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## Employer discrimination and the immutability of ethnic hierarchies: A field experiment

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# Employer discrimination and the immutability of ethnic hierarchies

A Field Experiment

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# Employer discrimination and the immutability of ethnic hierarchies <sup>a</sup>

A field experiment

by

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## Abstract

How pervasive is labor market discrimination against immigrants and what options do policymakers and migrants have to reduce it? To answer these questions, we conducted a field experiment on employer discrimination in Sweden. Going beyond existing work, we test for a large range of applicant characteristics using a factorial design. We examine whether migrants can affect their employment chances – by adopting citizenship, acquiring work experience, or signaling religious practice – or whether fixed traits such as country of birth or gender are more consequential. We find no evidence that immigrants can affect their employment chances by any of the tested means. Rather, ethnic hierarchies are critical: callback rates decline precipitously with the degree of ethno-cultural distance, leaving Iraqis and Somalis, especially if they are male, with much reduced employment chances. These findings highlight that immigrants have few tools at their disposal to escape ethnic penalties and that efforts to reduce discrimination must address employer prejudice.

**Keywords:** country of birth, citizenship, gender, work-experience, religion, discrimination, field experiment, labor market

**JEL-codes:** J7, J23

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## Table of contents

1	Introduction .....	3
2	Results.....	8
2.1	Registry data analysis.....	8
2.2	CV experiment results.....	13
3	Discussion.....	23
4	Materials and methods.....	25
4.1	Empirical strategy for the registry data analysis.....	25
4.2	Design of the CV study.....	27
	Appendix.....	34
	References.....	43

## 1 Introduction

The economic integration of immigrants is one of the most pressing policy issues facing Europe today. Though many Western European countries have experienced large-scale migration for decades, their records of integrating migrants into domestic labor markets remain patchy at best. Millions of immigrants and their descendants remain unemployed or underpaid (Dancygier and Laitin 2014). As a result, a growing literature has begun to investigate barriers to immigrants' economic success, with one important strand focusing on the hurdles that migrants encounter during the hiring process. This research has consistently found that job applicants with immigrant origins are less likely to be invited to interviews than are natives (Baert et al. 2015; Bursell 2007; Carlsson and Rooth 2007; Kaas and Manger 2012; McGinnity et al. 2009). Indeed, a recent meta-analysis by Zschrint and Ruedin (2016) concludes that discrimination against immigrants is a robust finding of 42 correspondence studies conducted in OECD countries during the last 15 years.<sup>1</sup>

Sweden, the focus of this paper, fits this trend. Like many OECD countries, it has a substantial and diverse immigrant-origin population of nearly two million (representing seventeen percent of the population), and though Swedish attitudes towards immigrants are generally more favorable compared to those of its European neighbors (e.g., Strabac and Listhaug 2008), immigrants in Sweden face considerably higher unemployment rates and lower wages than do natives (Beijron 2016; Heggeman and Gärdqvist 2016). Probing the locus of discrimination is therefore a key concern.

Given that discrimination is pervasive, what options do policymakers and migrants have at their disposal to escape ethnic penalties? Existing research has demonstrated that foreign origin presents an obstacle in the recruitment process, but it has less frequently considered ways in which migrants can improve their chances on the labor market. If discrimination is largely driven by comparatively fixed ascriptive traits such as ethnicity or gender, states should design policies to help break down employer stereotypes. If, by contrast, employers respond to attributes over which immigrants have some control, such

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<sup>1</sup> For a recent review of correspondence tests beyond immigrants and labor markets, see Rich (2014). For a foundational study, see Bertrand and Mullainathan (2004). For a recent review that also covers the various techniques available to researchers when conducting field experiments on discrimination, see Bertrand and Duflo (2017).

as work experience, citizenship, or signals of religiosity, different policy prescriptions emerge.

To provide answers to these questions, we carried out a correspondence study in Sweden's seven largest cities and sent applications from fictitious immigrants and natives to restaurant and café jobs. Our outcome of interest is whether applicants received a call-back for an interview. Going beyond much existing work, our study employs a factorial design. This design allows us to efficiently test for the impact of a larger range of immigrant characteristics (ascriptive traits, changeable attributes, as well as their combinations) than has been the case in the previous literature. Importantly, this approach permits us to test whether origin-based penalties diminish or persist once we vary attributes that employers could systematically link to specific countries of origin.

To establish the effects and nature of ethnic penalties, we consider four origin countries and examine whether ethnic hierarchies are at work. Research on social distance has demonstrated hierarchical orderings, whereby West Europeans and Northern Americans are positively stereotyped, followed by Southern and Eastern Europeans, Asians and Africans (Hagendoorn 1995). In an attempt to select sizable groups that have been shown to occupy different rungs on the ethnic hierarchy ladder, we signal that the applicant was born in Sweden, Poland, Iraq or Somalia. Such hierarchies have been found to influence views about what groups are preferred neighbors or marriage partners (e.g., Hagendoorn 1995; Hagendoorn and Hraba 1989; Strabac and Listhaug 2008). We test whether they also extend to labor markets. Additionally, we investigate whether another comparatively fixed trait – gender – interacts with different ethnicities to shape employer hiring decisions.

