


ARTICLE

Discrimination in marketized welfare services: a field experiment on Swedish schools

Jonas Larsson Taghizadeh*  and Per Adman

Department of Government, Uppsala University, Box 514, Uppsala 751 20, Sweden

*Corresponding author, email: Jonas.larsson@statsvet.uu.se

(Received 24 February 2022; revised 25 October 2022; accepted 13 November 2022; first published online 15 December 2022)

Abstract

Providers' tendency to cream skim clients according to certain sociodemographic traits is widely believed to increase under marketization, and therefore also discrimination. However, due to a lack of experimental research, little is known about the presence of discrimination in marketized welfare services and of the potential drivers of such biased treatment. The lack of research is particularly evident in regard to socioeconomic status (SES) discrimination and publicly financed for-profit providers. Moreover, competition, an important aspect of marketization, has not been investigated. Focusing on the interesting case of the Swedish school sector, we aim to improve knowledge on these matters. In a field experiment, 3,430 elementary school principals were randomly contacted through e-mail by parents with Arabic- or Swedish-sounding names and in low- or high-socioeconomic professions. The fictional parents were interested in placing their children at the school. The Swedish school sector resembles marketized public services in several Western countries. The results show clear signs of ethnic as well as SES discrimination, particularly in regard to more qualitative aspects of the replies. However, we find no significant differences in discrimination between public and private/for-profit schools and depending on the degree of competition in the school market.

Keywords: marketization; cream skimming; field experiment; discrimination; school vouchers; for-profit

Introduction

Today, public services are frequently offered in markets with both public and private providers. These markets are believed to increase cost efficiency and provide citizens with more choice opportunities. Critics, on the other hand, argue that market-based incentives may lead to cream skimming (this argument is common in the public debate; for academic examples, see e.g., Jilke et al., 2018; West et al., 2006; Lacireno-Paquet et al., 2002; Six, 2003), i.e., the intentional selection or avoidance of certain clients in order to reach program goals (cf. Lipsky, 1983). If this criticism is correct, then the ideal of equal treatment – a cornerstone of public sector service – is no

© The Author(s), 2022. Published by Cambridge University Press. This is an Open Access article, distributed under the terms of the Creative Commons Attribution licence (<https://creativecommons.org/licenses/by/4.0/>), which permits unrestricted re-use, distribution, and reproduction in any medium, provided the original work is properly cited.

longer upheld. Such a state of affairs would indeed be challenging for proponents of market-based reforms.

What, then, is known about the presence of discrimination in marketized welfare services, and what do we know about the potential drivers of discrimination in such markets? In fact, despite the rapid increase in private actors in welfare sectors all over the world, experimental research on the matter is conspicuously limited. Field experiments are perhaps the most reliable method of investigating discrimination (cf. Pager and Shepherd, 2008; Quillian, 2006). However, only a small number of published experiments on the subject exist. Interestingly, the findings are mixed, not always indicating that private providers are more likely than public providers to discriminate (Jilke et al., 2018; Milkman et al., 2015; Brown and Hilbig, 2021; Oberfield and Incantalupo, 2021).

However, prior field experiments have several limitations. First and foremost, the effect of *competition levels* between providers has not been studied. Second, we know of only one experiment analyzing discrimination among publicly financed *for-profit providers*, e.g., providers with the strongest incentives to cream skim (Jilke et al., 2018). Finally, previous experiments have ignored socioeconomic status (SES) discrimination. This may be of importance since service providers could be expected to cream skim clients based on SES (see more on our contributions in the next section).

Pursuing a twofold purpose, our article contributes on all of these points. First, we study whether discrimination is more present when local competition is stronger. This purpose is new to the literature, focusing on a previously neglected aspect of marketization. Second, we investigate differences in discrimination levels between different types of providers, including for-profit providers. Using an email correspondence experiment, we study the email replies from 3,430 Swedish elementary school principals who were contacted by fictional parents interested in placing their children at each principal's school. The ethnicities of the fictional parents were randomized (Swedish- or Arabic-sounding names), as were their professions (low or high SES). The study covers all school principals working at a unique school in Sweden. A notable number of the principals are employed at publicly financed for-profit schools, allowing a nuanced picture of the importance of various types of providers. Moreover, high-quality registry data were added to the experiment, allowing the investigation of competition effects, a new approach in the research field.

Discrimination indicators include whether the emails from our fictional parents received responses, how many of the posed questions were answered, and to what extent emails were friendly and promoted the school in question. The latter qualitative dimension we refer to as *promotion* is new to the literature. Promotion takes place when public officials selectively highlight positive service qualities and provide information about service availability, which could be used to cream skim clients.

We are agnostic about the results of our study. Client discrimination may very well occur unrelated to the competition level or type of provider. For example, as argued by Lipsky (1983), public officials tend to cream skim and discriminate in general to manage resource scarcity. Only further empirical investigation can reveal which of these theoretical perspectives is empirically accurate.

Our study focuses on schools in the Scandinavian welfare state of Sweden, i.e., a notably different context than the countries studied in previous experimental

research of this kind (Belgium and the US; see references below). Moreover, the elements and levels of marketization in the Swedish case currently resemble how public services are offered in many sectors in the Western world, making more general conclusions possible (see the institutional context section).

The findings of our study may have important societal implications. The Swedish school choice reform has led to increased school segregation in terms of ethnicity and SES in Sweden (Holmlund et al., 2019), and students with parents with higher education are much more likely to attend private schools (cf. Table 17 in Appendix J). While schools are not allowed to discriminate against students, principals may influence the applicant pool via the kinds of messages and information sent to potential clients. For example, being informed about admission processes and quality differences between schools could affect school choice decisions (Hastings and Weinstein, 2008; Hoxby and Turner, 2013; Kapor et al., 2020). If school segregation is partly a result of differences between schools in such ‘informal discrimination’, then it could have adverse effects not only for the affected students but for social mobility and social cohesion in society more generally. School segregation prevents important intergroup contact from taking place and may create hostility between groups (cf. Pettigrew and Tropp, 2006). Furthermore, the concentration of less affluent students in public schools could reinforce existing social inequalities. Students attending private schools have higher grades and national test scores in Sweden than those attending public schools after adjusting for student composition. While it is not clear whether these differences are a result of grading or school quality differences (Holmlund et al., 2019), the higher scores in private schools could potentially lead to better life and career chances among their students.

Prior Empirical Research and Our Contributions

The last decade has seen a growing number of empirical studies of public officials’ discrimination of clients. Discrimination based on ethnicity has been particularly acknowledged and here refers to unequal treatment based on physical characteristics (e.g., skin/hair) or cultural factors (e.g., name or language). Field experiments have become the preferred design in this literature since they allow a more objective assessment of the extent to which discrimination takes place and do not suffer from unrepresentative samples or social desirability bias (cf. Pager and Shepherd, 2008; Quillian, 2006). They also make it possible to capture discrimination in real-world contexts. Most existing studies are based on correspondence experiments, where emails from fictional persons of varying ethnicities are sent to public officials. In the US, clear signs of ethnic discrimination have been found, even in terms of response rates (for a meta-study focusing especially on the US, see Costa, 2017). Research in Europe also reports biased client treatment, although discrimination levels seem to be lower and primarily concern the quality and friendliness of responses but not response rates (see, for example, Hemker and Rink, 2017; Olsen et al., 2020; Larsson Taghizadeh, 2022a).

Clearly, ethnic discrimination against clients occurs among public officials in Western democracies. Thus, a natural next step is to examine whether some officials or organizations are more likely to discriminate than others. In contrast to our

knowledge on the prevalence of discrimination, much less is known about the contextual factors that can explain differences in discrimination against clients.

One important contextual factor that has repeatedly been suggested to cause biased client treatment is marketization. Marketization here refers to a process where market mechanisms such as competition, private provision and economic incentives are introduced into the public sector to make it more economically efficient (cf. Pollitt and Bouckhaert, 2011). Critics of marketization argue that market-based incentives may lead to “cream skimming” defined as the intentional selection – or avoidance – of certain groups in an effort to reach program goals or work in a cost-efficient manner (cf. Lipsky 1983, 107). However, the experimental discrimination literature does not reveal whether discrimination is more common in marketized welfare services or what the potential drivers of biased treatment in such sectors may be. It almost exclusively concerns public services operating as public monopolies and/or within less competitive environments. Private providers are largely missing, and the effects of different levels of competition between providers are not investigated. While there exists a relatively large non-experimental and quasi-experimental literature on marketization and cream-skimming (see e.g., Zimmer and Guarino, 2013; Lacireno-Paquet et al., 2002; West et al., 2006), more field experiments are needed, as they generally allow a more objective assessment of the extent to which discrimination takes place.

We identify only four published discrimination experiments on public and private welfare services operating within competitive environments (Jilke et al., 2018; Milkman et al., 2015; Brown and Hilbig, 2021; Oberfield and Incantalupo, 2021). All but one of these studies (Jilke et al., 2018) focus on the US and only two of them cover publicly financed services, reducing their generalizability outside these areas. In regard to ethnicity, the results of these studies are mixed. Both Jilke et al. (2018) and Milkman et al. (2015) find more bias among private than public providers when studying Belgian elderly care and US universities, respectively. However, Brown and Hilbig (2021) and Oberfield and Incantalupo (2021) find no such differences when analyzing colleges and elementary schools in the US. In regard to other discrimination forms, Brown and Hilbig (2021) find public-private differences in discrimination against students with criminal records.

