

The Unequal Distribution of Opportunity: A National Audit Study of Bureaucratic Discrimination in Primary School Access

Forthcoming in *American Journal of Political Science*.

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Acknowledgements: Asmus Leth Olsen acknowledges funding from the Independent Research Fund Denmark for the project ‘The Behavioral Citizen as a Layman Public Administration Scientist’ (ID: 8046-00034A). Invaluable comments on the manuscript have been provided by: Anders Berg-Sørensen, Jens Olav Dahlgaard, and Martin Vinæs Larsen. Excellent research assistance came from Cecilie Weischer and Anders Woller Nielsen.

Abstract: Administrators can use their discretion to discriminate in the provision of public services via two mechanisms. They make decisions to allocate public services, allowing them to discriminate via allocative exclusion. They can also discriminate by targeting administrative burdens towards out-groups to make bureaucratic processes more onerous. While prior audit studies only examine the use of administrative burdens, we offer evidence of both mechanisms. We sent a request to all Danish primary schools (n=1,698) from an in-group (a typical Danish name) and out-group (a Muslim name) parent asking if it was possible to move his child to the school. While both groups received similar response rates, we find large differences in discrimination via allocative exclusion: Danes received a clear acceptance 25% of the time, compared to 15% for Muslims. Muslims also faced greater administrative burdens in the form of additional questions.

Replication Materials: The data, code, and any additional materials required to replicate all 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/BFEBHQ>.

Word count: 9,971

Public officials are called upon to follow certain normative and legal expectations: equal treatment before the law, equal access to public services. Such values animate the concept of a Weberian bureaucracy, inform public personnel rules, and fit with basic notions of fairness. Whether bureaucrats meet such ideals in practice is another question. The discretion inherent in their jobs gives them the opportunity to discriminate across groups. Understanding how such discretion is used is central to understanding whether the state offers equal treatment in practice.

The ability of policymakers to understand if and to what degree discrimination is taking place has been aided by the growth of audit studies over the last fifteen years (Bertrand and Mullainathan 2004; Pager 2003, see Gaddis 2018 for a history of the use of the technique). This tool is increasingly used to document discriminatory behavior by public officials (Costa, 2017) but empirical tests typically involve relatively low-cost commitments on the part of the public actor – providing information about a service – and are often measured only in terms of whether the individual receives a response or not, or the tone and informational value of the response (Hemker and Rink 2017). Such responses are assumed to influence, but do not directly measure, access to public services. By contrast, we employ an audit study where bureaucrats are asked to make a real and costly commitment. In doing so, we draw attention to the different mechanisms by which bureaucrats act as the *de facto* gatekeepers of critical public services.

We examine access to primary education, which matters profoundly for later-life outcomes (Chetty, Friedman, and Rockoff 2014; Heckman 2008). Education is also central to cultivating the civic skills needed for citizenship (Mettler 2002). In our context of Denmark, minority families who experience a more welcoming educational setting become more integrated into the political process (Hjortskov, Andersen, and Jakobsen 2018). But the promise of education as a force for economic and civic inclusion is undercut if it is experienced as less accessible by immigrant communities. As in other Scandinavian welfare states, students of non-western background lag natives in educational performance (Danish Statistics 2018; Greve and Krassel 2015, 5), but we know little about whether they have also experienced discrimination in access to schools (Andersen and Guul 2019).

To advance the theoretical understanding of discrimination in public services, we distinguish between two administrative mechanisms by which bureaucrats may use their discretion: allocative exclusion and administrative burdens. One role that bureaucrats play in budget and policy implementation processes is to allocate how public resources are deployed. In doing so, they can help or hurt different groups. We test if officials engage in discrimination via allocative exclusion by offering school places to families with a Muslim name less frequently than to their Danish peers. We therefore examine actual decisions to allocate public resources, not just responses to requests for information. To our knowledge, no audit study of public sector discrimination has taken this approach.

Second, bureaucrats can apply more indirect forms of discrimination, by imposing administrative burdens differentially across groups (Herd and Moynihan 2018). Administrators might decline to share information with, be less welcoming toward, or demand more documentation from out-groups (Heinrich 2018; Jilke, Van Dooren and Rhys 2018). The applicant might not receive a direct rejection, and still participate in the bureaucratic process, but under less favorable circumstances. We examine if bureaucrats impose greater compliance and psychological costs on Muslim families.

The setting allows us to consider how a pattern of shifting demographics and an influx of migrants into Europe might result in patterns of discrimination in public services. Muslims in Europe often play a double out-group role, differentiated both in religion and ethnicity from predominantly Christian natives (Bloom, Arikan, and Courtemanche 2015). The risk of anti-Muslim bias has been exacerbated by the refugee crisis, which has encouraged anti-immigrant politics (Czymara 2019; Hangartner et al. 2019). Exposure to non-western immigrants affects both trust and political attitudes on crime among native Danes (Dinesen and Sønderskov 2015; Hjorth 2020). Danes do not appear to be unusual relative to the rest of Europe – in surveys they are as or more welcoming to refugees and immigrants as residents of other European countries, and less concerned with religious differences (Bansak, Hainmueller, and Hangartner 2016; Heath and Richards 2019).

While there is strong evidence that perceived out-group threats shape mass public opinion (Hainmueller and Hiscox 2010), our analysis offers evidence of how bureaucratic gatekeepers respond when

providing public services to Muslims. The limited existing pool of audit studies on the experience of Muslims in Europe suggests little discrimination in terms of responses, though with more mixed evidence on the tone and quality of information (Adman and Jansson 2017; Grohs, Adam and Knill; Hemker and Rink 2017). Is bureaucratic behavior exempt from the negative attitudes toward Muslims described above?

We answer these questions using a national field experiment where putative Muslim and Danish families request to transfer their child to a local school ($n=1,698$). The majority of audit study research in public sector settings has focused on political actors (Costa 2017) rather than bureaucrats, and the evidence on bureaucratic behavior is more mixed (e.g., Lowande and Proctor 2020). Furthermore, more than 80% of audit studies designed to assess discrimination by public officials come from a US setting (Costa 2017), thereby reflecting both the particular contours of American racial tensions and issues such as voting rights (White, Nathan and Faller 2015). While the prevalence of bureaucratic discrimination may be less well-established in other settings, it is no less important. The large differences in responses we find – 25% of those with Danish names were offered a position, compared to 15% of those with Muslim names – offers unambiguous evidence of discrimination via allocative exclusion. We also find that Danish bureaucrats discriminate in how they impose administrative burdens, seeking more information and offering a less welcoming tone to Muslims.

Mechanisms of Bureaucratic Discrimination: Allocative Exclusion and Administrative Burden

Individuals use observable cues – such as race, gender, or religion – to evaluate others, sorting them into pre-defined groups that hold positive and negative associations (Bodenhausen 1990). People tend to view those who share their own group characteristics more positively, and are more suspicious of out-group members (Williams and Reilly 1998). Negative beliefs of out-group members may be implicit, leading to unconscious bias. More explicit, or taste-based, discrimination reflects a deep-felt animus that is hard to displace (Becker 1971). Another possibility is statistical discrimination (Arrow 1972), where individuals view members of a group as having, on average, some characteristic that poses a risk or cost to them.

