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Article — Published Version

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American Journal of Political Science

Provided in Cooperation with:

WZB Berlin Social Science Center

Suggested Citation: Schaeffer, Merlin; Krakowski, Krzysztof; Olsen, Asmus Leth (2024) : Correcting misperceptions about ethno-racial discrimination: The limits of evidence-based awareness raising to promote support for equal-treatment policies, American Journal of Political Science, ISSN 1540-5907, Wiley, Hoboken, NJ, Iss. Early View, pp. 1-22,
<https://doi.org/10.1111/ajps.12933>

This Version is available at:

<https://hdl.handle.net/10419/313539>

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ARTICLE

Correcting misperceptions about ethno-racial discrimination: The limits of evidence-based awareness raising to promote support for equal-treatment policies

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Abstract

The disadvantages experienced by minorities and lack of societal remedies are partly attributable to native-majority citizens' limited awareness of minority hardships. We investigate whether informing citizens about field-experimental audits on ethno-racial discrimination increases their recognition of the issue and support for equal-treatment policies. Extending a largely US-centric research frontier, we focus on beliefs about discrimination faced by Muslims in Denmark. To further comprehension, we test three types of framing: a scientist stressing credibility, a lawyer emphasizing the legal breach, or a minority expressing grief. Our survey experiment ($n = 4,800$) shows that citizens are generally aware of discrimination and tend to overperceive its extent. Communicating audit evidence corrects misperceptions but does not change recognition or policy support, regardless of framing or initial misperception. Only combining priming, correction, and framing temporarily increases recognition and donations to support groups. These findings suggest that audit-based awareness campaigns have limited immediate success beyond donations acknowledging minority hardships.

To what extent are majority citizens aware of the disadvantages minorities face in society and can awareness-raising initiatives increase recognition of these issues and garner support for equal-treatment policies? This question addresses the prevalent notion among social movement activists, policymakers, and social scientists that the disadvantages experienced by minorities and the lack of societal remedies for them are partly attributable to the majority of citizens' lacking awareness of the hardships that minorities encounter.

One of the most significant hardships faced by minorities is outright discrimination by actors in

influential gatekeeping positions, such as employers, landlords, or insurance agents. This type of discrimination is characterized by gatekeepers who wrongfully give less favorable consideration to minority applicants and citizens, thereby limiting their socioeconomic opportunities and access to political representation of their interests.

Because gatekeepers typically deny engaging in discrimination, social scientists have developed a sophisticated field-experimental methodology based on fictitious applications and citizen requests to identify and describe the extent of outright discrimination that minorities face. Recent examples include studies on labor-market discrimination of transgender people (Granberg et al., 2020) or discrimination of students with a disability by public primary schools (Rivera & Tilcsik, 2023). Field-experimental audits indicate the prevalence and persistence of outright discrimination,

Verification Materials: The materials required to verify the computational reproducibility of the results, procedures and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/HRUXYI>.

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particularly toward three minorities: Black and Latino Americans in the United States (Butler & Broockman, 2011; Kline et al., 2022; Yemane, 2020) and immigrants and their descendants with Muslim-sounding names in Europe (Hemker & Rink, 2017; Olsen et al., 2022).

This study investigates whether the significant efforts of social scientists to identify and describe the extent of ethno-racial discrimination also succeed in raising public awareness of the issue. Psychological research shows that laypeople require a causal link between a disadvantage and a person's identity or status to frame an event as discrimination (Apfelbaum et al., 2017; Major & Dover, 2016). In the 1960s, British sociologist Daniel (1968) and his team pioneered the first officially published field-experimental audit study to provide persuasive evidence of this causal link to members of the British Parliament, successfully leading to a revision of the "Race Relations Act" prohibiting discrimination in hiring, housing, and public services (Gaddis, 2018). Their work drew inspiration from earlier in-person audits conducted by social movement activists and civic organizations in the United States to advocate for improved intergroup relations (Cherry & Bendick, 2018). Thus, field-experimental audits originated as a tool for evidence-based advocacy. Aiming to reassess their original purpose, this study investigates whether the methodological rigor of field-experimental audits, which has established them as a powerful tool for discrimination research across political science, economics, and sociology, translates into persuasive power for citizens, influencing their recognition of discrimination and policy support.

To do so, this study reports the results of a survey experiment that employs a tailored method to measure misperceptions about the extent of discrimination against immigrants and their descendants with a Muslim-sounding name, by eliciting Danish native-majority citizens' beliefs about the results of field-experimental audits testing for discrimination. The study then investigates whether informing respondents about the results of the audit increases their recognition of the significance of the issue and their support for equal-treatment policies. The experiment is inspired by Haaland and Roth (2023) seminal study on US citizens' misperceptions of the extent of labor-market discrimination against Black Americans, which found that communication of field-experimental evidence can converge beliefs about the extent of racial discrimination but does not change support for pro-Black policies. We build on this work and advance it in two ways.

First, while Haaland and Roth (2023) focus on the impact of experimentally induced belief shifts on policy preferences, this study redirects attention to the practical application of field-experimental audits in fostering support for antidiscrimination policies and consequently explores the political communication

strategies needed to bridge the gap between the mere provision of evidence and the support for political action. We argue that previous research on correcting misperceptions has overlooked key insights from framing theory. This oversight is especially relevant in the present context, as field-experimental audits are based on fictional applications and requests, and rely on counterfactual logic that only works in the aggregate. We contend that because field experiments do not relate to real individuals and because the single rejections provide no evidence of discriminatory intent, lay audiences may struggle to comprehend their scientific credibility, their normative significance, and their personal significance for potentially affected minorities. Consequently, we test the importance of three ideal types of framing field-experimental evidence: an independent researcher framing the evidence as credible, a lawyer framing the evidence as a breach of the law, or a potentially affected minority framing the evidence as causing them personal grief.

Second, considering the history and legacy of slavery and racism in the United States, alongside their salience in public discourse, one might doubt whether Haaland and Roth (2023) counterintuitive findings generalize to other countries, other ethno-racial minorities who do not trace their ancestry to abducted and enslaved peoples, and discrimination in other social fields than hiring. To address this gap, we extend this line of research to discrimination across multiple social fields—including domains particularly relevant for political science, such as discrimination by politicians and public administrators—and against immigrants and their descendants with a Muslim-sounding name in the North-European context of Denmark. Denmark's historically homogeneous population, social-democratic welfare state, and consensus-oriented multiparty system contrast sharply with the United States, meanwhile immigrants and their descendants from predominantly Muslim countries face discriminatory dynamics distinct from those experienced by Black Americans. These dynamics, often characterized by ostracism due to perceived incompatibility between their norms and values and those of the West, have instead been compared to those experienced by Latino immigrants and their descendants in the United States, a minority for whom there is also a substantial field-experimental literature documenting discrimination (Einstein & Glick, 2017; Landgrave et al., 2023; White et al., 2015).

We conducted the preregistered survey experiment on a representative sample of 4,800 native-born Danes with native-born parents. The findings reveal that most native-majority Danes are well-informed about the discrimination against persons with a Muslim-sounding name as measured by field-experimental audits, and even tend to overestimate its prevalence. When respondents were provided with information

from the audits, their understanding of the extent of ethno-racial discrimination became more accurate and consistent. However, communicating results did not change respondents' recognition of the problem of discrimination or their support for equal-treatment policies, even among those who initially underestimated the extent of discrimination indicated by field experiments—regardless of whether they were framed by researchers, lawyers, potentially affected persons, or not framed at all.

Thanks to a second control group without elicited misperceptions, our experimental design allowed us to disentangle and circumvent the potential priming effects of elicitation, whereby information treatment designs can prime respondents, including those in the control group, for the topic under investigation. In our case, such priming could artificially increase recognition of the issue of discrimination and, through that channel, raise support for equal-treatment policies already in the baseline. Accounting for priming, correction combined with framing by credible sources indeed temporarily increased recognition of the problem and donations to a minority-run support group, but it still did not suffice to increase support for equal-treatment policies. Several exploratory robustness tests address concerns about backlash tendencies and do not point to any treatment effect heterogeneity. Taken together, this implies limits of evidence-based awareness-raising campaigns. While citizens may donate to recognize minority hardships, they hesitate to endorse policies that could impact their own privileges.

THEORETICAL BACKGROUND

Ethno-racial discrimination, the unfair disadvantaging of a person or a group based on their race, language, religion, or any other marker that is perceived as indicating a distinct ancestry or cultural background (Lippert-Rasmussen, 2006; Wimmer, 2013), is pervasive across Western democracies. It can take different forms, including structural barriers entailed, for example, in residential segregation (Massey, 1990), lending practices of banks (Quillian et al., 2020), or legacies of disadvantage stemming from historical forms of oppression like slavery (O'Connell, 2012).

In one of its most outright forms, discrimination manifests in the unfair consideration of applications and requests by gatekeepers such as landlords or employers (Pager & Shepherd, 2008). Because gatekeepers typically deny engaging in discrimination, US-American social movement activists and civic organizations developed in-person audits in the mid-20th century to advocate for improved intergroup relations (Cherry & Bendick, 2018). Inspired by this

work, social scientists have refined this technique to identify and describe the extent of outright discrimination that minorities face. They typically use a field-experimental methodology where fictitious applications or requests elicit fewer positive replies to ethno-racial minority members despite all else being held equal. Country-comparative field experiments and several meta-analyses imply that ethno-racial discrimination is one of the most universal forms of discrimination that varies only in its degree of severity (Auspurg et al., 2019; Polavieja et al., 2023; Quillian & Midtbøen, 2021).

Various equal-treatment policies aim to mitigate discrimination, and there is evidence to support the effectiveness of at least some of them. Today, all Western democracies have laws prohibiting ethno-racial discrimination or have ratified binding international treaties that forbid it (Porat, 2005). However, these laws are not sufficient because they presume that discrimination is deliberate and overt and that it leaves prosecutable traces of evidence (Edelman et al., 2016). As a result, personnel managers, scholars, and policymakers have suggested various equal-treatment policies to mitigate discrimination and enhance the demographic diversity of organizations (Dobbin, 2009). These policies include diversity training and transparency initiatives to reduce the influence of stereotypes and other cognitive biases, mentoring programs to broaden informal social networks, and accountability initiatives that track progress in the representation of minorities (Dobbin & Kalev, 2021). While the evidential basis of diversity training, which is the most common form of equal-treatment policy, is doubtful (Chang et al., 2019; Devine & Ash, 2022), companies that have implemented mentoring and accountability initiatives have indeed observed an increase in employee diversity (Dobbin et al., 2015; Kalev et al., 2006).

