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Research Article

Caseload, time-pressure and discrimination: Are school principals who have less time for e-mail correspondence more likely to discriminate in their e-mail replies?

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Abstract: Street-level bureaucrats are assumed to use discriminatory practices against clients to handle high workloads and psychological exhaustion. However, empirical research on the relationships between caseloads, time pressure and discrimination is limited. This article is one of the first to study this topic using a large correspondence experiment that captures actual real-life discriminatory behaviour. Swedish school principals were randomly contacted via email by parents with Arabic- or Swedish-sounding names and with low-SES and high-SES professions who were interested in placing their children at the school. The principals' actual caseloads and perceived time pressure were captured using registry and survey data. The results reveal few robust effects. However, a slight tendency is seen in the results where principals who perceived that they had sufficient time to respond to emails were less likely to discriminate the Arabic and low-SES parents.

Keywords: Discrimination, Caseload, Coping strategies, Time pressure, Field experiment

Supplements: Pre-registration

The equal application of the law to all citizens is a vital normative goal of democratic governance and a cornerstone of public sector service. Despite this goal, numerous experiments have shown that public officials discriminate against ethnic minorities, even in established democracies (for reviews, see Costa, 2017 and Hansen & Tummers, 2020; for two recent studies, see Olsen et al., 2021 and Grohs et al., 2019). A crucial question for political science is whether public bureaucracies may be reorganized to reduce discriminatory behaviour. Are certain workplace characteristics among public officials associated with a higher risk of discrimination?

In contrast to our knowledge on the prevalence of discrimination, much less is known about the causes of discrimination and the degree to which contextual factors affect this behaviour (for reviews see Hardin & Banaji, 2013; Paluck & Green, 2009; Spencer et al., 2016). One factor that is often highlighted as associated with discriminatory behaviour is the working conditions and workload of public officials. In public administration research (Lipsky, 1980), street-level bureaucrats are assumed to use a wide variety of coping mechanisms to handle high workloads and psychological exhaustion (for a review of these mechanisms as well as the literature on coping, see Tummers et al., 2015). Some of the coping mechanisms are based on the discrimination of clients. For example, officials may selectively focus their attention, resources and efforts on some clients (*cream skimming*). They may also conversely try to decrease service availability, attractiveness or expectations from certain client groups (*rationing*) by, e.g., selectively increasing administrative burdens (cf. Jilke et al., 2018). Using

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these mechanisms, they can focus on less demanding and/or more successful clients to maximize the utilization of available personal and agency resources while increasing their odds of achieving their policy goals. Social psychology research has in turn showed that humans may be more likely to be unconsciously influenced by stereotypes and prejudice in situations that are very demanding of cognitive resources (cf. Fiske, 1998; Bodenhausen, 1990; Gilbert & Hixon, 1991; Ma et al., 2013). Under time pressure, individuals tend to unconsciously use mental shortcuts, such as stereotypes, to free up much-needed cognitive resources, and they are less likely to notice the effects of stereotypes and prejudices on their behaviour.

While there are numerous theories and hypotheses on the relationships between time pressure, caseloads and discrimination in both of these fields, empirical research is limited (for a review see Andersen & Guul, 2019). In this article, the effects of caseloads and time pressure on discriminatory behaviour will be tested in a large real-life field experiment. More specifically, I study the relationships between workplace conditions among principals and discrimination against parents based on ethnicity[1] and socioeconomic status (SES)[2] in Swedish elementary schools. Discrimination was captured using a correspondence experiment. A total of 3430 elementary school principals in Sweden were randomly contacted via emails from fictional parents interested in placing their children at the school and who asked them questions about the school and the admission procedure. The ethnicity (Swedish- and Arabic-sounding names) of the parents and their professions were randomized. The degree to which emails from the aliases received responses and the friendliness of the replies were studied. Using registry data, I captured the principal's actual caseloads as the number of students per school principal. A survey sent to the principals after the experiment was also used to capture their subjective perceptions of how much time they had for answering e-mails.

The study focuses on elementary school principals because schools are important public services that frequently engage with the general public, and the decisions made by principals have potentially large consequences for their clients. School principals are generally the victims of stress at a much higher degree than the population in general in Western countries, which creates the possibility that their caseloads unconsciously or consciously affect their actions (Läraren, 2020; The Conversation, 2019).

The study focuses on an empirical setting, which was rarely studied in this literature: A Scandinavian welfare state (Sweden). Most previous public administration research on coping mechanisms focuses on the US or the UK (Tummers et al., 2015). The same focus is found for social psychology research on cognitive resources, which has primarily been performed in the US. Sweden has a reputation for being an immigration-friendly welfare state and a well-functioning democracy (see, e.g., Migration Policy Group, 2014). Following decades of large-scale immigration, approximately 19 percent of the population is foreign-born (Statistics Sweden, 2019). Sweden is often described as a European outlier because public opinion shows particularly positive views on ethnic minorities compared to other countries (World Value Survey, 2014). From this perspective, we may be less likely to find discrimination in Sweden than in the US or the UK. However, the general discourse towards migrants from the Middle East has hardened in recent years, and recent experiments showed that they are often subject to discrimination in many societal arenas, similarly to many other European countries (e.g., Carlsson & Rooth, 2007; Bursell, 2014; Taghizadeh, 2021). Therefore, Sweden may be a more representative case of northern Europe than is often assumed.

Contributions

The present study makes three important empirical contributions to public administration research.

First, the effects of workload among public officials on *discriminatory behaviour against clients* are studied. Very few studies on this outcome were identified. Previous public administration research has primarily studied the effects on views towards clients (e.g., Schütze & Johansson, 2019; Blomberg et al., 2014) or indices of coping strategies, where most of these studies did not directly capture discrimination (Winter, 2002; Baviskar & Winter, 2017; Loon & Jakobsen, 2017). Social psychologists have in turn primarily studied the effects of cognitive load on prejudice and text-based descriptions of individuals rather than discriminatory acts towards individuals (e.g., Bodenhausen et al., 1998; Gilbert & Hixon, 1991). The clear exception is Andersen and Guul's (2019) study on Danish teachers in public administration research. Discrimination was captured using a vignette experiment where the teachers made a decision about a hypothetical case of a problematic student with a randomly assigned immigrant or non-immigrant name. The results showed that teachers were less likely to discriminate based on

ethnicity if they received extra resources or if the student in question would not be assigned to the teacher's class.