Due to the limited number of studies, several aspects of marketization have not been studied. We seek to address these limitations. First and foremost, the effect of *competition levels* between providers has not been studied. This is a crucial aspect of marketization (cf. Pollitt and Bouckhaert, 2011) but has not been taken into account in the experimental literature on discrimination. Second, publicly financed *for-profit providers* have largely been neglected and are therefore included in our study. For-profit providers should have stronger incentives to select applicants who are more profitable and avoid those they regard as costly, but we know of only one experiment on discrimination explicitly investigating such providers (Jilke et al., 2018).

Third, when evaluating the occurrence of biased treatment, prior research has not included treatments based on a potentially crucial characteristic: SES. Discrimination based on SES here refers to unequal treatment based on an individual’s education, occupation and/or income. SES is included in our study, since public officials are likely to cream skim based on this client characteristic and not only on ethnicity. To give an example from the education sector, principals should have

strong incentives to attract students with highly educated parents because such students are probably seen as less resource-demanding. Furthermore, these students seem more likely to raise average grades and to increase the school's reputation. Hence, SES could send clear signals on future costs, and service providers may be particularly prone to cream skim based on this characteristic.

Fourth, with two exceptions (Jilke et al., 2018; Brown and Hilbig, 2021), prior studies investigate only to what extent client emails are replied to. However, the larger literature on discrimination among public officials in Europe mainly finds discrimination in the quality and friendliness of replies (e.g., Hemker and Rink, 2017; Olsen et al., 2020). Although hardly investigated before, this tendency may very well occur in competitive markets, too. In addition, when investigating cream-skimming among providers, we argue that it is particularly important to analyze a new qualitative aspect we refer to as *promotion*. This denotes when public officials highlight positive service qualities and inform potential clients about service availability. This should be a highly relevant communicational aspect, as public officials may cream skim by selectively promoting their own service and thereby increasing certain clients' willingness to use that service. In our case, principals may highlight positive aspects of the schools, e.g., regarding pedagogic quality or the physical school environment, to certain clients and selectively inform certain parents about free slots. As a consequence, parents' willingness to apply to the school in question may be affected.

Theoretical Expectations and Mechanisms

New public management (NPM) reforms, with their emphasis on competition and measurable performance targets, have changed the conditions for all types of providers in many societal sectors (cf. Bohte and Meier, 2000; Bevan and Hood, 2006; Considine et al., 2020). There are several reasons why discrimination and cream skimming tendencies may increase as a result of the introduction of competition. With more competition, providers face greater pressure to decrease costs (cf. Le Grand and Bartlett, 1993; Savas, 2000). Furthermore, the risk of closure due to poor finances or a bad reputation is likely to be higher. While competition over more 'average clients' should be intensified, attracting less resource-demanding clients could potentially be even more beneficial when providers are subject to competition. Such clients may lower costs and improve the provider's reputation and therefore decrease economic risks, often associated with high competition. Finally, the broad use of measurable performance targets causes public officials to focus, sometimes almost exclusively, on measured tasks (cf. Bevan and Hood, 2006). As a consequence, more discrimination might follow among all sorts of providers, regardless of the type of ownership involved (see, e.g., Epp et al., 2014). An example from the school sector is the common use of average grades as a quality indicator, which could induce all kinds of schools to engage in cream skimming when competition is substantial.

The incentives to cream skim should be particularly strong in markets where providers are not fully compensated for recruiting costly clients (such as the Swedish school market; more on this is provided below). Incentives should also

be strong in markets where clients make their choices based on quality indicators that are not adjusted for client composition (e.g., grades, again, as in Sweden). However, even in systems where such objective quality measurements are available, using the school example, families could still see an intrinsic value in placing their child in a school with a certain student composition (high SES, few immigrants).

On the other hand, there are also reasons why we should not expect to find a relationship between competition and discrimination. First, a high level of competition could result in providers also competing over more costly clients, which should decrease discrimination. Second, as argued by Lipsky (1983), public officials in general are likely to use cream skimming. It is one of several coping mechanisms often used to handle a high workload. Discrimination could then follow when staffing and budgetary resources are scarce, regardless of competition levels. Finally, if discrimination is primarily caused by discriminatory attitudes that are not largely affected/activated by the organizational context in which officials operate, then we should not expect to find a causal relationship between competition and any form of discrimination. Biased treatment in general social situations – i.e., not particularly focusing on public officials and clients but on communication between individuals in general – has been related to taste-based motives that are both conscious and unconscious (i.e., motives outside the discriminator's awareness; Greenwald and Banaji, 1995). Hence, a principal may unknowingly discriminate against, e.g., individuals from the Middle East due to subconscious negative feelings toward individuals belonging to that group, unrelated to the competition levels her/his school is exposed to.

We now move on to our theoretical expectations regarding our second research question. As argued by several scholars, there are reasonable reasons to expect private providers to discriminate more than public ones. Translated to the context of our study, private schools (both for- and non-profit) are generally more vulnerable than public schools. Owners do not necessarily protect them, e.g., if they have economic problems or suffer from a bad reputation. Hence, private schools face a higher risk of closure/bankruptcy. By focusing their recruitment efforts on self-perpetuating students from high SES families, private schools can lower their costs for teachers and hence economic risks in general. Such students also automatically improve school results (in terms of grades) and school reputation, which makes future recruitment of more students easier. Everything being equal, from a rational choice perspective, incentives for cream skimming should therefore be stronger among private providers, regardless of whether they are for-profit or not. For-profit providers should have the strongest incentives to adopt cream skimming, however, as their owners can obtain a direct financial gain from not attracting costly or low-performing clients who may damage the provider's reputation.

As with competition, however, there are also sound reasons to expect similar levels of discrimination among all sorts of providers. High workload and coping strategies might be just as frequent among public actors as private for-profit ones. Moreover, taste-based discrimination may of course be present among publicly employed officials and not just among private or even for-profit officials. Furthermore, public providers may also have strong incentives to discriminate against less affluent clients due to the overrepresentation of such clients using their services (cf. Table 17 in Appendix J).

Institutional Context

Sweden is known as a well-functioning, egalitarian and tolerant democracy. However, while the country's economic equality and earnings/education mobility are still high according to OECD standards, they have decreased rapidly in recent decades (OECD, 2015; 2018). Following decades of large-scale immigration, today, the population is rather diverse, ethnically, linguistically and culturally. Approximately 19 per cent is foreign-born, with a substantial share with a background in the Middle East (approximately four per cent of the Swedish population was born in Syria, Iraq, or Somalia alone; Statistics Sweden, 2019). In general, Swedish public opinion shows comparatively positive views on ethnic minorities (World Value Survey, 2014). However, the right-wing populist party the Swedish Democrats has grown to be the third largest, with 17.5 per cent of the votes in the 2018 election. It mobilizes particularly against immigrants with a background in the Middle East, and the general discourse surrounding this group has hardened. Moreover, immigrants from this part of the world have been found to be discriminated against in both the labor and housing markets and, to some extent, in interactions with public officials and politicians (e.g., Bursell, 2014; Larsson Taghizadeh et al., 2022). In sum, from an SES as well as ethnic and political perspective, Sweden might be less divergent than its reputation holds.

Regarding the school sector, a comprehensive school choice reform in the 1990s radically changed the Swedish school system. Today's relatively high frequency of Swedish private schools was one consequence (cf. Table 17 in Appendix J). The proportion of students attending public schools in Sweden is still close to the OECD mean, however (OECD, 2017). What is somewhat unusual about this country is the existence of publicly financed private *for-profit* schools that are allowed to return profits to owners. Besides Sweden, we know of the existence of such schools in the US and Estonia (cf. OECD, 2017). However, in other sectors, publicly financed for-profit providers are internationally common, e.g., healthcare and elderly care (Välfärdsutredningen, 2016; Hoppania et al., 2022). Hence, findings based on the Swedish school sector are of potential relevance both to other countries and to other sectors, although it is difficult to know more precisely to what extent our results are generalizable in this sense.

In Sweden, municipalities (local governments) have administrative responsibility for organizing and financing schools (therefore, competition is measured at this level, c.f. below). Parents can freely choose between schools in their municipality and are encouraged to actively do so. There are 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 to adhere to the Swedish curriculum for compulsory schools. Forty-three per cent of Swedish municipalities do not compensate for low-SES and immigrant students; in the municipalities that do have compensatory systems, only a relatively small part of the school budget is redistributed (SKL, 2018). A voucher system is used. Hence, none of the 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.

The same criteria apply to public schools, although students are always guaranteed a slot in the public school nearest to their home.

Method

In our correspondence experiment, every elementary school principal governing a unique school in Sweden was randomly contacted via emails from fictional parents ($N = 3430$).¹ Emails were sent out in several waves between 11 and 14 January 2020.² A factorial design was employed based on ethnicity, SES, and gender³. Factorial designs are a standard approach in the field (cf. Brown and Hilbig, 2021; Oberfield and Incantalupo, 2021) and are based on the basic idea that experimental units take on all possible combinations of the levels of the factors of interest. Accordingly, the school principals' email addresses were randomly divided into eight groups corresponding to the aliases and SES signals used (see Table 1). An advantage of factorial designs is their efficiency with respect to using 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). As all treatment conditions are randomly distributed in the sample in a balanced way (cf. Appendix C, Table 7), we can estimate the individual independent effects of each treatment in a single model with little loss of efficiency. While it does not make a difference for the results if all three treatments are included in the statistical models or not, again showing that the randomization procedure was successful, we include them in all models to be prudent.