Yet, extensive research, particularly in North America, has documented the reluctance of native-majority citizens to embrace equal-treatment policies (Pew Research Center, 2019; Tuch & Hughes, 2011), and one of the main reasons for this lack of support seems to be the underestimation of the extent of discrimination (Bobo & Kluegel, 1993; Teney et al., 2022). These results have led scholars to conclude that an "effective way to foster support for diversity policies among employees is to inform them about the role of discrimination" (Scarborough et al., 2019, p. 207). This situation parallels research on misperceptions about immigration (Lutz & Bitschnau, 2023) or the extent of inequality (Gimpelson & Treisman, 2018; Hauser & Norton, 2017), demonstrating a broader pattern where inaccurate perceptions shape policy preferences.

An emerging line of survey-experimental research further supports this conclusion as it reveals that

citizens' policy preferences on various topics are influenced by faulty beliefs but can change in response to evidence-based information treatments that correct these misperceptions (Bursztyn & Yang, 2022). This has been well established in the area of immigration policy, where information treatments about the population shares of immigrants or related sociodemographic facts, such as their unemployment or crime rate, have been found to increase citizens' support for more liberal immigration policies (Alesina et al., 2023; Carnahan et al., 2021; Grigorieff et al., 2020; Thorson & Abdelaaty, 2023). A similar literature focuses on understanding how evidence of socioeconomic disparities influences people's political preferences and prompts support for political action (Alesina et al., 2021; Kuziemko et al., 2015; McCall et al., 2017; Thompson & Trejo, 2024), including the context of ethno-racial gaps (Brown et al., 2023; Callaghan et al., 2021; Peyton et al., 2022).

However, most research has focused on the impact of descriptive evidence of disparities, leaving the persuasiveness of complex causal evidence less understood. One notable exception is Alesina et al. (2024), who show that informing teachers about their results on an implicit association test reduces grading discrimination against students of immigrant origin. While audit studies originated as evidence-based advocacy tools of activists and civic associations, their evolution into a sophisticated field-experimental methodology qualifies them as complex evidence. But given their origins and the demonstrated persuasiveness of descriptive and recently also complex evidence, we hypothesize that correcting native-majority citizens' misperceptions about the extent of ethno-racial discrimination by informing them about the results of field-experimental audits increases their recognition of discrimination as a social problem and their support for equal-treatment policies (Hypothesis 1).

Misperceptions about ethno-racial discrimination

Misperceptions conceptually imply the possibility of accurate perceptions, and in the case of misperceiving the extent of discrimination, under- and overperceptions are also possible. Despite this, prior research has not discussed or defined what an accurate perception of the extent of discrimination is and how evidence-based awareness-raising could help to make perceptions more accurate.

The methodological consequence is that prior research has not definitively established the central presumption that native-majority citizens typically underestimate the prevalence of discrimination. Prior

studies have used standard survey methodology to gauge perceptions of discrimination on rating scales, which capture respondents' subjective evaluations of how much discrimination ethno-racial minorities face, or the extent to which discrimination is widespread and significant (Safi, 2023). However, these assessments cannot be validated against facts and should thus be viewed as measures of the subjective recognition of ethno-racial discrimination as a significant problem, rather than perceptions of its extent.

The only exception is Haaland and Roth (2023), who adopt a unique approach by assessing misperceptions by eliciting respondents' beliefs regarding the results of a field-experimental audit examining labor-market discrimination (see "Data and Methods"). Contrary to previous assumptions, their findings indicate that most citizens overestimate the level of discrimination faced by Black Americans. Furthermore, regardless of whether respondents initially underestimated or correctly perceived the extent of discrimination as estimated through field-experimental audits, informing them about the results of the audits had no discernible impact on their support for equal-treatment policies.

The theory of motivated reasoning provides a potential explanation for these sobering results (Flynn et al., 2017; Kunda, 1990). Corrective information tends to be effective in countering misperceptions resulting from wrong prior information or faulty guessing. In contrast, corrective information is less effective in countering misperceptions arising from directional bias, which involves biased beliefs aligned with a person's ideological predispositions (Hopkins et al., 2019; Kalla & Broockman, 2020; Kalla & Broockman, 2023). In the case of Haaland and Roth (2023), Republicans tend to underperceive discrimination against Black Americans, whereas Democrats tend to overperceive it. Consequently, correcting their misperceptions not only updates factual information but also challenges their partisan identity, potentially triggering backlash and rendering the correction ineffective. Accuracy-motivated bias poses another hurdle, as individuals may dismiss information from sources they consider partisan and unreliable (Druckman & McGrath, 2019). Distinguishing between directional and accuracy-motivated bias is complex (Little, 2021; Tappin et al., 2020); therefore, we employ the broader term "politically motivated bias."

To investigate whether correcting misperceptions about the extent of ethno-racial discrimination works in the absence of politically motivated bias, we conducted our study in Denmark. Strong political polarization and the undeniable legacy of slavery and racism in the United States make politically motivated bias an exceptionally strong problem there. Denmark contrasts the United States in this respect. Denmark has a multiparty parliamentary system with

comparatively low political polarization (Dinesen et al., 2020). Consequently, we find no evidence of politically motivated bias in misperceptions regarding discrimination (see “Results”). Moreover, Denmark has one of the historically most ethnically homogeneous populations in the West (Fearon, 2003), a social-democratic welfare regime (Esping-Andersen, 1990), and a dominant ethno-religious minority demarcated by religion rather than race—immigrants and their descendants from countries with a predominantly Muslim population. Compared to Black Americans, this group experiences ostracization due to perceived incompatibility between their norms and values and those of the West (Rytter & Pedersen, 2014; Sniderman et al., 2014), which aligns more closely with the experiences of Asian and especially Latino immigrants and their descendants in the United States (Alba, 2005; Kim, 1999; Zolberg & Woon, 1999). We can thus think of our case as a most different system with respect to the United States (Petersen & Aarøe, 2013).

In conclusion, we hypothesize that correcting native-majority citizens’ misperceptions about the extent of discrimination by informing them about the results of field experiments testing for discrimination increases their recognition of discrimination as a social problem and their support for equal-treatment policies in a European context, at least among those who underestimate the prevalence of discrimination (Hypothesis 2).

Framing by credible sources

As this study refocuses on the original practical application of field-experimental audits in fostering support for antidiscrimination policies, it becomes apparent that previous work on correcting misperceptions has largely disregarded important insights from framing theory. Framing refers to the process by which individuals use interpretive schemas to understand and make sense of events or issues (Goffman, 1974), and framing effects describe the subsequent influence of these schemas on attitudes and opinions (Chong & Druckman, 2007; Druckman, 2001). When certain aspects or considerations of an issue are emphasized, individuals tend to prioritize those aspects when forming their judgments. In the context of evidence-based information treatments, the way information is framed can shape how individuals understand and respond to that information. Intentional framing can thus be a powerful tool to modify citizens’ understanding of sociopolitical issues like discrimination and, consequently, alter their attitudes and policy preferences (Benford & Snow, 2000; Chong & Druckman, 2007).

Another line of literature demonstrates that citizens perceive evidence-based information treatments as more trustworthy and persuasive when it originates from a source they consider credible (Druckman, 2001; Druckman & McGrath, 2019). Eagly and Chaiken’s (1993) review highlights that this credibility can stem from three sources: (a) source objectivity, (b) source expertise surpassing that of the information recipient, and (c) sympathy and empathy with the source.

In our case, the issue at hand pertains to a lower response rate for applications or requests sent by fictitious minority members, which social scientists proficient in the subject readily frame as evidence of illegitimate and harmful discrimination. However, it remains uncertain whether citizens, who lack specialized knowledge, would employ the same frame to interpret this event. The refinement of activists’ in-person audits into field experiments has introduced the use of fictitious applications and requests, as well as counterfactual logic that only applies in the aggregate. Consequently, field-experimental audits do not involve real individuals, and individual rejections do not provide evidence of discriminatory intent.

Drawing upon framing theory and all three sources of credibility, we posit that lay citizens may find it challenging to ascertain whether differential return rates between fictional applications constitute credible evidence of discrimination unless (a, objectivity) an independent researcher affiliated with a reputable university confirms it. Furthermore, citizens may also struggle to understand the normative implications of differential return rates between fictional applications, as these rates do not provide direct evidence of prosecutable discriminatory intent, unless (b, expertise) a lawyer with expertise in these matters explains that the differential return rates indicate a violation of the law. Lastly, citizens may struggle to grasp the personal implications of differential return rates between fictional applications unless (c, sympathy and empathy) a potentially affected minority member explains how these results cause them personal distress.

In summary, we hypothesize that correcting misperceptions among native-majority citizens regarding the extent of discrimination is effective if field-experimental findings are framed as scientifically credible by an independent researcher, as indicative of a violation of the law by a lawyer, or as personally distressing by a potentially affected person (Hypothesis 3).

EXPERIMENTAL DESIGN

To investigate misperceptions of the extent of ethno-racial discrimination among native-majority citizens

and determine whether informing them about field-experimental evidence can enhance their recognition of this issue, we conducted a survey experiment in Denmark that was modeled after the work of Haaland and Roth (2023).

Denmark, while differing from the United States in important regards discussed above, shares similarities with other European countries in terms of increased ethno-racial diversity due to immigration, and the opinion climate associated with immigration, suggesting that patterns found in Denmark likely reflect similar relationships in other European contexts. The UN reported that in 2020, 12.4% of Denmark's population comprised foreign-born individuals, slightly below the European average of 14% (United Nations, 2020). The largest minorities in Denmark originate from Turkey, Poland, Syria, Romania, Germany, and Iraq, resulting from guest-worker immigration in the 1970s, refugee immigration from the 1990s onwards, EU internal migration, and subsequent family reunification (Danmarks Statistik, 2022). Anti-immigrant sentiment is similar to other European countries, with 42% believing that immigration negatively impacts the country (Supporting Information (SI) page 2). This is consistent with field-experimental audits. Quillian and Midtbøen (2021) report a labor-market discrimination ratio of 1.44 for Denmark, which is comparable to the international average of 1.45.

Our experiment was conducted from September to October 2022, among 4,800 native-born adult Danes with native-born parents (Schaeffer et al., 2024). Participants were recruited by the Danish survey company Epinion from an ongoing online panel and received compensation in points based on survey length, redeemable for gift cards or similar incentives. The survey company provided post-stratification weights to further ensure representativity. The study involved no deception and participants were explicitly informed of this fact at the outset. The Institutional Review Board at the Department of Sociology, University of Copenhagen approved the project.