Second, using a field experiment, the current study analyses the effects of workload on discriminatory behaviour in a *real-life setting*. Relatively limited experimental research has been performed on street-level bureaucracy (Tummers et al., 2015; Grimmelikhuijsen et al., 2017; Hansen & Tummers, 2020). Consequently, most of the studies on coping mechanisms in public administration research are based on surveys (e.g., Winter, 2002; Baviskar & Winter, 2017; Loon & Jakobsen, 2017). Surveys do not allow an objective assessment of the extent to which discrimination occurs, and they are potentially subject to methodological problems, such as unrepresentative samples, omitted variable bias and social desirability bias. The most credible and relevant studies on this topic in public administration research (e.g., Andersen and Guul, 2019) and social psychology research are based on vignette or laboratory experiments. These methodologies suffer less from the above-mentioned problems, but discrimination is captured using hypothetical situations that are not always generalizable to real-life scenarios. Most of the laboratory studies in social psychology were also performed on students and may not be generalizable to public officials.

Third, the effects of workload on both ethnic discrimination and SES discrimination are studied by including treatments on the professions of the clients (high SES: dentist, low SES: care assistant). To the best of my knowledge, no similar studies on this topic exist. However, if workload and time pressure among public officials are related to ethnic discrimination towards clients, then we should see similar effects based on SES. As a result of statistical discrimination, immigrant clients may be associated with perceived costs related to language skills and potential negative policy outcome trajectories (e.g., in grades). The SES of clients may send even clearer signals about costs and future workload, which increases the likelihood of statistical discrimination, such as via cream skimming/rationing. For example, school principals who are under a high workload have strong incentives to attract students with highly educated parents while deterring resource-demanding students. Principals under stressful working conditions are also more likely to unconsciously treat high-SES parents more favourably because they are seen as ingroup members or due to their higher social status.

Institutional Context: The Swedish School System

Municipalities (local governments) in Sweden have administrative responsibility for organizing and financing the elementary school system. Parents freely choose between all schools in their municipality: public schools managed by the municipalities, private schools run by for-profit businesses, and non-profit private schools run by associations and foundations. All schools are obliged to follow the same laws and adhere to the Swedish curriculum for compulsory schools. A voucher system is used, and no students have to pay for their education. If a private school is oversubscribed, it can choose students based on proximity to the school, waiting lists (by date of application), and/or priority for children whose older siblings are already enrolled in the school. The same criteria apply to public schools, but students are always guaranteed a slot in the public school nearest to their home. Schools are not allowed to discriminate against students in Sweden and most other countries, but frictions in the choice process may allow schools to influence who applies. For example, schools may influence the applicant pool via informal messages and information sent to potential clients. Therefore, the focus is on social interactions between principals and parents interested in placing their child at the school.

Methods

Discrimination was captured using a correspondence experiment. Every elementary school principal governing a unique school in Sweden[3] was contacted via one email from a randomly assigned fictional parent (N=3430). The emails were sent out in several waves between January 11 and 14, 2020.[4]

Treatments

A factorial design was used based on the ethnicity, SES, and gender of the parent[5]. The email addresses of the school principals were randomly divided into eight groups that corresponded to the aliases used in the emails. An advantage of factorial designs is their efficiency for use in experimental subjects. For a given number of

treatments, factorial designs require fewer experimental subjects than alternative experimental designs to maintain the same level of statistical power (cf. Collins et al., 2009).

For ethnic treatment, immigrants with a background in the Middle East were chosen. This group constitutes an important and politized ethnic minority group in Sweden, as previously mentioned.[6] I chose the male name Mahmoud and the female name Fatimah for the Arab parents, both with the surname Hassan, which is rather common in Sweden (Statistics Sweden, 2020). Couples from Arabic countries rarely choose traditionally Swedish names (or surnames) for their children to my knowledge, and the names chosen should therefore clearly signal ethnicity.

Names of individuals belonging to ethnic minorities are more strongly associated with low education, low income and disadvantageous employment positions (Elchardus & Siongers, 2011). Therefore, I chose the names Kevin and Melissa for the Swedish parents because they were associated with SES levels similar to the Arabic names according to a pre-test on upper secondary school principals (see appendix E, see Jilke et al., 2018 for a similar pre-test). All four names are used in Sweden today, and the mean age of the names varies between 15 (Melissa) and 35 (Mahmoud).

The presence of SES discrimination was studied by signalling highly skilled professions (dentist) in half of the e-mails and low-skilled professions in the other half (care assistant). The parents shared the same profession. These professions are common among immigrants and native-born individuals, but there is an underrepresentation of men among care assistants (only 10 percent, Socialstyrelsen, 2017). In contrast to care assistants, who require only a high school education, dentists require extensive university education and a licence to practice. Their average wage (47,400 SEK/month) is almost twice the wages of care assistants (24,800 SEK; Statistics Sweden, 2020). Therefore, the chosen professions should clearly signal SES.

Content of the Emails and Coding Scheme

The letter sent to the principals is shown in Figures 1 and 2 in appendix F. The emails were written as if they were sent by someone who was considering moving to the municipality and placing his or her son at the school. They included three straightforward and not particularly time-consuming questions about the school profile, the registration procedure and open slots.

The coding scheme consisted of three variables (see Table 5 and the coding examples in appendix B). Two of these variables measured formal aspects, i.e., whether any reply was received within two weeks and how many of the three questions were answered. Replies were registered from anyone working at the school or the municipality of the school, except for autoreplies and noninformative emails from principals who had left their position, the latter being excluded from the sample. The third variable was an index that measured the friendliness of the emails (0-5).[7] More specifically, it captures whether the replier invites future contact (1p), welcomes the sender to the municipality/school (1p), invites the sender to visit the school (1p), provides additional information not directly related to the three questions (1p) and/or use the name of the sender (1p).