Regarding the ethnicity treatment, Swedish-sounding and Arabic-sounding names are easily distinguished from each other. We tried to avoid stereotypical names that might signal SES or religious beliefs. More specifically, we chose the male name Mahmoud and the female name Fatimah⁴. Both were ascribed the surname Hassan, which is rather common in Sweden (Statistics Sweden, 2017). Commonly, names have a certain SES association. Names of individuals belonging to ethnic minorities are more strongly associated with low education and low income levels and disadvantageous employment positions (Aldrin, 2017; Elchardus and Siongers, 2011). Therefore, in a study such as this one, majority group names should convey SES levels similar to those of minority group names. In a pre-study, we asked upper secondary school principals to assess the education/income levels of school parents with different names (see Appendix D, Table 8). The results are in line with those of previous Swedish studies on names (Aldrin, 2017). We chose the Swedish names Kevin and Melissa, as they were associated with SES levels similar to those of the Arabic names used in this study. The Swedish surname that we employ, Andersson, is very common and unlikely to be associated with any particular SES level.⁵

SES discrimination is investigated by signaling highly skilled professions (dentist) in half of the e-mails and signaling low-skilled professions in the other half (care assistant). These professions are common among both immigrants and native-born individuals (Socialstyrelsen, 2018). In contrast to care assistants, who require only a high school education, dentists require extensive university education as well as a license to practice. Their average wage (47,400 SEK/month) is almost twice that of

Table 1. Our treatments

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

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 1. The letter sent to the school principals.

care assistants’ (24,800 SEK; Statistics Sweden, 2020). Hence, the chosen professions should clearly signal SES.

The emails sent to the principals were written as if sent by someone considering moving to the municipality (see Figure 1; translated from Swedish (see Appendix E, Figure 2). In this way, we prevented unusual names from arousing suspicion among principals in small municipalities where “everyone knows everyone.” We wanted the email questions to be straightforward and not particularly time-consuming to answer but also not too easily answered. We chose three relatively simple but important questions regarding school profile, the registration procedure and open slots.

The coding scheme consisted of 5 variables (see Table 2).⁶ Two of these measured formal aspects, i.e., whether any reply was received within two weeks and how many of the three questions were answered. For the former variable, replies were registered from anyone working at the school or the municipality of the school, excluding autoreplies and noninformative emails from principals who had left their position. The remaining variables were of a more qualitative character, with one of them being an index measuring the friendliness of the emails.⁷ The last two variables belonged to our promotion dimension and were based on information that could be used to motivate parents to choose or not choose a particular school. More precisely, we measured (a) information about open school slots and (b) whether there was any additional positive information in the principal’s reply. The latter variable concerned only information not directly related to the three questions asked by the parent. The first two variables as well as parts of our friendliness index (such as whether the principals use the name of the sender when replying) have been used in

Table 2. Measures of discrimination

Dimension	Variable	Variable description	Mean	Std. dev.
Formal correctness	Reply (1,0)	Whether the recipient responded to the email within 2 weeks	0.749	0.434
Formal correctness	Questions answered (0-3)	How many of the three questions were answered	1.653	1.226
Friendliness	Friendliness index (0-4)	The replier invited future contact (1p), welcomed the sender to the municipality/school (1p), invited the sender to visit the school (1p) and/or used the name of the sender when replying (1p)	0.958	1.028
Promotion	Open slots (1,0)	The replier mentioned that there are definitely/likely open slots at the school	0.249	0.433
Promotion	Positive information (1,0)	The replier highlighted positive information about the school or the municipality	0.284	0.451

previous research (cf. Brown and Hilbig, 2021; Olsen et al., 2020). The promotion variables are new to the literature. The coding of the variables is described more in depth in Appendix I.

Of the original 3,430 emails sent, 3,394 were ultimately included in the dataset. Thirty-one emails bounced back and were therefore excluded. Five emails were omitted, as the principal answered that she/he had left the position and did not forward the email 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 five variables taken together. No signs of spillover or disclosure of the experiment were found. Ethical concerns are discussed in Appendix A.

Results

General Discrimination Effects

Before turning to our research questions, the occurrence of discrimination in general is discussed (i.e., regardless of the type of provider or competition level). Multiple linear regression (OLS with several independent variables) is used in all models to make the results easier to interpret.⁸ Table 3 (row 1) displays the ethnic discrimination effects for all types of schools, i.e., the *treatment effect*⁹ of signing emails with an Arabic-sounding name versus a Swedish-sounding name. In terms of the response rate and the number of questions answered, we find small and non-significant negative effects (models 1 and 2). However, we observe relatively large and statistically significant negative effects for the other variables. Responses to emails signed with Arabic names are rated as less friendly (model 3, -0.192 friendliness points)¹⁰, are less likely to indicate that there are open slots in the school (model 4, 3.2 percentage points less likely) and are less likely to contain positive information about the school or municipality (model 5, 3.9 percentage points less likely). For socioeconomic discrimination, tendencies are similar. We find statistically significant evidence of low SES parents being discriminated against more for all qualitative

Table 3. Treatment effects on five discrimination outcomes of having an Arabic-sounding name, having a low-SES profession and being a woman

	Reply	Questions answered	Friendliness index	Open slots	Positive information
	(1)	(2)	(3)	(4)	(5)
Arab	-0.008 (0.017)	-0.022 (0.045)	-0.192*** (0.036)	-0.032* (0.014)	-0.039* (0.016)
Low SES	-0.014 (0.016)	-0.037 (0.041)	-0.126*** (0.034)	-0.049*** (0.014)	-0.063*** (0.017)
Woman	-0.016 (0.013)	-0.030 (0.039)	0.019 (0.034)	0.002 (0.015)	0.000 (0.017)
Constant	0.769*** (0.016)	1.698*** (0.042)	1.108*** (0.046)	0.289*** (0.018)	0.355*** (0.018)
N	3394	3394	3394	3394	3394
adj. R ²	-0.000	-0.000	0.012	0.004	0.006

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level.

and promoting aspects of the replies (row 2, models 3-5). Hence, both for ethnicity and SES, discrimination is found regarding the informal and promoting aspects of the emails but not regarding formal aspects, a result highly similar to that of prior research on general client discrimination effects in Sweden and Europe in general (Hemker and Rink, 2017; Olsen et al., 2020). The discrimination coefficients remain largely unchanged after controlling for school ownership and competition (see Appendix F, Table 12), suggesting that these contextual factors may not be the most important drivers behind the effects we observe.

Competition Effects

Having observed the expected general discrimination effects, we now turn to our research question regarding competition effects. The relationship between school competition in the municipality and discrimination is investigated in Table 4. The Herfindahl–Hirschman Index (HHI, Rhoades, 1993) is our main measure of competition. It is well established and frequently used to examine, e.g., the school sector. We calculate it by squaring the market share (proportion) of each school competing in the local school market (municipality) and then summarizing the resulting numbers (based on high-quality registry data from Statistics Sweden). The result is proportional to the average market share, weighted by market share. As such, it can range from 0 to 1.0, moving from a large number of schools with small market shares to a single monopolistic school. Swedish school markets are on average quite competitive, although there are notable differences between municipalities. The mean HHI in the dataset is 0.084; the lowest value is 0.006, the highest is 1, and the standard deviation is 0.088. The models include a large number of controls. Hence, the results should not be caused by other municipality characteristics, such as population size.

Models 1, 3, 5, 7, and 9 in Table 4 concern ethnic discrimination. The results are mixed. Negative interaction coefficients in models 1, 7 and 9 (row 4) indicate that

Table 4. Treatment effects on five discrimination outcomes of having an Arabic-sounding name and having a low-SES profession, by level of school competition in the municipality (HHI)

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HHI	0.133 (0.154)	0.029 (0.141)	0.054 (0.449)	-0.054 (0.407)	0.867* (0.375)	0.862* (0.362)	0.285+ (0.165)	0.162 (0.157)	0.201 (0.156)	0.063 (0.166)
Arab	0.001 (0.026)	-0.009 (0.018)	-0.031 (0.064)	-0.027 (0.045)	-0.189*** (0.048)	-0.187*** (0.036)	-0.021 (0.020)	0.031* (0.015)	-0.029 (0.021)	-0.039* (0.016)
Low SES	-0.012 (0.016)	-0.025 (0.022)	-0.031 (0.041)	-0.060 (0.059)	-0.112*** (0.033)	-0.115* (0.047)	-0.044** (0.014)	-0.061** (0.019)	-0.057*** (0.016)	-0.078*** (0.022)
HHI *Arab	-0.115 (0.195)		0.047 (0.527)		0.027 (0.421)		-0.111 (0.189)		-0.108 (0.171)	
HHI *Low SES		0.147 (0.171)		0.350 (0.502)		0.046 (0.472)		0.203 (0.188)		0.247 (0.201)
Constant	-1.540 (1.296)	-1.577 (1.301)	-8.575** (3.200)	-8.665** (3.202)	3.435 (2.638)	3.423 (2.653)	1.883+ (1.111)	1.831 (1.112)	1.635 (1.130)	1.572 (1.133)
N	3394	3394	3394	3394	3394	3394	3394	3394	3394	3394
adj. R ²	0.002	0.002	0.005	0.005	0.041	0.041	0.021	0.021	0.023	0.024