We preregistered the main hypotheses, experimental design, sample definition, questionnaires, and an initial R script, which was based on a pilot among 200 respondents. The analyses presented in this article deviate from the preregistration to increase brevity without affecting the conclusions (SI page 3 lists all deviations).

As illustrated in Figure 1, the experiment consisted of seven consecutive steps, which include (1) eliciting misperceptions about the extent of ethno-racial discrimination uncovered in a field-experimental audit, (2) correcting these misperceptions by presenting participants with the results of the audit, (3) framing the correction, (4) measuring the main outcome, (5a/b) measuring additional outcomes, (6) eliciting updated misperceptions regarding another audit, and (7) a

question asking participants to donate a potential win from a raft.

This design resulted in six equally sized experimental groups ($n = 800$), to which participants were randomly assigned. Two of these groups served as control groups: "Control 0," which skipped all elicitations of misperceptions and experimental treatments and was only surveyed about outcome questions, and "No correction," which had their misperceptions elicited in steps (1) and (6) but did not receive any treatments. Eliciting misperceptions of the extent of discrimination causes those in the "No correction" group to consider the result of a field experiment testing for discrimination. It is possible that this elicitation has a priming effect, raising recognition of the problem of discrimination and support for policies even in the baseline. Having two control groups allows us to tease apart priming and correction effects.

The other four groups were treatment groups. The "Correction" group had their misperceptions elicited and corrected but received no framing. The "Researcher," "Lawyer," and "affected" groups had their misperceptions elicited and corrected, like "Correction," but also received a framing. The "No correction" and "Correction" groups replicate the experimental design of Haaland and Roth (2023), while the additional four experimental groups extend it. Four of the seven steps in the experiment included additional randomized treatments that are independent of the six main experimental groups (see below). The experiment used block randomization and was implemented as a full factorial survey. The replication package includes balance tables that demonstrate the successful randomization of participants.

Elicitation of misperceptions about the extent of ethno-racial discrimination

Following Haaland and Roth (2023), we elicit misperceptions about ethno-racial discrimination by surveying respondents' beliefs regarding field-experimental results. This approach allows for the measurement of under- and overperceptions by comparing responses to the established results. However, unlike traditional survey items, it does not capture perceptions of the overall prevalence of discrimination in society. This is because field-experimental audits, while providing causal evidence, are limited by specific design factors such as the names used to signal ethnicity and race, samples of houses or jobs targeted, or the specific application stages observed (e.g., callbacks vs. hiring offers) (Heckman, 1998). This trade-off seems justified by our focus on whether field-experimental audits can influence public opinion, and the fundamental limitation that empirical research can only address measurable misperceptions.

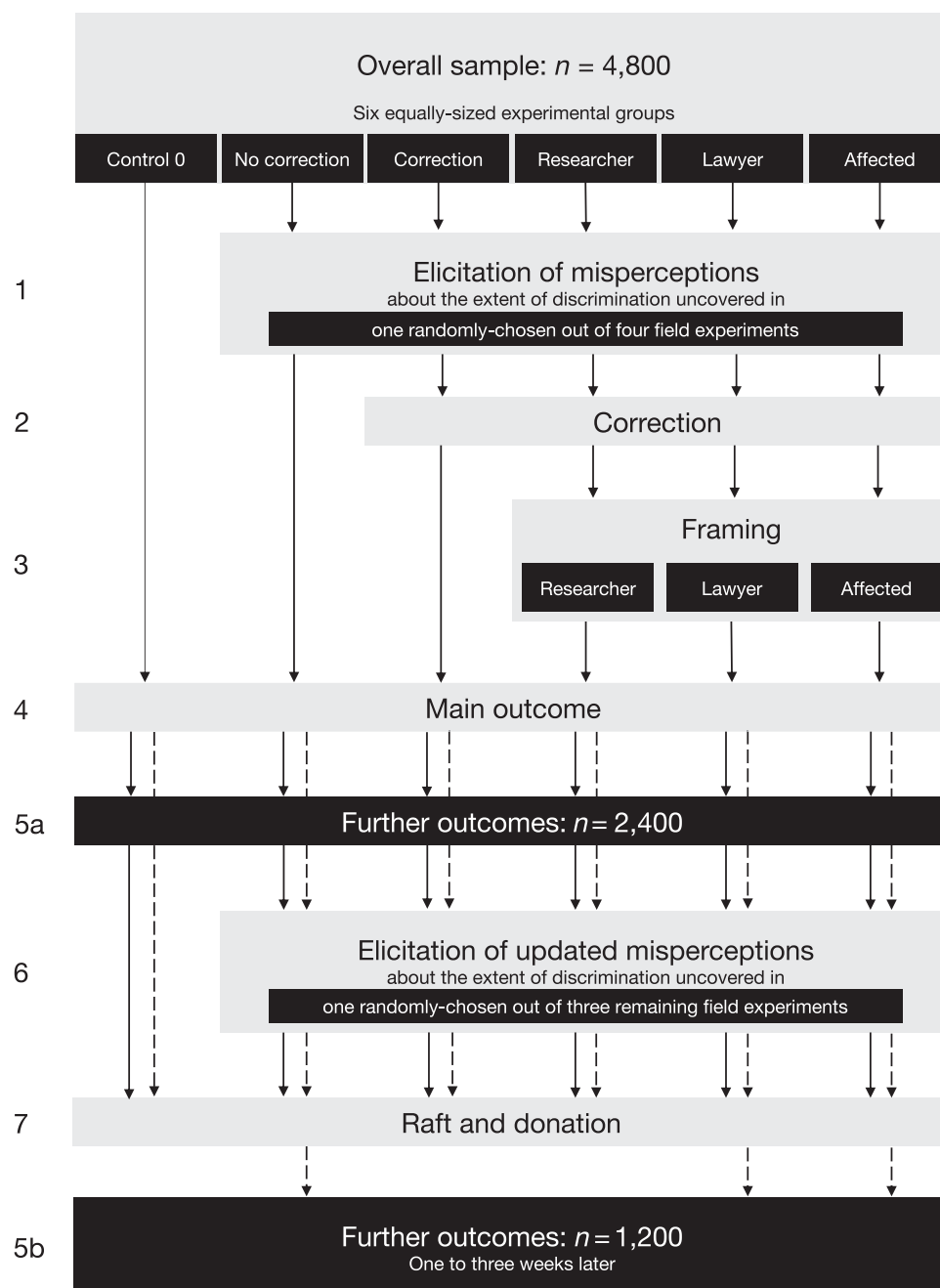


FIGURE 1 Experimental design. *Note:* Representation of experimental design with randomized elements in white. Parallel solid and dashed arrows indicate a random 50/50 split of the sample.

We began our experiment by informing participants that we were interested in their thoughts on a recent social science study. We then introduced them to one of four field experiments, selected randomly, regarding discrimination against individuals with a Muslim-sounding name in Denmark. These experiments included (1) Olsen et al. (2022) on discrimination by primary schools, (2) Dinesen et al. (2021) on discrimination by municipality politicians,

(3) Dahl and Krog (2018) on labor-market discrimination, and (4) Herby and Nielsen (2015) on housing discrimination.

The introduction to Olsen et al. (2022) and the elicitation of misperceptions regarding the extent of discrimination are provided below. The introductions and elicitation for the other three field experiments are identical except for the italicized text in brackets (SI pages 4–5).

Researchers from the University of Copenhagen have conducted a study on discrimination against non-Western minorities [*in Danish primary schools*]. They did so by sending out fictitious applications to [*primary schools in Denmark, where a parent requested to transfer their child to 3rd grade at the given school*].

The fictitious applications were exactly the same except for one thing: the name of [*the father of the child who was to be admitted to the school and who sent the email*]. Half of the [*applications*] had a typical Danish-sounding name such as “Peter Nielsen.” The other half of the [*applications*] had a typical Muslim-sounding name such as “Mohammad Osman.”

The researchers wanted to find out if [*schools*] would view the exact same [*application*] more positively if it was sent by a [*parent*] with a Danish-sounding name compared to a [*parent*] with a Muslim-sounding name.

Out of 100 fictitious [*applications*] with Danish-sounding names, [25] of the [*applications*] were [*accepted by the school*].

What do you think: out of 100 fictitious [*applications*] with Muslim-sounding names, how many of the [*applications*] were accepted by the [*school*]?

Unlike Haaland and Roth (2023), we measured the outcome of the field experiments as the number of positive replies out of 100 applications. In addition, respondents were shown the relative difference implied by the absolute figure as they typed it on their screen before submitting their answers. For example, if they entered that 15 out of 100 applications with a Muslim-sounding name received a positive reply (the correct answer), a text below the figure explained: “This means that primary schools were 67% more likely to admit a pupil with a Danish-sounding name compared to a pupil with a Muslim-sounding name.” Our design aimed to support respondents by providing both an absolute and a relative perspective on this challenging task.

At the very end of the survey, subsequent to the assessment of all outcomes, participants were asked to reevaluate their perception of the extent of ethno-racial discrimination in a randomly selected field experiment from the three studies they had not previously encountered. This enabled us to examine

whether participants successfully learn from corrective information and can transfer their updated understanding of discrimination to a new social context.

Correction

To examine the impact of informing citizens about field-experimental evidence on discrimination, the participants in the four treatment groups were subsequently provided with the findings of the study. Below we show the correction for Olsen et al. (2022). The corrections for the other three audits are identical except for the italicized text in brackets (SI page 6).

The researchers found that out of 100 fictitious [*applications*] with Muslim-sounding names, [15 of the applications got the student admitted to the school]. In comparison, [25] out of 100 [*applications got the student admitted to the school*] when the [*application*] had a Danish-sounding name. This means that [*primary schools were 67%*] more likely to admit a [*pupil*] with a Danish-sounding name compared to a [*pupil*] with a Muslim-sounding name.

Framing

To test our novel hypothesis that citizens require framing by credible sources to fully comprehend the social significance of field-experimental evidence, participants in three experimental groups were shown a split screen next (Figure 2). On the screen, the result of the audit was framed either as scientifically credible by a researcher, normatively alarming by a lawyer, or personally saddening by a potentially affected person. Care was taken to ensure that the statements were similar in style to isolate the effects of their different contents. Note that by jointly randomizing source (researcher, lawyer, minority member) and framing (credible, alarming, saddening), this design ensures realistic messages but precludes the separation of their independent effects. To uphold our commitment to participants that the study would be free of deception, all framing individuals were genuine people, identified by their full names and organizational affiliations, and had provided prior consent for their statements to be quoted as expressions of their personal and professional convictions.

We asked #Full Name, who is a lawyer, for a comment on the study. #S/he explained: “The results are alarming. Discrimination like this is prohibited by Danish law.”