Nonresponses were coded as zero for all variables to avoid conditioning on a posttreatment variable and inducing selection bias (cf. Coppock, 2019). Two research assistants independently coded emails following instructions and a coding form. After coding all of the responses independently, the assistants were instructed to pay special attention to emails where their codings differed to reconcile the final coding. For the simplest variables (reply and the variables forming the friendliness index), a third assistant reviewed differing cases. The names of the fictitious emailers were removed before coding started.

Of the original 3430 emails sent, 3394 units of analysis were ultimately included in the dataset, and registry data were available for 3133 units. Thirty-one emails bounced back and were excluded from the analysis. Five emails were also excluded because the principal replied that she or he had left the position, and the email was not forwarded further. In the few cases where several responses were received from a principal or a school, the overall "best" response was chosen; i.e., the one that scored highest on the three variables taken together. No signs of spillover or disclosure of the experiments were found in the email responses. Ethical concerns are discussed in appendix C.

Measuring Caseloads and Perceived Time for Contacts

Using registry data, I measured *the number of students per full-time principal*. Each school is governed by one head principal or a team of principals, including deputy principals. The experiment suggests that they have the main

responsibility for answering emails from parents. A total of 86.91 percent of the email's responses were written by head principals, 2.4 percent were written by deputy principals, and 10.69 percent were not written by principals[8] (primarily administrative assistants). Therefore, assuming that the magnitude of email correspondence is related to the number of students at the school, this variable should provide a rough estimate of the full-time personnel resources available for answering emails. The data supported this presumption. As seen in Table 11 in appendix H, schools with a higher number of full-time principals after controlling for the number of students were generally more likely to respond to emails and answer more questions. Table 11 also shows that responses from schools with a higher number of students per principal scored lower in terms of friendliness. This measurement is consistent with how caseloads are generally measured in the public administration literature, i.e., the number of clients on a caseworker's caseload or the amount of time available to a caseworker per case (cf. Winter, 2002; Tummers & Rocco, 2015; Van Berkel & Knies, 2016). Registry data were available for most of the principals included in this field experiment (approximately 92 percent), and the sample should therefore be representative of the full experimental dataset (see Table 10 in appendix G). No statistically significant differences in caseloads were found between the treatment groups, which suggests that the randomisation was successful (see Table 16 in appendix I).

The registry-based variable was complemented with survey data that directly captured the degree to which the principals felt that they had sufficient time to respond to emails. The survey question used was one of several questions that were asked about their work environment. All of the survey questions, including our second survey on discriminatory attitudes, may be found in the pre-analysis plan. Perceived resources about time for contacts among the principals was measured using the following survey question: *Do you feel you have enough time to respond to emails and/or phone calls?* (0=never, 10=always). The disadvantage of this measure is that it did not seem to correlate with being more likely to answer emails or write friendly emails. The correlation (Pearson's r) with the objective caseload measure was small (-0.1204) but statistically significant at the 0.1 percent level, which suggests that the two measures are potentially related but measured different things. Only approximately 25 percent of the principals contacted in the field experiment participated in the survey, which was sent out via email after the field experiment. The low response rate may have generated a selection bias where principals with smaller workloads were more likely to participate. However, no large differences were found between participants and non-participants in terms of objective caseload (number of students per principal, see Table 10 in appendix G). The survey sample also appeared to be relatively representative of the full experimental sample in comparison to municipality- and school-level characteristics (Table 10 in appendix G) and the general discrimination effects (Table 1 below). The treatments also appeared fairly balanced in the survey dataset relative to school and municipality characteristics (Table 9 in appendix G). The results are also presented with controls for municipality characteristics (Table 13 in appendix H).

Results

The Presence of Discrimination

Examination of the general presence of discrimination (see Table 1 below) revealed no negative discrimination effects on the response rate or the number of questions answered (models 1–6 in Table 1). However, statistically significant negative discrimination effects on the friendliness index were observed against our Arabic and low-SES aliases (models 7–9) in all of the samples, including the survey sample. The responses to these aliases were rated as less friendly on average (a decrease of approximately 10 to 16 percent in friendliness points from the baseline of 1.46 to 1.63 friendliness points).

Effects of Principal Caseloads on Discrimination using Registry Data

Table 2 shows whether principals with larger caseloads are more likely to discriminate against parents using an interaction variable between the number of students (in hundreds) per full-time principal (see Table 2, row 1). Most of the coefficients were positive, which indicate that principals with larger caseloads were *less likely* to discriminate. However, the estimates were generally small and close to zero (changes of around or less than one percent from the baseline/intercept) The only statistically significant interaction effect was a positive effect on questions answered (in model 4), which was only significant below the 10-percent level. An increase in the

caseload of 100 students per principal resulted in .04 more questions answered from the Low-SES parents (a change of 2.25 percent in questions answered from the baseline of 1.773 questions).

Table 1
Discrimination effects

	Reply			Questions answered			Friendliness index		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Arab	-.008	-.012	-.014	-.022	-.020	.040	-.225***	-.236***	-.267**
	(.017)	(.017)	(.030)	(.045)	(.045)	(.085)	(.045)	(.047)	(.098)
Low-SES	-.014	-.013	-.026	-.037	-.031	-.052	-.198***	-.211***	-.157*
	(.016)	(.016)	(.025)	(.041)	(.042)	(.080)	(.043)	(.046)	(.080)
Woman	-.016	-.013	.017	-.030	-.028	-.042	.025	.034	.102
	(.013)	(.014)	(.031)	(.039)	(.041)	(.091)	(.043)	(.047)	(.098)
Constant	.769***	.773***	.839***	1.698***	1.709***	1.895***	1.460***	1.482***	1.632***
	(.016)	(.016)	(.030)	(.042)	(.044)	(.081)	(.060)	(.062)	(.108)
Data	Full	Registry	Survey	Full	Registry	Survey	Full	Registry	Survey
Adj. R^2	-.000	-.000	-.001	-.000	-.001	-.002	.013	.014	.012
N	3394	3133	878	3394	3133	878	3394	3133	878

Notes: $^{+}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$. Standard errors in parentheses are clustered at the municipal level. Full: all principals contacted in the field experiment (minus bounces). Registry: all principals contacted in the field experiment with available registry data. Survey: all principals contacted in the field experiment with available survey data.