Without more specific information about the individual, group membership becomes a cue used to discriminate.

While prior audit studies have sought to examine the motivations behind discrimination, there is no single overarching theory. Much of the empirical research focuses on private rental and labor market discrimination (see, Flage, 2018 and Zschrnt and Ruedin 2016, respectively), rather than our topic of bureaucratic discrimination. To structure our analysis and propositions, we distinguish between two mechanisms of bureaucratic discrimination. Bureaucrats are constrained in their ability to discriminate by law, but retain discretion to make decisions on how to allocate resources, convey if the applicant is welcome or not, or to make the process of engaging with the state more or less onerous. The use of such discretion has been examined in the context of street-level bureaucrats (Maynard-Moody and Musheno 2003), but, as we show in our analysis, is also available to the type of back-office administrators we examine here. Such actors might have limited daily interaction with the public but still respond to their petitions and determine specific cases (Soss, Fording and Schram 2011). Being one step removed from the public, such patterns of bureaucratic discrimination are less easy for clients to observe. Regardless of whether the bureaucrat is at the frontline or back-office, the risk for clients of public services is that valued rights and benefits may be denied through the type of allocative decisions and administrative burdens we describe here (see also Heinrich 2018; Herd and Moynihan 2018; Jilke Van Dooren and Rys 2018).

For our context, we define discrimination via allocative exclusion as a discretionary administrative choice to allocate public resources differentially across groups. For example, some groups may be systematically more likely to be told by bureaucrats that spaces in a job-training program, school or public housing are unavailable. The consequence of discrimination via allocative exclusion is to stop the process for an individual, systematically denying members of discriminated groups a fair opportunity to access public services. While it is difficult to determine if any single decision is driven by discrimination, aggregate differences in allocation of resources across groups with otherwise identical rights to a service can be taken as evidence of discrimination.

Administrators may also discriminate in how they apply administrative burdens, i.e. learning, compliance and psychological costs (Herd and Moynihan 2018). Learning costs are the time and effort expended to learn about a program or service, ascertain eligibility status, the nature of benefits, conditions to be satisfied and how to gain access. Compliance costs are costs incurred as an individual provides information and documentation to demonstrate standing. Bureaucratic demands for such information can make the process of seeking rights and benefits more onerous, especially if targeted toward out-groups. Psychological costs include stigma and alienation from participating in unpopular programs, diminished autonomy if experiencing disempowering processes, as well as frustrations and stresses that arise from interacting with the state.

Prior audit studies have demonstrated evidence of discrimination by examining how public officials mitigate learning costs in their role as information providers. The most common outcome studied in this literature is responses to requests for information (Costa 2017). Hemler and Rink (2017) rightfully point out that there are other dimensions to bureaucratic discrimination. The quality of bureaucratic responses matters, since unhelpful information also increases learning costs (White, Nathan, and Faller 2015). Psychological costs have also been addressed, usually in terms of the friendliness of tone, or using the sender's name in response letters, which is assumed to convey a sense of welcome.

Applying our framework to prior audit studies of bureaucratic discrimination (see Supporting Information, SI Table A1, p2) highlights that prior studies have not examined discrimination via allocative exclusion, and that only some forms of discrimination via administrative burdens have been studied. Absent from prior work is a consideration of compliance costs, where bureaucrats make access to public services contingent on satisfying requests for information to document the person's eligibility, based on formal and sometimes informal criteria. While the ability to impose such costs are central to street-level bureaucracy accounts of discrimination (Lipsky 1980), they are less visible in audit studies of the same topic.

Audit Studies of Bureaucratic Discrimination

The first published audit studies of discrimination by public officials (Butler and Broockman, 2011) and bureaucrats (White, Nathan, and Faller 2015) are relatively recent. By 2017, Costa reviewed 41 such studies of public officials. In this section, we focus only on studies of bureaucratic discrimination akin to our theoretical sample of interest, rather than examples from the private sector, or involving politicians or political appointees.

Some work set in the US examines discrimination in voting processes, following Butler and Broockman (2011). Local election clerks were less likely to respond to Latino constituent questions about voting (White, Nathan, and Faller, 2015). The quality of the responses was also lower for Latinos relative to whites, less likely to convey accurate information. A study of local election clerks in Wisconsin found that senders who revealed their partisan identity were more likely to receive a response, with the result being driven by greater responsiveness to Republican constituents (Porter and Rogowski 2018).

Another US study compared the responses of putatively black and white requests for information, such as opening hours and enrollment forms, about common local public services from librarians, treasurers, sheriffs, school officials, job centers and county clerks (Giulietti, Tonin, and Vlassoupoulos 2017). Requests from black constituents were less likely to receive a response. Blacks were also less likely to receive a friendly response. The quality of the response, measured as length, was no different across races.

Other studies show less evidence of bureaucratic discrimination. In the context of US public housing, Einstein and Glick (2017) do not find racial differences in responses by officials to requests for aid. However, Hispanics were about 20 percentage points less likely to be saluted by name. Carnes and Holbein (2019) examine if public officials are more apt to vary their responses based on the socioeconomic status of constituents. They find that school principals provide the same rate of response to parents seeking information about school music and art programs, regardless of whether the respondent described financial hardships or not.

Prior work in a variety of settings provides limited and mixed evidence on whether Muslims face discrimination. In an audit study of Chinese local government, requests for information from the mayor's office about a welfare program for the unemployed (with the expectation that the response came from a

lower-level bureaucrat) resulted in response rates that were about one-third lower for ethnically Muslim names (Distelhorst and Hou 2017). The length of responses was also approximately half as long for Muslim respondents. It is generally difficult to separate out bias against Middle-Eastern names from a more general anti-Muslim bias. Pfaff et al. (2020) find that putative Muslim parents with non-Arabic names still faced discrimination, being about 5 percentage points less likely to receive a response from US school principals.

Other research does not detect marked anti-Muslim bias in responses, but find some differences in the content of the responses. Local Swedish public officials did not discriminate between Arabic and Swedish names in terms of response rates, but offered more informal and friendly responses to native Swedes (Adman and Jansson 2017). Grohs, Adam and Knill (2016) find no general pattern of discrimination between German and Turkish names among local German officials in terms of speed of response, and service orientation, which includes not just friendliness, but also provision of additional helpful information. Turkish respondents did receive less complete responses, though the size of the differences were not large. Another audit study in Germany offered somewhat different results: Hemker and Rink (2017) find no difference in response rates for requests for information about cash benefits, but find that Romanian and Turkish names receive markedly lower-quality responses than natives, with the content of the responses providing less accurate information that made access to benefits appear more onerous.