We asked #Full Name, who is a non-Western minority person, for a comment on the study. #S/he explained: "The results make me sad. It worries me to hear that people with a name like mine are not treated equally."

We recruited an equal number of two female and two male researchers, lawyers, and potentially affected persons, and randomized which individual participants were exposed to (overall $4 \times 3 = 12$ real persons). Furthermore, among the potentially affected persons, one woman and one man were Black and of Somali origin, while the other two were of Arabic origin, and none of them wore a Hijab or any other religious symbol. This ensures generalizability of potential framing effects. All researchers were recruited from Aarhus University to emphasize their independent expertise since the audits were conducted by researchers from the University of Copenhagen. The lawyers were all employed at the Danish Institute for Human Rights, a publicly funded organization that provides policy advice, offers legal counseling, and conducts independent research. The SI contains the profile photos and full names of all 12 individuals who gave their consent to the publication of these materials (SI page 7).

Outcomes

To assess the effectiveness of our interventions, we analyzed their impact on attitudinal and hypothetical behavioral outcomes. First, we surveyed the main outcome of our study, which is support for name-blind applications, among all participants: "It has been suggested that removing names from applications and requests could reduce discrimination.

How much are you for or against making it mandatory to remove names from applications and citizen requests?" Participants were asked to rate their response on a 5-point Likert scale, ranging from "Totally against" to "Totally in favor." We chose support for name-blind applications as the main outcome because it is a tangible solution to the discrimination revealed by the audits presented to participants, and it is also the main outcome of Haaland and Roth (2023). Although the policy has been criticized for its potential to negatively impact minorities (Behaghel et al., 2015), it is unlikely that our respondents were aware of these criticisms or could have independently grasped the complex backlash dynamics. In addition to the main outcome, our study includes three attitudinal outcomes, which were measured as additive scales: Recognition of ethno-racial discrimination as a significant problem, reasons for ethno-racial discrimination against "non-Western minorities" (the term commonly used in public Danish discourse), and support for further equal-treatment policies (see Figures 5–9).

We then assessed participants' updated beliefs about the extent of discrimination based on one of the three remaining field experiments that they have not been previously surveyed about, selected at random. This time, participants received no correction or framing until the study was completed. We concluded the survey with a hypothetical behavioral outcome: "We will draw a prize of DKK 5,000 [730 USD] among all participants. If you win, you can split the money between yourself, Dansk Kvindesamfund [the Danish Women's Society], and Indvandrer Kvindecenret [the Immigrant Women's Center]. How will you distribute the money?" We operationalize the behavioral outcome as the amount of money participants allocate to the Immigrant Women's Center in the event of winning the prize.

We asked #Full Name, who is a university researcher, for a comment on the study. S/he explained:

"The results are credible. The study was conducted according to the highest standards of evidence within the research field."



#Full Name, #Organization

FIGURE 2 Framing. *Note:* Schematic representation of split screen framing.

Split sample and delayed measurement of outcomes

Survey experiments, particularly those dealing with morally sensitive topics, are susceptible to experimenter demand effects. Additionally, there are concerns about the durability of the effects as outcomes are often measured only minutes after the treatments. To address these concerns, only half of each experimental group was exposed to the three additional attitudinal outcome questions ($n = 2,400$). The other half were invited to another survey 1–3 weeks after the initial survey. Due to budget constraints, this follow-up only included the “No Correction” control group, and the “Lawyer” and “Affected” treatment groups ($n = 1,200$). Initial analysis, based on the independent sample of respondents surveyed immediately, suggested stronger treatment effects within the “Lawyer” and “Affected” groups compared to the “Researcher” and “Correction” groups. Respondents from the “Control 0” group were not re-invited as they were not exposed to any experimental intervention with potential fading effects over time. Our decision to invite follow-up survey respondents based on initial results in the independent sample of respondents who were surveyed immediately was preregistered, but its implementation deviated slightly from the original plan due to logistical constraints (SI page 3).

Estimation strategy

We analyzed the data using post-stratification-weighted OLS with robust standard errors. The SI compares weighted and unweighted demographics to results from Danish population registers (SI page 21). It also includes unweighted analyses that are more precise and consistent with those presented in the main article. Our main predictor is a dummy-coded variable representing the experimental groups, with “No correction” or “Control 0” as the reference. For most analyses, we estimate treatment effects in comparison to the “No correction” group. To improve the precision of the treatment effect estimates, we control for participants’ age, gender, education, household size, employment status, the field-experimental audit from which their misperception was elicited, and the bias of their misperception. It is worth noting that when analyzing priming effects by comparing treatment effects against the “Control 0” group, we cannot control for the latter two pretreatment variables, as “Control 0” participants did not participate in the misperception elicitation. Continuous predictor variables are z -standardized based on their distribution in the overall sample, and continuous outcome variables are z -standardized based on the mean and standard deviation of the control group in the respective analysis.

Due to missing values only in the outcomes (ranging from 2% for recognition of discrimination as a social problem to 6% for support for name-blind applications), we use casewise deletion. The SI investigates differential missingness across treatment conditions, implements Lee (2009) bounds, and detects a degree of sensitivity to missing data on one of the overall five outcomes (SI page 24).

RESULTS

Misperceptions about ethno-racial discrimination

How aware are native-majority citizens of the extent of ethno-racial discrimination in their society? Haaland and Roth (2023) found that, respectively, 70% and 81% of US citizens accurately estimate or even overestimate the extent of labor- and housing-market discrimination against Black Americans estimated through field-experimental audits. Our study shows that this also applies to native-majority citizens’ perceptions of discrimination against immigrants and their descendants with a Muslim-sounding name in a European setting.

Figure 3 displays the expectations of native-majority Danes regarding the results of four different field-experimental audits. The top two rows show histograms for each audit, while panel e expresses the same data as a cumulative distribution function centered around the return rate identified in the audit. Finally, panel f displays exploratory predictors of these expectations, also centered around the return rate identified in the field experiments—accordingly negative values indicate over- and positive underperceptions.

Figure 3 reveals that between 62.2% (schools) and 82.4% (political representation) of native-majority citizens have an accurate or inflated perception of the extent of discrimination estimated through field-experimental audits (those who expected return rates equal to or smaller than the dashed lines), while between 5.8% and 13% underperceive it (those who expected return rates between the dashed and solid lines). However, these figures should be treated with caution because of the hard cutoffs between accurate, inflated, and underperceived. This is not the case for the approximately 10.9% who believe that primary schools are fair since the return rate for applications with a Danish name was known to the respondents. Only between 5.6% and 2.4% believe in fair treatment in the other three fields. Finally, a significant percentage of native-majority citizens expect to be disadvantaged compared to immigrants and their descendants with a Muslim-sounding name,

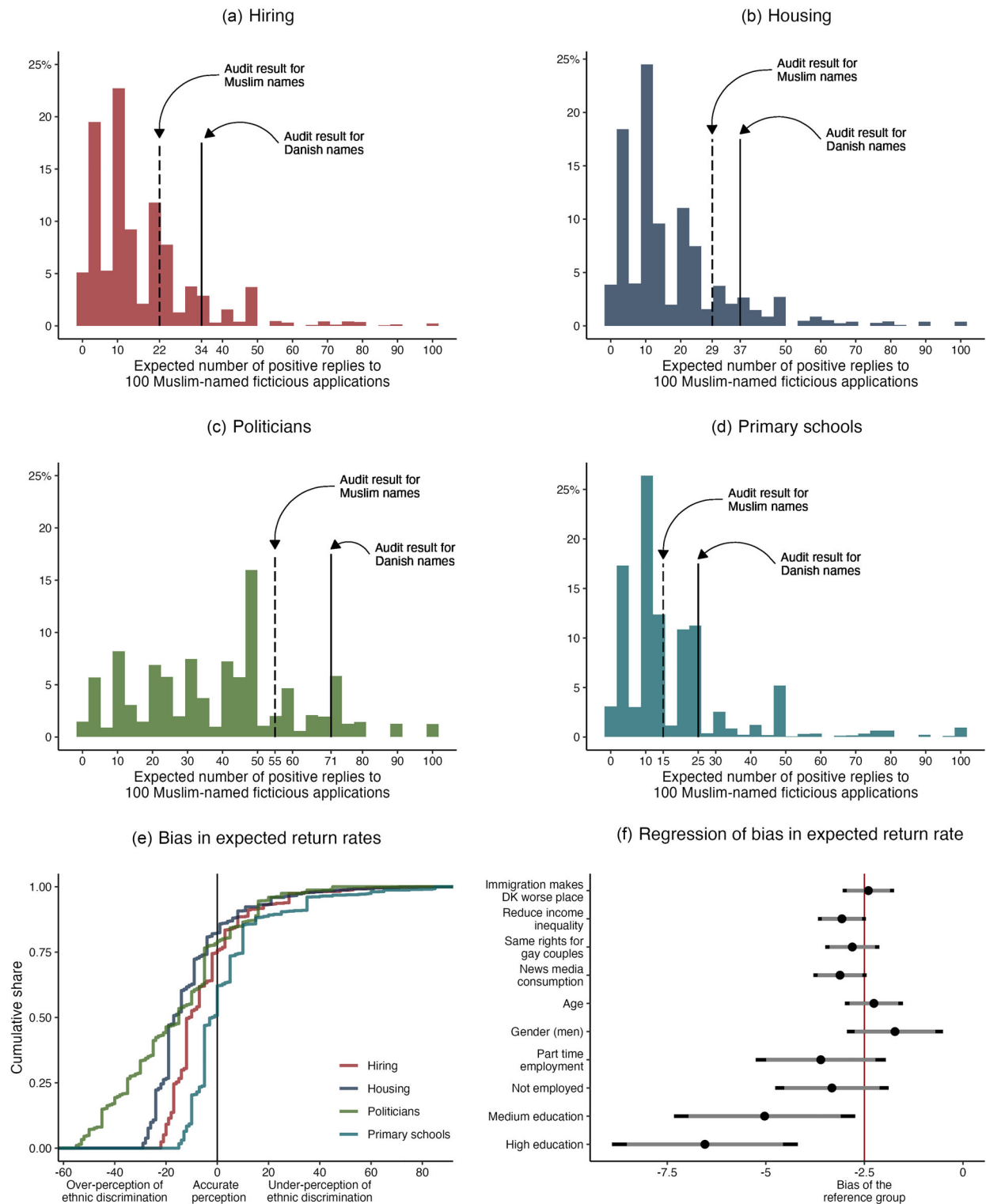


FIGURE 3 Misperceptions about the extent of ethno-racial discrimination. *Note:* Post-stratification weighted histograms (panels a to d) and cumulative-distribution functions (panel e). Ordinary-least-squares estimates with 90% and 95% confidence intervals (panel f) additionally adjusted for the type of field experiment with reference categories: female, gender, full-time employment, and low education.

especially by primary schools (14.6%) but also by landlords (9.3%), employers (8.9%), and politicians (5.5%).

