this and the 8 profiles were constructed to be quite informative about the workers' qualifications for this task. The financial task consisted of 10 multiple-choice questions which involved both computational skills and financial knowledge. For example, the workers were asked to calculate the balance on a savings account after a year given the initial balance and the interest rate. In another question, they had to indicate the most volatile asset in a given list.

The content of all profiles is in Appendix 3.C.1 (page 187 onwards). Here, we describe the sections featured in the profiles:

Summary. This section describes the workers' self-reported personal strengths, weaknesses, and their opinion about their own financial skills or skills that they find important (e.g. "learning new things").

Demographics. This section includes mostly information about the workers' demographics—age, marital status, and number of children. It also provides information about whether the worker has a driving license and how many surveys he or she has completed in the past (based on the agency's records).

Education. This section provides information about the workers' level of education,

field of studies, and favorite subjects (e.g. Math, Risk Management).

Work. This section provides information about the workers' job sector, current position, years of experience in the current role, and job responsibilities (e.g. communication with governmental offices, database administration). In the case of one profile, we refer to the previous position instead of the current one because the worker is not employed. We truthfully mention that this worker is on parental leave.

Certificates. This section summarizes the workers' results on three real-effort tasks that should signal their abilities in mathematics and finance and general effort. In the math task, workers were asked to answer 10 mathematics questions within a limited time. The questions are inspired by those of Bohren et al. (2019), for example: (i) "Which of the following is an integer multiple of 11?" (ii) "$16 < x + 8 < 26$. Which of the following could x be?" The workers always chose from four options. In the financial knowledge quiz, the workers were asked to answer 5 multiple-choice questions that aimed to test whether they understand the concepts of inflation, exchange rate, company shares, etc. When preparing this task, we adapted examples from the Czech National Bank and other sources with financial literacy tests. In the slider task, which is frequently used in the experimental literature (e.g., Gill and Prowse, 2019; Bradler, Neckermann, and Warnke, 2019; Gill and Prowse, 2012), the workers had to position 48 sliders at the exact position of 50 during a limited time. Each slider was initially positioned at a random number between 0 and 100.

We chose these tasks because we hypothesized that the assistants would disclose information depending on its relevance for the hiring task. A priori, the financial knowledge quiz seemed to have the highest predictive power for the workers' performance on the task with a series of financial decisions, while the slider task seemed to be the least relevant.

Judging the workers' performance on the three tasks without a reference point would be difficult for the assistants, especially in the case of the first profile that assistants would see. Thus, we included the average score of all workers who took part in the survey for each task.

Volunteering.[8] This section informs about the workers' observed donations for a good cause. MEDIAN provided us with the data on the frequency of workers' donations in

[8]This section is missing in 4 profiles because we found it hardly realistic that individuals would voluntarily report that they never donated to a charity.

past surveys. Each time their respondents completed a survey, they were redirected to the agency's page where they had to decide whether their survey completion fee should be transferred to their bank account, donated to a charity from a list, or whether they wanted to give it up. If a worker chose to send his or her fee to a charity in the past, we mention on his or her profile in what percentage of surveys the worker made the decision to donate. Furthermore, at the end of our survey with the workers, we asked the participants whether they would like to complete another survey in the upcoming days and donate a fee from participating in that survey to a charity of their choice. If a worker chose "yes" and MEDIAN later confirmed that the worker chose to donate his or her money *after* filling in the other questionnaire, we mentioned the worker's donation decision in his or her profile.

Skills. This section enumerates the workers' self-reported skills, such as Microsoft Office experience, English language proficiency, familiarity with online banking, experience with data analysis, customer service, product management, and so on. We included the information about online banking because we expected that the assistants may relate it to financial literacy and thus to the workers' performance on the hiring task.

Interests. This section provides information about the workers' leisure-time activities and interests, e.g. sports, traveling, or reading news about finance/business/economics in newspapers or on the Internet.

We populated each section of a profile only with true information gathered from the same worker. Since our workers could decide how many details to provide about themselves (in our survey and previous surveys with the data collection agency), the resulting 8 profiles differ somewhat in length. Specifically, they contain between 24 and 35 pieces of information.

Surveying workers with diverse educational and professional backgrounds enabled us to construct "low- and high-quality" profiles. We associate profile quality with the worker's suitability for the financial (hiring) task. As previous research has documented a positive correlation between a person's financial literacy and education (Lusardi, Mitchell, and Curto, 2010), we categorize profiles as low-quality if they are for who completed at most secondary education, while the high-quality profiles are for workers with a university de-

gree[9]. Half of the profiles are classified as low-quality.

The low- and high-quality profiles differ along several other dimensions besides education. In particular, the low-quality profiles represent mostly workers from low-skilled occupations, whose self-reported skills and job responsibilities tend to signal that they are less-suitable candidates for the financial task[10]. Moreover, the low-quality workers do not use online banking, report only partial knowledge of English (compared to good knowledge for the high-quality ones), and made no charity donations. An example of a high-quality profile is Ondřej's profile in Appendix 3.C.1; an example of a low-quality profile is Lucie's profile in Appendix 3.C.1.

3.2.3 Experiment with assistants

We remind the reader that Figure 3.A.2 in the Appendix provides a depiction of the flow of the experiment with the assistants. The full instructions for the assistants (translated from Czech) can be found in Appendix 3.C.1.

Instructions, incentives, and the information selection task

In the beginning, the subjects were informed that they would act as assistants for recruiting workers in our online labor market. We emphasized that this is not a traditional survey that asks about hypothetical situations and that their decisions may have real financial consequences for other respondents.

Next, the assistants learnt that they would see 8 CVs and their task would be to select information they would like to disclose to another survey participant, who would act as a hiring manager. The assistants knew that the hiring manager would see only the information disclosed about a worker, along with the name on the CV, when making the hiring

[9]Heterogeneity along the quality dimension is an important element of our experimental design because we might expect differential treatment of female workers with lower qualifications. For instance, Bohren, Imas, and Rosenberg (2019) ran an experiment on a large online platform in which they observed strong discrimination against female users with novice accounts but favorable treatment for women with a history of positive reviews.

[10]Note that both types of profiles include "positive" as well as "negative" information. This is natural given that we used real data. However, the low-quality profiles contain more information that may put a candidate at a disadvantage compared to the high-quality counterparts. An added value of having profiles with "mixed" information is that such ambiguity might reveal implicit discrimination (Cunningham and de Quidt, 2022). For example, due to self-image or social-image concerns, an assistant may be reluctant to select solely unfavorable information about a worker whose group the assistant dislikes or finds less competent. However, disclosing the worker's weaknesses together with less relevant positive characteristics could help the assistant disguise his or her bias.

decision for the financial task. If an assistant decided not to disclose any information about a worker, the manager would see only an empty profile with a name.

We incentivized the assistants to take the disclosure task seriously in the following manner. If a manager found the disclosed information useful, he or she could allocate to the assistant an additional bonus of up to 50 Czech crowns ($\sim$ \$2); this bonus did not cost the managers anything (it was a pure reward) and the assistants knew that. Furthermore, the assistants knew that the managers would make multiple hiring decisions during a limited time, so the simplified versions of the CVs would be of great help to them. Finally, the assistants were informed that the managers would benefit financially from hiring workers with good performance on the financial task. Hired workers would also earn additional money.

We included a comprehension check at the end of the instructions. Specifically, we aimed to test the assistants' general understanding of (i) their task, (ii) the managers' role and the information available to them, and (iii) the incentives that they (assistants) have. The assistants had to evaluate whether each of three statements was true or false in order to proceed to the information selection task. We showed the correct answers on the next page along with a scheme summarizing the key points of the instructions.

In the main task, each assistant selected information from the same set of 8 different profiles, which were shown sequentially and their order was randomized. To indicate the selection, the assistants had to tick information they wanted to send to a manager directly in the CVs. As a default, no specific information was preselected, i.e. the assistants had to actively select what to disclose. There was no limit on the amount of information pieces the assistants could select. After the assistants selected information from each profile, we showed them a preview of what a manager would see about a specific worker based on their selection. We allowed the assistants to return to the previous page to change their disclosure choices.

Treatments

To study the effects of workers' gender on assistants' disclosure, we randomly assigned a name to a profile to form a CV (independently across profiles and assistants)[11]. Orthogo-

[11]To cleanly identify the causal effects of the workers' gender on disclosure, we had to compare CVs with different names but the same information content. However, it was practically impossible to construct identical profiles based on data from different workers because CVs contained numerous pieces of

nally to gender, we varied workers' nationality. Hence, we used a 2x2 design; an assistant could potentially see a profile in four different versions: local male, local female, foreign male, and foreign female. To mitigate the effect of specific names, each profile had a different set of names that could be attached to it. The full list of names is presented in Table 3.B.3[12].

Our further discussion and analysis are more narrowly focused on the importance of workers' gender for the assistants' disclosure decisions while the nationality dimension, whose effects are still being explored, will be presented in the extended version of our paper.

To summarize and pin down our nomenclature, a *profile* is a nameless set of information representing a real worker and a *CV* is a profile with a fictitious name attached to it. Each assistant sees the same 8 profiles (in a random order).

The assistants were not informed that workers' names were fictitious. Including this information could make the subjects suspicious about the real gender and nationality of the workers behind the profiles, which would introduce a confound difficult to control for. Moreover, it could jeopardize our effort to make the main task as realistic and important as possible and reduce the assistants' effort.

Our manipulation of attributes of interest with the help of a first name is somewhat less salient compared to previous literature on discrimination, which uses both a first name and a surname. We did not use the surnames because we were concerned that the assistants may think that we disrespected the workers' anonymity by providing personally identifiable information.

We included a manipulation check to test whether our treatment was salient enough. Specifically, after the assistants finished information selection from the last CV, we asked them about the gender of that last worker. At this stage, the assistants could not return to the last CV to check the name. We did not inform the assistants beforehand that we planned to check their attention later to avoid the experimenter demand effect. For the same reason, we did not include a manipulation check after each CV; only the last one. Correct answers on the manipulation check were incentivized by an extra bonus. We observe that 92% of assistants accurately identified the gender of their last CV.

information. For this reason, we assigned fictitious names to real profiles.

[12]We also displayed the IDs of workers, invented by us, next to the workers' names to substitute for the lack of surnames and to make the task more realistic. Our IDs do not reveal the identity of the real workers.

Tables 3.B.4 and 3.B.5 demonstrate that the randomization was successful, i.e. the treatment arms are well balanced and the observables are jointly unrelated to a treatment status.

Outcomes

Capturing communication in a disciplined manner is difficult. Even the simple form of communication that we restrict to—disclosure—results in a large amount of possible patterns. To avoid data mining, we pre-specified to inspect a small set of outcome variables: https://www.socialscienceregistry.org/trials/8662.