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for all treatments and a number of municipal level variables (municipality population, proportion foreign born, population growth, mean income, proportion voted for Swedish Democrats). The choice of municipality controls is discussed in depth in appendix G. Larger HHI values indicate lower competition.

parents with Arabic-sounding names are treated worse in less-competitive school markets. The reverse is true for models 3 and 5. In the case of SES discrimination, there are positive interaction coefficients in all models, indicating that low SES parents are treated better in less competitive school markets (row 5, models 2, 4, 6, 8 and 10). However, none of the interaction effects are statistically significant and in most of the models, the coefficients are very small.¹¹ Furthermore, most of the individual treatment effects (rows 2-3 in models 5-10) of the low SES and Arabic-sounding names remain substantially and statistically significant even when the competition interactions are included. A complementary measure of competition also showed statistically nonsignificant and very weak interaction effects (see Table 11 in Appendix F). In sum, the overall picture is that biased treatment does not appear to be strongly related to competition. The results in Table 4 as well as Tables 5 and 6 are robust to numerous robustness tests, including controls for potential confounders such as student composition and student test scores as well as removing all controls (see Appendices G and H, Tables 13 to 16).

Provider Effects

We now turn to our second research question regarding provider effects. For ethnic discrimination, the interaction coefficients in rows 4-5 in Table 5 show private/public and for-profit/public differences in discrimination (see also Table 9). Public schools refer to all schools owned by Swedish municipalities (78.2 per cent of all observations). Private schools refer to all schools not owned by Swedish municipalities, including corporations, cooperatives and non-profit associations and foundations (21.8 per cent). For-profit schools are a subset among private schools and refer to schools run as corporations (13.1 per cent).¹² We analyze the latter schools in separate models, as cream skimming and discrimination may be even more likely among this private school subset. By testing the interaction effects between the type of ownership and the Arabic name treatment, we investigate whether private and for-profit schools are more likely to discriminate than public schools. If ethnic discrimination is more common among private schools, the interaction coefficients in row 4 should be large, negative and statistically significant. However, the table shows only one statistically significant interaction effect. In addition, it is positive, suggesting that private schools more frequently inform Arabic aliases about open school slots (model 7: OLS: $P=0.022$, Logit: $P=0.052$). The interaction effect is not robust to multiple hypothesis testing and is not statistically significant above the 5-percent level when using logistic regression. In addition, the other interaction coefficients for private schools (row 4, models 1, 3, 5, and 9) generally show substantially very small interaction effects. The same goes for for-profit schools (row 5).¹³ In sum, the results do not indicate clear differences in ethnic discrimination between private schools and public schools or between for-profit and public schools.

Similarly, the models in Table 6 show private/public and for-profit/public differences in SES discrimination. Only the interaction effect in model 10 is statistically significant (row 5, OLS: $P=0.035$, Logit: 0.007). This shows that for-profit schools are less likely than public schools to inform low SES parents about positive aspects of the school or municipality. The direction of the other interaction effects in rows 4-5 shows a similar pattern (8 of 10 coefficients are negative). Some of these could be

Table 5. Treatment effects on five discrimination outcomes of having an Arabic-sounding name, by school type (private/for-profit school vs. a public school)

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Private	-0.021 (0.029)		0.078 (0.092)		-0.020 (0.062)		-0.159*** (0.027)		-0.157*** (0.022)	
For-profit		-0.031 (0.033)		0.043 (0.110)		-0.019 (0.073)		-0.181*** (0.030)		-0.151*** (0.025)
Arab	-0.013 (0.020)	-0.013 (0.020)	-0.014 (0.052)	-0.014 (0.052)	-0.200*** (0.043)	-0.200*** (0.043)	-0.048** (0.018)	-0.048** (0.018)	-0.045* (0.019)	-0.045* (0.019)
Private *arab	0.020 (0.035)		-0.038 (0.102)		0.040 (0.081)		0.076* (0.033)		0.031 (0.036)	
For-profit *arab		0.018 (.044)		-0.059 (0.126)		-0.051 (0.099)		0.054 (0.040)		0.006 (0.038)
Constant	0.773*** (0.018)	0.772*** (0.019)	1.681*** (0.048)	1.670*** (0.048)	1.112*** (0.049)	1.113*** (0.049)	0.323*** (0.019)	0.319*** (0.020)	0.368*** (0.019)	0.374*** (0.020)
N	3394	3099	3394	3099	3394	3099	3394	3099	3394	3099
adj. R ²	-0.001	-0.001	-0.001	-0.001	0.011	0.013	0.018	0.020	0.022	0.020

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. School ownership reference category: public school. Standard errors in parentheses are clustered at the municipal level. The models also include controls for the SES and gender treatments.

Table 6. Treatment effects on five discrimination outcomes of having a low SES profession, by school type (private/for-profit school vs. a public school)

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Private	-0.007 (0.024)		0.121 ⁺ (0.071)		-0.021 (0.057)		-0.107*** (0.023)		-0.130*** (0.022)	
For-profit		-0.018 (0.033)		0.064 (0.089)		-0.052 (0.074)		-0.139*** (0.022)		-0.106*** (0.029)
Low SES	-0.013 (0.018)	-0.013 (0.018)	-0.011 (0.047)	-0.011 (0.047)	-0.135*** (0.037)	-0.135*** (0.037)	-0.041* (0.016)	-0.042** (0.016)	-0.056** (0.018)	-0.056** (0.018)
Private *Low SES	-0.008 (0.044)		-0.123 (0.107)		0.040 (0.083)		-0.027 (0.029)		-0.023 (0.030)	
For-profit *Low SES		-0.008 (0.051)		-0.093 (0.120)		0.013 (0.108)		-0.028 (0.033)		-0.077* (0.037)
Constant	0.770*** (0.017)	0.770*** (0.018)	1.671*** (0.045)	1.667*** (0.047)	1.112*** (0.047)	1.118*** (0.049)	0.311*** (0.019)	0.313*** (0.019)	0.362*** (0.019)	0.368*** (0.020)
N	3394	3099	3394	3099	3394	3099	3394	3099	3394	3099
adj. R ²	-0.001	-0.001	-0.000	-0.001	0.011	0.013	0.017	0.016	0.022	0.021

Note: ⁺ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. School ownership reference category: public school. Standard errors in parentheses are clustered at the municipal level. The models also include controls for the ethnicity and gender treatments.

considered to be of substantial size.¹⁴ However, the parameters carry much uncertainty, as they are not statistically significant. Hence, even though there is a slight tendency of private schools discriminating more based on SES, the safest conclusion is that the hypothesis does not gain support.

Conclusion

It is often argued that market-based incentives may lead providers of public services to discriminate clients. However, previous experimental research is limited, and the findings are mixed, not always indicating that private providers are more likely to discriminate.

In this study, we addressed several limitations in prior scholarly work on discrimination in marketized welfare services. First, we focused not only on ownership and acknowledged the important marketization aspect of the competition level between providers. Second, we included publicly financed private for-profit providers, which should have the strongest incentives to cream skim. Third, we acknowledged discrimination based on SES, a client characteristic based on which service providers could be expected to cream skim clients. Fourth, we analyzed the quality and friendliness of replies and not only whether emails were replied to or not. All of these points should facilitate the discovery of discrimination effects potentially related to specific aspects of the marketization reform such as the introduction of private providers and competition.

Nevertheless, our results do not indicate that discrimination in the Swedish school market varies depending on these factors. We do not find clear evidence of principals at private schools discriminating along ethnic lines more than principals at public schools, and the same is true when we compare discrimination between for-profit schools and public schools. This goes both for ethnic and socioeconomic discrimination, although we – especially when looking at the substantial sizes of the coefficients – find some traces of private/for-profit schools treating low-SES parents less well than high-SES parents, although almost all of these effects are not statistically significant. We also do not find any robust impact of the level of market competition among schools on discrimination. For ownership and competition to qualify as the main drivers of discrimination, more consistent evidence would be needed.

What we do find is a strong *general discrimination effect*. In line with the more conventional and extensive research on public officials' treatment of clients, which tends to not focus on marketized welfare services, principals working at both public and private schools were found to discriminate based on ethnicity as well as SES from the qualitative aspects of the emails. Here, our results highlight the importance of a new qualitative dimension of email replies (promotion) that could potentially be used to cream skim clients. Our Arabic and low SES parents were less likely to be informed about positive aspects of the schools and were also less likely to be informed about open school slots. This finding is of substantial importance, as discrimination of this kind could be used to deter certain parents from asking further questions or visiting the school and ultimately affect their choice of school. The occurrence of such biased treatment conflicts with fundamental principles of public sector service such as equal treatment.

Our best guess is that other discriminatory mechanisms – also present among public schools – are most likely at work behind the discrimination effects we found.