A study of Belgian elder-care facilities found that Flemish and Mahgrebian names (from North Africa and typically Muslim) feature similar rates of responses, quality of information, and tone (Jilke, Van Dooren, and Rhys 2018). However, private facilities provided less enrollment information and less comprehensive information to Mahgrebian names relative to public providers. Such a pattern may reflect statistical discrimination observed elsewhere in private provision of public services. For example, Bergman and McFarlin (2018) find, that schools are generally less likely to respond to putative students signaling poor behavior or low achievement, but only charter schools discriminated against disabled students, and this discrimination was not present in states that compensated for the costs of disabled.

Overall, audit studies of bureaucratic discrimination offer a mixed picture. There is certainly evidence of discrimination in some studies, but not in others. This mixed picture holds true for the treatment

of populations of Middle-Eastern, Turkish or Arabic names that bureaucrats might suppose to be Muslims. There is, therefore, compelling reason to more deeply explore the potential for bureaucratic discrimination in general, and toward this group in particular.

Analytical Approach

Rather than replicate the approach of prior audit studies, we examine previously unaddressed mechanisms of discrimination. Across prior work, bureaucratic interactions are presented as one where the individual seeks information from the state: what do I need to vote? Where can I find a library? Can you provide information about housing? The most significant request placed upon the administrator is for a meeting with a school official (Pfaff et al. 2020). While provision of information is an important bureaucratic role, bureaucrats also make decisions about the allocation of resources, and are themselves the demanders of information. Our analytical approach attempts to capture these two distinct roles.

Our primary dependent variable is an actual administrative decision on the allocation of public resources, one that directly captures discrimination in provision of access to public services. This is distinct from providing information because, as Giulietti, Tonin, and Vlassopoulos (2017, 3) note, “(f)ailing to provide information about a service is not equivalent to denying access to a service.” We also examine how administrative responses distribute compliance costs to different groups.

The use of the audit studies to examine discrimination involves analytical choices depending on the goal of the project. Some designs focus on identifying the underlying motivations for discrimination by, for example, trying to avoid confounding of socioeconomic status (SES) of the different names being provided to ensure any differences reflect racial or ethnic discrimination rather than class discrimination (e.g. Jilke, Van Dooren and Rhys 2018). Here, we prioritize documenting the existence and scale of discrimination faced by representative members of a population of interest via actual bureaucratic decisions, while also testing if characteristics of that group moderate patterns of discrimination.

Our primary experimental treatment is a typical Muslim or native Danish name to capture patterns of discrimination for students seeking to gain entry to a primary school. We include a signal about how diligent

the student is to examine if this moderates how Muslims are treated. Public officials may use statistical discrimination as a tool to protect organizational performance in light of demands for public services that exceed available resources. School bureaucrats may be especially sensitive to the possibility that students are seeking a transfer because of an inability to succeed in another school (Bergman and McFarlin 2018). Danish teachers are conscious of the costs of non-native Danish students (Andersen and Guul 2019). The school administrators we survey have been also shown to engage in labor-market discrimination against equally qualified Muslim candidates, although this pattern appears to be strongest amongst lower-performing schools, suggesting taste-based rather than statistical discrimination (Guul, Villadsen, and Wulff 2018). If diligent Muslim students face less discrimination, the results are consistent with patterns of statistical discrimination. If such students receive the same level of discrimination regardless of performance, the results are more consistent with taste-based discrimination.¹

Empirical Setting

As in other European countries, the growing Muslim population in Denmark has experienced unequal outcomes. Non-Western immigrants and their descendants (primarily from Turkey, Lebanon, Iraq, and most recently Syria) now comprise 8.5% of the Danish population, a significant change over the last 30 years (Statistics Denmark 2018a), and consistent with demographic changes throughout Europe. At the same time, public employment remains dominated by native Danes. Even as the non-western student population constitutes 10% of all students (and rising), only 2.4% of the teachers have a non-western background (Olesen 2016). Middle-Eastern applicants face higher discrimination in Danish labor markets (Dahl and Krog 2018), including when applying for jobs as teachers in schools (Guul, Villadsen, and Wulff 2018).

¹ The study was pre-registered: <http://egap.org/registration-details/2381>. We diverge from the pre-registration to limit our focus only to the two variables that were subject to experimental manipulation and causal inference rather than those conditional on post-treatment responses. However, our core interest is aligned with the pre-registration: whether the email receives a positive or negative response.

Muslims are also more likely to be sanctioned by Danish employment agency bureaucrats (Pedersen, Stritch, and Thuesen 2018) and do less well in terms of educational outcomes (Danish Statistics 2018; Greve and Krassel 2015, 2). In the empirical setting we study, there is a strong negative relationship between share of non-western students at the school and the school's academic performance (see SI Table B1 and Figure B1, p3).

All Danish children are mandated to receive 10 years of education, starting from the year the child turns six, which can be fulfilled by attending a free publicly-funded municipal primary school, a private school or by home schooling. Some 700,000 students are in primary education, with 86% attending a public school, and the rest in private schools. Public primary schools are funded and governed by municipalities within a formal framework from the Ministry of Education. Private schools are non-profit institutions that are required to meet certain standards in terms of curricula and teaching. While private, they receive a public subsidy based on the average cost per pupil and charge only minor tuition fees. Private schools cover a diverse set of institutions, spanning everything from religious to socially progressive schools (Danish Ministry of Higher Education and Science 2016).

Children are allocated to a school based on where they live, but parents may seek to move their child to a different school if a place is available. Every year 30,000 to 40,000 students transfer between schools.² Public school transfers occur via two different processes. Either the municipal school administration initiates the transfer, or the parents and the receiving school agree to a transfer (Ministry of Education 2019). Parents tend to transfer their children because they have moved, or because they are unhappy with the conditions or fit at their child's current school (Epinion 2017). Conditions for the school transfer and the number of places available at a school is decided by the local municipal council.

Private primary schools enjoy greater autonomy to decide which students to accept or reject, and are not subjected to binding requirements of admitting pupils from a specific school district, unlike their public

² An exact number is difficult to provide as some number of transfers are due to certain schools not providing teaching above a certain grade which forces parents to find another school.

counterparts (Ministry of Education, 2019). Our robustness tests therefore separate the results by school type.

Experimental Design

The research design is an audit study with a 2x2 factorial design with experimental manipulation of the sender's name (Muslim/Danish) and information about the pupil's educational performance respectively. We include the entire primary school system in Denmark. The Danish Ministry of Education database provided information on primary schools, giving 1,922 results in total as of 26th October 2016. Once we excluded duplicate emails for schools and specialist schools that only serve 10th grade students 1,698 unique institutions remained. We use the publicly available email address a parent would use in seeking information from the school. Figure 1 provides the distribution of school size (i.e., number of enrolled students) and the share of non-native Danish students in each school. Panel A shows the school size, with a mean of around 400 students. In panel B, the average proportion of non-native Danish students is about 10%, with a long right tail with number of schools with large shares of non-native Danish students.