Since immigrants cannot easily change perceptions of ethnic hierarchies (or their gender), we next turn to attributes that immigrants can to some degree affect: the adoption of citizenship, the accumulation of skills, and the salience of their religious practice. Scholars have documented a positive association between naturalization and economic outcomes: Immigrant citizens are more likely to be employed and to earn higher wages than are those who have not naturalized (e.g., Bevelander 2000; Chiswick 1978; Kogan 2003; Steinhardt and Wedemaier 2012, Engdahl 2014). The resulting policy implication may seem straightforward: Immigrants who want to improve their labor market outcomes

should become citizens.<sup>2</sup> Yet, much of the existing work on the citizenship-employment link is observational and cross-sectional. As a result, we cannot easily disentangle whether citizenship acquisition indeed increases earnings or employment options, or whether unobservable characteristics – such as increased motivation to work or to stay in the country – causes both naturalization *and* better economic outcomes.<sup>3</sup>

Moreover, in settings where the barriers of citizenship are comparatively low, such as in Sweden, the effects of citizenship are unclear. Naturalization could signal legal status, integration, language skills or intention to stay in the country – all of which could lead employers to privilege citizens. Though plausible, this mechanism remains largely untested. We therefore vary immigrant citizenship and examine whether employers are more likely to call back immigrants who have acquired citizenship.

Another way in which immigrants can potentially boost their employment chances is by accumulating work experience early on. Research has shown that refugees who face long employment bans upon entry are much less likely to be employed once these bans are lifted compared to refugees who face shorter bans (Marbach et al. 2017). These results are consistent with so-called scarring effects found among natives, whereby early experiences of unemployment reduce wages and employment prospects in later years, especially among the low-skilled (e.g., Burgess et al. 2003; Gregg and Tominey 2005; Mroz and Savage 2006). It is less clear, however, whether these effects are due to discrimination by employers, and moreover, whether they also apply to immigrants. Our study consequently investigates whether immigrants are less likely to receive callbacks from employers if they lack relevant prior work.<sup>4</sup>

Finally, work by Adida and colleagues (2016) demonstrates that employers are especially likely to discriminate against immigrants who are Muslim (holding constant country of birth). One mechanism that can generate this religiously-based discrimination are employer concerns that daily religious practice reduces productivity. We therefore

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<sup>2</sup> Note that a positive effect of citizenship in the hiring process would not necessarily suggest that naturalization requirements should be eased (thus potentially reducing citizenship's ability to signal integration), but rather that, in the context of a fixed set of requirements, immigrants should be encouraged (e.g., via information campaigns) to acquire citizenship.

<sup>3</sup> Findings from studies that can better isolate the effect of citizenship by relying on longitudinal registry data are mixed (see Engdahl 2014; Steinhardt 2012).

<sup>4</sup> Baert et al. (2017) also study the effect of work experience but do so by holding it constant within immigrant-native pairs and varying it across pairs. They find that immigrants receive the same number of callbacks once applicants have 20 years of experience.



examine whether migrants who signal that they are religiously active receive fewer callbacks than those who do not.

Our main findings are as follows. First, employer behavior follows ethnic hierarchies: Native Swedes receive the most responses (21%), followed by those born in Poland (17%), Iraq (10%), and then Somalia (5%). The callback rates for native Swedes are thus *four* times higher than they are for applicants born in Somalia.

Moreover, gender significantly affects employment prospects. Across groups, women receive higher callback rates than do men, leading to staggering gaps in the recruitment process: The callback rate for Somali men is *ten* times smaller than that for Swedish or Polish women. Groups that feature prominently in the debate about integration problems – young, low-skilled, Muslim men – thus also encounter the most discrimination, even when their other background characteristics and applications are identical to those of natives

Second, there is little evidence that characteristics over which immigrants have some control can influence employer hiring decisions and, by implication, that these characteristics generate hierarchically-ordered ethnic penalties. We do not find that the acquisition of citizenship can even out ethnic hierarchies. The overall effect of citizenship is zero, and it remains non-existent across groups. In contrast to arguments about the economic returns to citizenship, we find that employers do not prefer immigrant citizens over non-citizens. Furthermore, there is some evidence that previous work experience does improve the employment chances of migrants, but effects are small and imprecisely estimated, and affect natives in the same way as immigrants. Increased experience will therefore not reduce immigrant-native employment inequalities. Finally, we do not find evidence of a penalty tied to religious activity. If anything, signals of religious practice slightly raise callback rates, but overall ethnicity trumps these signals.

Our paper advances existing scholarship in several ways. First, we employ the factorial design in the context of a correspondence study. While this design has been extensively used in consumer research (Green and Srinivasan 1978) and, more recently, in opinion surveys about immigrants (e.g., Hainmueller and Hopkins 2015), correspondence testers have not realized the methodology's full potential. In particular, using factorial designs one can study the effects of several factors using the same sample size that would be

required for the study of one independent variable (Collins et al. 2009). Substantively, this permits us to test whether variables that signal integration drive ethnic hierarchies.