Table 2
Effects of the interaction between a principal's caseload and the Arab and low-SES treatments on replies, questions answered and friendliness index.

	Reply		Questions answered		Friendliness index	
	(1)	(2)	(3)	(4)	(5)	(6)
Caseload*Arab	-.006		-.010		.007	
	(.006)		(.019)		(.022)	
Caseload*Low-SES		.009		.040+		.009
		(.007)		(.021)		(.024)
Caseload	-.001	-.009	.003	-.022	-.078***	-.079***
	(.005)	(.005)	(.016)	(.015)	(.014)	.016)
Arab	.005	-.012	.011	-.021	-.256**	-.235***
	(.026)	(.017)	(.078)	(.045)	(.087)	(.046)
Low-SES	-.012	-.040	-.030	-.147+	-.210***	-.237**
	(.016)	(.028)	(.042)	(.075)	(.046)	(.089)
Constant	.775***	.797***	1.701***	1.773***	1.704***	1.706***
	(.021)	(.022)	(.061)	(.059)	(.074)	(.081)
N	3133	3133	3133	3133	3133	3133
adj. R^2	-.000	-.000	-.001	-.000	.026	.026

Notes: $^{+}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for gender treatment. Caseload=100 students per full time principal.

Effects of Perceived Time Resources on Discrimination using Survey Data

Using the time-contacts interaction in Table 3 below, I tested whether principals who perceived that they had sufficient time to respond to emails were less likely to discriminate parents based on ethnicity and SES in their email replies.

Consistent with the expectations, most interaction effects were positive, which indicated that more time was associated with less discrimination. For ethnic discrimination, two positive statistically significant interaction effects below the 10-percent level were found on reply (p=0.089, model 1) and questions answered (p=0.054, model 3). Principals who answered the survey and who felt to a higher degree that they had sufficient time to answer emails (1 step up on a 0–10 scale) were 1.5 percentage points more likely to respond to emails from our Arab aliases vs. our Swedish aliases (from a baseline of 87 percent) and answered .054 more questions (an increase of 2.8 percent from a baseline of 1.95 questions). For the SES discrimination, a positive statistically significant interaction effect below the 5-percent level was found on reply (p=.019, model 2). Principals who felt they had more time to answer emails were 2.1 percentage points more likely to respond to emails from our low-SES aliases vs. our high-SES aliases (from a baseline of 89 percent). Marginal effects are illustrated in Figure 4 in appendix H. If municipality-level characteristics were controlled for, the positive interaction effect in model 3 was also statistically significant at the 5-percent level (Table 12 in appendix H). However, if a control for student test results was added (see Table 13 in appendix H), only the statistically significant effect of SES remained.

Table 3
Effects of the interaction between perceived time resources for answering emails and the Arab and low-SES treatments on replies, questions answered and the friendliness index.

	Reply		Questions answered		Friendliness index	
	(1)	(2)	(3)	(4)	(5)	(6)
Time contacts*Arab	.015^{+}		.054^{+}		.001	
	(.009)		(.028)		(.031)	
Time contacts*		.021*		.025		-.011
Low-SES		(.009)		(.029)		(.033)
Time contacts	-.005	-.009	-.009	.003	-.017	-.010
	(.006)	(.006)	(.019)	(.021)	(.025)	(.030)
Arab	-.098^{+}	-.014	-.265	.023	-.270	-.265**
	(.057)	(.030)	(.175)	(.083)	(.202)	(.099)
Low-SES	-.025	-.142**	-.045	-.182	-.161*	-.099
	(.025)	(.054)	(.079)	(.176)	(.079)	(.199)
Constant	.869***	.890***	1.950***	1.877	1.724***	1.688***
	(.044)	(.046)	(.140)	(.142)	(.195)	(.222)
N	878	878	878	878	878	878
adj. R^2	-.000	.003	.001	.004	.011	.011

Notes: $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for gender treatment.

Discussion and Conclusion

This article assessed the relationships between caseloads, time pressure and discriminatory behaviour using a combination of field experimental data, registry data and survey data. Overall, the results did not show a strong robust relationship between caseloads/time pressure and discrimination. However, a slight tendency was seen in the results where principals who perceived that they had sufficient time to respond to emails were less likely to discriminate the Arabic and low-SES aliases. Notably, the effects were primarily found in formal aspects of

the replies (e.g., response rate) rather than informal aspects (e.g., friendliness). None of the effects were statistically significant after the critical p-values were adjusted for multiple hypothesis testing.[9]

The results contrast with a few studies in public administration research that found stronger effects between caseloads and outcomes, such as client-oriented coping strategies (Winter, 2002; Loon & Jakobsen, 2007) and discrimination (Andersen & Guul, 2019). The current results also contrast with a few laboratory experiments in social psychology that found clearer effects between cognitive load and (text-based) prejudiced behaviour towards fictional individuals (Bodenhausen et al., 1998; Gilbert & Hixon, 1991).

These differences in results may be explained by the contextual differences in the policy area and country studied. The differences may also be the result of differences in outcome variables. The current study focused on discriminatory behaviour in a real-life setting and most previous studies were survey- or computer-based laboratory experiments. Another explanation is statistical power. The statistical power in the current registry data sample was higher than all of the reviewed previous studies based on the number of observations and the variables used. However, the statistical power in the survey sample was lower than that in some previous studies (e.g., Loon & Jakobsen, 2007; Andersen & Guul, 2019: studies 1 and 2 but not study 3), which may have made it more difficult to identify statistically significant interaction effects on this part of the study. Based on my power calculations (power and sampsi in stata), the survey sample may be too small (the power level is below .8) to completely exclude (alpha=.05) the presence of effects on response rates below .067 and questions answered or the friendliness index below .19. The corresponding numbers for the registry data sample were .02 and .1.