[FIGURE 1 ABOUT HERE]

Treatments

All schools received one of four versions of an email with the same core request: to transfer a third-grade boy to the school. Third-grade students are typically about nine years old. The identity of the sender was manipulated by both randomly assigning the sender email address with a clear indication of the name and assigning the name at the end of the emails. The sender has a typical Danish native name or typical Muslim name. For ease of reading, we use the term “Danish name” to represent a parent with a traditional Danish name. This does not mean, of course, that those with a “Muslim name” are not also Danish. Muslims in Europe are assigned overlapping identities – as immigrant, religious out-group, and often a member of a lower SES – that matter for how they are perceived and treated in reality. In selecting our names for

treatments, we therefore employ the most common first and last native Danish and Muslim male names: “Peter Nielsen” and “Mohammad Osman.”³ Our focus on understanding typical experiences of Muslims raises a legitimate concern that the names do not convey equal social status, which may be what drives discrimination.⁴ However, this concern is mitigated by evidence from previous research that sought to estimate the effects of SES versus ethnic/religious signals in labor market discrimination in Denmark. Dahl and Krog (2018) tested 16 of the most popular Middle-Eastern names compared to high and low native-Dane SES names, and concluded that “the differences between treatment groups are caused by the ethnic trait and are not associated with SES” (Dahl and Krog, 2018, 403). Furthermore, they also find no significant difference in callback for job applications for the most common Middle-Eastern names which suggest that possible differences in the religious connotations of the names are not driving the effect (Dahl and Krog, 2018). Specifically, the first name “Mohammad” that we use had a callback rate no different than the seven other minority names in their study (see SI Table C1, p4).

Both the parent and child are male in order to maximize our potential to identify discrimination as previous research in the Danish labor market finds that Muslim males face greater discrimination than

³ Osman is the second most common last name (the most common is another spelling of Mohammad - “Mohamed” - see Statistics Denmark 2018b). Specifically, 5177 men in Denmark have the name Mohammad and 1122 male and females have the name “Osman”. “Mohammad Osman” is not the most common combination of none-native Danish names but does occur (13 individuals have this exact name). However, given that existing research in a Danish context finds no meaningful difference in effects of different Middle Eastern names (Dahl and Krog, 2018), “Mohammad Osman” serves the purpose of providing a familiar and distinctively Muslim name.

⁴ In this, our approach is consistent with some of the critiques of audit studies that argue for a better understanding of SES associated with names, which is that relying on unusual minority names, e.g. DeShawn and Tanisha for African-Americans, may misstate aggregate patterns of discrimination (Gaddis 2017).

females (Dahl and Krog 2018). This also implies that whatever effects we may find will likely be upper bound estimates for the true discrimination observed in the full immigrant population in Denmark.

Information about the child's ability is manipulated by describing the reason for the transfer. In one condition, the school is also told that things are not going well at his current school as a reason for the transfer. In the "diligent" condition the father also mentions that his son's current teachers say he does well with his homework. As our main focus is on ethno-religious discrimination we only report findings on the effects of the child's ability in the robustness section.

Each of the four conditions were randomly assigned to an equal number of the 1,698 schools. As a result, 850 schools received a request from Muhammad Osman and 848 from Peter Nielsen. The treatments take the following form:

Email from:

**Danish treatment:* peter_nielsen@hjemme-email.dk*

**Muslim treatment:* mohammad_osman@hjemme-email.dk*

Hi [school name]. I am writing because we would like to move our son to another school.

**Non-diligent student treatment:* He is in 3rd grade and we don't think that it is going very well at this school.*

**Diligent student treatment:* He is in 3rd grade and the teachers say that he is doing well with his homework, but we still don't think that it is going very well at this school.*

We would like to know if he can change to your school. Do you have a place at your school? And can I get to know more about the school?

Kind regards

**Danish treatment:* Peter Nielsen

**Muslim treatment:* Mohammad Osman

Table 1 report descriptive statistics for background characteristics across the four treatment groups which shows good balance on various school characteristics.⁵

[TABLE 1 ABOUT HERE]

Ethical Considerations

⁵ Due to technical issue the emails where send out on two separate days. However, the number of emails sent on each of the two days is constant across the treatments.

Field experiments inevitably raise ethical issues. However, reviews of these concerns conclude that deception and involuntary participation is acceptable in cases where the research could not be undertaken without it, the topic addressed has substantive importance and where it imposes minimal harm on subjects (Zchirnt 2019). Researchers have an ethical obligation to maximize the benefits of the study while minimizing the potential harms. To maximize the benefits we chose the topic of school access based on evidence of the importance of schooling experiences for later life outcomes (Chetty, Friedman, and Rockoff 2014), and the dearth of evidence on discrimination in school access. Non-western immigrants perform poorly in Danish elementary schools, contributing to later life socio-economic disparities (Danish Statistics 2018a). Understanding potential barriers to school transfer for Muslim pupils in the Danish setting therefore has real policy importance. In other areas, the causal power of the audit studies technique has not just uncovered discrimination; it has shaped policy debates (Pager 2007). We have communicated the results with Danish school administrators and relevant stakeholders. Such evidence can reduce bias. For example, Alesina et al. (2018) find that exposing teachers to evidence that they hold stereotyped views led them to partially reduce a previously observed bias against immigrant students in grading.

To minimize risks we undertook a number of steps. First, we formulated a relatively straightforward request, requiring minimal effort on the part of the subject. Second, all responses to our request received a follow-up email within 24 hours to formally end the correspondence with the school. The follow-up made it clear that the sender was no longer interested in sending their child to the school. Importantly, 50% of the responding schools responded within two days and more than 95% responded within 6 days, minimizing the time the experiment was in the field. To protect respondents, individual responses are anonymized in data replication materials and the granularity of school-level data is reduced. It is important to note that it is impossible to say if any individual respondent is behaving in a discriminatory fashion since there may be legitimate reasons for exclusion, and thus they cannot face liability for their responses.

Dependent Variables

To give a sense of the nature of the responses, Table 2 provides an illustrative sample of ten actual responses, anonymized and translated into English.

[TABLE 2 ABOUT HERE]

Table 3 provides descriptive statistics for the dependent variables of the study across the four conditions. First, “Response” is the response rate. We do treat responses that are notifications of forwarding of the question or auto-reply as actual responses, leaving a true response rate of 80.3% or 1,364 email responses.⁶ This compares favorably to the highest response rates of previous bureaucratic audit studies, which range between 70 and 80% (Giulietti, Tonin, and Vlassoupoulos 2017; Grohs, Adam, and Knill 2016; Hemker and Rink 2017; Porter and Rogowski 2018; White, Nathan, and Faller 2015, Lowande and Proctor 2020), and is higher than the response rate of public officials generally (53%), and audit studies of school officials in particular (between 43 to 53%) (Bergman and McFarlin 2018; Costa 2017; Pfaff et al. 2020).

The next variables capture whether the responses indicate if the school has room for the student or not, broken into three categories. “Clear reject” denotes if it is clearly expressed that there is not a place at the school. Hedged responses are excluded from this category (e.g., “unfortunately there is not a place, but maybe there will be one next month”). This standard resulted in 25% receiving a clear rejection among received responses. However, we also code non-responses as rejects in order not to condition the decision variable on the response outcome which would make a causal interpretation of the results problematic (Kalla et al. 2018, Coppock 2019).⁷ As a result, 40% are defined as rejections. Table 2 includes examples of rejection, in responses 5 to 7. “Clear accept” denotes a clear affirmative offer of a spot at the school

⁶ Emails were forwarded in 85 instances. We do not find any evidence of that the forwarding of emails was affected by the treatment. See SI Table D1, p5.