Second, we are among the first to examine whether citizenship can improve the employment chances of immigrants by reducing employer discrimination. Our results indicate that in the Swedish context, naturalization should not necessarily be considered a tool for economic integration. If citizenship improves employment outcomes at all, it is unlikely to do so by influencing employer discrimination.

Third, by selecting several immigrant groups we are able to establish the significance of deep-seated ethnic hierarchies and, by implication, the severity of ethnic penalties. Existing research has begun to examine such hierarchies in the labor market, but results have been mixed.<sup>5</sup> This inconclusiveness may be caused by employers differing in the heuristics they use (e.g., some employers might associate a minority group with citizenship or work experience, while others do not). Our factorial design allows us to examine whether ethnic hierarchies persist once several important background characteristics are varied.

Finally, the fact that ascriptive characteristics like ethnic origin and gender critically impact employment outcomes, and that these fixed traits have much larger effects than do attributes that policy and migrant behavior can actually influence, has significant policy implications. Specifically, efforts that seek to improve immigrants' economic integration will fall short if they are only targeted at immigrants. Instead, our results suggest that policies that focus on breaking down prejudice among natives may also be effective.

The remainder of this paper proceeds as follows. To provide background on immigrant economic integration in Sweden and to place the results of our study into context, the next section draws on population-based registry data to present employment rates of all individuals aged 15-64 who are born in Sweden, Poland, Iraq and the Horn of Africa. These nationwide statistics document that the ethnic hierarchies that we find in our experiment are indeed widespread. But we also discuss how they are less helpful in establishing the magnitude and nature of employer discrimination, or in identifying the

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<sup>5</sup> Weichselbaumer (2016) and Baert et al. (2017) document hierarchies in the German and Belgian labor markets, respectively, while Andriessen et al.'s Dutch study (2012) does not.

causal effects that citizenship acquisition, work experience, and country of birth have on employers' recruitment choices. We therefore next turn to our correspondence study and discuss its experimental design and empirical results. The subsequent section concludes with a discussion of the main implications of our study and is followed by a section detailing the methods employed in this article.

## 2 Results

### 2.1 Registry data analysis

Like many European countries, Sweden's immigrant-origin population consists of a mix of labor migrants, family migrants, and refugees, hailing from both within and outside of the European Union (EU). At the end of 2016 the ten countries with the largest stock of migrants were (in descending order): Finland, Syria, Iraq, Poland, Iran, the former Yugoslavia, Somalia, Bosnia and Herzegovina, Germany, and Turkey.<sup>6</sup> Though employment rates vary across groups, the overall economic performance of migrants falls well below that of natives. When compared to other OECD countries, Sweden features the highest gap in employment rates between immigrants and natives.<sup>7</sup>

To provide some background on this employment gap and to illustrate the inferential problems inherent in trying to estimate discrimination using observational data, we draw on population-wide government registers. These registry data cover the entire Swedish population and contain information on a range of individual characteristics, such as employment status, wage income, country or region of birth, citizenship acquisition, gender, level of education, age and residential location. They allow us to analyze the employment situation of close to six million individuals born in Sweden, Poland, Iraq and the country-group that is often called the Horn of Africa (comprising Somalia, Eritrea, Ethiopia, Sudan and Djibouti)<sup>8</sup> as well as many other countries in 2015.

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<sup>6</sup> When we began the experimental study (early 2016), Iraqi-born immigrants were still the largest group among Swedish residents born in the Middle-East. By the end of 2016 Syria had surpassed Iraq as the largest sending country.

<sup>7</sup> The immigrant-native employment gap is 17.6 percentage points in Sweden, followed by 15.7, 14.8, and 13.6 points in Belgium, the Netherlands, and France, respectively. The OECD average is 5.4 (OECD 2016; own calculations based on 2014 data).

<sup>8</sup> For reasons of confidentiality, the "county-of-birth" variable sometimes aggregates several countries into a regional category. In our case, Somalia is grouped with Eritrea, Ethiopia, Sudan and Djibouti. Within this group, the Somali-born constitute the majority, followed by the Eritrea- and Ethiopia-born, respectively. There are very few residents born in Sudan or Djibouti. We discuss this aggregation further in the section on methods and materials.

We first examine the extent to which variation in observable characteristics such as country of birth, education and citizenship contributes to the immigrant-native employment gap, and we do so employing the Oaxaca-Blinder (OB) decomposition, which is based on the intuition that differences in outcomes across groups can arise due to differences in the *distribution* of characteristics as well as due to differences in the *returns* to these characteristics (Blinder 1973; Oaxaca 1973). For instance, an immigrant group might face lower employment rates than natives because its members have less education than do natives; because its members reap lower returns to their education than do natives; or both.

We first estimate group-wise regressions (Table 2) and then use the results of these regressions to decompose the mean outcome differences between immigrant groups and natives into an “explained” and an “unexplained” part (Table 3). Whereas the explained part refers to the difference in outcomes that are due to differences in the distribution of characteristics, the unexplained part refers to differences in returns. Since the literature largely rests on the broad idea that discrimination occurs when equal cases are treated differently (cf. Pager and Shepherd 2008), the unexplained part has often been interpreted as a measure of discrimination (e.g. Fortin et al. 2011; Oaxaca and Ransom 1994; Reimers 1983). For more details, see the section on methods and materials at the end of this report.