Finally, the differences in results may be explained by the choice of independent variables. The measurement of personnel resources relative to the number of clients (students) may not provide an accurate view of the degree to which principals are exposed to time pressure when contacted by clients. For each case, the workload can vary greatly (cf. Jewell & Glaser, 2006), and the level of work pressure may vary with quality or efficiency expectations (cf. Lipsky, 1980). However, similar measures were used in many previous studies (e.g., Winter, 2002; Loon & Jakobsen, 2007). The results were similar when they were adjusted for the composition of the students (see appendix H, Table 14). The caseload measure correlated with the outcome where the clearest discrimination effects were found, i.e., friendliness. Principals with larger caseloads generally wrote less friendly emails (cf. Table 11 in appendix H). However, no clear effects of caseload on discrimination were found in the current study. Hence, even where effects were expected, they did not appear. My survey-based measure, which provided a direct measure on time pressure in relation to answering emails, showed similar results. From this perspective, my results question whether a strong relationship exists between workloads and discriminatory behaviour.

Given the limited number of studies on the topic and their limitations (including my own study), it is however clear that more studies are needed to draw safe conclusions about the relationships between caseloads, time pressure and the prevalence of discrimination. In particular, we need larger N-experimental studies in various contexts that capture actual discriminatory behaviour towards clients.

Notes

1. Ethnic discrimination was defined as unequal treatment based on physical characteristics (e.g., skin/hair) or cultural factors (e.g., name or language).
2. SES discrimination was defined as unequal treatment based on the individual's education, occupation and/or income.
3. See appendix D.
4. The study followed the pre-registered pre-analysis plan (https://osf.io/3bju7?view_only=3ed1f21c95844935b379396f8e77588f). The pre-analysis plan covered a large project that include numerous hypotheses for testing in several articles. Another article (Taghizadeh, 2021) focusing on discrimination based on SES has been accepted for publication in local government studies.
5. Gender was randomised to test for gender-based discrimination (in other articles) and ensure that the results were generalisable to men and women.
6. Four percent of the Swedish population was born in Syria, Iraq, or Somalia alone (Statistics Sweden 2019).

7. A principal component analysis supported treating the items as one dimension. Using a principal component analysis with varimax rotation, the eigenvalue of the only dimension that survived the Kaiser criterion was 1.8. This dimension explained 36 percent of the total variance. The factor loadings varied between 0.38 and 0.52.
8. The results remained largely the same when these responses were removed from the analysis or when replies from non-principals were controlled for (e.g., administrative assistants).
9. The statistically significant effects found using the survey data were not robust to Bonferroni or Benjamini-Hochberg (false discoveryrate=0.05) adjusted critical p-values based on the 12 tests in the article.

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Appendix

Appendix A. The treatments

Table 4
The treatments

Swedish-sounding name				Arabic-sounding name			
Low SES		High SES		Low SES		High SES	
Man	Woman	Man	Woman	Man	Woman	Man	Woman

Appendix B. Email-dependent variables and coding examples.

Table 5
Summary statistics for email-dependent variables (N=3394)

Dimension	Variable	Variable description	Mean	Std. dev.
Formal correctness	Reply (1,0)	Did the recipient respond to the email within 2 weeks?	0.749	0.434
Formal correctness	Questions answered (0-3)	How many of the three questions are answered?	1.653	1.226
Friendliness	Friendliness index (0-5)	The replier invites future contact (1p), welcomes the sender to the municipality/school (1p), invites the sender to visit the school (1p), provides additional information not directly related to the three questions (1p) and/or use the name of the sender (1p)	1.261	1.282

To illustrate the coding procedure for the dependent variables, two complete email answers are presented in Figure 3. In the first email, all three questions about the school profile, open school slots and how to apply are answered. Therefore, the variable for the number of questions answered was assigned the value 3 (cf. Table 5 above). The sender is welcomed in a friendly manner, her or his name is used, and she or he is invited to reach out for future contact and to visit the school. Therefore, the variable friendliness index was given the value 4. For email 2, the principal scored only 2 on the number of questions answered (only the questions on how to apply to the school and on the school profile are answered). The friendliness index variable was given a value of 1 because the response contained additional information about the school (information about average grades) but no other signs of friendliness.

Figure 1
Two examples of email replies from the school principals

Answer 1

Hi name! For starters, I would like to welcome you to our school and our municipality. We have no defined profile. Depending on the grade your child is going to, you sign up in different ways. Year 1 enrolment occurs through the Pedagogical Planning Unit, and year 2 and 3 enrolment occurs through me. There is a queue at school today for grade 1. For grades 2 and 3, there is no queue, but all slots are filled right now, so the school is unfortunately full at the moment. Hope this information is helpful. Feel free to contact me again if you have more questions and/or want to visit the school.

Best regards, name

Answer 2

You go to municipalityx.se and apply for school. It is the elementary school administration that places children. Our school does not have a special profile; however, our students have the highest grades in the municipality!

/principal x

Appendix C. Research Ethics

To reduce social desirability bias and capture real-life discrimination, the research subjects were not made aware that they were part of a field experiment. Discrimination is potentially sensitive, and the results would not have been accurate if the elementary school principals had been informed and asked to participate beforehand. However, I strived to decrease the potential negative effects of the experiment as much as possible. First, the principals were anonymised, and only aggregate-level tendencies are shown and not specific answers. The emails and the discrimination effects are presented in a way that prevents the identification of the municipalities from which they were sent. Second, I minimised the time the principals spent on emails by keeping the questions simple. Some principals asked questions in their replies, but these questions were not answered to prevent them from working additional hours. The experiments will generate numerous articles. No similar experiments have been performed to my knowledge on the research subjects, and no future experiments will be implemented by the author on Swedish public officials (without seeking consent) so that future encounters between principals and citizens will not be affected and to avoid using more of their working time. Data on how much time the principals spent replying to our emails in the experiment are provided below. The design was approved by the Swedish ethical review board (decision 2017/234 and decision 2018/371).