Importantly, education is the only significant predictor of these perceptions, as the exploratory analysis in panel f shows. The better educated tend to over-perceive the extent of discrimination more. Though true beliefs remain impossible to measure definitively, these results suggest that left-leaning respondents are not deliberately exaggerating answers to signal political convictions; political attitudes, especially whether immigration makes Denmark a worse place to live, do not predict misperceived discrimination, suggesting that the measured perceptions do not have politically motivated bias. This conclusion finds further support in an additional analysis of the European Social Survey: native-majority Danes have remarkably accurate beliefs about the number of immigrants in their country (SI page 2), although these beliefs are typically regarded as being strongly influenced by politically motivated bias.

Nevertheless, one might argue that respondents expressed not their personal convictions about the extent of discrimination, but their perceptions of what academics would find. While the latter is indeed what the survey asked of the respondents, this would be problematic only if respondents doubted the credibility of academics' field-experimental audits. However, the SI refutes this concern. Inflated perceptions are not due to distrusting the methods of (presumably left-leaning) academics; they are given precisely by those who find the method credible and expect it to yield results that warrant policy action (SI page 23).

Learning about the extent of discrimination from field-experimental audits

Do native-majority citizens update their understanding of the extent of discrimination when they are presented with results of field-experimental audits? Our research indicates that they do. Panel A of Figure 4 demonstrates a scatter plot and linear fit of the association between respondents' initial misperception on the X-axis (the same data as Figure 3) and their misperception concerning another field experiment, which we assessed at the end of the survey on the Y-axis. Panel B shows the nonlinear relations between initial and subsequent misperceptions and confirms the linear estimates as providing a robust approximation of the overall trend. The figure pools respondents, regardless of the field experiments they were surveyed about, but only comprises those who were either informed about the initial field experiment result ("Correction") or those who were not ("No correction") from the six experimental groups.

For respondents who were not informed, the linear fit is akin to a 45-degree angle-bisecting line. This means that if respondents overestimate the extent of labor-market discrimination, they will typically overestimate the extent of discrimination by primary schools to the same degree. However, for those who were informed about the results of the initial field experiment, the strength of this association is significantly decreased; in fact, roughly halved. This suggests that respondents adjust the extent to which they over- or underperceive the severity of discrimination upon being informed about field-experimental evidence. The SI further supports this result by considering respondents from all experimental groups. Across combinations of field experiments, informing respondents about the results of a field experiment reduces the association between initially and subsequently measured misperception by approximately 70% and significantly decreases the absolute bias in the latter (SI pages 8–9).

It is essential to emphasize that these findings constitute genuine learning not just the repetition of facts. Respondents are cognitively processing a finding from one study to improve their expectations regarding another study about a different social field, with a different baseline discrimination rate, and several minutes and item batteries later. Therefore, we conclude that native-majority citizens learn and reduce their misperceptions about the extent of ethno-racial discrimination in response to being informed about social science evidence.

Awareness raising based on field-experimental audits

Does an improved understanding of the extent of ethno-racial discrimination, at least among those initially underestimating it, promote recognition and support for equal-treatment policies? Our results suggest that this is not the case, which refutes Hypotheses 1 and 2. Panel A of Figures 5–9 illustrates the distribution of outcomes in the control group, which represents public opinion on ethno-racial discrimination in Denmark. Panel B of Figures 5–9 shows the average treatment effects of informing participants about the result of a field-experimental audit, on the additive indices of the respective outcomes, thus indicating how much the average distribution in panel a is shifted by informing citizens about the results of the audit, in terms of standard deviations.

Ethno-racial discrimination is more often recognized as a social problem than rejected (Figure 5), and most equal-treatment policies find more support than rejection (Figure 8). However, three questions stand out. Danish native-majority citizens tend to reject the idea that ethno-racial inequalities are largely the

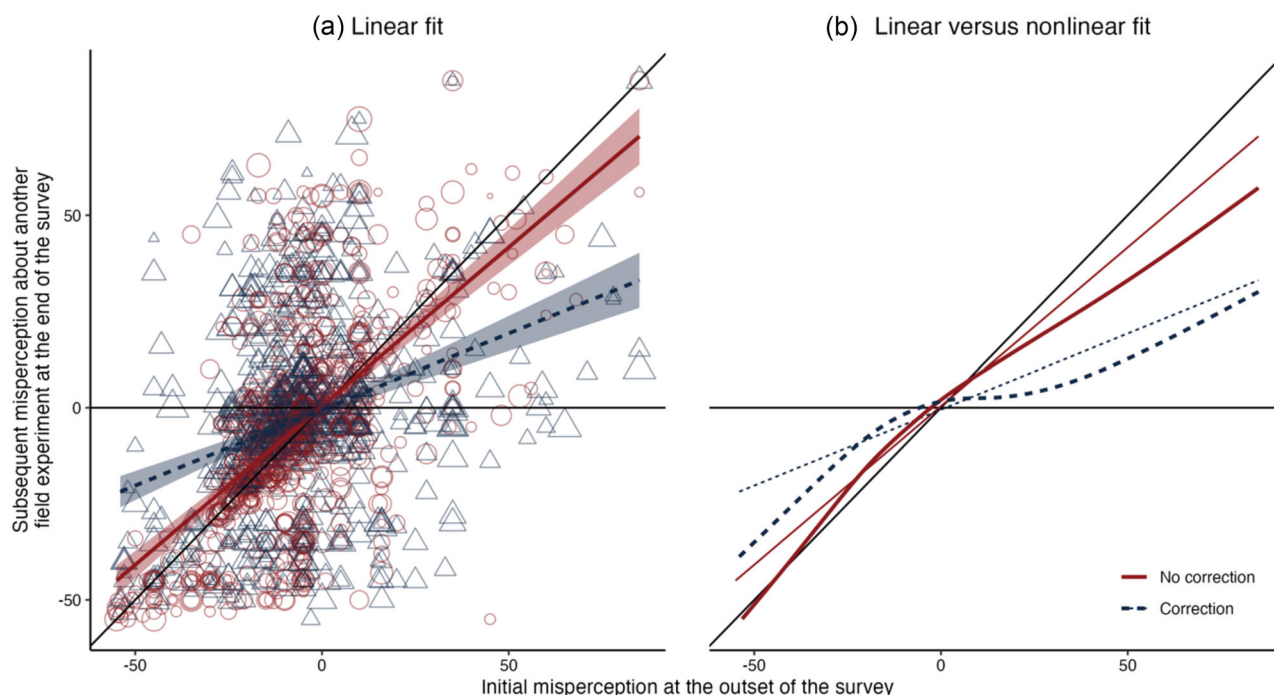


FIGURE 4 Learning about the extent of ethno-racial discrimination from audit evidence. *Note:* Post-stratification-weighted ordinary-least-squares estimates with 95% confidence intervals (panel a). Comparison of ordinary least squares to nonlinear generalized-additive-model estimates (panel b).

result of discrimination (Figure 5), they regard a clash between non-Western and Danish norms and values as a main reason for discrimination more than wrong stereotypes (Figure 6), and they are largely rejecting the idea of mandatory name-blind applications (Figure 7). Additionally, targets for the representation of minorities are the second least popular equal-treatment policy (Figure 8), suggesting less support for policies that imply a more fundamental reform of how

society is organized. Ironically, while public awareness campaigns are the most popular antidiscrimination measure, this article demonstrates their ineffectiveness. Overall, there is ample room for treatment effects to raise support for equal-treatment policies before any ceiling would be reached.

Throughout panel b of Figures 5–9, we further observe that informing citizens about the results of field-experimental audits does not change their

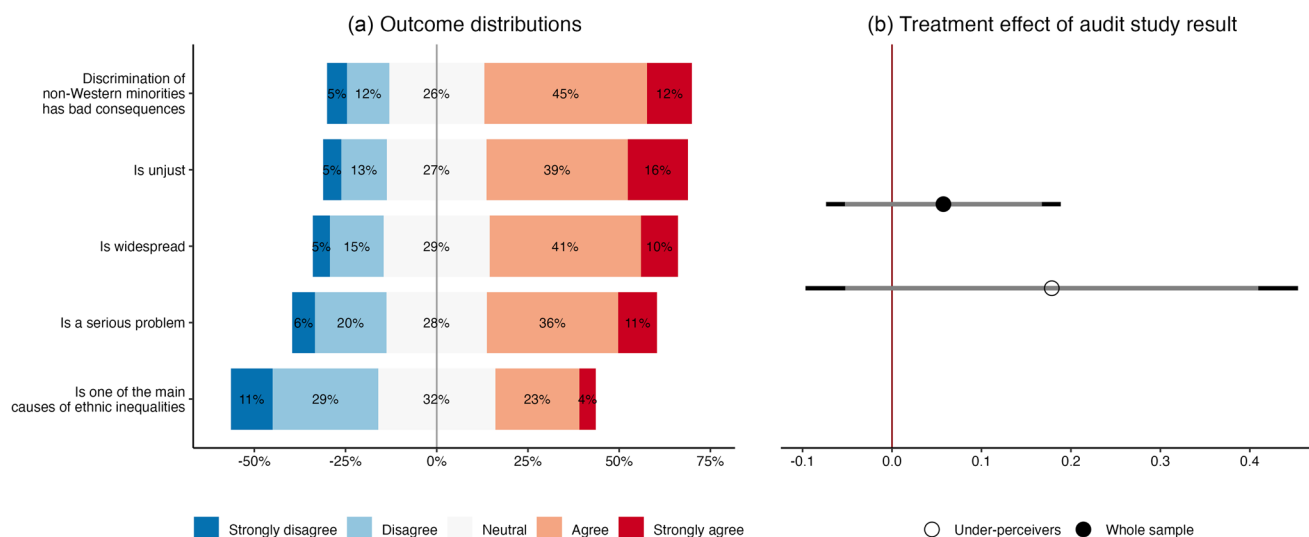


FIGURE 5 Effect of audit study result on recognition of discrimination as a significant problem. *Note:* Post-stratification-weighted outcomes (panel a). Ordinary-least-squares estimates with 90% and 95% confidence intervals. Effect sizes in standard deviations of the control group (panel b). Full regression tables on Supporting Information pages 10–11.