Table 1 reports group-wise regressions and average employment rates. We observe an ordering consistent with the theory of ethnic hierarchies, which predicts that individuals rank ethnic groups on the basis of social distance. In particular, our results show that native Swedes have the highest employment rates (86%), followed by those born in Poland (73%), Iraq (60%), and finally those born in the Horn of Africa (58%).

Furthermore, the coefficient estimates for most variables differ across groups. The estimates for educational attainment are positive across the board, but the size is generally larger for Swedes than it is for immigrants, suggesting that natives reap higher labor market returns from education. Moreover, among immigrants the impact varies considerably between groups.<sup>9</sup>

In addition to demographic variables, Table 1 includes variables specific to foreign-born groups, of which time in Sweden has a positive, if decreasing relationship with

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<sup>9</sup> The reference category is those who have not completed 9 years of elementary school.

employment for all groups.<sup>10</sup> However, the impact is substantively larger for Iraqi-born as well as those born on the Horn of Africa. Most interestingly, citizenship appears to be strongly related to employment for all three foreign-born groups, raising the employment probability by between 7 and 13 percentage points, depending on the country of birth.

On the whole, Table 1 presents significant differences in the returns to characteristics across immigrants and natives, specifically to education and gender. The decomposition results in Table 2 further confirm that the employment gaps are substantial (ranging between 14 and 29 percentage points; see row 1) and generally not driven by differences in characteristics. Differences in characteristics explain very little (the unexplained part accounts for about 94% of the gap). Note that the negative sign for the explained part in the case of Polish- and Iraqi-born individuals implies that differences in characteristics, most notably in demographics, work to the benefit of these two groups. Among those born on the Horn of Africa, observable characteristics do contribute to the reduced employment odds vis-à-vis the Swedish-born, but even here the lion's share of this group's employment penalty cannot be explained by the distribution of these characteristics (the unexplained part contributes 84% (23.83 out of 28.48 points) to the overall employment gap).

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<sup>10</sup> In the case of those born in Iraq or the Horn of Africa, "Time in Sweden" turns negative only after well over 50 years, whereas it turns negative after 27 years for the Polish-born. However, immigrants from these countries that have lived in Sweden for such long stretches of time are rare, and in the case of those originating from Iraq or the Horn of Africa close to non-existent.

Table 1. Correlates of Having Employment in 2015 by Country of Birth

	(1) Sweden	(2) Foreign-born	(3) Poland	(4) Iraq	(5) Horn of Africa
Mean outcome	0.8643	0.6908	0.7251	0.5957	0.5795
Gender (Female)	0.0018*** (0.0003)	-0.0420*** (0.0008)	-0.0280*** (0.0038)	-0.1010*** (0.0028)	-0.0885*** (0.0034)
<i>Education (level attained):</i>					
Elementary school	0.1007*** (0.0031)	0.0603*** (0.0020)	0.0118 (0.0144)	0.0386*** (0.0053)	0.0669*** (0.0059)
Upper secondary school	0.2685*** (0.0030)	0.1944*** (0.0016)	0.1862*** (0.0125)	0.1752*** (0.0045)	0.2109*** (0.0046)
Post-secondary (≤2 years)	0.2801*** (0.0031)	0.1922*** (0.0023)	0.1702*** (0.0144)	0.1641*** (0.0075)	0.2221*** (0.0089)
Post-secondary (≥2 years)	0.3242*** (0.0030)	0.2445*** (0.0016)	0.2234*** (0.0125)	0.2377*** (0.0044)	0.2515*** (0.0059)
Graduate School (PhD)	0.3486*** (0.0032)	0.3274*** (0.0030)	0.2847*** (0.0180)	0.3296*** (0.0178)	0.2011*** (0.0254)
Age	0.0146*** (0.0001)	0.0167*** (0.0002)	0.0226*** (0.0011)	0.0110*** (0.0008)	0.0074*** (0.0010)
Age^2	-0.0002*** (0.0000)	-0.0003*** (0.0000)	-0.0003*** (0.0000)	-0.0003*** (0.0000)	-0.0002*** (0.0000)
Citizen		0.0698*** (0.0011)	0.1257*** (0.0049)	0.0680*** (0.0050)	0.1183*** (0.0050)
Time in Sweden		0.0139*** (0.0001)	0.0027*** (0.0007)	0.0309*** (0.0008)	0.0273*** (0.0008)
Time in Sweden^2		-0.0002*** (0.0000)	-0.0001*** (0.0000)	-0.0006*** (0.0000)	-0.0005*** (0.0000)
Constant	0.3698*** (0.0033)	0.1521*** (0.0049)	0.1368*** (0.0254)	0.1715*** (0.0157)	0.2161*** (0.0186)
Observations	4,755,922	1,112,526	55,597	105,707	70,587
Adj. R-squared	0.0705	0.0982	0.0647	0.131	0.177

Notes: The dependent variable is a dummy indicating whether an individual had any wage income in 2015 (1) or not (0). OLS coefficients; robust standard errors in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 2. Oaxaca-Blinder Decomposition of Results