Table 6
Subjects' estimated time cost

Media word count	658
Estimated time cost per subject	16.45
minutes	2543
Number of impacted subjects	697.21
Estimated total time cost (hours)	

Notes: assuming an average typing speed of 40 words per minute and no time cost for subjects who did not reply

Appendix D. How the List of Principals was Generated

No schools are shared between principals, and the study is very close to being a population study of all elementary school principals in Sweden.

To minimise the risk of spillover effects and detection, the list of principal email addresses was generated based on registry data following these rules: (1) Only one unique email address per principal was allowed, (2) Principals working at several school units (based on school codes) were contacted only once, (3) If there were several principals working at a school (e.g., vice-principals), the email address to the main principal was used, (4) In the cases where the email addresses for the principals in the registry data contradicted the homepages (according to web-scraping), the addresses on the homepages were used, (5) When no email to directly contact the principal was available for a school, and we had no reason to believe that the school's principal was already included in our dataset, we contacted the school directly via its common address, and (6) 36 principals that participated in a pilot study were not included in the list of email addresses. These rules resulted in 543 unique email addresses to schools that were not used, and some of these email addresses led to the exclusion of principals from the final sample.

Appendix E. Choice of Swedish Names

Names generally carry an ethnic association and a certain SES association, and names belonging to ethnic minorities are sometimes associated with low SES (Elchardus and Siongers 2011). To avoid the possibility that the results were driven by SES associations connected to the chosen names, it was important to choose names that were associated with the same SES levels. Before performing the experiment, a smaller study was performed using upper secondary school principals (i.e., not from an elementary school) who rated names on a 100-point scale based on SES (0=low income/education, 100=high income/education). Of the 715 schools that received the invitation, 252 (35.2 percent) answered all of the questions in the survey.

Table 7
Results from upper secondary school principals rating names on a 100-point scale on income/education

Names	Observations	Mean	Standard deviation
Kevin	261	39.15	18.21
Mahmoud	258	41.30	19.32
Melissa	255	46.87	18.51
Fatima	255	46.97	19.24
(Ebba) for comparison	252	60.06	16.14
(Daniel) for comparison	253	57.33	13.80

Table 8
The number of individuals in Sweden in 2020 named Kevin, Mahmoud, Melissa and Fatima and their mean age according to the Swedish name registry

Names	Number of men	Number of women	Mean age
Kevin	14 659	26	18
Mahmoud	5338	1180	35
Melissa	2	4725	15
Fatima	0	6207	32

Appendix F. Letter Sent to the Principals

Figure 2
The letter in English

Questions about your school

Hi!

My name is Name, and my family and I are thinking of moving to your municipality. Does your school have a special profile? How do you register? Are there open slots available at your school?

Where we move depends on how the job opportunities are (my husband and I are both dentists/nursing assistants), but it is of course also important that we find a good school for our son.

Thank you for taking the time to reply (preferably via email if possible).

Sincerely,

Name Surname

Figure 3
The original letter in Swedish

Frågor om er skola

Hej! Mitt namn är *Namn*, jag och min familj funderar på att flytta till er kommun. Har er skola någon speciell inriktning/profil? Hur anmäler man sig? Finns det plats på skolan?

Vart vi flyttar beror på hur det är med jobb-möjligheter (min man och jag är båda *Tandläkare*/*Vårdbiträden*) men det är förstås också viktigt att vi hittar en bra skola åt vår son. Tack för att du tog dig tid att svara (gärna via mail om möjligt).

Vänliga hälsningar *Namn Efternamn*

Appendix G. Balance Tests and Comparisons Between the Samples

Table 9
Balance between the different treatment groups in the survey sample

	Arab	Swedish	Low-SES	High-SES
Time contacts	5.684	5.489	5.439	5.726
	(2.792)	(2.882)	(2.911)	(2.763)
Private school	.228	.243	.210^{+}	.261^{+}
	(.420)	(.430)	(.408)	(.439)
For-profit	.128	.136	.143	.121
	(.334)	(.343)	(.351)	(.327)
Number students	244.0	240.9	250.1	.235
	(191.0)	(172.3)	(170.7)	(.191.4)
Population municipality	132.2	149.5	143.8	138.3
(thousands)	(234.6)	(251.4)	(254.4)	(232.2)
Proportion of foreign born in municipality	.182	.180	.186	.176
	(.075)	(.074)	(.078)	(.071)
Mean income in municipality	299.1	298.9	300.1	298.0
(thousands, kr)	(41.75)	(42.18)	(44.25)	(39.60)
Percent voted Swedish Democrats in	12.40	12.67	12.55	12.52
municipality	(4.603	(4.994)	(4.819)	(4.799)
N	430	448	433	445

Notes: $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ t-tests for significant differences in Means (Arab vs Swedish, low-SES vs high-SES). Means with standard deviations in parentheses.

Table 10
Comparisons of school and municipality characteristics across the different datasets

Sample	Full	Registry data	Survey data
Arab treatment	.501	.497	.490
	(.500)	(.500)	(.500)
Low-SES treatment	.499	.496	.493
	(.500)	(.500)	(.500)
Woman treatment	.500	.502	.503
	(.500)	(.500)	(.500)
Caseload		2.920	2.843
		(1.948)	(1.928)
Private school	.218	.227	.236
	(.413)	(.419)	(.525)
For-profit school	.131	.139	.132
	(.338)	(.346)	(.339)
Number of students	245.3	258.5	242.4
	(176.8)	(177.7)	(181.6)
Population municipality	150.2	154.4	141.0
(thousands)	(249.6)	(252.8)	(243.3)
Proportion of foreign born in municipality	.186	.188	.181
	(.074)	(.075)	(.074)
Mean income in municipality	299.8	300.9	299.0
(thousands, kr)	(41.13)	(41.58)	(41.95)
Percent voted Swedish Democrats in	12.97	12.97	12.54
municipality	(5.050)	(4.989)	(4.806)
N	3394	3133	878

Notes: Means with standard deviations in parentheses. Full: all principals contacted in the field experiment (minus bounces). Registry: all principals contacted in the field experiment with available registry data. Survey: all principals contacted in the field experiment with available survey data.