Disclosure-related outcomes. We adopt a "top-down" approach to study the effects of workers' gender on disclosure. This means that our primary outcome of interest is the overall share of information pieces that an assistant discloses from a profile. Subsequently, we study the shares of disclosed information pieces in the sections described above (e.g. Demographics, Education, Work Experience). If the treatment significantly affects disclosure from a specific section, we take a closer look at the content of this section to understand which pieces of information drive the effect. For example, if we observe treatment effects on disclosure from the Demographics section, we additionally compare across treatment arms how frequently assistants disclose information about workers' age, marital status, number of children, driving license, and number of completed surveys.

Attention-related outcomes. To study possible drivers of (potential) disclosure discrimination, we additionally collected data on assistants' attention allocated to the workers' profiles. Specifically, we recorded the time that each assistant spent on selecting information from each profile. As we did not impose any limit on time that assistants should spend per profile, the subjects could move through profiles as quickly as they wanted. We also measured how frequently assistants chose to learn more about some specific pieces of information in the profiles. For this purpose, we embedded 4-6 buttons in each profile (in the Demographics, Education, Work Experience, Certificates, and Volunteering sections), next to information pieces that may not be self-explanatory, potentially causing assistants to be interested in further details. For instance, a button next to the slider-task results (Certificates section) informed assistants about the nature of this task if the person clicked on it: *The slider task is a mechanical task in which participants had to center within a 2-minute limit as many sliders as possible (max. 48) with a random initial position.* The content and position of these "learn-more" (or "more

information") buttons within the profiles can be seen in Appendix 3.C.1 (page 187 onwards). Our outcome variable is the total number of assistants' clicks on the "learn-more" buttons in a profile. This measure also captures repeated clicks on the same button.

3.2.4 Managers' hiring decisions

After running the experiment with the assistants, we conducted a large-scale survey with a different sample of respondents who acted as hiring managers. The purpose of this data collection, which was performed in cooperation with the same research agency (Data Collect), was twofold. First, it was necessary to conduct this survey not to deceive our experimental subjects. We promised the assistants that information that they would select about the workers would be shown to another survey respondent and that this respondent would decide how to reward their effort. Second, we intended to receive evidence on the consequences of potential discrimination in disclosure for whether the workers were hired. The discussion of the results from the managers' survey is beyond the scope of this dissertation chapter.

Each manager was matched with a random assistant[13] and saw information that the assistant selected from the 8 profiles with the same names. The order of profiles was randomized. For similar reasons as for assistants, the managers did not know that the names were fictitious. Immediately after managers saw a worker's profile pre-processed by an assistant, they made a hiring decision in relation to the worker[14]. After the managers made all 8 hiring decisions, they could reward an assistant whom they were paired with by a real bonus if they found the assistant's selection of information useful.

3.3 Identification

To quantify the effect of gender on disclosure of information passed by an assistant to a manager, we employed the 2-way Fixed Effects Model. Each assistant i sees 8 profiles indexed by j.

Baseline regressions

[13]By chance, a few assistants were paired with two managers. In these cases, we randomly chose one of the assigned managers and recorded his or her decision while calculating the extra rewards to the assistants. Consequently, we had to recruit additional managers to reward the unmatched assistants.

[14]Hiring decisions were incentivized with a small bonus, which was increasing in the worker's performance on the task with a series with financial decisions.

We start by estimating the following regression model:

$$Y_{ij} = \eta + \tau T_{ij}^{FEM} + \mu_i + \phi_j + \xi_{ij}, \tag{3.1}$$

Y_{ij} is an outcome variable (e.g. share of disclosed information pieces by assistant i in profile j). T_{ij}^{FEM} is an indicator of whether assistant i saw profile j with a female name. We control for unobservables fixed over assistants and profiles by including assistant fixed effects μ_i, as well as dummies for the profiles ϕ_j. The coefficient of interest is τ, which shows the effect of female gender on assistants' disclosure or attention.

Heterogeneity

We are also interested in whether the treatment effects differ for subgroups of assistants with different characteristics (in particular, assistants' with different gender or attitudes toward women) and for profiles of different quality. To examine these heterogeneous effects, we augment equation (3.1) by including interactions of the treatment indicator with the heterogeneity variables.

Clustering

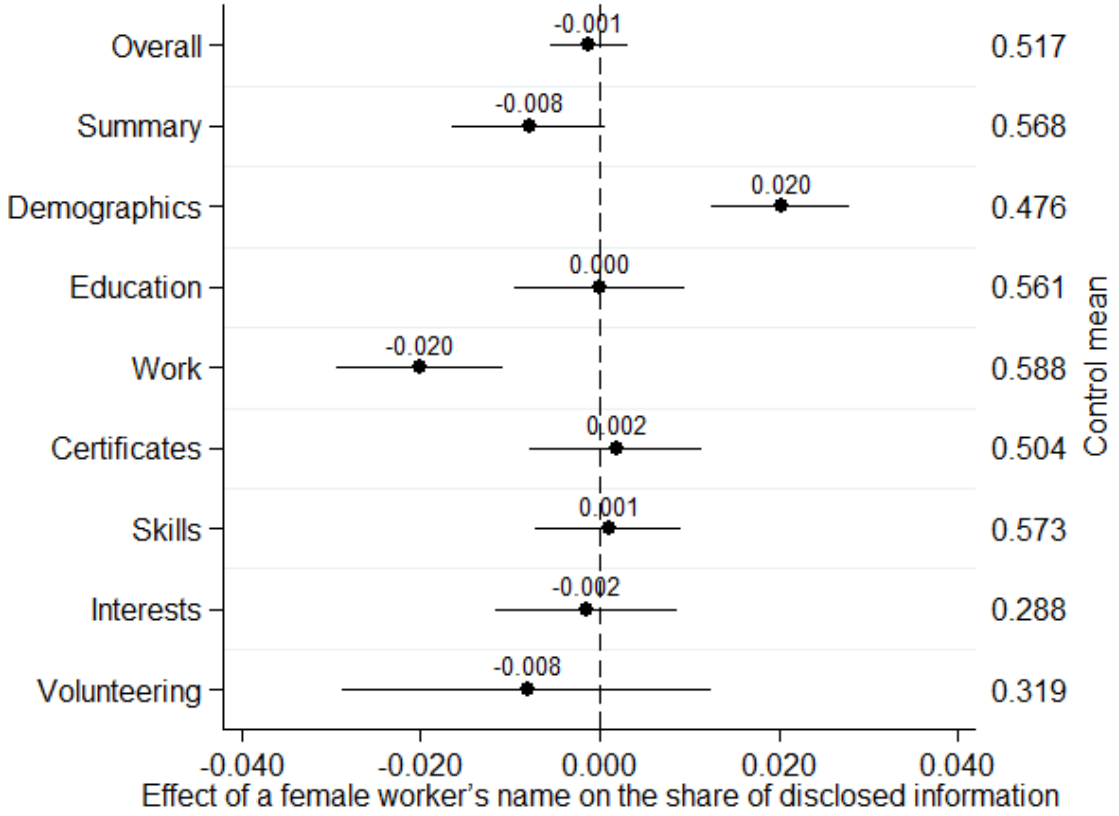
In all models, we cluster the errors at the assistants' level to address potential correlation across profiles.

3.4 Results

This section presents the results from our experiment with the assistants. Specifically, we discuss that assistants seem to rely on gender stereotypes when disclosing information about female workers.

Figure 3.1 illustrates the causal effects of a female name on the share of disclosed information from a worker's whole CV and from the particular sections. The sizes of the control means indicate that the assistants tend to provide a nontrivial amount of information about the workers and their disclosure covers a diverse set of profile sections. The assistants select on average 51.7% of information (around 16 pieces) from a male worker's profile. The assistants disclose the most about the male workers' work experience, self-reported skills, personal qualities, and education while they tend to neglect the information about the workers' interests and volunteering activities. Assigning a female name to a profile significantly increases the amount of information disclosed from

Figure 3.1: Effect of a female name on the disclosure of information (overall and from each section)



Notes: Coefficient plots. Each row corresponds to the regression of the share of disclosed information in the corresponding category (left axis) on the indicator of female name on a CV (with assistants' and profiles' fixed effects). The points represent the estimated coefficients and the bars represent the 95% confidence intervals. The control means (right column) are simple means of the share of disclosed information in the corresponding category over CVs with male names.

Demographics and decreases the amount of information disclosed from Work. In particular, the assistants disclose on average 2 percentage points (pp) more information from Demographics for a female CV than for a male CV ($p<0.01$; 4.2% increase relative to the control mean[15]). At the same time, they select on average 2pp less information from Work for a female CV than for a male CV ($p<0.01$; 3.4% decrease). To gain deeper insights into these treatment effects, we study which pieces of information drive these differences and run the pre-specified heterogeneity analyses for each of the two profile

[15]When presenting percent changes throughout this paper, we always compare the treatment effects to the control mean (i.e. the average value of the outcome in the Male group) or to the control mean in a specific subsample (e.g. male assistants) in the case of heterogeneity analyses. We omit the description of the baseline group in the text, but its specification can be found in the notes for the corresponding tables or figures.

sections.

Workers' gender and disclosure of demographic information

Table 3.1 shows the results of regressions in which all information pieces from Demographics serve as dependent variables. Assistants are 2.4pp more likely to disclose information about marital status and 8.2pp more likely to disclose information about the number of children if a worker has a female name (p<0.01 in both cases). This corresponds to an increase of 6.3% and 31.4%, respectively, compared to the control means. The finding that assistants provide family-related information more frequently in the case of female workers[16] suggests that they may find it more relevant for hiring women. Correspondence studies (e.g. Becker, Fernandes, and Weichselbaumer, 2019; Petit, 2007) systematically document that hiring discrimination against women prevails among those applicants whose demographics signal a higher likelihood of becoming pregnant or overoccupied with childcare. Hence, the tendency revealed to signal this kind of information for women (even in our online context) suggests its prominent role in discrimination against women.

Table 3.1: Effect of a female worker's name on the disclosure of Demographic information

	(1) Age	(2) Marital status	(3) Children	(4) Driving license	(5) Surveys
Female	-0.005	0.024***	0.082***	-0.003	0.000
	(0.006)	(0.008)	(0.010)	(0.006)	(0.007)
Control mean	0.753	0.384	0.261	0.715	0.250
Observations	6056	5299	6056	6056	6056

Note: Regressions of specific demographic information pieces on the Female treatment indicator. *Surveys* contains the actual number of surveys that a worker completed in the past. All regressions include profile and assistant fixed effects. Standard errors (in parentheses) are clustered at the assistant level. The control means are the average values of the outcomes in the male-CVs group.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next, we discuss whether the Female treatment effects on the disclosure of workers' demographics differ among different subgroups of assistants or workers' profiles. In one of

[16] In one of the profiles, we (truthfully) mention that the worker is on parental leave. The assistants are 1.6pp more likely to disclose this information if a worker is female (p=0.65; 2.6% increase compared to the control mean). The results are presented in Column 1 of Table 2 and are based on the OLS regression ($N = 757$) in which the assistants' characteristics (age, gender, household size, educational and regional dummies, and a recruitment-experience dummy) are included.

these analyses, we split the sample by assistants' bias against women. We constructed this variable in the following manner. At the end of our experiment, we asked the assistants to what extent they agree or disagree with different statements in relation to gender roles and stereotypes. For instance, they had to express their (dis)agreement with whether women should be more responsible for household chores than men or whether boys are more talented in technical subjects and math relative to girls. The assistants answered on a 5-point Likert scale where 1 stood for "fully agree" and 5 represented "fully disagree". To construct an index indicating tolerance to women, we first ensured that higher values always imply "better" perception of women and then calculated the average of each assistant's responses to all statements. For ease of interpretation in the heterogeneity analysis, we use a dummy variable (called "biased against women") equal to one if the value of the tolerance index is less than or equal to the median[17].