For example, discrimination may primarily be a result of widespread conscious or unconscious discriminatory attitudes (cf. Larsson Taghizadeh, 2022b). What speaks in favor of this view is that similar discrimination effects as those found here have been found in other public services operating under different conditions (e.g., public monopolies) in other countries (cf. Costa, 2017). In addition, our results do not exclude the possibility that marketization/NPM reforms introduce incentives that result in some degree of discrimination among all providers (including public ones), regardless of ownership and levels of competition. Only a longitudinal study wherein discrimination levels are captured before and after such reforms would be able to safely determine whether this is the case. Another avenue for future research would be to utilize other methods to capture discrimination. Private providers may have (exclusive) opportunities to discriminate that correspondence experiments such as this cannot capture. For example, in the Swedish case, such providers may manipulate queues (which they administer themselves), that may be studied using a more case-based method. While our study provides valuable findings regarding unequal treatment of clients following a controversial marketization reform, more studies covering other countries and/or utilizing other methods are needed before we can draw safe conclusions regarding the effects of marketization on discrimination.

Funding and data availability. An anonymized version of the data underlying this article will be shared on reasonable request to the corresponding author. The design was approved by the Swedish ethical review board (see 2017/234 and 2018/371). This work was supported by the Swedish Research Council for Health, Working Life and Welfare (2019-00504).

Conflict of interest disclosure. The authors declare none.

Notes

1 In other words, no schools are shared between principals, and the study is very close to being a population study of all elementary school principals in Sweden. For more information on how our list of principal email addresses was generated, see Appendix B.

2 The study follows the preregistered preanalysis plan (https://osf.io/3bju7?view_only=3ed1f21c95844935b379396f8e77588f). The preanalysis plan covers a large project that includes numerous hypotheses to be tested in several articles.

3 The randomization also took gender into account to ensure that the results are generalizable to both men and women and to test for gender-based discrimination in other articles.

4 The name Fatimah may signal religious beliefs, as one of the daughters of Mohammad carried this name. However, we do not think this is a problem, as she is likely not well known, especially among non-Muslims. Only 0.5 per cent (18/3430) of the principals who replied to our emails had Middle Eastern-sounding names.

5 It would be problematic if the names chosen by highly educated immigrants from Arabic countries were less likely to be perceived as Arabic names than those given by immigrants with a low level of education (cf. Gaddis, 2017). However, to our knowledge, immigrants from Arabic countries in Sweden rarely choose traditionally Swedish names (or surnames) for their children. Furthermore, most of those who replied to the emails had Swedish-sounding names and are therefore unlikely to be able to distinguish between Arabic-sounding names in terms of SES.

6 Note that the preanalysis plan included slightly different sets of dependent variables. The reply and question-answered variables used in the article are identical to those used in the preanalysis plan. The friendliness variables mentioned in the plan (welcomed, future contact, and use name) were merged into a single index in the article (together with one newly created friendliness variable, i.e., visit school), as they can be treated as one dimension based on the principal component analyses. The preanalysis plan opened up the possibility of creating indices as long as the factor loadings of each indicator were at least 0.4. The

additional information variable mentioned in the preanalysis plan was refined into the positive information variable in the article, as most of the additional information was positive. Finally, the article included a new open school slot variable that appeared inductively when our assistants reviewed the email responses, which is theoretically interesting from a cream-skimming perspective.

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 satisfied the Kaiser criterion was 1.5. This dimension explained 37 per cent of the total variance. The factor loadings varied between 0.44 and 0.54.

8 The results remain almost identical if we instead run logit models for the models with binary outcomes and poisson models for the count outcomes in terms of statistical significance and the direction of the effects. We comment on exceptions to this rule in the text.

9 We chose to cluster our standard errors at the municipal level to be on the safe side since observations in the same municipality may not be perfectly independent from each other. However, we have also run all models without the clustering and it does not influence the results substantially.

10 See Table 10 in Appendix F for the individual effects of the friendliness variables. Statistically significant discrimination effects are found for all friendliness variables except the welcomed variable in terms of ethnicity and all variables except the name variable in terms of SES.

11 When interpreting substantial effect sizes of the interaction variables, it should be kept in mind that the HHI index is rather skewed (mean=0.084 and std=0.088) and that the questions answered and friendliness index run from 0-3 and 0-4 respectively (the remaining measures are dichotomous). Therefore, e.g., the predicted difference in discrimination level – when going from the HHI mean minus one standard deviation to the HHI mean plus one standard deviation – is negligible for most models. The only possible exceptions are the effect of HHI*Arab on positive information and the effect of HHI*SES on open slots and positive information, although these effects are also of a rather modest size.

12 Cooperatives are not counted as for-profit because their purpose is to benefit their members, who participate in running the schools, and not to generate profit for shareholders.

13 C.f., the logic for evaluating substantial effects employed in endnote 11 above. The only possible exception here is model 9, i.e., the positive interaction effect on positive information in Table 5, being approximately 3 percentage points (c.f., the statistically significant general discrimination effect in Table 3, model 5).

14 Using similar logic as that in endnotes 11 and 13 above, for private schools, the effects in model 3 and 7 in Table 6 might be considered to be of substantial size, i.e., on questions answered and open slots (3-4 percentage points' difference). The same applies to models 4 (questions answered) and 8 (open slots, 3 percentage points' difference) for for-profit schools.

References

- Aldrin, E.** (2017), 'Assessing Names? Effects of Name-Based Stereotypes on Teachers' Evaluations of Pupils' Texts', *Names*, **65**(1), 3–14.
- Bevan, G. and Hood, C.** (2006), 'What's measured is what matters: targets and gaming in the English public health care system', *Public Administration*, **84**(3), 517–538.
- Bohte, J. and Meier, K. J.** (2000), 'Goal displacement: Assessing the motivation for organizational cheating', *Public Administration Review*, **60**(2), 173–182.
- Brown, J. and Hilbig, H.** (2021), 'Locked Out of College: When Admissions Bureaucrats Do and Do Not Discriminate', *British Journal of Political Science*, 1–11.
- Bursell, M.** (2014), 'The multiple burdens of foreign-named men—evidence from a field experiment on gendered ethnic hiring discrimination in Sweden', *European Sociological Review*, **30**(3), 399–409.
- Collins, L. M., Dziak, J. J. and Runze, L.** (2009), 'Design of experiments with multiple independent variables: a resource management perspective on complete and reduced factorial designs', *Psychological Methods*, **14**(3), 202.
- Considine, M., O'Sullivan, S., McGann, M. and Nguyen, P.** (2020), 'Contracting personalization by results: Comparing marketization reforms in the UK and Australia', *Public Administration*, **98**(4), 873–890.
- Coppock, A.** (2019), 'Avoiding Post-Treatment Bias in Audit Experiments', *Journal of Experimental Political Science*, **6**(1), 1–4.
- Costa, M.** (2017), 'How responsive are political elites? A meta-analysis of experiments on public officials', *Journal of Experimental Political Science*, **4**(3), 241–254.