	Sweden vs. Foreign-born	Sweden vs. Poland	Sweden vs. Iraq	Sweden vs. Horn of Africa
<b>Difference</b>	0.1734	0.1392	0.2686	0.2848
P(Natives)	0.8643	0.8643	0.8643	0.8643
P(Foreign-born)	0.6908	0.7251	0.5957	0.5795
<b>Explained</b>	0.0096 (0.0002)	-0.0117 (0.0008)	-0.0017 (0.0010)	0.0465 (0.0018)
<b>Unexplained</b>	0.1638 (0.0005)	0.1509 (0.0020)	0.2703 (0.0017)	0.2383 (0.0025)

Notes: The first row presents the difference between the native and foreign-born group in the proportion employed. The second and third rows report the share of natives and foreign-born being employed. The second block ("Explained") reports the size of the employment gap that is due to group differences in characteristics. The third block ("Unexplained") reports the size of the employment gap that is due to group differences in returns to characteristics. For included covariates, see Table 1. Standard errors in parentheses

These results help us establish a set of important empirical regularities. First, in addition to an immigrant penalty, employment gaps appear to be consistent with an ethnic hierarchy, which predicts that individuals rank ethnic groups on the basis of social distance. Our results show that those born in the Horn of Africa face the steepest penalty,

followed by individuals born in Iraq, and then Poland. Second, citizenship is positively associated with employment and, further, it appears to narrow immigrant-native employment gaps (even when time in Sweden is controlled for).

Yet, the analyses thus far also face shortcomings. First, it is possible that we did not capture important variables that lead to both employment gaps between immigrants and natives and to the specific ethnic hierarchies we observe. For example, variation in confidence, language skills, networks and search intensity across groups could, in principle, drive group-based employment differences. Likewise, the four groups could be applying to jobs in different sectors or requiring different skill levels. In short, on the basis of these observational data, it cannot be ruled out that unobserved variables rather than employer discrimination account for a large portion of the immigrant-native employment gap.

Second, though we document a positive association between citizenship and employment, this effect is difficult to interpret. It is possible that employers are more likely to hire citizens than non-citizen immigrants because citizenship can signal a greater commitment to stay in the country, and it might also reduce administrative or legal hurdles that can arise in the hiring of non-citizens (Bloemraad 2008). In line with models of statistical discrimination (Arrow 1972, Phelps 1972), the information revealed by citizenship could therefore lessen employer discrimination.

At the same time, citizenship acquisition could correlate with important immigrant attributes. Immigrants who are particularly motivated to assimilate, to put down roots, to start a family, or to move up economically might be more likely to acquire citizenship *and* to exhibit favorable employment outcomes. In other words, citizenship itself might not contribute to their employment, but the factors that lead to citizenship acquisition also lead to improved economic outcomes. The registry data do not allow us to pit this selection pathway against the pathway that directly links citizenship to reduced discrimination among employers.

Third, it is also challenging to measure the effects of prior work experience and immigrant status on employment outcomes using observational data. Though we could have included a variable measuring prior work experience, this inclusion would almost certainly lead to post-treatment bias. That is, if being born abroad reduces the odds of

acquiring work experience due to employer discrimination, the inclusion of prior work experience would lead to a biased estimate of the effect of having been born in a foreign country on employment.

Finally, note that other variables that we do include – such as citizenship acquisition and educational attainment – are also plausibly affected by country of birth and hence also suffer from post-treatment bias, making it difficult to assess the impact of these variables; of country of birth generally; and of ethnic hierarchies associated with the countries in our study specifically.

In short, though the analyses relying on registry data provide us with a good overview of the variables that are plausibly associated with employment inequalities, they are clearly not suited for estimating the extent to which employer discrimination produces these unequal outcomes. Moreover, these observational data do not include all potentially relevant variables (such as religious activity), and they also do not permit us to isolate the causal role that specific individual characteristics – such as country of birth, gender, citizenship or work experience – play when employers evaluate job applicants. Because the registry data do not give us much traction when estimating the magnitude and nature of employer discrimination, we next turn to a correspondence study that allows us to identify some of the characteristics driving employers' decision-making.

## 2.2 CV experiment results

Our field experiment is designed to estimate the impact of two types of factors: relatively fixed attributes that individuals cannot easily modify and relatively malleable features over which individuals have some control. Identifying which type of factor generates employer discrimination is critical if we want to make headway in improving immigrants' labor market integration. Testing studies<sup>11</sup> in hiring processes offer a way to measure discrimination and are applied to a growing number of countries to unveil discrimination against different groups. In this study, we sent fictitious resumes and cover letters (electronically or via mail) to hiring companies in which the country of birth, gender, citizenship, previous work experience, and religious activity of the applicant vary (we

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<sup>11</sup> Two main techniques of field experiment used for assessing ethnic discrimination can be distinguished: situation testing (or audit testing in the US terminology) and correspondence testing (Riach and Rich 2002). This project will use correspondence testing. In the situation testing approach, which has become the standard ILO methodology, real individuals are hired to apply for the same job. See Heckman (1998) for a critique of situation/audit studies.



also vary the applicant's name, selecting first and last names that are common in the origin country).