Only men reveal significantly more demographic information if the worker has a female name, as demonstrated in column 1 of Table 3.B.6. A male assistant selects 3.2pp more demographic information about a female worker relative to his average disclosure of 49.4% from Demographics in the case of a male worker ($p<0.01$; 6.5%). In contrast, a female assistant selects only 0.7pp more demographic information about a female worker relative to her average disclosure of 45.8% from Demographics in the case of a male worker ($p=0.16$; 1.5%). Table 3.B.7 illustrates that, compared to women, men provide significantly more information about female workers' marital status and number of children.

The assistants who are more likely to agree with traditional gender roles and stereotypes tend to disclose more demographic information about female workers relative to more tolerant assistants; this is captured by the marginally significant interaction term in column 2 of Table 3.B.6. This tendency suggests that stereotypes play a role in the differential treatment of women.

The effect of Female treatment on disclosure of demographic information is similar regardless of the profile quality (column 3 of Table 3.B.6). Therefore, women in various fields seem to face a similar treatment in this context.

[17]Although we elicited the assistants' attitudes toward women after the main task (thus, after the treatment assignment), the index constructed—tolerance toward women—is balanced across treatment arms (see Table 3.B.4).

Workers' gender and disclosure of work-related information

The negative effect of Female treatment on work-related information disclosure is driven especially by information about job responsibilities. Table 3.2 shows the results of regressions in which all information pieces from the Work section serve as dependent variables. The assistants are on average 7.2pp (p<0.01) and 1.8pp (p<0.10) less likely to disclose information about a worker's job responsibilities[18] and work area, respectively, if the worker has a female name (this corresponds to, respectively, a 12.6% and 2.5% decrease relative to control means).

Table 3.2: Effect of a female worker's name on the disclosure of Work information

	(1) Status	(2) Area	(3) Position	(4) Experience	(5) Any responsibilities
Female	0.016	-0.018*	0.013	0.003	-0.072***
	(0.035)	(0.010)	(0.009)	(0.010)	(0.010)
Control mean	0.621	0.729	0.756	0.625	0.570
Observations	757	6056	6056	5299	6056

Notes: Regressions of specific work information pieces on the Female treatment indicator. *Status* is a binary variable equal to 1 if the assistant disclosed information that the worker is on parental leave (this information piece is present only in one profile). *Any responsibilities* is a binary variable equal to 1 if the assistant disclosed at least one job responsibility from the worker's profile. Regressions in Columns (2)-(5) include profile and assistant fixed effects. In these cases, the standard errors (in parentheses) are clustered at the assistant level. Column (1) is based on the OLS regression with the treatment indicator and assistants' age, gender, household size, educational and regional dummies, and recruitment experience (robust standard errors in parentheses). The control means are the average values of the outcomes in the male-CVs group.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The heterogeneity analysis by profile quality in column 3 of Table 3.B.8 reveals that the negative Female effect on work-related information disclosure is concentrated mainly among the low-quality profiles. Specifically, in high-quality profiles, the assistants disclose on average 0.5pp (p=0.51) less information about Work from female compared to male CVs (0.8% decrease); in low-quality profiles, disclosure of work-related information from female CVs is 3.6pp lower than from male CVs (p<0.01, 6.3% decrease). The main contributor to this lower disclosure from female low-quality CVs is the information about

[18] In an alternative specification, we used the number of disclosed responsibilities as a dependent variable instead of a dummy indicating whether at least one job responsibility is disclosed (the workers' profiles include between 1 and 3 job responsibilities). We find that assistants select 0.12 fewer job responsibilities from CVs with female names (p<0.01; the average number of disclosed responsibilities from male CVs = 1.04).

job responsibilities (column 4 of Table 3.B.9).

In line with the earlier finding that assistants provide more family-related information about female workers, differences in disclosure of the work-related information may also be connected to gender stereotypes. This link is supported by (i) the finding that assistants are 3.5pp more likely to provide information about job positions from female low-quality CVs than from male low-quality CVs (column 2 of Table 3.B.9, $p<0.05$)[19] and (ii) the observation that our low-quality profiles tend to feature female-dominated occupations (e.g. cashier, postal delivery, administrative worker). To show more explicitly that assistants tend to over-provide stereotypical information about female-workers' jobs, we run heterogeneity analyses by female- vs. male-dominated occupations in Table 3.B.10. The positive Female effect on the disclosure of a job position is clearly concentrated among the profiles with female-dominated occupations[20]. Additional heterogeneity analysis by assistants' gender reveals that male assistants are 6.4pp more likely to provide information about job positions from female low-quality CVs (Table 3.B.11, $p<0.01$, a 7.9% increase relative to their mean disclosure from male low-quality CVs). In comparison, female assistants are only 0.5pp more likely to disclose information about job positions from female low-quality CVs ($p=0.81$, 0.6% relative to their mean disclosure from male low-quality CVs)[21]. Taken together with the earlier observation that men select more family-related information about female workers, this result suggests that gender stereotypes play a more prominent role in the selection of information by male assistants.

We conclude this section by commenting on attention outcomes. There are no significant effects of a female name on attention outcomes, but there seems to be a tendency to lower attention to female CVs (see column 1 of Table 3.B.12 for the time spent on a CV and column 3 of Table 3.B.12 for the clicks on the "learn-more" buttons). However, we do not have data on assistants' attention to *all* individual information pieces because we only recorded the time that the assistants spent on the entire CV, and the "learn-more" buttons were presented only next to pieces that were likely to require additional explana-

[19] There are no such differences in the case of high-quality CVs.

[20] We classified profiles 1, 5, 7 as female-dominated, and 2, 6, 8 as male-dominated; profiles 3 and 4 are ambiguous, so we excluded them from this analysis. We also ran the same heterogeneity analyses restricted further to profiles with even more obvious classification as female- or male-dominated occupations and the results hold, although they lose significance in the most restrictive specification due to the substantial sample reduction (these analyses are available upon request).

[21] We continued to split the sample by profile quality instead of gender-dominated occupations because this is our pre-specified heterogeneity analysis. The assistants' gender differences are confirmed by the specification that classifies profiles into "female-" and "male-dominated" groups.

tion. Therefore, we leave to future work the investigation of the attentional underpinning of discrimination in the disclosure of the specific information pieces that we identified.

3.5 Conclusion

We use a novel experimental design to study discrimination in information transmission in the context of hiring. We create an online labor market in which our main subjects, respondents who act as human resource assistants, select information about workers for other respondents, who act as hiring managers. The managers inspect only the information selected and make hiring decisions about the workers. The exogenous variation in our experiment comes from random names that we assign to the workers' profiles to signal gender.

Our results indicate that assistants tend to disclose information differently depending on the gender and nationality of the workers. First, we document that assistants provide more information about family and less information about work from female CVs. A closer look at the pieces of information disclosed suggests that differential disclosure is driven by gender stereotypes. In particular, the selection from female CVs is more likely to contain information about marital status, the number of children, and a female-dominated occupation than the selection from male CVs.

Our findings have several practical implications. First, HR assistants may discriminate unintentionally, and thus simply informing them about our findings may induce them to rethink their practices and adjust their training programs. Second, our research invites the design of more discrimination-proof communication protocols. Although some businesses are already using standardized hiring processes with prescribed rules, our discussions with human resource professionals suggest that this is not always the case and that there is room for (more subtle) differential communication about different groups of candidates. Finally, the emphasis that our assistants put on family-related information for females suggests the importance of a more general societal problem related to childcare and unequal gender roles. Among other things, this calls for expansion of affordable childcare availability and parental leave programs that minimize the (perceived) loss of firms related to childcare and that promote shared parental leave between fathers and mothers.

Our work can serve as a motivation for investigating what other channels, similar to differential disclosure, may underlie biases in hiring of female employees. For example, using gendered language in job-position descriptions that emphasizes masculine-associated traits as desired qualities may discourage many talented women from applying.