⁷ For a parent making the request, a non-response will be closer to a rejection than the other dependent variables. In the results section we discuss possible implications if non-responses are treated as unclear responses instead of a rejection decision.

generally or in a specific class. Again, hedged responses are excluded. Using this standard, 20% received a clear acceptance to the request to change school. Responses 8 to 10 in Table 2 are examples of clear acceptances. “Unclear response” is if an ambiguous answer is given regarding whether there is room. An unclear answer should be understood as an answer that creates doubt for the parent. Using this standard, 40% received an unclear response. Responses 1 to 4 in Table 2 are examples of unclear responses. All decision variables were coded by two coders who were blinded to the treatment each response belonged to and with the two treatment names removed from the response mails. Each of the three decision outcomes have high levels of intercoder reliability, with Cronbach alpha values of more than 0.9.

Another set of variables capture compliance costs, where families were asked to provide information. First, “Simple Question” captures questions about the name of the child and other basic factual matters which occur in 19% of the responses. In addition, we coded for more complex questions about underlying reasons for the school transfer. These were much fewer, present in only 6% of responses. Finally, we coded for requests for the parent to call the school or meet in person. Both requests occur each in 37% and 35% of the responses respectively. For all outcomes non-responses are coded to 0. However, a non-response may also create additional burdens. For instance, parents seeking the desired information would have to make repeated contacts to the school or search for the information independently. This burden could be viewed as analogous to asking parents to respond to a simple question. We therefore also present results where a non-response is treated as a simple question.

A final set of variables capture psychological costs by coding the friendliness of the greeting tone in the response (for similar codings see Einstein and Glick 2017, White et al 2015). Some 22% do not contain a greeting and the majority of these are non-responses which are coded to contain no greeting. About 4% contain a greeting with no name, 51% contain the informal greeting (e.g., “hi”) or similar with name, and 22% use a formal greeting (e.g., “dear”) with name.

Among the received emails a clear majority of 72% are responses by females, 96% of all responders have a typical native Danish name, slightly more than 1% have a typical Muslim name, and the remainder are others or not classifiable. For the formal position there is more variation with a large share of responses

from the school principal, other managers like vice-principals, and finally also from secretaries. Consistent with the logic that principals will delegate more in larger public organizations, there is much lower probability that the principal authored the response as school size increases (for more see SI Table E1, p6).

[TABLE 3 ABOUT HERE]

Results

Response Rate

Figure 2 presents the probability of receiving a response. Muslim senders are not significantly different from native Danes in terms of the likelihood of receiving a response (-1.4 percentage points (pp), 95% confidence interval (ci) [-5.2 pp, 2.4 pp]). These results stands in contrast to most US bureaucratic audit studies which have found that ethnicity and race generate sizable response effects (Costa 2017), but is consistent with response rates to Muslim names in European bureaucracies (Adman and Jansson 2017; Grohs, Adam, and Knill 2016; Hemker and Rink 2017; Jilke, Van Dooren and Rhys 2018).

[FIGURE 2 ABOUT HERE]

Decision to Accept or Reject Students

Next, we turn to the question of how public officials allocate transfer opportunities. Figure 3 shows evidence of discrimination via allocative exclusion. In panel C, the Muslim name sender is associated with a 9.8 percentage point lower probability of obtaining a clear acceptance (95%-ci [-13.6 pp to -6.0 pp]). With an acceptance rate among native Danish requests of about 25%, this implies that only about 15% of Muslims receive a clear acceptance. Panel A of Figure 3 shows that Muslim requests receive 7.4 percentage points more clear rejections (95%-ci [2.8 pp, 12.1 pp]). There is no difference in unclear responses (2.4 pp, 95%-ci [-2.3 pp, 7 pp]). This division between rejections and unclear response depends partly on how we define non-response emails. Without non-responses, Muslims would receive about 5 percentage points more rejections, although such estimates cannot be treated as causal given that they are conditioned on non-

responses (Coppock 2019). Furthermore, the finding that Muslim requests are substantively less likely to get a clear acceptance than native Danes does not depend upon how non-responses are treated.⁸

[FIGURE 3 ABOUT HERE]

Compliance Costs: Additional Questions and Requests

Figure 4 offers evidence of discrimination via compliance costs. Muslim requests have 8.4 percentage points higher probability of containing one or more simple questions (95%-ci [4.7 pp, 12.1 pp]).⁹ This is substantive as only 15% of native Danish requests receive one or more questions. For more complex questions there is no significant or substantive difference but these questions are also fairly infrequent overall (0.5 pp, 95%-ci [-1.9 pp, 2.8 pp]). There is also no difference across treatments for requests to meet (-0.1 pp, 95%-ci [-4.6 pp, 4.4 pp]). However, for requests to call the school, the probability is 14 percentage points lower for Muslim respondents (95%-ci [-18.5 pp, -9.4 pp]). If we interpret phone calls as a more

⁸ About 12 percent of all requests are offered a spot on a waiting-list (see SI Table F1, p7). Requests from Muslim fathers are about 4 percentage points (95%-ci [1.2 pp, 7.3 pp]) more likely to be offered a spot on the waitlist. Formally only private schools can offer a spot on a waiting list, which is why 92% of the offers in our data is provided at private schools. Furthermore, about 90% of waiting-list offers are provided after a “clear rejection.” Thus, while Muslims are slightly more likely to be waitlisted this merely reflects that they are much more likely to be rejected and cannot be provided a causal interpretation due to post-treatment bias.

⁹ As noted in the methods section, we could also see a non-response as creating a burden similar to that of a simple question. If we code non-responses as being a simple question we get a slightly larger estimate of 10.2 pp (95%-ci [5.6 pp, 14.8 pp]).

informal mode of communication, it suggests that native Danish requests received a more informal request for additional information.

[FIGURE 4 ABOUT HERE]

Psychological Costs: Tone of Responses

Figure 5 examines email tone and use of respondent name, which have been used as a proxy for friendliness, reflecting efforts by the public official to reduce psychological costs by making the requester feel welcome. In panel A we see no significant differences in the probability of receiving no greeting, with the majority of these being non-responses coded to no greeting (1.5 pp, 95%-ci [-2.5 pp, 5.4 pp]). In panel B we see that Muslim senders are about 2 percentage points more likely to be greeted without the use of their name (95%-ci [0.4 pp, 4.3 pp]). However, this form of greeting is very rare in the data. The informal greeting “hi” with the name of the recipient is far more common and provided less frequently to Muslim senders by 7.5 percentage points (95%-ci [-12.3 pp, -2.8 pp]). Muslim and Danes are not significantly different in terms of receiving a formal greeting of “Dear” with their name (3.7 pp, 95%-ci [-0.3 pp, 7.7 pp]).