All applications are sent by fictitious immigrants who came to Sweden as children and who were born in 1995.<sup>12</sup> We chose this group because we wanted to minimize presumed immigrant-native differences in unobserved characteristics such as language skills or the quality of education. Moreover, the future of immigrant integration hinges on the younger generation, making this group substantively important. Note that this choice (vs. selecting older migrants who arrived as adults) likely reduces the amount of discrimination we observe.

Applications were sent to jobs in the restaurant and café sector. While we hold the sector constant, there is some differentiation within the advertised jobs, as some require prior experience (e.g., as a cashier or waiter). We control for this difference in the analysis. The main outcome of interest is whether or not the applicant received a callback for an interview.

We selected the restaurant and café sector because, first, it is the sector that currently employs the plurality of immigrants.<sup>13</sup> Second, this sector employs relatively young workers, and is an important entry point into the labor market.<sup>14</sup> Finally, given the large size and growth potential of the service sector, it is important to estimate the extent of discrimination that currently prevails.

When considering the generalizability of our findings, several aspects should be kept in mind. On the whole, we expect intermediate levels of discrimination when compared to other sectors: On the one hand, a large immigrant workforce is present in this sector, so our findings could present a lower bound of discrimination. On the other hand, because the jobs involve face-to-face interactions, employers might be concerned about customer prejudice, thereby raising levels of discrimination. In sum, we expect that our results should best generalize to service jobs that already employ migrant workers, require relatively few skills and involve face-to-face contact with customers. For more details on

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<sup>12</sup> Since the applications were sent out during 2016 and 2017, the employers would have inferred that the age of the applicant was about 21 or 22.

<sup>13</sup> In 2015, the Hotels and Restaurants sector employed 20% of Sweden's foreign-born population. The sector employs over 165,000 workers, almost 40% of whom are foreign-born. It employs more women than men, largely in restaurants and cafés (Statistics Sweden 2015).

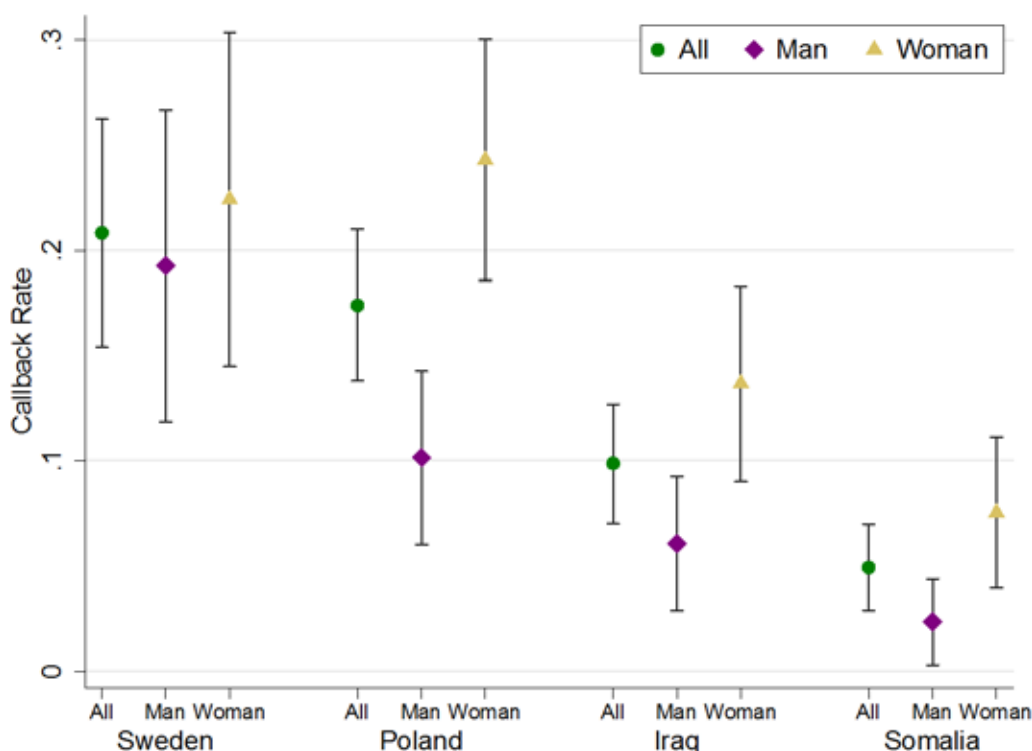
<sup>14</sup> Among employed persons aged 15-24, one in ten works in the Hotels and Restaurants sector, whereas the corresponding figure for those aged 25-54 is one in 35 (Statistics Sweden 2015).

the design of the correspondence test, see the section on methods and materials at the end of this report.

We first display the callback rates by the two ascriptive characteristics that we manipulate: the applicants' country of birth and gender. Figure 1 demonstrates that ethnic hierarchies indeed govern employer responses. Callback rates are highest for native Swedes (21%), followed by applicants born in Poland (17%), Iraq (10%), and Somalia (5%), respectively. The difference in callback rates between the Polish-born and the Iraqi and Somalia-born are thus substantively large and statistically significant ( $p < 0.01$ ), as is the difference between immigrants born in Iraq vs. Somalia ( $p < 0.01$ ). These experimental results are in line with our observational findings, suggesting in turn that the ethnic ordering of employment rates can at least in part be attributed to employer behavior, rather than simply to unobservable characteristics on which these groups vary.