3.A Appendix figures

Figure 3.A.1: Overview of the project

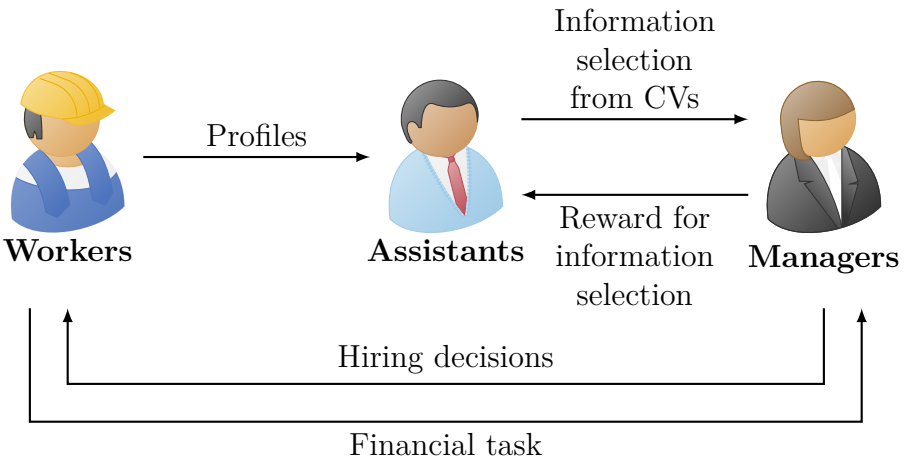
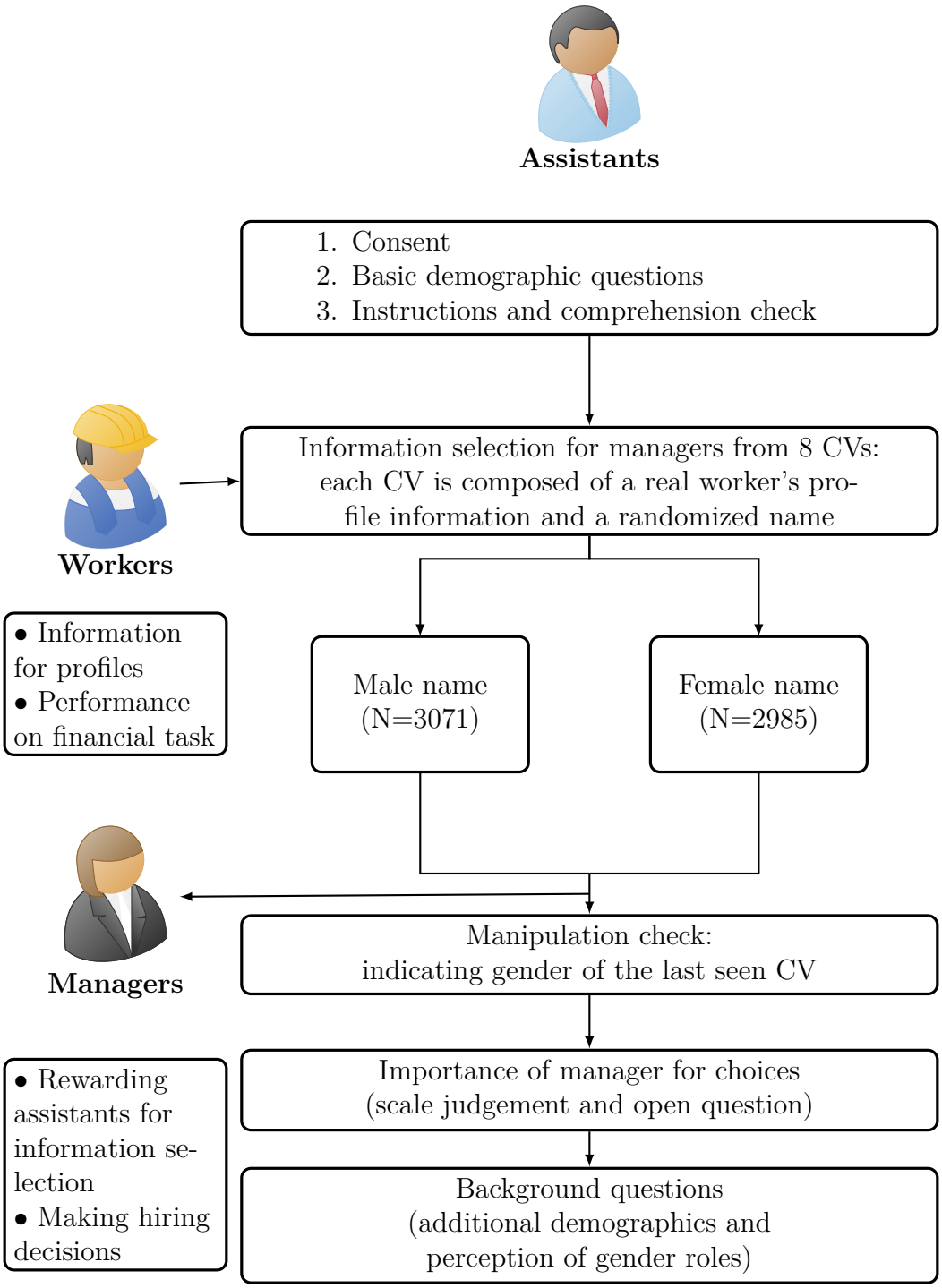


Figure 3.A.2: Flow of the experiment with assistants and the connections to the surveys with workers and managers



3.B Appendix tables

Table 3.B.1: Demographic composition of our sample of assistants compared to the general Czech population

	Mean: experiment (assistants)	Mean: Demographic Yearbook of the Czech Republic 2020
Gender		
Male	0.51	0.51
Female	0.49	0.49
Age group		
18 to 24 years	0.103	0.102
25 to 34 years	0.211	0.209
35 to 44 years	0.255	0.257
45 to 54 years	0.233	0.233
55 to 64 years	0.198	0.199
Education		
Primary and secondary Without national school-leaving exam	0.414	0.417
Secondary with national school-leaving exam	0.375	0.373
University	0.211	0.210
Region (NUTS 2)		
Prague	0.127	0.127
Central Bohemia	0.130	0.129
Southwest	0.114	0.115
Northwest	0.104	0.104
Northeast	0.141	0.140
Southeast	0.156	0.159
Central Moravia	0.116	0.113
Moravian-Silesian	0.112	0.113

Notes: This table compares the shares of selected socio-demographic groups in our experiment (N=757) to the corresponding shares received from the Demographic Yearbook of the Czech Republic 2020.

Table 3.B.2: Summary statistics for assistants' sample

	(1) mean	(2) sd	(3) p50	(4) min	(5) max
Female	0.49	0.50	0.00	0.00	1.00
Age	42.04	12.92	41.00	18.00	64.00
Household size	2.76	1.19	3.00	1.00	6.00
Primary and secondary	0.41	0.49	0.00	0.00	1.00
Education without national school-leaving exam					
Secondary education	0.38	0.48	0.00	0.00	1.00
With national school-leaving exam					
University degree	0.21	0.41	0.00	0.00	1.00
Prague	0.13	0.33	0.00	0.00	1.00
Central Bohemia	0.13	0.34	0.00	0.00	1.00
Southwest	0.11	0.32	0.00	0.00	1.00
Northwest	0.10	0.31	0.00	0.00	1.00
Northeast	0.14	0.35	0.00	0.00	1.00
Southeast	0.16	0.36	0.00	0.00	1.00
Central Moravia	0.12	0.32	0.00	0.00	1.00
Moravian Silesian	0.11	0.32	0.00	0.00	1.00
Employed	0.75	0.43	1.00	0.00	1.00
Unemployed	0.02	0.14	0.00	0.00	1.00
Household net monthly income > 50,000 CZK	0.31	0.46	0.00	0.00	1.00
Has recruitment experience	0.13	0.33	0.00	0.00	1.00
Thought about the hiring manager	8.15	1.98	9.00	0.00	10.00
Correctly identified last worker's gender	0.92	0.27	1.00	0.00	1.00

Notes: This table presents the summary statistics for 757 assistants. 6 assistants ($< 1\%$) and 72 assistants (9.5%) did not record their employment status and income, respectively. We chose CZK 50,000 as a threshold value for income because net monthly household income of a median subject lies between CZK 40,001 and 50,000. An assistant could select whether he/she did not think about the manager at all (0 on a numeric scale) or a lot (10 on a numeric scale) while selecting information about workers.

Table 3.B.3: List of workers' names used in the experiment

Profile	Name	Gender
1	**PETR**	Male
	VOLODYMYR	Male
	ADÉLA	Female
	OLEKSANDRA	Female
2	**ONDŘEJ**	Male
	EVGENIY	Male
	KATEŘINA	Female
	YEKATERINA	Female
3	**JINDŘICH**	Male
	MYKHAILO	Male
	MARKÉTA	Female
	OLESYA	Female
4	**VOJTĚCH**	Male
	YURIY	Male
	ZDEŇKA	Female
	VASILISA	Female
5	**MATĚJ**	Male
	DMITRIY	Male
	LUCIE	Female
	KSENIYA	Female
6	**JIŘÍ**	Male
	OLEXIY	Male
	JITKA	Female
	OLENA	Female
7	**ZDENĚK**	Male
	VASILY	Male
	ALŽBĚTA	Female
	YELYZAVETA	Female
8	**RADEK**	Male
	ANATOLIY	Male
	BOŽENA	Female
	VARVARA	Female

Notes: This table shows the list of workers' names used in our experiment. Each profile had two female and male names: a local and foreign-sounding name. We do not present worker's nationality that each name is likely to signal because the focus of this chapter is on the gender aspect of (possible) disclosure discrimination.

Table 3.B.4: Randomization check I (assistants)

	(1) Male (control)	(2) Female Treatment	(3) t-test	(4) N
Female	0.49	0.49	0.76	6056
Age	42.23	41.83	0.22	6056
Household size	2.73	2.79	0.05	6056
Primary and secondary education without national school-leaving exam	0.41	0.42	0.76	6056
Secondary education with national school-leaving exam	0.38	0.37	0.17	6056
University degree	0.20	0.22	0.21	6056
Prague	0.13	0.12	0.56	6056
Central Bohemia	0.13	0.13	0.73	6056
Southwest	0.11	0.11	0.82	6056
Northwest	0.10	0.11	0.12	6056
Southeast	0.15	0.16	0.27	6056
Northeast	0.14	0.14	0.82	6056
Central Moravia	0.12	0.11	0.11	6056
Moravia-Silesia	0.12	0.11	0.16	6056
Employed	0.75	0.75	0.84	6008
Unemployed	0.02	0.02	0.82	6008
Income is missing	0.10	0.09	0.60	6056
Household net monthly income $>$ 50,000 CZK	0.29	0.33	0.01	5480
Has recruitment experience (dummy)	0.13	0.12	0.07	6056
Thought about the hiring manager	8.15	8.14	0.79	6056
Correctly identified last worker's gender	0.92	0.91	0.32	6056
Tolerance to women	3.26	3.25	0.61	6056
Mobile survey completion	0.40	0.42	0.36	6056
N	3071	2985		

Notes: Means of assistants' characteristics in the control and treatment group. Column (3) reports p-values of t-test for the hypothesis that the means are equal across the four treatment arms. The tolerance index was constructed by taking averages of responses to 7 questions regarding women (all measured on a scale from 1 to 5; when necessary, we recoded responses so that 5 would mean the highest tolerance).

Table 3.B.5: Randomization check II (assistants)

	(1) Female Treatment
Female	-0.005
	(0.013)
Age	-0.000
	(0.001)
Household size	0.009
	(0.006)
Primary and secondary	-0.022
education without national school-leaving exam	(0.018)
Secondary education with national school-leaving exam	-0.033*
	(0.018)
Central Bohemia	0.008
	(0.026)
Southwest	0.009
	(0.027)
Northwest	0.039
	(0.027)
Southeast	0.025
	(0.025)
Northeast	0.010
	(0.025)
Central Moravia	-0.026
	(0.026)
Moravian Silesian	-0.017
	(0.027)
Income is missing	-0.015
	(0.022)
Has recruitment experience	-0.040**
	(0.020)
Tolerance to women	-0.004
	(0.009)
Correctly identified last worker's gender	-0.030
	(0.024)
Mobile survey completion	0.006
	(0.014)
Thought about the hiring manager	-0.000
	(0.003)
Constant	0.545***
	(0.057)
N	6056
F	1.180
p-value of F-test	0.268

Notes: Regression of the treatment indicator on assistants' characteristics. Standard errors in parentheses. We include only covariates which do not have missings because our treatment effects are estimated on the full sample. If we include the high-income and employed dummies, the p-value of F-test is 0.217.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.6: Heterogeneity analyses for the effect of a female worker's name on the share of disclosed Demographic information

	Share of disclosed pieces (Demographics)		
	(1)	(2)	(3)
Female (a)	0.032***	0.013**	0.018***
	(0.006)	(0.006)	(0.005)
Female * Female assistant (b)	-0.025***		
	(0.008)		
Female * Biased against women (c)		0.013*	
		(0.008)	
Female * Low-quality profile (d)			0.005
			(0.007)
(a) + (b)	0.007		
	(0.005)		
(a) + (c)		0.026***	
		(0.005)	
(a) + (d)			0.023***
			(0.005)
Control mean	0.494	0.455	0.477
N	6056	6056	6056