[FIGURE 5 ABOUT HERE]

Robustness Tests

The full OLS results as reported in figure 2 to 5 are provided in Table 4 with the inclusion of the diligent student treatment. We did not find any effect of the student diligence treatment on the response rate or other outcome variables, including the decision to accept or reject students, apart from diligent student cues receiving slightly more simple questions (95%-ci [0.1 pp, 7.5 pp]). We also show how the effect of student diligence has no meaningful moderating effect on the Muslim treatment (see SI Table G1, p8). In short, signaling that the student seeking a school place is diligent does not substantively reduce the discrimination

that Muslim families faced. Logit specifications of all models and ordered probit models of the main results on the decision show substantively similar results (see SI Table H1 and H2, p9). The same is true if we add regional fixed effects (SI Table I1, p10).

[TABLE 4 ABOUT HERE]

Given that private schools have more autonomy in selecting students, we tested if they differ from public schools in response rates or the decision to accept students by ethnic group. There are no meaningful differences between public and private schools on these dimensions (see SI Table J1-J3, p11-12). We also estimate the key models while interacting the Muslim treatment with the rural or urban local setting of the school without finding much variation (see SI Table K1, p13). We interact the Muslim treatment with a dummy indicating an above median level non-western students currently enrolled at the school (see SI Table L1). Again, we do not find any indication of significant differences of the effect of the treatment. The same is true if we interact the Muslim treatment with the school grade average (see SI Table L1, p14).

Conclusion

This study conceptually and empirically expands the use of audit studies to examine public sector bureaucratic discrimination. We identify and test different mechanisms of bureaucratic discrimination, finding evidence of both discrimination via allocative exclusion and administrative burdens. Unlike previous audit studies, we can observe not just how administrators respond to requests for information, but also how they use their role of gatekeepers of public services to make consequential decisions on how public resources are allocated. School officials were willing to differentially provide access to education to native Danes relative to Muslims, with Danes receiving a clear acceptance to a school transfer request 25% of the time, compared to 15% for Muslim senders.

The importance of examining allocative exclusion is underlined by the fact that if our study used only the most frequently-used measure of discrimination in audit studies – response rates – we would have

observed no difference in treatment across groups. The findings therefore reinforce calls to move beyond response rates in audit studies to examine other mechanisms of bureaucratic discrimination (Hemker and Rink 2017).

Our framework also demonstrates the value of understanding the variety of ways bureaucrats can employ administrative burdens to discriminate, which includes imposing higher psychological and compliance costs on out-group members. Bureaucrats are not just providers of information, but also demanders of information. Muslim families are more likely to be asked to provide information with no guarantee of acceptance, while a Danish family is more likely to be asked to call in person in the context of a successful admission. A preference for a more informal phone call may also indicate higher social trust, which is consistent with the use of a more welcoming tone for native Danes. The findings are all the more salient since they occur in a context – a universal benefit in a generous welfare state – where administrative burdens are generally less pronounced (Herd and Moynihan 2018).

As discussed above, we limited the interaction with bureaucrats to an initial response, quickly telling them soon after that the position was no longer needed. One limitation is that we cannot say conclusively that bureaucrats would not have eventually provided places to students if given more time. However, the evidence we have lends little support to this possibility. First, the high response rates mean that few officials were left to respond. Second, the evidence on compliance burdens shows that even among those who did not immediately offer a place, they still used administrative burdens to discriminate.

Our findings are all the more important given that much of the research on bureaucratic discrimination comes from the US, even as European and especially Scandinavian countries have become more ethnically and religiously heterogeneous. We offer evidence of bureaucratic discrimination in a Scandinavian welfare state that features a universalistic ethic (Esping-Andersen 2013), but one where immigration has challenged that ethic, leading to the ‘ethnicization’ of welfare state politics (Hjorth 2016). We show that in policy choices made far from electoral processes, bureaucrats discriminate against ethnic minorities in the provision of public services. In this context, the evidence of bureaucratic discrimination we offer is important not just because it raises awareness of the problem to policymakers, but also because it

can help solve the problem by holding a mirror to public officials, encouraging them to align their ideals and their actions (Alesina et al. 2018).

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[overgange/skolestart-og-boernehaveklassen/frit-skolevalg](https://uvm.dk/folkeskolen/fag-timetal-og-overgange/skolestart-og-boernehaveklassen/frit-skolevalg) (Accessed May 15, 2019).

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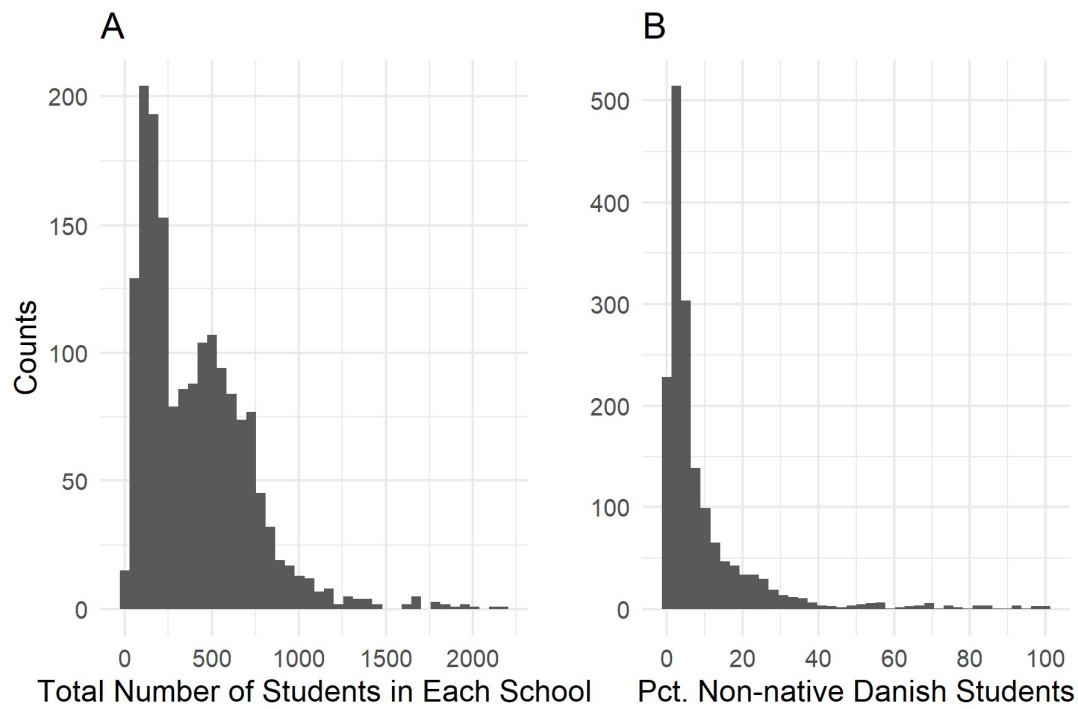
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Figure 1: Descriptive Statistics for the Schools in the Sample



Note: Histograms with number of schools on the y-axis. The sample is slightly reduced on both measures due to missing data. N=1,675.