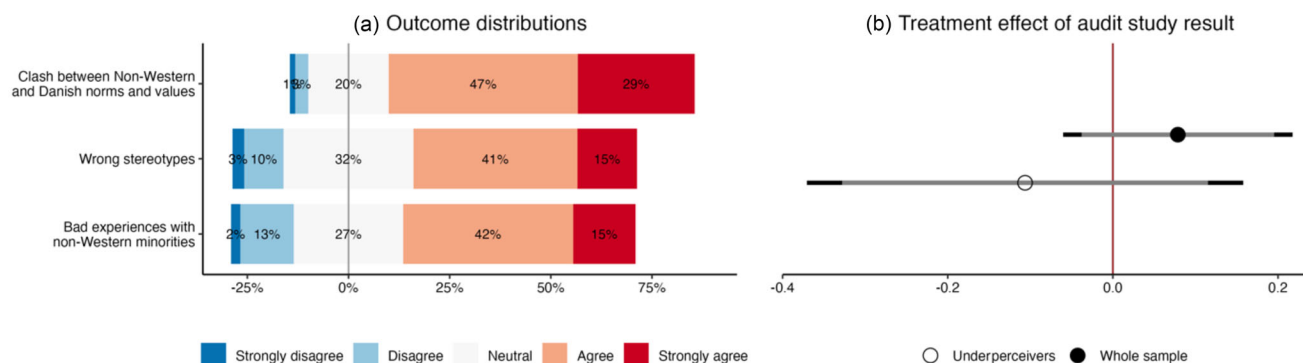


FIGURE 6 Effect of audit study result on reasons for discrimination. *Note:* Post-stratification-weighted outcomes (panel a). Ordinary-least-squares estimates with 90% and 95% confidence intervals. Effect sizes in standard deviations of the control group (panel b). Full regression tables on Supporting Information pages 10–11.

views on discrimination and equal-treatment policies. Estimates are insignificant not only for the overall sample (black-filled dot) but also for respondents who underperceived the extent of discrimination and were corrected in the intended way (hollow dots). This finding is further supported by nonlinear marginal effects based on Hainmueller et al. (2019) kernel smoothing estimator, which further demonstrates that there is no effect heterogeneity by initial misperception (SI page 15). One might raise concerns about the statistical power of these analyses due to the relatively small sample size of underperceivers. However, two observations support our interpretation. The point estimates among underperceivers do not indicate stronger treatment effects compared to the entire sample and four out of the five treatment effects for underperceivers point in the opposite direction than hypothesized. Nonlinear marginal effects confirm these two observations (SI page 15).

Accordingly, we conclude that regardless of their initial misperception, informing citizens about the results of field-experimental audits does not change their recognition of discrimination as a significant problem, their recognition of its reasons, and it does not change their support for equal-treatment policies or their willingness to donate to a minority-run support group. Thus, Haaland and Roth’s (2023) find-

ings may not be explained by US exceptionalism. In a very different context, across different social fields (education, politics, housing, and hiring), without any indication of politically motivated bias in Danish citizen’s misperceptions of the extent of discrimination, and with respect to a very different type of minority, we come to the same and thus more universal conclusion than previously thought.

Framing by credible sources

Do native-majority citizens alter their opinions of discrimination and equal-treatment policies if independent experts from renowned organizations or potentially affected persons help them frame the credibility and social significance of differential return rates between fictional applications? Contrary to Hypothesis 3, our results suggest that none of the three types of framing make a significant difference, regardless of the personal characteristics of the framing person, and regardless of the initial misperceptions of the participants. Figure 10 shows the treatment effects of the three types of framing and thus displays whether the additional framing of field-experimental results shifts the public opinion displayed in panel a of Figures 5–9. All 36 effect estimates are statistically insignificant,

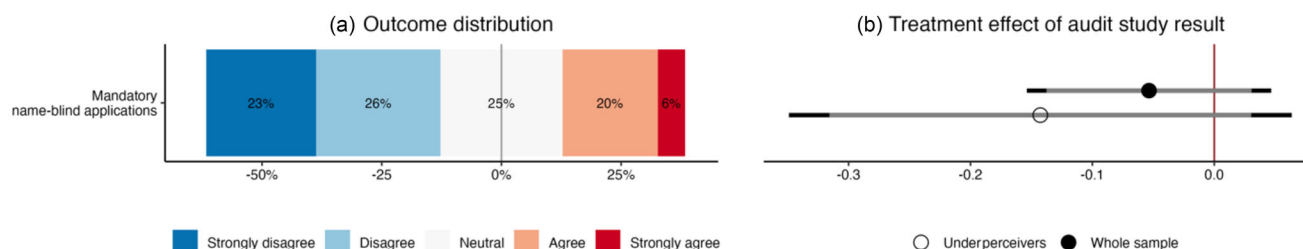


FIGURE 7 Effect of audit study result on support for name-blind applications. *Note:* Post-stratification-weighted outcomes (panel a). Ordinary-least-squares estimates with 90% and 95% confidence intervals. Effect sizes in standard deviations of the control group (panel b). Full regression tables on Supporting Information pages 10–11.

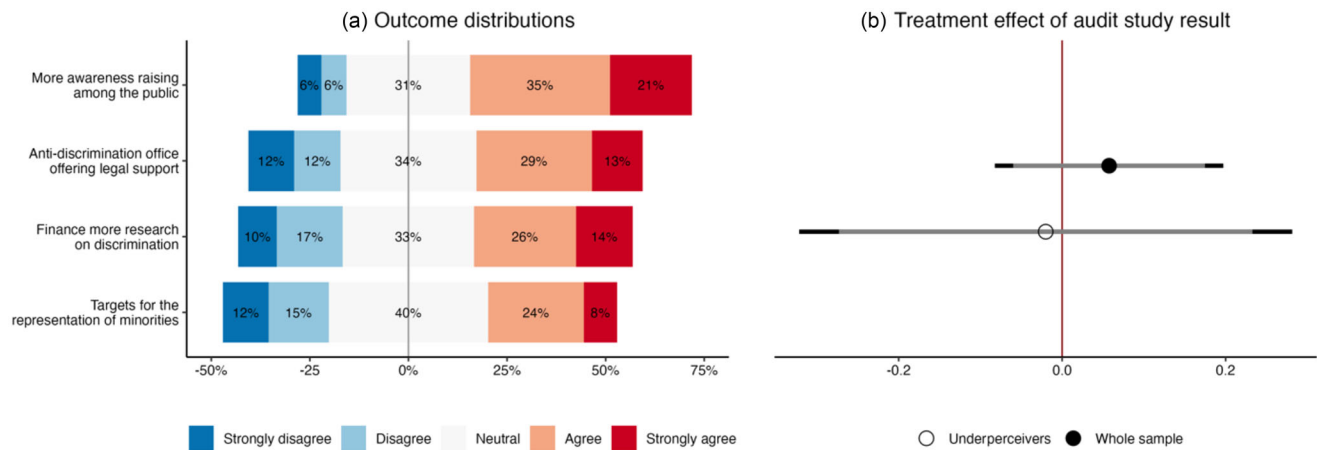


FIGURE 8 Effect of audit study result on support for further equal-treatment policies. *Note:* Post-stratification-weighted outcomes (panel a). Ordinary-least-squares estimates with 90% and 95% confidence intervals. Effect sizes in standard deviations of the control group (panel b). Full regression tables on Supporting Information pages 10–11.

and, with only two exceptions, the effect sizes for those who underperceived the extent of discrimination are smaller than for the overall sample and in several instances point in the opposite direction than the one expected. The SI shows that this finding is not driven by any particular one of the 12 persons who conveyed the framing messages to the participants; the conclusion holds despite idiosyncratic personal differences between the credible sources in terms of gender, skin color, age, or attractiveness (SI page 18).

Priming

Do the above null-findings result from the initial elicitation of misperceptions, which asks participants to carefully consider the extent of ethno-racial discrimination indicated by a field-experimental audit and thereby potentially raise recognition of the problem of discrimination and support for policies already in

the control group? Our study suggests that there is indeed a concealed priming effect, a concern that is often overlooked in information-treatment experiments. However, due to our novel use of two control groups, we can effectively distinguish between priming and information treatment. Figure 11 shows results in the same style as Figure 10 but with an important difference: Now “Control 0” respondents, who were never informed about field-experimental audits nor asked to consider their result, are the control group. This implies that we cannot estimate treatment effects among underperceivers because this information is not known for “Control 0” respondents. Figure 11 displays five treatment effects for each outcome: The mere priming effect of the elicitation, the combined priming and information effect, and finally the combined effects of priming, information, and framing by a researcher, lawyer, or potentially affected person.

Apart from donations, our study indicates no evidence of a pure priming effect and minimal evidence

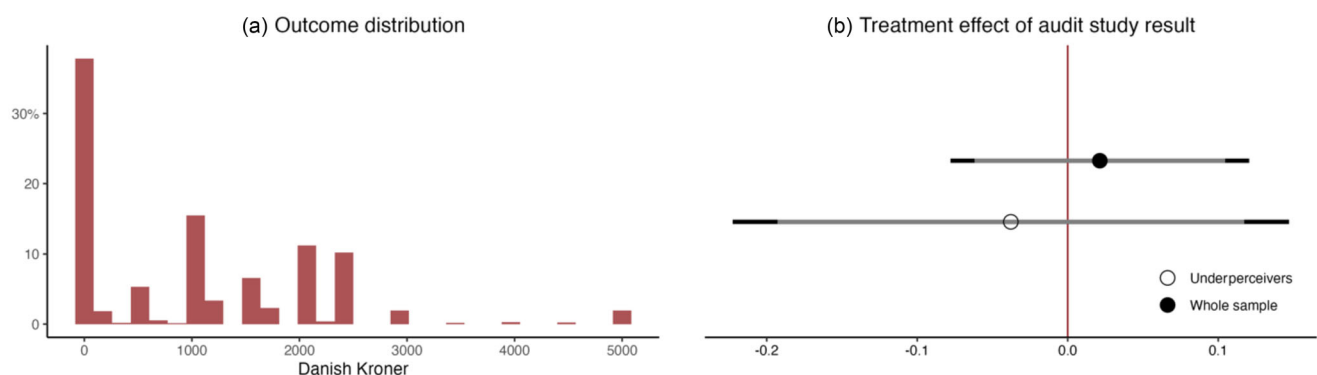


FIGURE 9 Effect of audit study result on donation to Immigrant Women's Center. *Note:* Post-stratification-weighted outcomes (panel a). Ordinary-least-squares estimates with 90% and 95% confidence intervals. Effect sizes in standard deviations of the control group (panel b). Full regression tables on Supporting Information pages 10–11.