- Elchardus, M. and Siongers, J. (2011), 'First names as collective identifiers: an empirical analysis of the social meanings of first names', *Cultural Sociology*, 5(3), 403–422.
- Epp, C. R., Maynard-Moody, S. and Haider-Markel, D. P. (2014), *Pulled over: How police stops define race and citizenship*, Chicago, IL: University of Chicago press.
- Gaddis, S. M. (2017), 'How black are Lakisha and Jamal? Racial perceptions from names used in correspondence audit studies', *Sociological Science*, 4, 469–489.
- Greenwald, A. G. and Banaji, M. R. (1995), 'Implicit social cognition: attitudes, self-esteem, and stereotypes', *Psychological Review*, 102(1), 4.
- Hastings, J. S. and Weinstein, J. M. (2008), 'Information, school choice, and academic achievement: Evidence from two experiments', *The Quarterly Journal of Economics*, 123(4), 1373–1414.
- Hemker, J. and Rink, A. (2017), 'Multiple dimensions of bureaucratic discrimination: Evidence from German Welfare Offices', *American Journal of Political Science*, 61(4), 786–803.
- Holmlund, H., Sjögren, A. and Öckert, B. (2019), Jämlikhet i möjligheter och utfall i den svenska skolan. *Bilaga 7. SOU 2019: 40*.
- Hoppania, H. K., Karsio, O., Näre, L., Vaittinen, T. and Zechner, M. (2022), Financialization of Eldercare in a Nordic Welfare State, *Journal of Social Policy*, 1–19.
- Hoxby, C. and Turner, S. (2013), 'Expanding college opportunities for high-achieving, low income students', <https://www8.gsb.columbia.edu/programs/sites/programs/files/finance/Applied%20Microeconomics/Caroline%20Hoxby.pdf> (accessed 05.05.2021).
- Jilke, S., Van Dooren, W. and Rys, S. (2018), 'Discrimination and Administrative Burden in Public Service Markets: Does a Public–Private Difference Exist?', *Journal of Public Administration Research and Theory*, 28(3), 423–439.
- Kapor, A. J., Neilson, C. A. and Zimmerman, S. D. (2020), 'Heterogeneous beliefs and school choice mechanisms', *American Economic Review*, 110, 1274–1315.
- Lacireno-Paquet, N., Holyoke, T. T., Moser, M. and Henig, J. R. (2002), 'Creaming versus cropping: Charter school enrollment practices in response to market incentives', *Educational Evaluation and Policy Analysis*, 24(2), 145–158.
- Larsson Taghizadeh, J. (2022a), 'Are more affluent parents treated more favourably by elementary school principals? Socioeconomic discrimination among local Swedish public officials', *Local Government Studies*, 1–33. <https://doi.org/10.1080/03003930.2021.1986391>
- Larsson Taghizadeh, J. (2022b), 'The effects of implicit biases on real-life client discrimination among public officials', *Research & Politics*, 9(2).
- Larsson Taghizadeh, J., Åström, A. and Adman, P. (2022), 'Do politicians discriminate against ethnic minority constituents? A field experiment on social interactions between citizens and Swedish local politicians', *Parliamentary Affairs*, 75(1), 154–172.
- Le Grand, J. and Bartlett, W. (1993), *Quasi-markets and social policy*, London: Palgrave Macmillan.
- Lipsky, M. (1983), *Street-Level Bureaucracy: The Dilemmas of the Individual in Public Service*, New York, NY: Russell Sage Foundation.
- Milkman, K. L., Akinola, M. and Chugh, D. (2015), 'What happens before? A field experiment exploring how pay and representation differentially shape bias on the pathway into organizations', *Journal of Applied Psychology*, 100(6), 1678.
- Oberfield, Z. W. and Incantalupo, M. B. (2021), 'Racial Discrimination and Street-Level Managers: Performance, Publicness, and Group Bias', *Public Administration Review*, 81(6), 1055–1070.
- OECD. (2015), 'Sweden Policy brief. Inequality', <https://www.oecd.org/sweden/sweden-achieving-greater-equality-of-opportunities-and-outcomes.pdf> (accessed 05.05.2021).
- OECD. (2017), 'School choice and school vouchers: An OECD perspective', <https://www.oecd.org/education/School-choice-and-school-vouchers-an-OECD-perspective.pdf> (accessed 05.05.2021).
- OECD. (2018), 'A broken social elevator? How to promote social mobility. How does Sweden compare?', <https://www.oecd.org/sweden/social-mobility-2018-SWE-EN.pdf> (accessed 05.05.2021).
- Olsen, A.L., Kyhse-Andersen, J.H. and Moynihan, D. (2020), 'The unequal distribution of opportunity: A national audit study of bureaucratic discrimination in primary school access', *American Journal of Political Science*.
- Pager, D. and Shepherd, H. (2008), 'The sociology of discrimination: Racial discrimination in employment, housing, credit, and consumer markets', *Annual Review of Sociology*, 34, 181–209.

- Pettigrew, T. F., and Tropp, L. R.** (2006), A meta-analytic test of intergroup contact theory, *Journal of Personality and Social Psychology*, **90**(5), 751.
- Pollitt, C. and Bouckhaert, G.** (2011), *Public Management Reform: A Comparative Analysis*, Oxford: Oxford University Press.
- Quillian, L.** (2006), 'New approaches to understanding racial prejudice and discrimination', *Annual Review of Sociology*, **32**, 299–328.
- Rhoades, S. A.** (1993), 'The herfindahl-hirschman index', *Federal Reserve Bulletin*, **79**, 188–189.
- Savas, E. S.** (2000), *Privatization and Public – Private Partnerships*, New York, NY: Chatham House.
- Six, P.** (2003), 'Giving consumers of British public services more choice: what can be learned from recent history?', *Journal of Social Policy*, **32**(2), 239–270.
- SKL.** (2018), 'Socioekonomisk resursfördelning i skola och förskola', <https://webbutik.skr.se/bilder/artiklar/pdf/7585-697-1.pdf?issuusl=ignore> (accessed 05.05.2021).
- Socialstyrelsen.** (2018), 'Statistik om Legitimerad Hälso- Och Sjukvårdspersonal 2016 Samt Arbetsmarknadsstatus 2015', <https://www.socialstyrelsen.se/statistik-och-data/statistik/statistikammen/halso-och-sjukvardspersonal/> (accessed 05.05.2021).
- Statistics Sweden.** (2017), 'Swedish Name Register', <https://www.scb.se/hitta-statistik/sok/?query¼sök6pa°6namnandlang¼sv> (accessed 05.05.2021).
- Statistics Sweden.** (2019), 'Statistikdatabasen', <https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/> (accessed 05.05.2021).
- Statistics Sweden.** (2020), 'Lönesök – Hur mycket tjänar?', <https://www.scb.se/hitta-statistik/sverige/isiffror/lonelok/Search/?lon=tandl%C3%A4kare> (accessed 05.05.2021).
- Välfärdsutredningen.** (2016), *Ordning och reda i välfärden*, Stockholm: Finansdepartementet.
- West, A., Ingram, D. and Hind, A.** (2006), "Skimming the cream" admissions to charter schools in the United States and to autonomous schools in England', *Educational Policy*, **20**(4), 615–639.
- World Value Survey.** (2014), 'World Values Survey Wave 6: 2010-2014', <http://www.worldvaluessurvey.org/WVOnline.jsp> (accessed 05.05.2021).
- Zimmer, R. W. and Guarino, C. M.** (2013), 'Is there empirical evidence that charter schools "push out" low-performing students?', *Educational Evaluation and Policy Analysis*, **35**(4), 461–480.

Appendix

A. 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, we strived to decrease the potential negative effects of the experiment as much as possible. First, the principals were anonymized, and only aggregate-level tendencies are shown and not specific answers. The emails and the discrimination effects are presented in a way that prevents identification of the schools or the municipalities from which they were sent. Second, we minimized 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. To the best of our knowledge, no similar experiment has been conducted on the research subjects, and the authors will not implement any future experiments on them (without seeking consent) to avoid affecting future encounters between them and citizens. The design was approved by the Swedish ethical review board.

B. Generating the List of Principals

To minimize 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 those on the homepages (according to web-scraping), the addresses on the homepages were used, (5) When no email for directly contacting 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 principals being excluded from the final sample. Approximately 5 percent of the emails (180/3430) were answered by nonprincipals (e.g., an administrator at the school or municipality) according to the responses.

C. Randomization Checks

Most of the studies cited in this article did not test for balance across the treatment groups. Hence, they provided no direct evidence that the randomization was successful. Thanks to the availability of register data, balance can be tested in the present study. As shown in Table 7, the randomization procedure resulted

Table 7. Balance between the different treatment groups in the dataset, for school and municipal level variables (means)

	Swedish	Arab	High-SES	Low-SES	All
<i>School level variables</i>					
Private school	0.216 (0.411)	0.220 (0.415)	0.213 (0.410)	0.223 (0.416)	0.218 (0.413)
For-profit school	0.129 (0.336)	0.133 (0.339)	0.118 ⁺ (0.322)	0.145 ⁺ (0.352)	0.131 (0.338)
Proportion with parents with higher education	0.583 (0.168)	0.586 (0.172)	0.586 (0.171)	0.583 (0.169)	0.585 (0.170)
Number of students	243.7 (174.7)	246.8 (178.9)	242.0 (180.6)	248.5 (172.5)	245.2 (176.6)
<i>Municipal level variables</i>					
Population municipality (thousands)	151.4 (252.3)	149.1 (247.0)	144.7 (243.0)	155.8 (256.0)	150.2 (249.6)
Population growth (proportion)	1.008 (0.009)	1.009 (0.009)	1.008 (0.009)	1.009 (0.009)	1.009 (0.009)
Prop foreign born in municipality	0.185 (0.074)	0.187 (0.075)	0.183 (0.072)	0.190 (0.076)	0.186 (0.074)
Percent voted Swedish democrats in municipality	12.90 (5.008)	13.04 (5.092)	12.98 (5.018)	12.96 (5.083)	12.97 (5.050)
Mean income in municipality (thousands, kr)	300.5 (43.04)	299.1 (39.14)	298.9 (39.70)	300.7 (42.51)	299.8 (41.13)
N	1692	1702	1700	1694	3394

Notes: ⁺ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$; t tests are for significant differences in means. Standard deviations are in parentheses. Units of analysis = schools.

in an overall high level of balance between the different treatments in terms of both school and municipality characteristics. There was only one case where significant differences in the means were found between the treatment groups (tested with t tests). For-profit schools were slightly more likely to receive emails from low-SES aliases than from high-SES aliases. However, as the private school variable was generally well balanced between the treatment groups and often resulted in similar effects as the public variable, it is unlikely that the imbalance for for-profit schools was driving the results. To be on the safe side, entropy balancing and coarsened exact matching methods were used to determine whether increasing the balance in this variable would change the results. However, these additional analyses resulted in highly similar results.

D. Choice of Swedish Names

Before conducting the experiment, a smaller study was conducted where upper secondary school administrators (i.e., not from an elementary school) had to rate names on a 100-point scale based on socioeconomic status (0=low income/education, 100=high income/education). Of the 715 schools that received the invitation, 252 (35.2 percent) answered all survey questions.

Table 8. Results from our study where upper secondary school administrators rated 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

E. Original Swedish Letters Sent to the Principals

Frågor om er skola

Hej! Mitt namn är *Namm*, 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 *Namm Efternamn*

Figure 2. The original letters in Swedish.