Table 1: Descriptive Statistics and Balance Test

	<i>Muslim Name</i>		<i>Danish Name</i>		F-test (p-values)	
	<i>Diligent student</i>	<i>Non-diligent student</i>	<i>Diligent student</i>	<i>Non-diligent student</i>	For all four conditions	Sender name blocks
Total emails sent	425	425	424	424		
Above median number of students	50%	46%	52.6%	48.8%	p=0.29	p=0.27
Above median percent non-native Danish students	48.8%	49.6%	52.6%	48.6%	p=0.63	p=0.57
Above median grades	51.2%	47.4%	47.9%	44%	p=0.34	p=0.23
Percent of schools in four main urban areas	35.8%	31.3%	33.7%	31.6%	p=0.48	p=0.71
Percent of public schools	71.1%	64.5%	69.3%	67%	p=0.19	p=0.86

Table 2: Example of 10 Responses from the Schools

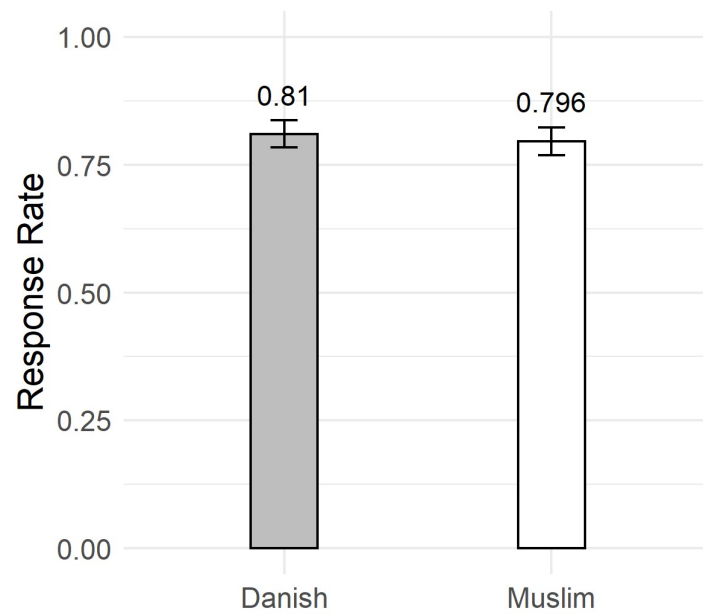
1. Hello <first name>. I would like to know a little more about your son. Where does he live and what school does he go to? Once I have received this information, I would go with it to one from the management. Good day. Sincerely <sender>
2. Hi <first name>. Thanks for your mail. Before I can answer you if we have room, I need to know your address so we can check if you live in our school district. . . Sincerely <sender>
3. Dear <first name> <surname> . Thank you for your mail with inquiry about a possible school transfer. Where do you live? Where is your son at school now? What is the reason you want a school transfer? You are welcome to contact me at phone. <phone number> or <phone number>, so we can talk more about any visit to <school name>... Sincerely <sender>
4. Hello <first name>. Thank you for your email concerning a school transfer for your son, and I need to have an in-depth chat with you to get a closer look at the problem. Can you call me at either mobile: <phone number> or my extension: <phone number>. Sincerely <sender>
5. Hi <firstname> Unfortunately, there is no room on <schoolname> in 3 rd grade unless you live in the school district. <sender>
6. Dear <firstname> Unfortunately, we are fully booked in the year group. So, we are not in a position to take in your son. Kind regards <sender>
7. Dear <firstname> <lastname> Unfortunately, we do not have any more places in 3 rd grade. You can find information about the school on <website> Kind regards <sender>
8. Good morning <firstname> Your son is welcome at <schoolname> and it would be fine to hold a meeting, so you can see the school, meet the teachers and we can match our expectations. Would you please contact me – so we can set a date and time Kind regards <sender>
9. Hi <firstname> We do have room for your son. We think you should stop by and see <schoolname> and have a chat with our principal before you decide. When would it suit you to visit us? Kind regards <sender>
10. Dear <firstname> <lastname> You are welcome to contact the school to get a tour. We have 26 students in 3 rd grade. So, there is also room for your child. On our website <website> you can read a lot more about the school. <sender>

Table 3: Means for the Dependent Variables across Treatments

	<i>Muslim Name</i>		<i>Danish Name</i>	
	<i>Diligent Student</i>	<i>No Student Info.</i>	<i>Diligent Student</i>	<i>No Student Info.</i>
Response	0.78	0.81	0.81	0.81
<i>Discretionary decision</i>				
Clear reject*	0.43	0.44	0.37	0.35
Unclear response	0.43	0.39	0.37	0.4
Clear accept	0.14	0.16	0.25	0.25
<i>Compliance costs</i>				
Simple Question	0.26	0.2	0.16	0.14
Complex Question	0.07	0.07	0.06	0.06
Request a phone call	0.29	0.3	0.44	0.44
Request to meet	0.35	0.34	0.34	0.35
<i>Psychological costs</i>				
No greeting*	0.24	0.22	0.22	0.21
Greeting with no name	0.06	0.05	0.03	0.03
Informal greeting	0.46	0.49	0.55	0.55
Formal greeting	0.24	0.24	0.2	0.21

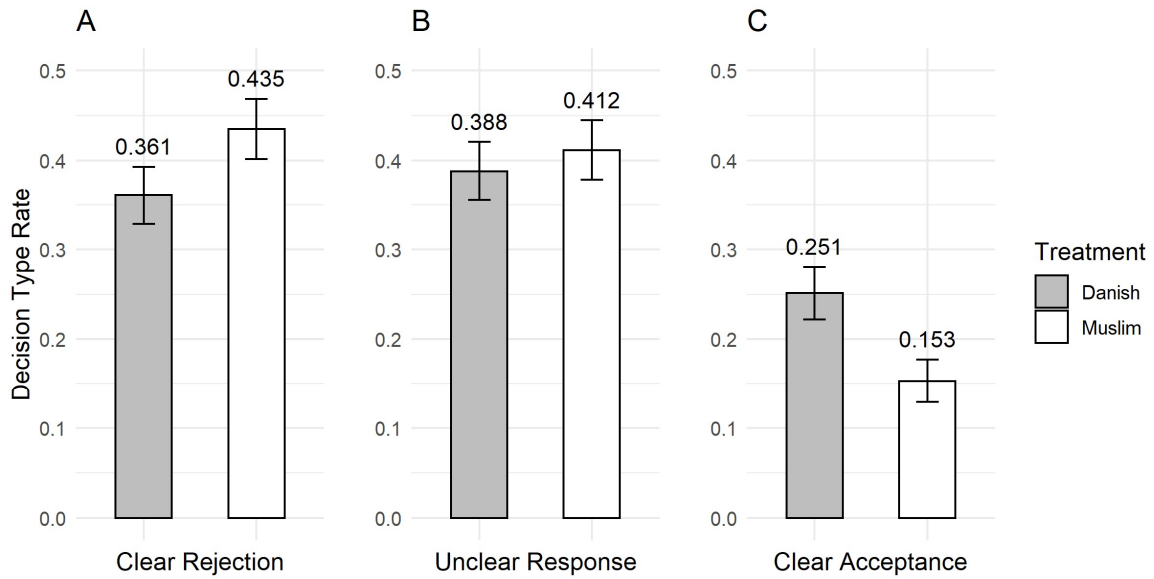
Note: All variables range from 0 to 1. N=1,698 for all variables. *Non-responses coded to 1 on these variables.