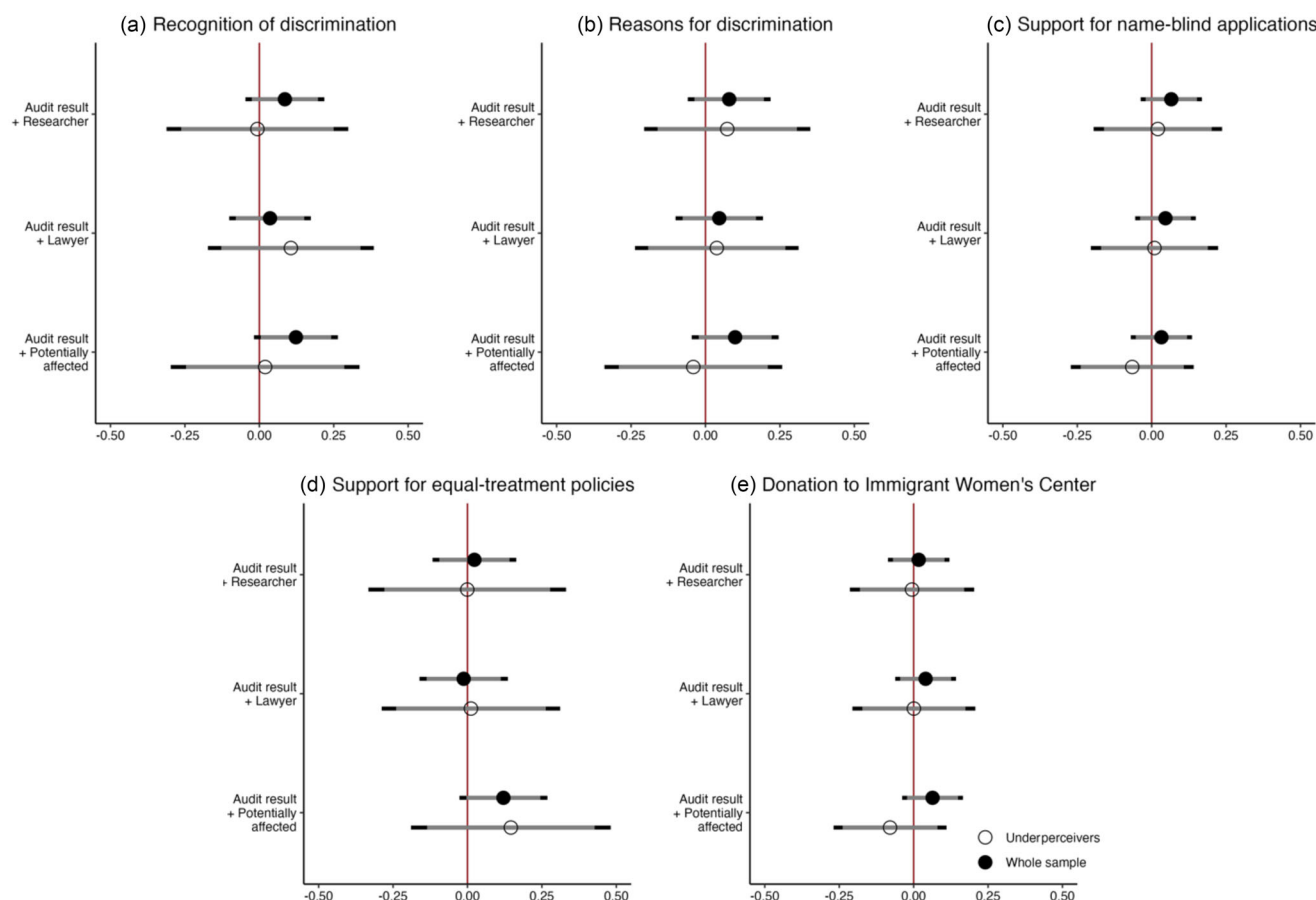


FIGURE 10 Effect of framed information. *Note:* Post-stratification-weighted outcomes (panel a). Ordinary-least-squares estimates with 90% and 95% confidence intervals. Effect sizes in standard deviations of the control group (panel b). Full regression tables on Supporting Information pages 10–11.

of a combined priming and information effect. The exception is a single significant effect on recognizing the negative consequences of discrimination. However, there is some indication that the combination of priming, information, and framing can increase recognition of discrimination as a significant social issue, especially if presented as scientifically credible by a researcher or emotionally impactful by a potentially affected person. However, even the combination of priming, information, and framing treatments does not suffice to significantly change people's views on the reasons for ethno-racial discrimination, their lack of support for name-blind applications, or their support for other equal-treatment policies. There is one exception suggesting that the combined three treatments do increase people's support for other equal-treatment policies if framed by a potentially affected person. We remain cautious about this singular finding, as it is not reflected in a similar tendency for support for name-blind applications.

One outcome stands out for which we find a clear priming effect with no additional importance of information and framing. When respondents were

asked to carefully consider the extent of discrimination, they significantly increased their donations to a minority-run support group. The average priming effect is around 200 Danish Kroner (\$30), and this outcome is usually considered a valid behavioral measure by many scholars. However, the overall pattern of findings leads us to interpret the result differently, suggesting that it may be easier for individuals to donate to express pity than to support fundamental changes that may undercut their group's privileges, especially given the slim chances of winning the raft (for different effects of hypothetical and consequential incentives on costly behaviors, see Liu & Tian, 2021).

Persistence of treatment effects

Do any of these effects persist over time? Our study suggests that they do not. A randomly selected subset of respondents from three experimental groups were asked a subset of the outcome questions 1–3 weeks after the treatments as part of another survey

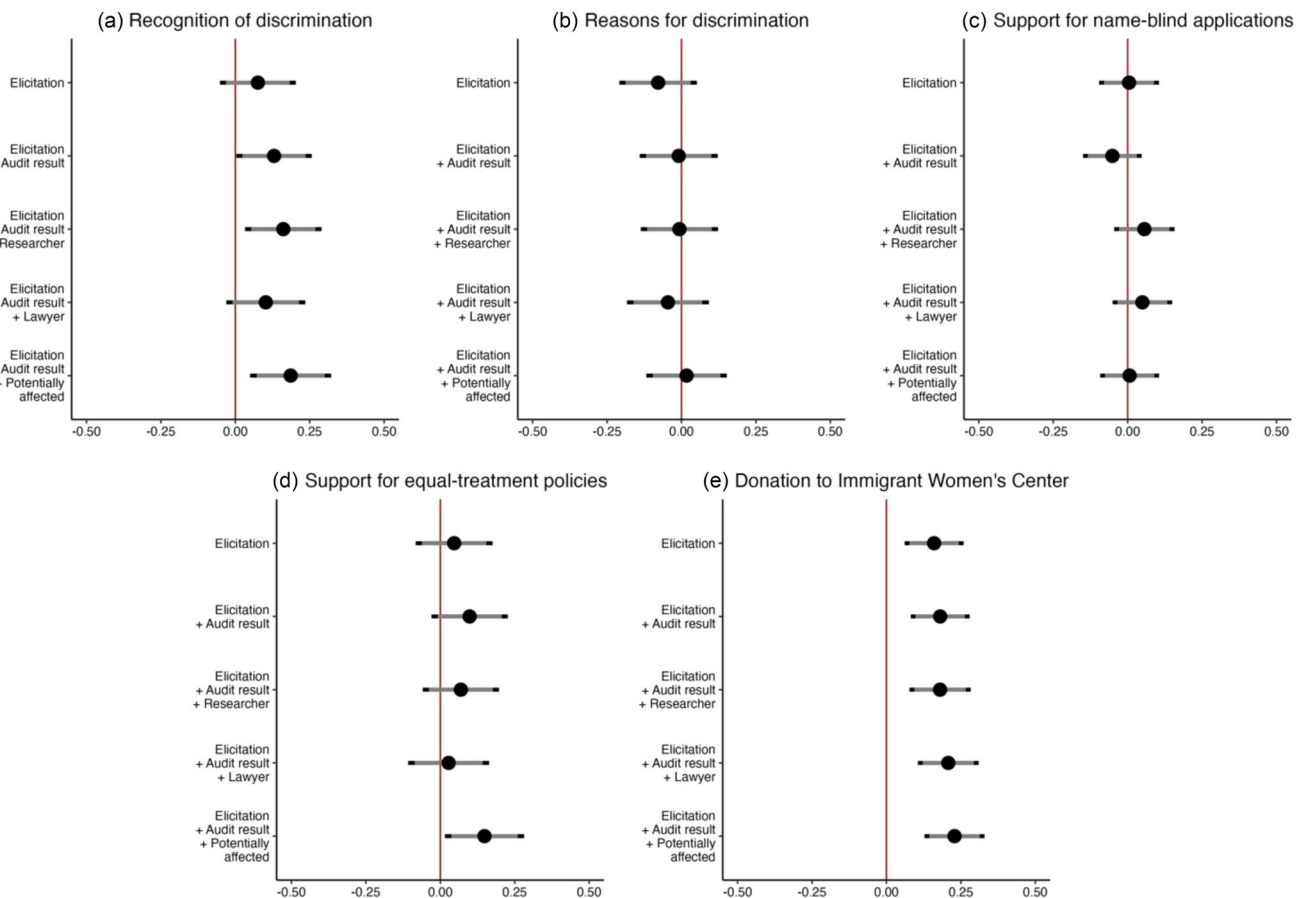


FIGURE 11 Priming effect of eliciting misperceptions of the extent of discrimination. *Note:* Post-stratification-weighted outcomes (panel a). Ordinary-least-squares estimates with 90% and 95% confidence intervals. Effect sizes in standard deviations of the control group (panel b). Full regression tables on Supporting Information page 12.

that was presented unobtrusively as a survey about “current political issues, especially climate change.” As illustrated in Figure 12, we find no evidence that our interventions had any enduring effects on the recognition of discrimination as a social problem or support for equal-treatment policies.