Appendix H. Other tests

Table 11
Effects of the number of principals, number of students and the number of students per principal on replies, questions answered and the friendliness index.

	Reply		Questions answered		Friendliness index	
	(1)	(2)	(3)	(4)	(5)	(6)
Number of principals	.062*		.316***		.093	
	(.031)		(.086)		(.098)	
Number of (100) students	-.007		-.005		-.091***	
	(.005)		(.014)		(.014)	
Caseload (100 students per		-.004		-.003		-.075***
full time principal)		(.004)		(.012)		.011)
arab	-.012	-.012	-.018	-.020	-.234***	-.235***
	(.017)	(.017)	(.044)	(.045)	(.047)	(.046)
Low-SES	-.013	-.013	-.034	-.030	-.206***	-.210***
	(.016)	(.016)	(.042)	(.042)	(.046)	(.046)
Woman	-.011	-.013	-.023	-.028	.041	.045
	(.014)	(.014)	(.040)	(.041)	(.046)	(.046)
Constant	.739***	.784***	1.453***	1.716***	1.622***	1.693***
	(.028)	(.019)	(.073)	(.052)	(.094)	(.070)
N	3137	3133	3137	3133	3137	3133
adj. R^2	.000	-.000	.003	-.001	.027	.027

Notes: $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for the gender treatment.

Table 12

Effects of the interaction between perceived time resources for answering emails and the Arab and low-SES treatments on replies, questions answered and the friendliness index when controlling for municipality level characteristics

	Reply		Questions answered		Friendliness index	
	(1)	(2)	(3)	(4)	(5)	(6)
Time contacts*Arab	.016^{+}		.056*		.002	
	(.009)		(.028)		(.030)	
Time contacts*		.022*		.025		-.005
Low-SES		(.009)		(.029)		(.031)
Time contacts	-.006	-.010	-.011	.003	-.021	-.017
	(.006)	(.006)	(.019)	(.021)	(.023)	(.026)
Arab	-.105^{+}	-.017	-.291^{+}	.023	-.285	-.274**
	(.056)	(.028)	(.175)	(.083)	(.189)	(.092)
Low-SES	-.024	-.147**	-.042	-.182	-.146^{+}	-.116
	(.024)	(.054)	(.078)	(.176)	(.078)	(.193)
Constant	1.200	1.351	-6.480	-6.379	20.449***	20.403***
	(1.454)	(1.461)	(4.764)	(4.829)	(5.348)	(5.329)
N	878	878	878	878	878	878
adj. R^2	.010	.014	.007	.004	.071	.071

Notes: $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for the gender treatment and a number of municipality level controls (population, proportion foreign born, population growth, mean income, and proportion who voted for the far right: Swedish Democrats).

Table 13

Effects of the interaction between perceived time resources for answering emails and the Arab and low-SES treatments on replies, questions answered and the friendliness index after controlling for student test results and municipality level characteristics

	Reply		Questions answered		Friendliness index	
	(1)	(2)	(3)	(4)	(5)	(6)
Time contacts*Arab	.008		.038		-.006	
	(.009)		(.031)		(.032)	
Time contacts*		.020*		.014		-.011
Low-SES		(.009)		(.030)		(.032)
Time contacts	.002	-.005	.006	.016	-.019	-.016
	(.006)	(.007)	(.021)	(.022)	(.024)	(.026)
Arab	-.071	-.026	-.192	.020	-.205	-.240*
	(.060)	(.028)	(.192)	(.089)	(.203)	(.092)
Low-SES	-.032	-.141*	-.061	-.143	-.161^{+}	-.101
	(.027)	(.058)	(.086)	(.184)	(.087)	(.199)
Student test results	.000	.000	-.003	-.002	-.007	-.007
	(.001)	(.001)	(.004)	(.004)	(.005)	(.005)
Constant	1.088	1.218	-9.271^{+}	-9.240^{+}	17.441**	17.374**
	(1.651)	(1.650)	(5.347)	(5.380)	(5.880)	(5.859)
N	775	775	775	775	775	775
adj. R^2	.007	.012	.007	.005	.063	.064

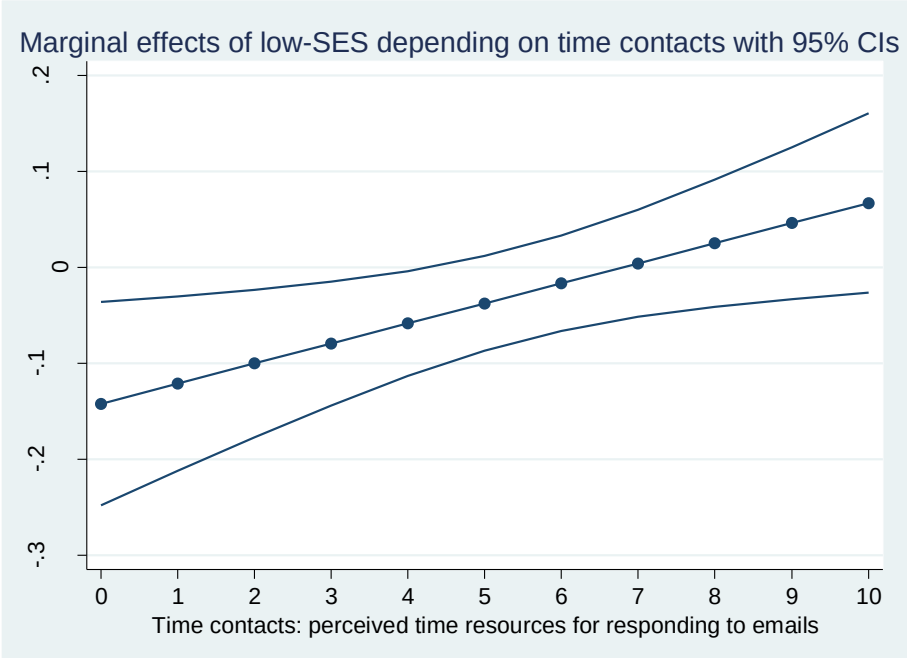
Notes: $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for the gender treatment and a number of municipality level controls.