Figure 2: Response Rate by Treatment



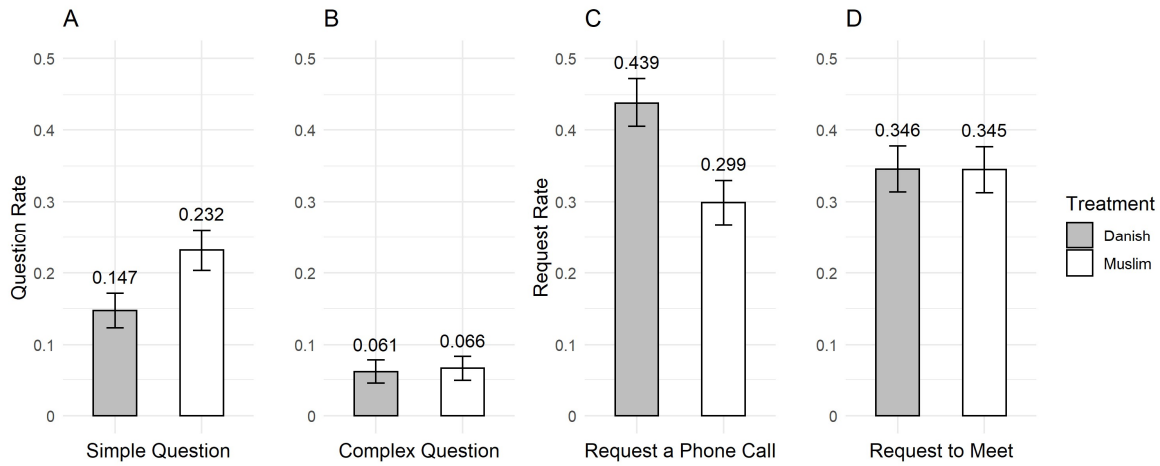
Note: OLS estimates with 95%-confidence intervals based on HC2 robust standard errors. See Table 4, model 1, for full OLS results including the diligent student treatment. N=1,698.

Figure 3: Decision Type Rate by Treatment



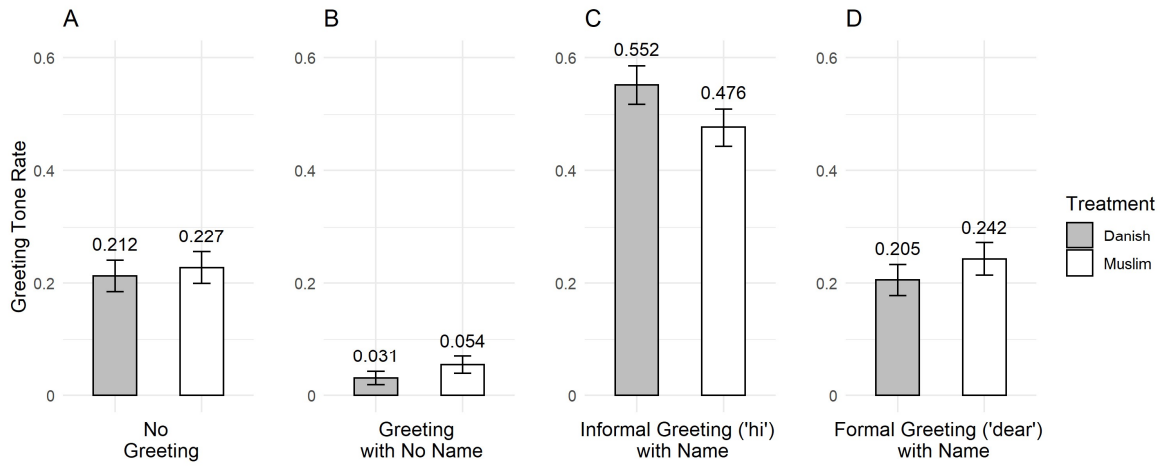
Note: OLS estimates with 95%-confidence intervals based on HC2 robust standard errors. See Table 4, models 2-4, for full OLS results including the diligent student treatment. Non-responses are coded to 1 on “clear rejection” and 0 on all other variables. N=1,698.

Figure 4: Compliance Costs by Treatment



Note: OLS estimates with 95%-confidence intervals based on HC2 robust standard errors. See Table 4, models 5-8, for full OLS results including the diligent student treatment. Non-responses are coded to 0 on all variables. N=1,698.

Figure 5: Psychological Costs by Treatment



Note: OLS estimates with 95%-confidence intervals based on HC2 robust standard errors. See Table 4, models 9-12, for full OLS results including the diligent student treatment. Non-responses are coded to 1 on “no greeting” and 0 on all other variables. N=1,698.

Table 4: Summary of Overall Results

	1	2	3	4	5	6	7	8	9	10	11	12
Muslim	-0.01 (0.02)	0.07* (0.02)	0.02 (0.02)	-0.10* (0.02)	0.08* (0.02)	0.00 (0.01)	-0.14* (0.02)	-0.00 (0.02)	0.01 (0.02)	0.02* (0.01)	-0.08* (0.02)	0.04 (0.02)
Diligent	-0.01 (0.02)	0.00 (0.02)	0.01 (0.02)	-0.01 (0.02)	0.04* (0.02)	0.00 (0.01)	-0.01 (0.02)	-0.00 (0.02)	0.02 (0.02)	0.00 (0.01)	-0.02 (0.02)	-0.00 (0.02)
Intercept	0.82* (0.02)	0.36* (0.02)	0.39* (0.02)	0.26* (0.02)	0.13* (0.02)	0.06* (0.01)	0.44* (0.02)	0.35* (0.02)	0.20* (0.02)	0.03* (0.01)	0.56* (0.02)	0.21* (0.02)
R ²	0.00	0.01	0.00	0.02	0.01	0.00	0.02	0.00	0.00	0.00	0.01	0.00
Adj. R ²	-0.00	0.00	-0.00	0.01	0.01	-0.00	0.02	-0.00	-0.00	0.00	0.00	0.00
Num. obs.	1698	1698	1698	1698	1698	1698	1698	1698	1698	1698	1698	1698
RMSE	0.40	0.49	0.49	0.40	0.39	0.24	0.48	0.48	0.41	0.20	0.50	0.42

Note: *p < 0.05. OLS coefficients with H2 robust standard errors. Models: 1: Response, 2: Clear reject, 3: Unclear response, 4: Clear accept, 5: Simple Question, 6: Complex Question, 7: Request a phone call, 8: Request to Meet, 9: No greeting, 10: Greeting with no name, 11: Informal greeting, 12: Formal greeting.