Notes: All regressions include profile and assistant fixed effects. Standard errors (in parentheses) are clustered at the assistant level. *Female* is a treatment indicator equal to 1 if a CV has a female name. *Biased against women* is equal to 1 if an index of tolerance toward women is less or equal to its median value (see Section 3.4 for the details about the construction of the tolerance index). The control means are the average values of the outcome in the male-CVs group and: in Column (1), a subsample of male assistants; in Column (2), a subsample of "tolerant" assistants; in Column (3), a subsample of high-quality profiles.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.7: Heterogeneity analysis by assistant's gender for the effect of a female worker's name on the disclosed Demographic information

	(1) Age	(2) Marital status	(3) Number of children	(4) Driving license	(5) Surveys
Female (a)	-0.005	0.055***	0.103***	0.001	0.004
	(0.007)	(0.012)	(0.015)	(0.008)	(0.011)
Female * Female assistant (b)	-0.001	-0.064***	-0.044**	-0.009	-0.008
	(0.011)	(0.016)	(0.020)	(0.011)	(0.014)
(a) + (b)	-0.005	-0.009	0.059***	-0.007	-0.004
	(0.008)	(0.011)	(0.013)	(0.008)	(0.009)
Control mean	0.781	0.423	0.292	0.717	0.242
N	6056	5299	6056	6056	6056

Notes: All regressions include profile and assistant fixed effects. Standard errors (in parentheses) are clustered at the assistant level. *Female* is a treatment indicator equal to 1 if a CV has a female name. The control means are the average values of the outcomes in the male-CVs group and subsample of male assistants. *Surveys* informs about the actual number of surveys that a worker completed in the past.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.8: Heterogeneity analyses for the effect of a female worker's name on the share of disclosed Work information

	Share of disclosed pieces (Work)		
	(1)	(2)	(3)
Female (a)	-0.012*	-0.021***	-0.005
	(0.007)	(0.007)	(0.007)
Female * Female assistant (b)	-0.017*		
	(0.009)		
Female * Biased against women (c)		0.002	
		(0.009)	
Female * Low-quality profile (d)			-0.031***
(a) + (b)	-0.029***		
	(0.007)		
(a) + (c)		-0.019***	
		(0.007)	
(a) + (d)			-0.036***
			(0.007)
Control mean	0.588	0.606	0.606
N	6056	6056	6056

Notes: All regressions include profile and assistant fixed effects. Standard errors (in parentheses) are clustered at the assistant level. *Female* is a treatment indicator equal to 1 if a CV has a female name. *Biased against women* is equal to 1 if an index of tolerance toward women is less or equal to its median value (see Section 3.4 for the details about the construction of the tolerance index). The control means are the average values of the outcome in the male-CVs group and: in Column (1), a subsample of male assistants; in Column (2), a subsample of tolerant assistants; in Column (3), a subsample of high-quality profiles.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.9: Heterogeneity analyses for the effect of a female worker's name on disclosed Work information by profile quality

	(1) Area	(2) Position	(3) Experience	(4) Any responsibilities
Female (a)	-0.021	-0.009	0.018	-0.002
	(0.013)	(0.012)	(0.014)	(0.014)
Female * Low-quality profile (b)	0.006	0.043**	-0.025	-0.140***
	(0.018)	(0.018)	(0.020)	(0.021)
(a) + (b)	-0.015	0.035**	-0.007	-0.142***
	(0.013)	(0.013)	(0.014)	(0.015)
Control mean	0.774	0.808	0.638	0.642
N	6056	6056	5299	6056

Notes: All regressions include profile and assistant fixed effects. Standard errors (in parentheses) are clustered at the assistant level. *Female* is a treatment indicator equal to 1 if a CV has a female name. *Any responsibilities* is a binary variable equal to 1 if an assistant disclosed at least one job responsibility from a CV. The control means are the average values of the outcomes in the male-CVs group and subsample of high-quality profiles.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.10: Heterogeneity analysis by female-dominated occupation for the effect of a female worker's name on disclosed Work information

	(1) Area	(2) Position	(3) Experience	(4) Any responsibilities
Female (a)	-0.012	-0.022	0.018	-0.009
	(0.015)	(0.014)	(0.015)	(0.017)
Female * Female-dominated job (b)	-0.010	0.055**	-0.023	-0.173***
	(0.022)	(0.022)	(0.022)	(0.026)
(a) + (b)	-0.021	0.034**	-0.005	-0.182***
	(0.016)	(0.016)	(0.016)	(0.018)
Control mean	0.765	0.831	0.638	0.633
N	4542	4542	4542	4542

Notes: All regressions include profile and assistant fixed effects. Standard errors (in parentheses) are clustered at the assistant level. *Female* is a treatment indicator equal to 1 if a CV has a female name. *Female-dominated job* is a binary variable equal to 1 if a CV is based on a profile with a female-dominated occupation (we classify profiles 1, 5, 7 as female-dominated, and 2, 6, 8 as male-dominated; profiles 3 and 4 are ambiguous so we exclude them from this analysis). *Any responsibilities* is a binary variable equal to 1 if an assistant disclosed at least one job responsibility from a CV. The control means are the average values of the outcomes in the subsample of CVs with male names and male-dominated occupations.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.11: Heterogeneity analyses for the effect of a female worker's name on disclosed Work information by profile quality and assistants' gender

	(1) Area	(2) Position	(3) Experience	(4) Any responsibilities
Female (a)	-0.028	-0.008	0.034*	0.037*
	(0.019)	(0.018)	(0.021)	(0.020)
Female * Female assistant (b)	0.014	-0.003	-0.033	-0.080***
	(0.026)	(0.023)	(0.028)	(0.028)
Female * Low-quality profile (c)	0.011	0.072***	-0.029	-0.178***
	(0.026)	(0.026)	(0.029)	(0.030)
Female assistant * Low-quality profile	-0.008	0.029	-0.014	-0.020
	(0.025)	(0.026)	(0.027)	(0.030)
Female * Female assistant * Low-quality profile (d)	-0.011	-0.056	0.008	0.077*
	(0.035)	(0.036)	(0.039)	(0.041)
(a) + (c)	-0.017	0.064***	0.005	-0.141***
	(0.019)	(0.019)	(0.021)	(0.021)
(a) + (b)	-0.014	-0.011	0.001	-0.043**
	(0.017)	(0.015)	(0.020)	(0.019)
(a) + (b) + (c) + (d)	-0.013	0.005	-0.020	-0.144***
	(0.019)	(0.019)	(0.018)	(0.020)
Control mean	0.778	0.808	0.615	0.643
N	6056	6056	5299	6056

Notes: All regressions include profile and assistant fixed effects. Standard errors (in parentheses) are clustered at the assistant level. *Female* is a treatment indicator equal to 1 if a CV has a female name. *Any responsibilities* is a binary variable equal to 1 if an assistant disclosed at least one job responsibility from a CV. The control means are the average values of the outcomes in the male-CVs group and subsample of male assistants and high-quality profiles. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.B.12: Effect of female name on attention measures

	(1) Time	(2) Learn-more clicks
Female	-32.869	-0.018
	(32.814)	(0.040)
Control mean	129.232	0.693
Observations	6056	6056

Notes: *Female* is a treatment indicator equal to 1 if a CV has a female name. *Time* is the number of seconds that an assistant spent on selecting information from a CV. *Learn-more clicks* is the number of clicks that an assistant made on "More information" buttons embedded in a CV. All regressions include profile and assistant fixed effects. The control means are the average values of the outcomes when we limit the sample to CVs with male names.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.C Instructions (translated from Czech)

3.C.1 Assistants' instructions

Hello,

Participation in this survey is totally voluntary. If you start the survey and you no longer wish to finish it, you can do so without any consequences.

If you decide to participate in the survey, make sure that you have enough time to finish it (i.e. at least **25 minutes**), please.

For completion of the survey, you will receive the **reward stated in the invitation**. In addition, you may receive a **bonus** whose amount depends partially on your decisions. You will receive the bonus points in February 2022 at latest, after the evaluation of the whole survey.

In contrast to traditional survey questions, which are about hypothetical situations, **you will now make decisions that might have real** (financial) **consequences for other participants of our online labor market**. Specifically, you will select information from profiles of workers.

We would like to assure you that **panel iVýzkumy.cz guarantees your total anonymity and the confidentiality of your answers.**

Please answer the questions truthfully, according to your own judgement and knowledge, regardless of whether your opinions adhere to mainstream attitudes or are politically correct. It is crucial for success of the survey that you go attentively through the whole survey and adhere to the instructions in each part of the survey.

If you are done reading the text above and agree to participate in this survey, please check "Yes". You will start the survey by pressing the button →.

◯ Yes
◯ No

[Next page]

What is your **gender**?

◯ Man

◯ Woman

What is your **age**?

Enter a number into the following field:

What is your **highest completed education**?

◯ Unfinished elementary

◯ Elementary

◯ Vocational or general secondary without state examination

◯ Secondary with state examination

◯ Higher professional

◯ University

In what **region** do you reside?

We want to know the region where you actually live, not the region of your permanent residency. Click on the arrow below to show the list of regions.

∨

How many **people** are there in **your household** (including you)?

◯ 1

◯ 2

◯ 3

◯ 4

◯ 5

◯ More, write how many:

[Next page]

In this survey, you will act as an **assistant in hiring workers** in our **online labor market**.

We emphasize that, in contrast to traditional survey questions, which are about hypothetical situations, you will now make **decisions** that might have **real** (financial) **consequences** for other participants of our survey.

[Next page]

Your task will be to **review 8** workers' **profiles** and **select** only those **pieces of information** that you would like to **provide** to another Czech participant of our survey – this person will act as a **hiring manager**.

The **manager** will **hire** workers for a **financial task**, which consists of a **series of various financial decisions**, e.g. about investments.

The **manager** will be deciding about each of these 8 people individually, i.e. he/she might **hire any number of people** (e.g. all 8 or even nobody).

[Next page]

The **manager** will be **busy** because he/she will have to make multiple hiring decisions during a limited time. Therefore, **your task** of **simplifying the profiles** is a crucial help to him/her.

Before making a hiring decision about a worker, the **manager** will see **only the information** that **you will select**, but he/she will never see the workers' original profiles.

Some pieces of information will have a **button** More information next to them that will enable you to better understand the corresponding piece of information, but this button **will be never displayed to the manager**. Hence, if you choose the corresponding piece of information, the manager will see only its content, but not the button with the additional information.

[Next page]

Each profile simplification may be important because **your information selection** might **impact** the manager's **decisions** and bear **financial consequences**.

Workers who are **hired will receive extra money**. The **manager** will receive a **higher reward** if the hired workers perform well on the financial task.

[Next page]

It is important that you select information for the **manager** diligently because he/she **will decide how to reward your effort**. This **reward** will be paid **in addition** to your participation fee.

If the **manager** finds **your information selection useful**, he/she can give you **up to 500 points**, which costs him/her nothing. If the manager finds that your information selection is not useful at all, he/she might give you 0 points.
[Note: participants were rewarded by the data collection agency's points with a conversion rate 10 points = 1 CZK.]

During the survey, you will be able to return to these instructions.