Table 14
Effects of the interaction between perceived time resources for answering emails and the Arab and low-SES treatments on replies, questions answered and the friendliness index when controlling for municipality level characteristics

	Reply		Questions answered		Friendliness index	
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign student case-loads*Arab	-.007		-.029		-.012	
	(.021)		(.036)		(.063)	
Low-SES student caseloads*Low-SES		.023		.110*		.034
		(.019)		(.048)		(.065)
Foreign student case-loads	-.011		-.028		-.065*	
	(.013)		(.037)		(.030)	
Low-SES student case-loads		-.037**		-.112***		-.082+
		(.014)		(.031)		(.043)
Arab	-.005	-.015	.013	-.027	-.178*	-.237***
	(.025)	(.018)	(.064)	(.045)	(.076)	(.047)
Low-SES	-.011	-.042	-.027	-.163*	-.211***	-.261**
	(.018)	(.028)	(.046)	(.070)	(.047)	(.088)
Constant	.779***	.818***	1.750***	1.852***	1.466***	1.574***
	(.020)	(.023)	(.056)	(.055)	(.065)	(.085)
N	2529	3059	2529	3059	2529	3059
adj. R^2	-.001	.002	-.001	.002	.014	.017

Notes: $^{+}p < .10$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for the gender treatment. Foreign student caseloads =100 students with a foreign background per full time principal. Low-SES student caseloads =100 students with parents who do not have higher education per full time principal.

Figure 4
Marginal effects of low-SES depending on time contacts (Table 3, model 2)



Appendix I. Descriptive statistics for the caseload measure

Figure 5
Histogram for the caseload measure

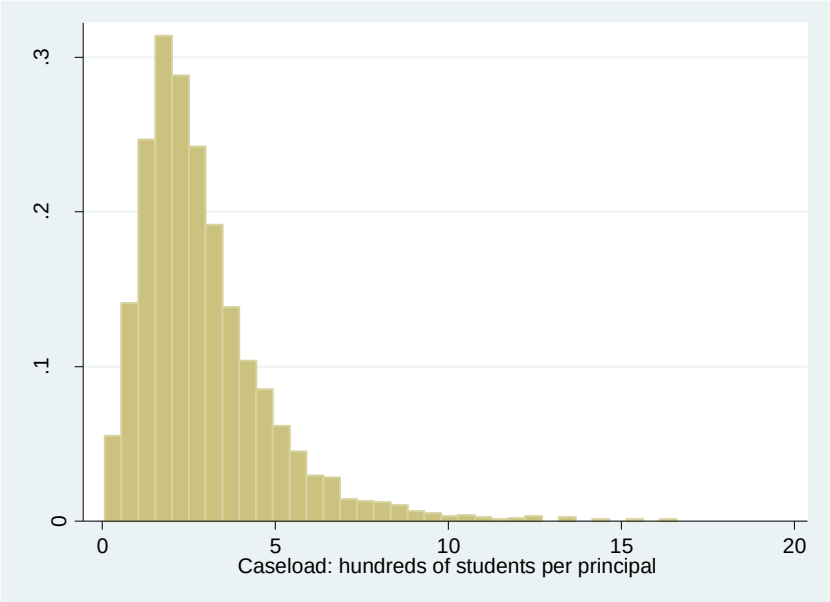


Table 15
Summary statistics for the caseload measure

	Mean	St.dev	Min	Max
Caseload	2.920	1.948	.0667	16.6
Caseload in schools where < 58% of parents have higher education	2.687	1.659	.19	16.2
Caseload in schools where ≥ 58% of parents have higher education	3.154	2.178	.067	16.6
Caseload in schools where < 21% of students have a foreign background	3.300	1.929	.57	15.45
Caseload in schools where ≥ 21% of students have a foreign background	2.656	1.918	.067	16.6

Notes: Cut-off points based on the median in the samples. Proportion of students with higher education was positively associated with caseload (1.941***), and the proportion of students with a foreign background was negatively associated (-.449**). The lower caseloads in less affluent schools may be a result of these schools receiving extra funding from the government and that more affluent students are more likely to attend for-profit schools.

Table 16
Balance between the different treatment groups in the caseload registry data sample

	Arab	Swedish	Low-SES	High-SES	All
Caseloads	2.928	2.911	2.929	2.910	2.920
	(1.923)	(1.973)	(1.921)	(1.975)	(1.948)
Private school	.216	.212	.216	.212	.214
	(.412)	(.409)	(.412)	(.409)	(.410)
For-profit	.131	.128	.141^{+}	.118^{+}	.130
	(.338)	(.334)	(.348)	(.323)	(.336)
Number students	248.0	246.0	249.3	244.8	247.0
	(177.3)	(175.2)	(172.6)	(179.7)	(176.2)
Population municipality (thousands)	144.3	149.6	149.8	137.2	143.3
	(240.3)	(241.1)	(248.4)	(232.7)	(240.6)
Proportion of foreign born in municipality	.186	.184	.188	.181	.185
	(.074)	(.073)	(.075)	(.072)	(.074)
Mean income in municipality (thousands, kr)	298.8	300.0	300.2	298.5	299.4
	(38.70)	(43.20)	(42.30)	(39.68)	(41.00)
Percent voted Swedish Democrats in municipality	13.06	12.92	13.02	12.95	12.99
	(5.098)	(4.948)	(5.063)	(4.986)	(5.024)
N	1577	1556	1551	1582	3133

Notes: $^{+}$ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ t-tests for significant differences in Means (Arab vs Swedish, low-SES vs high-SES). Means with standard deviations in parentheses.