Furthermore, callback rates differ dramatically when we take into account both country of birth and gender. For instance, whereas only 2.5% of male Somali-born applicants receive an invitation to interview, the corresponding figure for female Somali-born applicants is 7.5% ( $p < 0.05$ ). Female applicants receive higher callback rates, and this tendency is especially pronounced among immigrant groups. The lower employment rates among women born in Iraq and Somalia that we observed in the registry data are therefore unlikely the result of employer discrimination and instead partly reflect other differences in the drivers of female employment across groups.

Figure 1. Callback Rates by Country of Birth and Gender



Note: Capped bars represent 95% confidence intervals

The results thus far establish the striking importance of ascriptive traits. Can immigrants escape the penalties imposed by their country of birth and gender? To answer this question, we first examine whether the adoption of citizenship influences employer hiring. Since the experimental design forces a correlation between being foreign-born and Swedish citizenship (i.e., all natives in the experiment are Swedish citizens), we move on to multivariate regression analysis.<sup>15</sup> To ease the interpretation of our results, we consistently present estimates that rely on a linear probability model. Results using a logistic model are in the Online Appendix, and are substantively similar unless otherwise noted. The most basic specification (Table 3, column 1) shows that citizenship has a positive, but very small, effect on the callback rate. Moreover, the estimate is not statistically significant at conventional levels. These results do not lend support for the hypothesis that employers are more likely to hire immigrants who have adopted Swedish citizenship. Rather, country of birth and associated ethnic hierarchies remain powerful.

<sup>15</sup> The correlation between the indicator for being born in Sweden and the citizenship indicator is Pearson's  $r = -0.3625$ .

In column 2 of Table 3 we add the further treatment conditions. Controlling for all covariates not only increases statistical power, but also allows us to compare the magnitude of the effect of country of birth to that of other characteristics. The point estimates for the effects of country of birth in column 2 closely track those found in column 1, suggesting that randomization succeeded. Further, ethnic penalties for Iraqi and Somali applicants are large compared to other treatment conditions.

If immigrants can't improve their labor market chances by adopting citizenship, does the accumulation of previous work experience help? We find that prior relevant work experience raises the probability of receiving a callback by about 3 percentage points, but the point estimate fails to reach statistical significance at conventional levels (Table 3, column 2).<sup>16</sup> The callback rates for jobs that require previous cashier and waiting experience are 4 percentage-points higher when compared to jobs that lack this requirement. Finally, explicit mentions of religious activity do not appear to harm applicants. The estimate for religious activity is close to zero and fails to reach significance at conventional levels.

Summing up the results thus far, ascriptive traits such as country of birth and gender are much more consequential at the recruitment stage than are characteristics over which immigrants and policymakers have some control, such as citizenship acquisition, the accumulation of work experience, or signals of religious activity. However, it is possible that the effects of some of these variables vary across groups. For instance, citizenship or previous work experience could matter for groups with generally less favorable integration and employment outcomes, while signals of religious activity could be consequential for immigrants originating from Muslim-majority countries (Adida et al. 2016).

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<sup>16</sup> When using the logistic regression model, the impact of experience is significant at  $p < 0.10$  (see Table A1 in the Online Appendix). Like in the linear probability model presented in the main text, however, the substantive size of the effect of experience is considerably smaller than that of ascriptive traits.

Table 3. CV Experiment Results. DV: Invited to interview (callback)

	(1)	(2)
<b>Mean outcome:</b>	<b>0.122</b>	
<b>Country of birth:</b>		
Poland	-0.026 (0.034)	-0.024 (0.034)
Iraq	-0.096*** (0.033)	-0.094*** (0.032)
Somalia	-0.146*** (0.031)	-0.144*** (0.031)
Citizenship (Yes)	0.014 (0.017)	0.014 (0.017)
Gender (Female)		0.079*** (0.016)
Religious Acitivity (Yes)		-0.005 (0.016)
Previous Experience (More)		0.026 (0.019)
Job requirement (Waiting experience)		0.043* (0.024)
Observations	1,492	1,492
R-squared	0.072	0.094
City Dummies	Yes	Yes
Time-trend	Yes	Yes
Adj. R-aquared	0.065	0.085

*Notes:* The dependent variable is a dummy indicating whether applicant was called to an interview (1) or not (0). OLS coefficients; robust standard errors in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

We now move on to analyses that aim to uncover such interactions.<sup>17</sup> Table 4 (column 1) interacts citizenship with the various countries of birth (recall that citizenship is held constant for the Swedish-born). The citizenship effect varies slightly across groups, but none of these interactions reach conventional levels of statistical significance.<sup>18</sup>

The second column of Table 4 presents the impact of being foreign-born conditional on having more or less previous experience. If statistical discrimination related to uncertainty about relevant background experience is at work, employers who receive CVs that contain information about additional previous experience should discriminate less. By contrast, theories of taste-based discrimination would predict that knowledge about additional previous work experience should not affect employer discrimination.