[Next page]

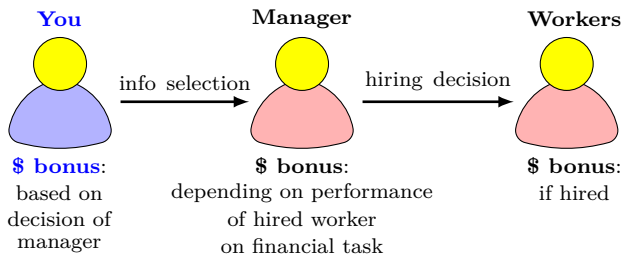
In this part, we would like to **check** your **understanding** of the task instructions that you just read. If you want to go through the **instructions one more time**, press the button ←.

For each of the following statements, please decide whether it is true or false.

	Yes	No
I will see profiles of **8 workers**. **My task** is to **select information** from these profiles for another Czech participant who will act as a **hiring manager**.	◯	◯
The manager is **hiring** people for a **financial task**. Besides information that I select, the **manager will NOT see the full profiles**. The **manager** will be **busy** making many hiring decisions.	◯	◯
The manager will determine **my bonus** according to how **useful** he/she finds **my information selection**.	◯	◯

[Next page]

All statements on the previous page were **CORRECT**.



[next page]

HERE you can recheck the instructions (they will open in a new tab).

Now you will look through a worker's profile. Please **select** the pieces of **information** that you would like to **provide to a manager** who will consider this worker for the **task** that consists of a **series of financial decisions**.

ONDŘEJ (ID 664)[22]

SUMMARY (based on self-evaluation)

☐ Ondřej has logical and technically oriented mindset. He behaves consistently and is eager to learn.

☐ OndŘej is sometimes inattentive to details and lacks self-confidence.

☐ According to Ondřej it is quite likely that [he/she] would be able to convince other people of [his/her] opinion in financial services.

DEMOGRAPHICS

☐ **Age:** 27

☐ **Marital status:** single

☐ **Number of children:** 0

☐ **Driving license:** yes

☐ **Number of completed online questionnaires:** 15 More information

[After clicking on "More information":]

The number of completed online questionnaires is a record from a database about the actual total number of online surveys that the worker has properly completed in the past. Note: This button with more information will never be displayed to the manager.

[22] To authentically illustrate the questionnaire of assistants, we present the 8 profiles in a random order and randomly select one of the corresponding names for each profile from Table 3.B.3. The presented order of profiles in this Appendix is: 2, 5, 7, 6, 1, 4, 8, 3. Note, however, that each assistant could see the profiles in a different order and with different names than displayed in this Appendix.

EDUCATION

- ☐ **Level:** university – master's degree
- ☐ **Area of studies:** economics
- ☐ **Favorite subject:** risk management More information
  [After clicking on "More information":]
  Risk management is an area of managing projects and processes that deals with determination and evaluation of their risks and undesirable effects. Note: This button with more information will never be displayed to the manager.
- ☐ **Favorite subject:** mathematics
- ☐ **Favorite subject:** financial analysis

WORK

- ☐ **Employment sector:** banking
- ☐ **Current position:** analyst
- ☐ **Work experience in the current position:** 2 years
- ☐ **Job responsibilities:** error analysis
- ☐ **Job responsibilities:** preparation of reports
- ☐ **Job responsibilities:** accounting control

CERTIFICATES (based on real tasks)

- ☐ Attained **6 points in a math test** (**average** of all candidates: **4** points) More information
  [After clicking on "More information":]
  The Math test included 10 questions that tested the knowledge of basic Math operations, equation solving, etc. Participants had 2.5 minutes to complete the test. Note: This button with more information will never be displayed to the manager.
- ☐ Attained **5 points in a financial-literacy quiz** (**average** of all candidates: **3.9** points) More information
  [After clicking on "More information":]

The financial-literacy quiz included 5 questions that tested the understanding of basic financial concepts (inflation, interest, etc.). Note: This button with more information will never be displayed to the manager.

☐ Attained **6 points in a slider task (average** of all candidates: **24** points) More information

[After clicking on "More information":]

The slider task is a mechanical task in which participants had to center within a 2-minute limit as many sliders as possible (max. 48) with a random initial position. Note: This button with more information will never be displayed to the manager.

VOLUNTEERING (based on real decisions)

☐ Completed a survey for free in order to **donate the money to a charity** More information

[After clicking on "More information":]

In the questionnaire, we asked the worker whether he or she is willing to participate in another survey in upcoming days and donate the reward from participation to a charity of own choice. If the worker agreed, he or she received later an invitation for a survey in which it was explicitly mentioned that the reward will be donated. We could verify whether the worker truly completed this survey. Note: This button with more information will never be displayed to the manager.

☐ **Donated own participation fee** in **56%** of completed online surveys

SKILLS

☐ **Microsoft Word:** advanced

☐ **Microsoft Excel:** advanced

☐ **Microsoft PowerPoint:** advanced

☐ **Internet banking:** using

☐ **English language:** good knowledge

☐ **Has experience with** data analysis

☐ **Has experience with** economics

☐ **Has experience with** data entry

INTERESTS

☐ Sport activities

☐ Traveling

☐ International news

[next page]

Based on **your** earlier **selection**, the hiring **manager will see** the following information:
***

ONDŘEJ (ID 664)

[At this point, the assistant saw what would be displayed to the manager about this worker based on the assistant's choices. This means also that the assistant could see that the More information buttons were going to be suppressed.]

***

If you want to return to the profile of the worker and **change your selection of information, press** button ←.

[next page]

HERE you can recheck the instructions (they will open in a new tab).

LUCIE (ID 141)

Now you will look through a worker's profile. Please **select** the pieces of **information** that you would like to **provide to a manager** who will consider this worker for the **task** that consists of a **series of financial decisions**.

SUMMARY (based on self-evaluation)

☐ Lucie is stress-resistant. Her strengths are credibility and responsibility.

☐ Lucie sometimes postpones things and is inattentive to details.

DEMOGRAPHICS

☐ **Age:** 31

☐ **Number of children:** 1

☐ **Driving license:** yes

☐ **Number of completed online questionnaires:** 15 More information
[After clicking on "More information":]
The number of completed online questionnaires is a record from a database about the actual total number of online surveys that the worker has properly completed in the past. Note: This button with more information will never be displayed to the manager.

EDUCATION

☐ **Level:** secondary (without school leaving exam)

☐ **Area of studies:** storage operator More information
[After clicking on "More information":]
A storage operator is primarily responsible for logistic operations with physical products: receipt of materials in a warehouse, management of warehouse records and administration, handling of materials, packing, and preparation of goods for expedition. Note: This button with more information will never be displayed to the manager.

WORK

☐ **Current work status:** on [maternal/parental] leave

☐ **Last employer:** post office

☐ **Last position:** delivery

☐ **Work experience in the last position:** 3 years

☐ **Job responsibilities:** communication with people

CERTIFICATES (based on real tasks)

☐ Attained **1 point in a math test** (**average** of all candidates: **4** points) More information

[After clicking on "More information":]

The Math test included 10 questions that tested the knowledge of basic Math operations, equation solving, etc. Participants had 2.5 minutes to complete the test. Note: This button with more information will never be displayed to the manager.

☐ Attained **2 points in a financial-literacy quiz** (**average** of all candidates: **3.9** points) More information

[After clicking on "More information":]

The financial-literacy quiz included 5 questions that tested the understanding of basic financial concepts (inflation, interest, etc.). Note: This button with more information will never be displayed to the manager.

☐ Attained **18 points in a slider task** (**average** of all candidates: **24** points) More information

[After clicking on "More information":]

The slider task is a mechanical task in which participants had to center within a 2-minute limit as many sliders as possible (max. 48) with a random initial position. Note: This button with more information will never be displayed to the manager.

SKILLS

☐ **Microsoft Word:** basic knowledge

☐ **Microsoft Excel:** basic knowledge

☐ **Microsoft PowerPoint:** basic knowledge

☐ **Internet banking:** not using

☐ **English language:** partial knowledge

☐ **Has experience with** data entry

INTERESTS

☐ Watching TV

☐ Sometimes reads Blesk [Blesk is a Czech tabloid]

[next page]

Based on **your** earlier **selection**, the hiring **manager will see** the following information:
***

LUCIE (ID 141)

[At this point, the assistant saw what would be displayed to the manager about this worker based on the assistant's choices.]

***

If you want to return to the profile of the worker and **change your selection of information, press** button ←.

[next page]

HERE you can recheck the instructions (they will open in a new tab).

Now you will look through a worker's profile. Please **select** the pieces of **information** that you would like to **provide to a manager** who will consider this worker for the **task** that consists of a **series of financial decisions**.

ЕЛИЗАВЕТА **(YELYZAVETA) (ID 812)**

SUMMARY (based on self-evaluation)

- ☐ Yelyzaveta is efficient. She is responsible and able to solve difficult and complex problems.
- ☐ Yelyzaveta sometimes postpones things. She is impulsive and bad at financial management.

DEMOGRAPHICS

- ☐ **Age:** 38
- ☐ **Marital status:** married
- ☐ **Number of children:** 1
- ☐ **Driving license:** yes

☐ **Number of completed online questionnaires:** 7 More information

[After clicking on "More information":]

The number of completed online questionnaires is a record from a database about the actual total number of online surveys that the worker has properly completed in the past. Note: This button with more information will never be displayed to the manager.

EDUCATION

☐ **Level:** secondary (without school leaving exam)

☐ **Area of studies:** administration

☐ **Favorite subject:** theory

☐ **Favorite subject:** practice

WORK

☐ **Employment sector:** trucking

☐ **Current position:** administrative worker

☐ **Work experience in the last position:** 6 years

☐ **Job responsibilities:** paperwork

CERTIFICATES (based on real tasks)

☐ Attained **3 points in a math test (average** of all candidates: **4** points) More information

[After clicking on "More information":]

The Math test included 10 questions that tested the knowledge of basic Math operations, equation solving, etc. Participants had 2.5 minutes to complete the test. Note: This button with more information will never be displayed to the manager.

☐ Attained **4 points in a financial-literacy quiz (average** of all candidates: **3.9** points) More information

[After clicking on "More information":]

The financial-literacy quiz included 5 questions that tested the understanding of basic financial concepts (inflation, interest, etc.). Note: This button with more information will never be displayed to the manager.

☐ Attained **44 points in a slider task (average** of all candidates: **24** points)
More information

[After clicking on "More information":]

The slider task is a mechanical task in which participants had to center within a 2-minute limit as many sliders as possible (max. 48) with a random initial position.

Note: This button with more information will never be displayed to the manager.

VOLUNTEERING (based on real decisions)

☐ **Donated own participation fee** in **100%** of completed online surveys

SKILLS

☐ **Microsoft Word:** professional

☐ **Microsoft Excel:** professional

☐ **Microsoft PowerPoint:** professional

☐ **Internet banking:** not using

☐ **English language:** partial knowledge

☐ **Has experience with** administrative work

☐ **Has experience with** building savings

INTERESTS

☐ Reading books

☐ Cooking

☐ Sport activities

☐ Reading business literature

[next page]

Based on **your** earlier **selection**, the hiring **manager will see** the following information:

***

ЕЛИЗАВЕТА **(YELYZAVETA) (ID 812)**

[At this point, the assistant saw what would be displayed to the manager about this worker based on the assistant's choices.]