Exploratory robustness analyses: Backlash and effect heterogeneity

Finally, we aim to address two concerns that may arise regarding our findings. First, it is important to note that correcting citizens’ misperceptions is not

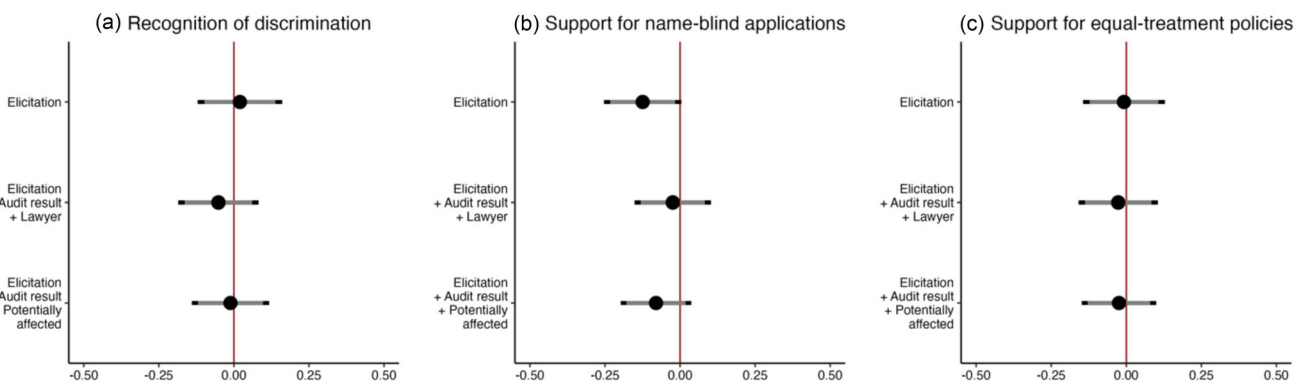


FIGURE 12 Priming, information, and framing effects after one to three weeks. *Note:* Post-stratification-weighted outcomes (panel a). Ordinary-least-squares estimates with 90% and 95% confidence intervals. Effect sizes in standard deviations of the control group (panel b). Full regression tables on Supporting Information pages 10–13.

a neutral operation; people often react negatively to persuasion and correction (Hopkins et al., 2019; Kalla & Broockman, 2020, 2023). As a result, our intervention may impact the outcomes through two opposing paths. On the one hand, it provides information, influencing respondents' misperceptions. On the other hand, it may result in backlash as a response to our attempt to persuade them, despite no indication of politically motivated bias in native-majority Danes' misperceptions. To address this concern, we employed two strategies through exploratory robustness tests, which reaffirm the null results discussed earlier (see SI pages 19–20).

Our first approach aims to mitigate the potential backlash by utilizing an instrument-variable procedure. Specifically, we estimate the effect of the updated misperception, measured at the end of the survey (see Step 6 in Figure 1), on all outcomes, including those assessed at least a week later. This variable should only reflect the updated misperception thanks to our initial correction (Step 2) without any backlash tendency. However, as the updated misperception is an observational variable, we instrument it using the randomized correction treatment, which allows us to separate the exogenous portion of updated beliefs (Peyton et al., 2022; Settele, 2022). Crucially, this strategy assumes the experimental treatments influence outcomes solely through their updating of misperceptions. The SI provides a discussion of this exclusion restriction's plausibility (SI page 20).

Our second strategy seeks to identify the potential backlash effect by employing a regression discontinuity design. We leverage the fact that the initially measured misperception is a continuous variable that exhibits a notable qualitative difference at the point of the return rate for applications with a Muslim-sounding name. Respondents who provided values below this threshold were corrected upwards, while those who provided values above it were corrected downwards. At this discontinuity, all respondents received the same factual information, implying that the correction had little effect on their updated beliefs. However, some were corrected to recognize a greater extent of the problem, which could arguably generate a stronger backlash among native-majority citizens compared to the reassurance that the problem is smaller than initially feared.

Second, one might question whether the treatment effects presented vary with sociopolitical factors. To address this concern, we employ machine-learning techniques to detect heterogeneous treatment effects, as proposed by Chernozhukov et al. (2022). In these exploratory analyses, we combine our two control groups and all treatment groups into a binary treatment indicator. As potential factors influencing the treatment effect, we examine four pretreatment mod-

erators (homophobia, xenophobia, support for redistribution, and frequency of news media consumption), the field experiment the respondents were introduced to, and all sociodemographic control variables (age, gender, household size, education, employment status, and region). The results indicate no significant differences between respondents who exhibited the most positive and the most negative responses to the treatment for all our outcomes (SI page 17). This reveals the limitations of using field-experimental audits to raise awareness not only on average but across all relevant sociopolitical groups.

CONCLUSION

We have studied the effectiveness of awareness-raising initiatives that are based on field-experimental audits. Our objective was to investigate whether the methodological strengths of these experiments, which have contributed to their widespread adoption across the social sciences, can effectively raise native-majority citizens' recognition of ethno-racial discrimination as a significant social problem and their support for equal-treatment policies, especially if combined with political framing strategies. Although audit studies, initially developed by activists and civic organizations to promote intergroup relations, have been instrumental in documenting discrimination and influencing policy, we recognized that native-majority citizens, lacking specialized knowledge, might not readily interpret lower response rates for fictitious applications or requests from minority members as evidence of illegitimate and harmful discriminatory practices. Therefore, our study considered the possibility that field-experimental audits require framing by an independent scientist to establish credibility, a lawyer to highlight the evidence as a violation of the law, or a potentially affected minority member to emphasize the personal distress caused by such discrimination.

Our findings demonstrate that informing native-majority citizens about audit evidence has a positive impact on their understanding of the extent of discrimination indicated through field-experimental audits. Their perceptions converge and become more accurate, allowing them to develop realistic expectations about discrimination in various social fields beyond the specific one they were informed about. However, contrary to a classic assumption in the social sciences, significant improvements in perceptions of the extent of discrimination do not lead to a corresponding change in the recognition of the problem or support for equal-treatment policies, even among those who initially underestimated the extent of discrimination, and regardless of whether the evidence

was presented with different frames from researchers, lawyers, or potentially affected persons.

Our study's findings both mirror and extend the research conducted by Haaland and Roth (2023) in a different context, focusing on a different minority group, spanning across various social fields including political representation and equal access to public services, and encompassing a native-majority population without any indication of politically motivated bias in their misperceptions of discrimination. Moreover, we carefully considered the effects of framing by credible sources and accounted for priming effects. The similarities in findings despite these profound differences and extensions suggest that the patterns observed in both studies have broader applicability and may be more universal across minorities facing fundamentally different stereotypes and underlying mechanisms of exclusion than previously believed. Related research indicates that raising awareness of discrimination may not translate into increased demands for anti-discrimination policies, even among immigrants and their descendants (Schaeffer & Kas 2024). That said, further research is needed to determine whether the reasons and mechanisms underlying citizens' lack of response to evidence of ethno-racial discrimination differ between contexts and targeted minorities.

The findings from our analysis offer several insights into why audit-based awareness raising may fail to raise recognition of the issue of discrimination and support for equal-treatment policies. The primary reason seems to be that most native-majority citizens are already aware of the prevalence of gatekeeper discrimination as evidenced through field-experimental audits and in fact tend to overestimate its prevalence; although they may still underestimate more covert forms of systemic discrimination and the enduring legacies of past discrimination.

But even among those initially underestimating the extent of ethno-racial discrimination and having their perceptions corrected as intended, recognition of the issue and policy preferences do not change, indicating the influence of alternative elements. Some of our descriptive results contain a hint. The most widely supported reason for discrimination against individuals with a Muslim-sounding name is the belief that it arises from a clash between non-Western and Danish norms and values. Additionally, approximately half of the native-majority population believes that discrimination is due to people having made negative experiences with Muslims in the past. This suggests that because field-experimental audits do not address perceived legitimacy and root causes of discrimination, their evidence may fail to impact support for equal treatment policies. Countering flawed stereotypes, such as those surrounding immigrants' unemployment or crime rates (Grigori-

eff et al., 2020), or employing narrative strategies that encourage perspective-taking (Kalla & Broockman, 2020; Kalla & Broockman, 2023) might prove more impactful. However, the latter may not always align with the expectation that researchers should prioritize research-based evidence, which is why we explored whether diverse framing and source strategies, involving potentially affected minorities, can enhance the persuasiveness of research-based evidence.

A final reason echoes a classic argument. Our novel use of two experimental control groups enabled us to discover that the mere elicitation of beliefs about discrimination, presumably serving as a reminder of a known and embarrassing issue, leads to increased donations (of a considerable but unlikely win from a raft) to a minority-run support group. This resonates with Blumer's (1958) classic formulation of group threat theory, suggesting that members of the dominant majority may donate to recognize minority hardship but are unlikely to endorse fundamental changes that undermine their privileged position.

On the surface, all of this leads to the sobering realization that the diligent efforts of social scientists to identify and describe the extent of discrimination using field-experimental audits do not directly translate into raising public awareness of the issue. However, it is important to note that this study focuses on the immediate effects on citizens and does not capture the intricate, delayed, and indirect processes through which social science research influences public perceptions of social problems. Field-experimental audits are repeatedly covered by news media and discussed in podcasts, shared on social media platforms, and become part of public discourse through politicians and opinion leaders. As an example, in the spring of 2023, a reform commission, aware of the Danish field-experimental audits, recommended to the Danish government the implementation of name-blind applications and the introduction of quotas to ensure that at least one applicant of immigrant origin is invited for interviews when a position is advertised in the public sector (Lindbjerg Greve, 2023). This example serves as a reminder that field-experimental audits can have a meaningful impact without necessarily changing public opinion. There are occasions when governments encounter challenging situations where they pursue the improvement of minority rights, regardless of, or even in opposition to, public opinion. In such circumstances, field-experimental evidence can help to legitimize these efforts.

ACKNOWLEDGMENTS

Merlin Schaeffer acknowledges funding from the Carlsberg Foundation for the project "Misperceptions about Ethnic Discrimination and Support for Equal Treatment Policies" (ID: CF21-0079). Parts of this

manuscript were presented at seminars at Sciences Po Paris, VIVE the Danish Center for Social Science Research, Heidelberg University, and the University of Copenhagen. We thank the participants for their useful feedback. We especially thank the editors of *AJPS*, three anonymous reviewers, Hanno Hilbig, Marco Giani, Nina Magdeburg, Joscha Legewie, Nicholas Sambanis, and Yasmin Mohamed Hassan for their helpful comments and suggestions. We are particularly grateful to Kristina Bakkær Simonsen, Martin Vinæs Larsen, Mathilde Cecchini, Jakob Majlund Holm, Tine Birkelund Thomsen, Nikolaj Nielsen, Tarek Hussein, Nanna Margrethe Krusaa, Loubna Mekhchoun, Mohamed Hassan Musse, Yasmin Mohamed Hassan, and Mohammed Albannay for framing field-experimental results for the study participants.

Open access funding enabled and organized by Projekt DEAL.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Schaeffer, Merlin, Krakowski Krakowski, and Asmus Leth Olsen. 2024. "Correcting misperceptions about ethno-racial discrimination: The limits of evidence-based awareness raising to promote support for equal-treatment policies." *American Journal of Political Science* 1–22. <https://doi.org/10.1111/ajps.12933>