F. Additional Analyses

Table 9. Treatment effects on five discrimination outcomes of having an Arabic/Swedish-sounding name, a low/high-SES profession and being a woman, depending on school type (public or private)

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Public	Private	Public	Private	Public	Private	Public	Private	Public	Private
Arab	-0.013 (0.020)	0.008 (0.031)	-0.013 (0.052)	-0.043 (0.086)	-0.201*** (0.043)	-0.165* (0.069)	-0.048** (0.018)	0.030 (0.025)	-0.045* (0.019)	-0.011 (0.029)
Low-SES	-0.013 (0.018)	-0.022 (0.039)	-0.011 (0.047)	-0.132 (0.093)	-0.135*** (0.037)	-0.098 (0.076)	-0.042** (0.016)	-0.075** (0.026)	-0.056** (0.018)	-0.082** (0.028)
Woman	-0.012 (0.016)	-0.029 (0.027)	-0.032 (0.045)	-0.030 (0.076)	0.029 (0.039)	-0.013 (0.074)	0.007 (0.018)	-0.012 (0.024)	-0.008 (0.019)	0.034 (0.023)
Constant	0.771*** (0.019)	0.763*** (0.029)	1.668*** (0.049)	1.803*** (0.085)	1.112*** (0.050)	1.096*** (0.082)	0.317*** (0.020)	0.183*** (0.026)	0.370*** (0.020)	0.203*** (0.026)
N	2654	740	2654	740	2654	740	2654	740	2654	740
adj. R ²	-0.000	-0.002	-0.001	-0.001	0.013	0.005	0.004	0.008	0.005	0.010

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level.

Table 10. Treatment effects on the four friendliness measures included in the friendliness index of having a low/high-SES profession, an Arabic/Swedish-sounding name and being a woman

	Future contact (1)	Welcomed (2)	Visit (3)	Use name (4)
Arab	-0.054*** (0.013)	-0.001 (0.016)	-0.045*** (0.011)	-0.092*** (0.017)
Low-SES	-0.025+ (0.014)	-0.056*** (0.013)	-0.046*** (0.012)	0.001 (0.019)
Woman	-0.017 (0.015)	0.022 (0.014)	0.012 (0.018)	0.012 (0.018)
Constant	0.249*** (0.018)	0.300*** (0.021)	0.187*** (0.014)	0.371*** (0.017)
N	3394	3394	3394	3394
adj. R ²	0.005	0.004	0.009	0.009

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level.

The HHI measure may overestimate competition in larger municipalities where there are more schools (and schools with more than 1,000 students are rare). Therefore, we test another measure of competition that is frequently used in the education literature: the private school market share (the proportion of students in the market attending private schools); see Table 11 below. In municipalities where this share is low, we expect most schools to operate as public monopolies (no competition). In municipalities where the share is higher, the diversity of schools is probably larger, and both public and private schools presumably have to use market principles to be competitive. Unlike the HHI measure, however, this alternative measure may ignore competition between public schools and overestimate competition in municipalities with few but large private schools. As both approaches provide similar results, our conclusions regarding competition effects appear to be rather reliable.

Table 11. Treatment effects on five discrimination outcomes of having an Arabic/Swedish-sounding name and a low/high-SES profession, by private school market share in the municipality

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Privshare	0.077 (0.119)	0.108 (0.122)	0.139 (0.342)	0.410 (0.349)	0.038 (0.288)	0.056 (0.272)	-0.087 (0.137)	0.047 (0.127)	-0.144 (0.129)	-0.024 (0.142)
Arab	-0.014 (0.027)	-0.009 (0.018)	-0.049 (0.077)	-0.026 (0.045)	-0.171** (0.062)	-0.187*** (0.036)	-0.067* (0.026)	-0.031* (0.015)	-0.049+ (0.027)	-0.038* (0.016)
Low-SES	-0.012 (0.016)	-0.009 (0.026)	-0.031 (0.041)	0.025 (0.068)	-0.113*** (0.033)	-0.091 (0.061)	-0.045** (0.014)	-0.042 (0.025)	-0.058*** (0.016)	-0.034 (0.028)
Privshare *Arab	0.037 (0.149)		0.152 (0.411)		-0.116 (0.321)		0.246 (0.158)		0.073 (0.126)	
Privshare *Low-SES		-0.023 (0.138)		-0.377 (0.363)		-0.151 (0.343)		-0.019 (0.128)		-0.160 (0.153)
Constant	0.769*** (0.022)	-0.974 (1.106)	-7.764** (2.768)	-7.840** (2.765)	7.107** (2.467)	7.078** (2.475)	2.973* (1.148)	2.967* (1.149)	2.046+ (1.071)	2.014+ (1.069)
N	3394	3394	3394	3394	3394	3394	3394	3394	3394	3394
adj. R ²	-0.001	0.002	0.005	0.005	0.037	0.037	0.020	0.019	0.023	0.023

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for all treatments and the same control variables included in the HHI models.

Table 12. Treatment effects on five discrimination outcomes of having an Arabic-sounding name, having a low-SES profession and being a woman, after controlling for school type and competition

	Reply	Questions answered	Friendliness index	Open slots	Positive information
	(1)	(2)	(3)	(4)	(5)
Arab	-0.008 (0.017)	-0.022 (0.045)	-0.188*** (0.036)	-0.031* (0.014)	-0.038* (0.015)
Low SES	-0.014 (0.015)	-0.038 (0.041)	-0.120*** (0.033)	-0.046** (0.014)	-0.060*** (0.017)
Woman	-0.016 (0.013)	-0.031 (0.039)	0.022 (0.034)	0.003 (0.015)	0.001 (0.017)
HHI	0.056 (0.110)	0.019 (0.331)	1.572*** (0.282)	0.425*** (0.120)	0.372*** (0.108)
Private	-0.009 (0.021)	0.059 (0.072)	0.042 (0.044)	-0.109*** (0.018)	-0.132*** (0.015)
Constant	0.766*** (0.022)	1.684*** (0.058)	0.961*** (0.052)	0.274*** (0.023)	0.330*** (0.021)
N	3394	3394	3394	3394	3394
adj. R ²	-0.001	-0.001	0.029	0.024	0.027

Note: † $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level.

G. Choice of Controls

In Table 4 in the manuscript, we include a number of controls since the level of competition between schools in a municipality could potentially correlate with numerous municipality-level factors that in turn could correlate with discrimination. For example, the HHI of a municipality correlates with the size of the municipality (-0.427***), population growth (-0.532***) and municipality mean income (-0.331***), which could affect both the economic resources the principals have at hand per student and the number of parents contacting them. Similarly, HHI correlates with the proportion of the population with a foreign background (0.358***) and the proportion voting for the far-right Sweden Democrats (0.156***), factors that more directly could affect how ethnic minorities are treated.

Tables 13 to 15 in the Appendix contain a number of robustness tests where we present results when controlling for student composition as well as student test scores. Controlling for these variables could potentially be important, as there may exist systematic differences between different schools and between different school markets in these regards that could also correlate with discriminatory behavior. For example, principals in underperforming schools with few students with parents with higher education may be more likely to discriminate as they are more likely to be under stress and since avoiding more demanding clients is a way to quickly raise the average grades. On the other hand, overperforming schools with more resourceful students may also discriminate demanding/costly students as their way of teaching and their available resources are adapted to a certain type of student composition. As there are differences – for example, in student composition (see Table 17 below) – between public and private schools, it may be important to control for these potential confounders to avoid omitted variable bias. However, as we see in Appendix H, the results remain largely the same when adding these controls.

H. Robustness Tests

Table 13. Treatment effects on five discrimination outcomes of having an Arabic/Swedish-sounding name by school type (private/for-profit school vs. a public school), controlling for student composition and test scores

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Private	-0.001 (0.043)		0.120 (0.124)		0.003 (0.080)		-0.152*** (0.029)		-0.151*** (0.023)	
For-profit		-0.010 (0.043)		0.075 (0.129)		0.042 (0.099)		-0.176*** (0.030)		-0.145*** (0.026)
Arab	-0.014 (0.021)	-0.014 (0.021)	-0.015 (0.054)	-0.015 (0.054)	-0.212*** (0.045)	-0.212*** (0.045)	-0.051** (0.019)	-0.051** (0.019)	-0.043* (0.019)	-0.043* (0.019)
Private *arab	0.015 (0.046)		-0.059 (0.119)		0.035 (0.099)		0.094* (0.038)		0.037 (0.034)	
For-profit *arab		0.010 (.058)		-0.085 (0.148)		-0.033 (0.135)		0.085* (0.042)		0.032 (0.041)
Constant	0.811*** (0.091)	0.809*** (0.093)	2.011*** (0.263)	2.056*** (0.271)	1.220*** (0.216)	1.252*** (0.235)	0.388*** (0.095)	0.409*** (0.108)	0.436*** (0.091)	0.451*** (0.099)
N	2984	2777	2984	2777	2984	2777	2984	2777	2984	2777
adj. R ²	-0.001	-0.001	0.002	0.001	0.023	0.022	0.023	0.022	0.029	0.022

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for all treatments as well as private schools (models 1, 3, 5, 7, 9) and for-profit schools (models 2, 4, 6, 8, 10), as well as the proportion of students with parents with higher education and the proportion of students passing the national tests in Swedish and Mathematics (not shown in the table).