***

If you want to return to the profile of the worker and **change your selection of information, press** button ←.

[next page]

HERE you can recheck the instructions (they will open in a new tab).

Now you will look through a worker's profile. Please, **select** the pieces of **information** that you would like to **provide to a manager** who will consider this worker for the **task** that consists of a **series of financial decisions**.

ОЛЕКСІЙ **(OLEXIY) (ID 347)**

SUMMARY (based on self-evaluation)

- ☐ Olexiy is even-tempered. He is good at solving difficult and complex problems and is creative.
- ☐ Olexiy is sometimes indecisive and fears mathematics.
- ☐ According to Olexiy, he could very probably convince others of his opinion in financial services.

DEMOGRAPHICS

- ☐ **Age:** 44
- ☐ **Marital status:** married
- ☐ **Number of children:** 1
- ☐ **Driving license:** yes
- ☐ **Number of completed online questionnaires:** 6 More information
  [After clicking on "More information":]
  The number of completed online questionnaires is a record from a database about

the actual total number of online surveys that the worker has properly completed in the past. Note: This button with more information will never be displayed to the manager.

EDUCATION

- ☐ **Level:** university – master's degree
- ☐ **Area of studies:** social geography
- ☐ **Favorite subject:** geography
- ☐ **Favorite subject:** English

WORK

- ☐ **Employment sector:** insurance
- ☐ **Current position:** product manager More information
  [After clicking on "More information":]
  Product manager is responsible for having an overview of the market, monitoring current trends and their identification. Based on the observations, he/she then creates strategic plans, including the design, creation and launch of new products. Note: This button with more information will never be shown to the manager.
- ☐ **Work experience in the last position:** 3 years
- ☐ **Job responsibilities:** product management
- ☐ **Job responsibilities:** content on intranet and web
- ☐ **Job responsibilities:** organization of testing of new products

CERTIFICATES (based on real tasks)

- ☐ Attained **2 points in a math test** (**average** of all candidates: **4** points) More information
  [After clicking on "More information":]
  The Math test included 10 questions that tested the knowledge of basic Math operations, equation solving, etc. Participants had 2.5 minutes to complete the test. Note: This button with more information will never be displayed to the manager.

☐ Attained **5 points in a financial-literacy quiz (average** of all candidates: **3.9** points) More information

[After clicking on "More information":]

The financial-literacy quiz included 5 questions that tested the understanding of basic financial concepts (inflation, interest, etc.). Note: This button with more information will never be displayed to the manager.

☐ Attained **41 points in a slider task (average** of all candidates: **24** points) More information

[After clicking on "More information":]

The slider task is a mechanical task in which participants had to center within a 2-minute limit as many sliders as possible (max. 48) with a random initial position. Note: This button with more information will never be displayed to the manager.

VOLUNTEERING (based on real decisions)

☐ **Donated own participation fee** in **100%** of completed online surveys

SKILLS

☐ **Microsoft Word:** basic knowledge

☐ **Microsoft Excel:** basic knowledge

☐ **Microsoft PowerPoint:** basic knowledge

☐ **Internet banking:** using

☐ **English language:** good knowledge

☐ **Has experience with** product management

☐ **Has experience with** with holding stocks and mutual funds

INTERESTS

☐ Reading books

☐ Finance/business/economics

[next page]

Based on **your** earlier **selection**, the hiring **manager will see** the following information:
***

ОЛЕКСІЙ **(OLEXIY) (ID 347)**

[At this point, the assistant saw what would be displayed to the manager about this worker based on the assistant's choices.]

***

If you want to return to the profile of the worker and **change your selection of information, press** button ←.

[next page]

HERE you can recheck the instructions (they will open in a new tab).

Now you will look through a worker's profile. Please, **select** the pieces of **information** that you would like to **provide to a manager** who will consider this worker for the **task** that consists of a **series of financial decisions**.

PETR (ID 778)

SUMMARY (based on self-evaluation)

☐ Petr's strengths are logical thinking and trustworthiness.

☐ Petr is sometimes unorganized and postpones things.

☐ According to Petr, it is important to keep learning new things.

DEMOGRAPHICS

☐ **Age:** 38

☐ **Marital status:** married

☐ **Number of children:** 2

☐ **Driving license:** yes

☐ **Number of completed online questionnaires:** 7 More information

[After clicking on "More information":]

The number of completed online questionnaires is a record from a database about the actual total number of online surveys that the worker has properly completed in the past. Note: This button with more information will never be displayed to the manager.

EDUCATION

☐ **Level:** secondary (with school leaving exam)

☐ **Area of studies:** business and service management

☐ **Favorite subject:** commodity expertise More information

[After clicking on "More information":]

It enables orientation in the main assortment groups in accordance with valid legislation and the requirements of business practice, it clarifies the issue of consumer properties, quality, evaluation of goods, defects of goods, labeling, and professional sale of goods. Note: This button with more information will never be shown to the manager.

☐ **Favorite subject:** mathematics

WORK

☐ **Employment sector:** trade – purchase and sale of goods

☐ **Current position:** cashier

☐ **Work experience in the current position:** 1 year

☐ **Job responsibilities:** communication

☐ **Job responsibilities:** service

☐ **Job responsibilities:** goods

CERTIFICATES (based on real tasks)

☐ Attained **5 points in a math test (average** of all candidates: **4** points) More information

[After clicking on "More information":]

The Math test included 10 questions that tested the knowledge of basic Math operations, equation solving, etc. Participants had 2.5 minutes to complete the test. Note: This button with more information will never be displayed to the manager.

☐ Attained **4 points in a financial-literacy quiz (average** of all candidates: **3.9** points) More information

[After clicking on "More information":]

The financial-literacy quiz included 5 questions that tested the understanding of basic financial concepts (inflation, interest, etc.). Note: This button with more information will never be displayed to the manager.

☐ Attained **9 points in a slider task (average** of all candidates: **24** points) More information

[After clicking on "More information":]

The slider task is a mechanical task in which participants had to center within a 2-minute limit as many sliders as possible (max. 48) with a random initial position. Note: This button with more information will never be displayed to the manager.

SKILLS

☐ **Microsoft Word:** basic knowledge

☐ **Microsoft Excel:** basic knowledge

☐ **Microsoft PowerPoint:** no experience

☐ **Internet banking:** not using

☐ **English language:** partial knowledge

☐ **Has experience with** customer service

INTERESTS

☐ Watching TV

☐ Trips to the countryside

[next page]

Based on **your** earlier **selection**, the hiring **manager will see** the following information:
***

PETR (ID 778)

[At this point, the assistant saw what would be displayed to the manager about this worker based on the assistant's choices.]

***

If you want to return to the profile of the worker and **change your selection of information, press** button ←.

[next page]

HERE you can recheck the instructions (they will open in a new tab).

Now you will look through a worker's profile. Please, **select** the pieces of **information** that you would like to **provide to a manager** who will consider this worker for the **task** that consists of a **series of financial decisions**.

ZDEŇKA (ID 459)

SUMMARY (based on self-evaluation)

☐ Zdeňka has logical thinking. She is responsible and courteous.

☐ Zdeňka is sometimes indecisive and lacks self-confidence.

☐ Zdeňka considers herself good at money management.

DEMOGRAPHICS

☐ **Age:** 30

☐ **Marital status:** single

☐ **Number of children:** 0

☐ **Driving license:** yes

☐ **Number of completed online questionnaires:** 151 More information

[After clicking on "More information":]

The number of completed online questionnaires is a record from a database about the actual total number of online surveys that the worker has properly completed in the past. Note: This button with more information will never be displayed to the manager.

EDUCATION

☐ **Level:** university – master's degree

☐ **Area of studies:** statistics

☐ **Favorite subject:** statistics

☐ **Favorite subject:** demographics

☐ **Favorite subject:** accounting

WORK

☐ **Employment sector:** marketing/management/advertising/media

☐ **Current position:** project field manager More information

[After clicking on "More information":]

Project field manager is responsible for smooth and efficient day-to-day progress of a project. He/She tries to learn and fulfill needs of clients, set goals and timelines, determine a budget, manage the work group, and control the progress of the project in order to meet standards and regulations. He/She also makes interim reports and evaluations and suggests improvements of processes. Note: This button with more information will never be shown to the manager.

☐ **Job responsibilities:** communication

☐ **Job responsibilities:** database management

☐ **Job responsibilities:** work organization

CERTIFICATES (based on real tasks)

☐ Attained **8 points in a math test (average** of all candidates: **4** points) More information

[After clicking on "More information":]

The Math test included 10 questions that tested the knowledge of basic Math operations, equation solving, etc. Participants had 2.5 minutes to complete the test. Note: This button with more information will never be displayed to the manager.

☐ Attained **4 points in a financial-literacy quiz (average** of all candidates: **3.9** points) More information

[After clicking on "More information":]

The financial-literacy quiz included 5 questions that tested the understanding of basic financial concepts (inflation, interest, etc.). Note: This button with more information will never be displayed to the manager.

☐ Attained **48 points in a slider task (average** of all candidates: **24** points) More information

[After clicking on "More information":]

The slider task is a mechanical task in which participants had to center within a 2-minute limit as many sliders as possible (max. 48) with a random initial position. Note: This button with more information will never be displayed to the manager.

VOLUNTEERING (based on real decisions)

☐ Completed a survey for free in order to **donate the money to a charity** More information

[After clicking on "More information":]

In the questionnaire, we asked the worker whether he or she is willing to participate in another survey in upcoming days and donate the reward from participation to a charity of own choice. If the worker agreed, he or she received later an invitation for a survey in which it was explicitly mentioned that the reward will be donated. We could verify whether the worker truly completed this survey. Note: This button with more information will never be displayed to the manager.

☐ **Donated own participation fee** in **69%** of completed online surveys

SKILLS

☐ **Microsoft Word:** advanced

☐ **Microsoft Excel:** advanced

☐ **Microsoft PowerPoint:** basic knowledge

☐ **Internet banking:** using

☐ **English language:** good knowledge

☐ **Has experience with** mathematics

☐ **Has experience with** data entry

☐ **Has experience with** data analysis

INTERESTS

☐ Sport activities

☐ Music

[next page]

Based on **your** earlier **selection**, the hiring **manager will see** the following information:
***

ZDEŇKA (ID 459)

[At this point, the assistant saw what would be displayed to the manager about this worker based on the assistant's choices.]

***

If you want to return to the profile of the worker and **change your selection of information, press** button ←.

[next page]

HERE you can recheck the instructions (they will open in a new tab).

Now you will look through a worker's profile. Please, **select** the pieces of **information** that you would like to **provide to a manager** who will consider this worker for the **task** that consists of a **series of financial decisions**.

АНАТОЛИЙ (ANATOLIY) (ID 235)

SUMMARY (based on self-evaluation)

- ☐ Anatoliy has a technically-oriented mindset and is hungry for knowledge.
- ☐ Anatoliy is sometimes direct in expressing controversial opinions and unwilling to comply with social norms.
- ☐ Anatoliy considers himself good at money management and he does not leave financial decisions to other family members.

DEMOGRAPHICS

- ☐ **Age:** 38
- ☐ **Marital status:** single
- ☐ **Number of children:** 0
- ☐ **Driving license:** yes
- ☐ **Number of completed online questionnaires:** 58 More information
  [After clicking on "More information":]
  The number of completed online questionnaires is a record from a database about the actual total number of online surveys that the worker has properly completed in the past. Note: This button with more information will never be displayed to the manager.

EDUCATION

- ☐ **Level:** university – master's degree
- ☐ **Area of studies:** electronics and communication technology
- ☐ **Favorite subject:** telecommunication networks
- ☐ **Favorite subject:** circuit theory More information
  [After clicking on "More information":]
  An electrical circuit is a conductive connection of electrical elements, e.g. resistors, diodes, and switches. Circuit theory applies physical laws and principles in the analysis of elementary phenomena in DC and AC electrical circuits, defines basic

circuit quantities (voltage, current) and basic circuit elements modeling all kinds of real energy interactions. The basic goal is the ability to calculate voltage and current anywhere in the circuit and based on them to assess the properties of electrical equipment. Note: This button with more information will never be displayed to the manager.

☐ **Favorite subject:** programming

WORK

☐ **Employment sector:** education

☐ **Current position:** IT administrator

☐ **Job responsibilities:** administration of computer network

☐ **Job responsibilities:** hardware maintenance

☐ **Work experience in the last position:** 5 years

CERTIFICATES (based on real tasks)

☐ Attained **3 points in a math test** (**average** of all candidates: **4** points) More information

[After clicking on "More information":]

The Math test included 10 questions that tested the knowledge of basic Math operations, equation solving, etc. Participants had 2.5 minutes to complete the test. Note: This button with more information will never be displayed to the manager.

☐ Attained **5 points in a financial-literacy quiz** (**average** of all candidates: **3.9** points) More information

[After clicking on "More information":]

The financial-literacy quiz included 5 questions that tested the understanding of basic financial concepts (inflation, interest, etc.). Note: This button with more information will never be displayed to the manager.

☐ Attained **12 points in a slider task** (**average** of all candidates: **24** points) More information

[After clicking on "More information":]

The slider task is a mechanical task in which participants had to center within a

2-minute limit as many sliders as possible (max. 48) with a random initial position.
Note: This button with more information will never be displayed to the manager.

VOLUNTEERING (based on real decisions)

☐ **Donated own participation fee** in **16%** of completed online surveys

SKILLS

☐ **Microsoft Word:** professional

☐ **Microsoft Excel:** professional

☐ **Microsoft PowerPoint:** basic knowledge

☐ **Internet banking:** using

☐ **English language:** good knowledge

☐ **Has experience with** economics

☐ **Has experience with** mathematics

☐ **Has experience with** holding stocks and mutual funds

INTERESTS

☐ Reading books

☐ Gardening

☐ News about finance/business/economics

[next page]

Based on **your** earlier **selection**, the hiring **manager will see** the following information:
***

АНАТОЛИЙ **(ANATOLIY) (ID 235)**

[At this point, the assistant saw what would be displayed to the manager about this worker based on the assistant's choices.]

***

If you want to return to the profile of the worker and **change your selection of information, press** button ←.

[next page]

HERE you can recheck the instructions (they will open in a new tab).

Now you will look through a worker's profile. Please, **select** the pieces of **information** that you would like to **provide to a manager** who will consider this worker for the **task** that consists of a **series of financial decisions**.

ОЛЕСЯ (OLESYA) (ID 585)

SUMMARY (based on self-evaluation)

- ☐ Olesya's strengths are courtesy and flexibility.
- ☐ Olesya is sometimes direct in expressing controversial opinions and has bad performance under pressure.
- ☐ Olesya considers herself good at money management and certainly does not leave financial decisions to other family members.
- ☐ According to Olesya, people should try again when they do not succeed the first time.

DEMOGRAPHICS

- ☐ **Age:** 34
- ☐ **Marital status:** single
- ☐ **Number of children:** 0
- ☐ **Driving license:** yes
- ☐ **Number of completed online questionnaires:** 9 More information
  [After clicking on "More information":]
  The number of completed online questionnaires is a record from a database about the actual total number of online surveys that the worker has properly completed in the past. Note: This button with more information will never be displayed to the manager.

EDUCATION

- ☐ **Level:** secondary (with school leaving exam)
- ☐ **Area of studies:** trade
- ☐ **Favorite subject:** law
- ☐ **Favorite subject:** accounting

WORK

- ☐ **Employment sector:** advertising
- ☐ **Current position:** project manager More information
  [After clicking on "More information":]
  Project manager proposes a structure and staffing of the implementation team for a specific project. He/She is then in charge of this project, divides everything into sub-tasks, and then checks and supervises their fulfillment. While working on the project, he/she cooperates in determining the financial requirements of the project, makes time estimates and updates them. He/she regularly prepares written reports on the status of the project. Note: This button with more information will never be shown to the manager.
- ☐ **Work experience in the current position:** 12 years
- ☐ **Job responsibilities:** communication with government offices
- ☐ **Job responsibilities:** invoicing
- ☐ **Job responsibilities:** communication with government

CERTIFICATES (based on real tasks)

- ☐ Attained **2 points in a math test** (**average** of all candidates: **4** points) More information
  [After clicking on "More information":]
  The Math test included 10 questions that tested the knowledge of basic Math operations, equation solving, etc. Participants had 2.5 minutes to complete the test. Note: This button with more information will never be displayed to the manager.

☐ Attained **5 points in a financial-literacy quiz** (**average** of all candidates: **3.9** points) More information

[After clicking on "More information":]

The financial-literacy quiz included 5 questions that tested the understanding of basic financial concepts (inflation, interest, etc.). Note: This button with more information will never be displayed to the manager.

☐ Attained **17 points in a slider task** (**average** of all candidates: **24** points) More information

[After clicking on "More information":]

The slider task is a mechanical task in which participants had to center within a 2-minute limit as many sliders as possible (max. 48) with a random initial position. Note: This button with more information will never be displayed to the manager.

SKILLS

☐ **Microsoft Word:** basic knowledge

☐ **Microsoft Excel:** basic knowledge

☐ **Microsoft PowerPoint:** basic knowledge

☐ **Internet banking:** not using

☐ **English language:** partial knowledge

☐ **Has experience with** data entry

☐ **Has experience with** customer service

INTERESTS

☐ Walks with the dog

☐ Sport activities

☐ Reading the newspaper

[next page]

Based on **your** earlier **selection**, the hiring **manager will see** the following information:

***

ОЛЕСЯ **(OLESYA) (ID 585)**

[At this point, the assistant saw what would be displayed to the manager about this worker based on the assistant's choices.]

***

If you want to return to the profile of the worker and **change your selection of information, press** button ←.

[next page]

If you are among 50 randomly chosen participants of this survey and you answer **correctly** the following **two questions**, you will earn **extra 200 points**.

In your opinion, what is the **country of origin of the last** worker whose profile you just saw?

◯ Czech Republic

◯ Post-Soviet country (e.g. Russia, Ukraine)

In your opinion, what is the **gender of the last** worker whose profile you just saw?

◯ Man

◯ Woman

[next page]

What guided your information selection for the hiring manager? **What did you try to achieve** with your information selection?

How much did you think about the hiring **manager** when selecting information about the workers for him/her?

Click on the slider to show the number that indicates its current position.

Not at all **Very much**

[next page]

Thank you for filling in the main part of our questionnaire. Now we would like to ask you to answer a couple of additional questions.

What is your **current employment situation**?

◯ Employed full-time
◯ Employed part-time
◯ Self-employed
◯ Unemployed but looking for a job
◯ Student, apprentice
◯ On maternal/parental leave / taking care of children
◯ Retired and not working
◯ In household
◯ Other
◯ I do not know / I do not want to answer

[next page]

Do you have **experience working in a hiring team**, e.g. Have you ever worked as a **human resource officer**?

◯ Yes
◯ No

[next page]

Please think about the **total net income of your household**. As net income, consider the **total amount that you have at your disposal**, **after taxes**—your income from work, state support, interest, etc.

To which **category** does the **net monthly income of your household** belong (total income of all members of the household together, without income of roommates)?

◯ No income
◯ Less than 15,000 Czech crowns

◯ 15,001-30,000 Czech crowns
◯ 30,001-40,000 Czech crowns
◯ 40,001-50,000 Czech crowns
◯ 50,001-75,000 Czech crowns
◯ 75,001-100,000 Czech crowns
◯ 100,001 and more Czech crowns
◯ I do not know / I do not want to answer

[new page]

Would you **mind** having as **your neighbor**:

	Definitely would mind	Somewhat would mind	Indifferent	Rather would NOT mind	Definitely would NOT mind
Czech	◯	◯	◯	◯	◯
Russian	◯	◯	◯	◯	◯
Ukrainian	◯	◯	◯	◯	◯
Chinese	◯	◯	◯	◯	◯
Mongol	◯	◯	◯	◯	◯
Indian	◯	◯	◯	◯	◯

To what extent do you **agree** with the following statements?

Foreigners from the countries of the former Soviet Union and Asia that are living long-term in the Czech Republic...

	Totally agree	Agree	I do not have an opinion	DISagree	Totally DIS-agree
present health risks (spreading diseases)	◯	◯	◯	◯	◯
cause criminality to increase	◯	◯	◯	◯	◯
threaten our way of life	◯	◯	◯	◯	◯
increase total unemployment	◯	◯	◯	◯	◯

To what extent do you **agree** with the following statements?

Foreigners **from the countries of the former Soviet Union and Asia** that are living long-term in the Czech Republic...

	Totally DIS-agree	DISgree	I do not have an opinion	Agree	Totally agree
help in resolving the problem of the ageing population	◯	◯	◯	◯	◯
contribute to developing the economy	◯	◯	◯	◯	◯
enrich our own culture	◯	◯	◯	◯	◯

[new page]

To what extent do you **agree** with the following statements?

	Totally agree	Agree	I do not have an opinion	DISagree	Totally DISagree
Women should always prioritize family over career.	◯	◯	◯	◯	◯
Women should take maternal leave after childbirth, not men.	◯	◯	◯	◯	◯
Women should take care of the household more than men.	◯	◯	◯	◯	◯
Women should take care of children more than men.	◯	◯	◯	◯	◯

To what extent do you **agree** with the following statements?

	Totally agree	Agree	I do not have an opinion	DISagree	Totally DISagree
Men are better managers than women.	◯	◯	◯	◯	◯
Financial provision for the family is foremost men's concern.	◯	◯	◯	◯	◯
Boys are more talented in technical fields and maths than girls.	◯	◯	◯	◯	◯

[next page]

Thank you for your participation. If you have any comments or questions concerning this survey, please write them in the field below. Your feedback is very important to us so that we can keep improving our research.

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