Table 14. Treatment effects on five discrimination outcomes of having an a low/high-SES profession by school type (private/for-profit school vs. a public school), controlling for student composition and test scores

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Private	0.013 (0.027)		0.156 ⁺ (0.081)		0.041 (0.065)		-0.085*** (0.025)		-0.112*** (0.025)	
For-profit		-0.009 (0.037)		0.060 (0.099)		0.045 (0.084)		-0.113*** (0.026)		-0.090** (0.032)
Low SES	-0.015 (0.019)	-0.015 (0.019)	-0.012 (0.052)	-0.012 (0.052)	-0.141*** (0.039)	-0.141*** (0.039)	-0.043* (0.017)	-0.043* (0.017)	-0.061*** (0.019)	-0.061*** (0.019)
Private *Low SES	-0.015 (0.048)		-0.132 (0.127)		-0.042 (0.093)		-0.039 (0.031)		-0.042 (0.032)	
For-profit *Low SES		0.006 (0.055)		-0.053 (0.137)		-0.038 (0.119)		-0.038 (0.036)		-0.073 ⁺ (0.040)
Constant	0.808*** (0.092)	0.809*** (0.094)	2.009*** (0.262)	2.060*** (0.270)	1.213*** (0.214)	1.252*** (0.237)	0.374*** (0.094)	0.400*** (0.107)	0.429*** (0.090)	0.444*** (0.099)
N	2984	2777	2984	2777	2984	2777	2984	2777	2984	2777
adj. R ²	-0.001	-0.001	0.002	0.001	0.023	0.022	0.022	0.022	0.030	0.023

Note: ⁺ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for all treatments as well as private schools (models 1, 3, 5, 7, 9) and for-profit schools (models 2, 4, 6, 8, 10), as well as the proportion of students with parents with higher education and the proportion of students passing the national tests in Swedish and Mathematics (not shown in the table).

Table 15. Treatment effects on five discrimination outcomes of having an Arabic-sounding name and having a low-SES profession by level of school competition in the municipality (HHI), controlling for student composition and test scores

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HHI	0.232 (0.153)	0.070 (0.152)	0.300 (0.458)	0.108 (0.449)	0.859* (0.384)	0.826* (0.398)	0.256 (0.175)	0.095 (0.179)	0.185 (0.172)	0.011 (0.194)
Arab	0.017 (0.026)	-0.012 (0.018)	-0.018 (0.064)	-0.030 (0.046)	-0.162** (0.051)	-0.199*** (0.038)	-0.013 (0.021)	-0.032* (0.016)	-0.011 (0.024)	-0.036* (0.017)
Low SES	-0.015 (0.017)	-0.019 (0.023)	-0.028 (0.045)	-0.055 (0.063)	-0.135*** (0.034)	-0.102* (0.048)	-0.046** (0.016)	-0.060** (0.022)	-0.064*** (0.016)	-0.075*** (0.022)
HHI *Arab	-0.348 (0.211)		-0.151 (0.584)		-0.453 (0.441)		-0.232 (0.205)		-0.298 (0.191)	
HHI *Low SES		0.046 (0.178)		0.334 (0.535)		-0.399 (0.460)		0.166 (0.209)		0.128 (0.212)
Constant	-1.618 (1.471)	-1.633 (1.475)	-8.184* (3.526)	-8.271* (3.528)	1.877 (2.952)	1.973 (2.963)	0.944 (1.260)	0.899 (1.262)	0.317 (1.380)	0.281 (1.383)
N	2984	2984	2984	2984	2984	2984	2984	2984	2984	2984
adj. R ²	0.005	0.003	0.009	0.009	0.047	0.047	0.025	0.025	0.030	0.030

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level. The models also include controls for all treatments, HHI and a number of municipal level variables (municipality population, proportion foreign born, population growth, mean income, proportion voted for Swedish Democrats), as well as the proportion of students with parents with higher education and the proportion of students passing the national tests in Swedish and Mathematics (not shown in the table)

Table 16. Treatment effects on five discrimination outcomes of having an Arabic-sounding name and having a low-SES profession by level of school competition in the municipality (HHI), without controls

	Reply		Questions answered		Friendliness index		Open slots		Positive information	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HHI	0.106 (0.146)	0.001 (0.121)	-0.062 (0.419)	-0.164 (0.347)	1.481*** (0.362)	1.458*** (0.335)	0.522*** (0.147)	0.385** (0.136)	0.201 (0.156)	0.322* (0.141)
Arab	-0.000 (0.025)	-0.008 (0.017)	-0.031 (0.062)	-0.022 (0.045)	-0.200*** (0.047)	-0.188*** (0.036)	-0.025 (0.020)	-0.031* (0.014)	-0.033 (0.020)	-0.038* (0.015)
Low SES	-0.014 (0.015)	-0.026 (0.022)	-0.037 (0.041)	-0.067 (0.061)	-0.120*** (0.033)	-0.138** (0.047)	-0.047** (0.014)	-0.068*** (0.019)	-0.061*** (0.017)	-0.087*** (0.022)
HHI *Arab	-0.099 (0.193)		0.102 (0.522)		0.148 (0.409)		-0.071 (0.186)		-0.059 (0.163)	
HHI *Low SES		0.147 (0.174)		0.362 (0.521)		0.221 (0.472)		0.256 (0.184)		0.313 (0.194)
Constant	0.760*** (0.022)	0.768*** (0.020)	1.703*** (0.057)	1.712*** (0.052)	0.978*** (0.055)	0.979*** (0.051)	0.243*** (0.022)	0.255*** (0.022)	0.294*** (0.022)	0.307*** (0.022)
N	3394	3394	3394	3394	3394	3394	3394	3394	3394	3394
adj. R ²	-0.000	-0.000	-0.001	-0.001	0.029	0.029	0.013	0.014	0.013	0.014

Note: + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Standard errors in parentheses are clustered at the municipal level.

I. Coding

Emails were independently coded by two assistants following instructions and a coding form written by the authors of this article. After coding all responses independently, the assistants paid special attention to emails where their coding differed to reconcile the final coding. However, regarding the simplest variables (reply and the variables forming the friendliness index), differing cases were reviewed by a third assistant. The names of the fictitious emailers were removed before coding started. Moreover, nonresponses were coded as zero for all variables, a standard approach in contemporary correspondence studies. The reason is to avoid selection bias induced by posttreatment variable conditioning (cf. Coppock, 2019). To illustrate the coding procedure for the three variables, two complete email answers are presented in Figure 3.

To illustrate the coding procedure, looking at the first email, all three questions are answered (regarding the school profile, open slots and how to apply). Hence, the variable for the number of questions answered is assigned the value 3 (cf. Table 2). Moreover, the sender is welcomed in a friendly way, her or his name is used, and she or he is invited to make future contact and visit the school. Hence, the friendliness index variable is given the maximum value of 4. The open school slots variable is assigned a value of 0, as the sender is not informed that there are open slots available. No additional positive information regarding the school or municipality is provided, so the positive information variable is therefore given the value 0. For email 2, the principal scores 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 email also includes additional positive information regarding the school (information about average grades). Concerning all the other variables, it receives values of 0. Hence, the first email is formally correct and friendly but does not involve any promotion. The second one is somewhat less formally correct and less friendly but includes some aspects of promotion.

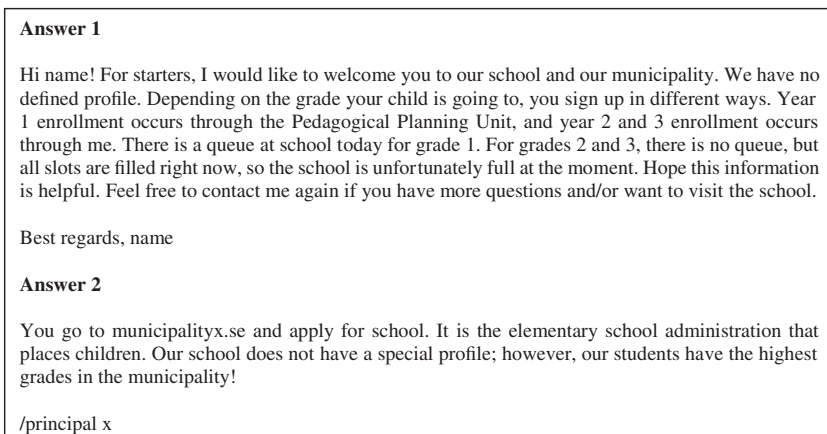


Figure 3. Two examples of email replies from the school principals.

J. Student Composition and School Results for Different School Types

Table 17. School composition and test results depending on school ownership

	Public schools (1)	All private schools (2)	For-profit private schools (3)
Mean proportion students with parents with higher education	0.584	0.703	0.697
Mean proportion students with a foreign background*	0.278	0.296	0.302
Mean proportion students passing national tests**	0.914	0.946	0.944
Number of students	692 905	156 617	111 907
Number of schools	2654	740	445

*data not available for 19 percent of all schools

** Differences in test scores between private/for-profit schools and public schools are not statistically significant after controlling for student composition

Cite this article: Larsson Taghizadeh J and Adman P (2024). Discrimination in marketized welfare services: a field experiment on Swedish schools. *Journal of Social Policy* 53, 1095–1125. <https://doi.org/10.1017/S0047279422000940>