<sup>17</sup> While the factorial design allows for the study of the main effects of several factors without the usual loss of statistical power, attempts to estimate the effects of *combinations* of treatments, as is done in our interaction analysis, will lead to lower statistical power, the degree to which is determined by the number of treatments that are interacted (Collins et al. 2009).

<sup>18</sup> Analyzing whether treatment effects differ across countries of birth, as we do in Table 4, raises concerns of multiple comparisons. In Table A4 of the Online Appendix we address this concern by presenting results controlling for the false discovery rate (Benjamini and Hochberg 1995). Using this more conservative measure of statistical significance, the main treatments effects (of country of birth and gender) remain significant but none of the differences in treatment effects across countries of birth. This should come as no surprise, given that only a few of the interaction effects in Table 4 are statistically significant, and barely so.

Column 2 of Table 4 reports that the interactions in question are substantively small and imprecisely estimated, thus failing to reach conventional levels of significance. Hence, these results are more consistent with taste-based than with statistical discrimination.

Turning to an additional aspect of experience, in our design we distinguished between two types of jobs, those that require previous waiting experience and those that do not. The former always receive applications indicating more previous experience whereas the latter receive applications of both types. This design forces a correlation<sup>19</sup> between previous experience and whether the job requires prior waiting experience, and therefore the interaction between country of birth and job requirement is also included in column 2 of Table 4. The interactions between job type and the country of birth are quite large and negative. This could suggest that employers are reluctant to hire immigrants for positions which, on average, are likely to entail more contact with customers. However, although the magnitudes of these interaction effects are fairly sizable, they are imprecisely estimated and do not reach conventional levels of statistical significance.

We also wanted to examine whether employers react to signals of religiosity. Existing work has shown that employers in France discriminate against Muslim applicants, even when country of birth and associated ethnic backgrounds are held constant (Adida et al. 2016). Our design does not allow us to tease apart religion, country of birth, and ethnicity: the great majority of Iraqi and Somali migrants are Muslim, and, to maintain realism, plausibility and external validity, we did not vary their denomination. We instead chose to include signals of religiosity. That is, holding denomination within a given country of birth constant, we assess whether *priming* religiosity influences employer behavior.<sup>20</sup> We do so because some of the concerns about employing Muslims could relate to the group's religious needs (e.g., the frequency of prayer or the provision of halal foods) rather than their denomination per se, and how these needs interfere with productivity or company

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<sup>19</sup> Pearson's  $r=0.5345$ .

<sup>20</sup> That countries of origin themselves carry signals of religious belonging is one of the most formidable obstacles facing correspondence testers interested in religious discrimination. Adida et al. (2010) suggest that the researcher uses groups that have the same country of origin, but that differ in their religious orientation as signaled by their names. Given our overarching goal of studying discrimination against the largest groups from Eastern Europe, the Middle East and Africa, we cannot use this strategy. Instead, we follow previous research that signals religious affiliation by having applicants report volunteer activities in religious groups (Drydakis 2010; Pierné 2013; Wallace 2014).

policies. Accordingly, if employers receive information that Muslim applicants indeed practice their religion, this could further depress callback rates.

To test this hypothesis, half of the Somali and Iraqi-born applicants in our sample indicated that they were active in a mosque in their city of residence. If bias against religiously-practicing Muslims were driving the results, we would expect Iraqis and Somalis to fare even worse when they include information about their religious involvement. Column 3 of Table 4 demonstrates that this is not the case. If anything, the ethnic penalty for religiously active Iraqi- and Somali-born applicants is smaller compared to the penalty among the non-active compatriots, providing suggestive evidence that discrimination against Iraqi- and Somali-born jobseekers is not solely driven by their degree of religiosity and the practical aspects related to accommodating religious Muslims at the workplace.<sup>21</sup>

Moreover, among Polish-born applicants, those who are religiously active (with applications stating involvement in a local Catholic Church), also face a smaller ethnic penalty. But this pattern contrasts with that of religiously active Swedish-born applicants (who indicate that they are active in a local Protestant church) who do receive fewer callbacks compared to their non-active counterparts.

In sum, the evidence does not support the conclusion that indications of religious practice among the foreign-born compound discrimination. We note, however, that this result must be interpreted with caution, for while our design differentially primes religiosity, it remains possible that employers' beliefs about applicants' religious practice do not in fact vary on the basis of these primes.

Interestingly, the differences in the effects of religious activity across natives and immigrants are more in line with the role congruity theory of prejudice (Eagly and Karau 2002), in that candidates born in the largely secular, but historically Protestant, Sweden appear to be evaluated less favorably when practicing their religion, whereas candidates born in more religious parts of the world do not face an additional penalty for practicing the dominant religion of their country of birth.

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<sup>21</sup> If we instead analyze the impact of religious activity on applicants of a given country of birth, we find that it has a small positive, but statistically insignificant, impact on the callback rates of Iraqi- and Polish-born applicants, and an impact that is negative, but close to zero, on Somali-born applicants.

Table 4. Heterogeneous effects of country of birth. DV: Invited to interview (callback)

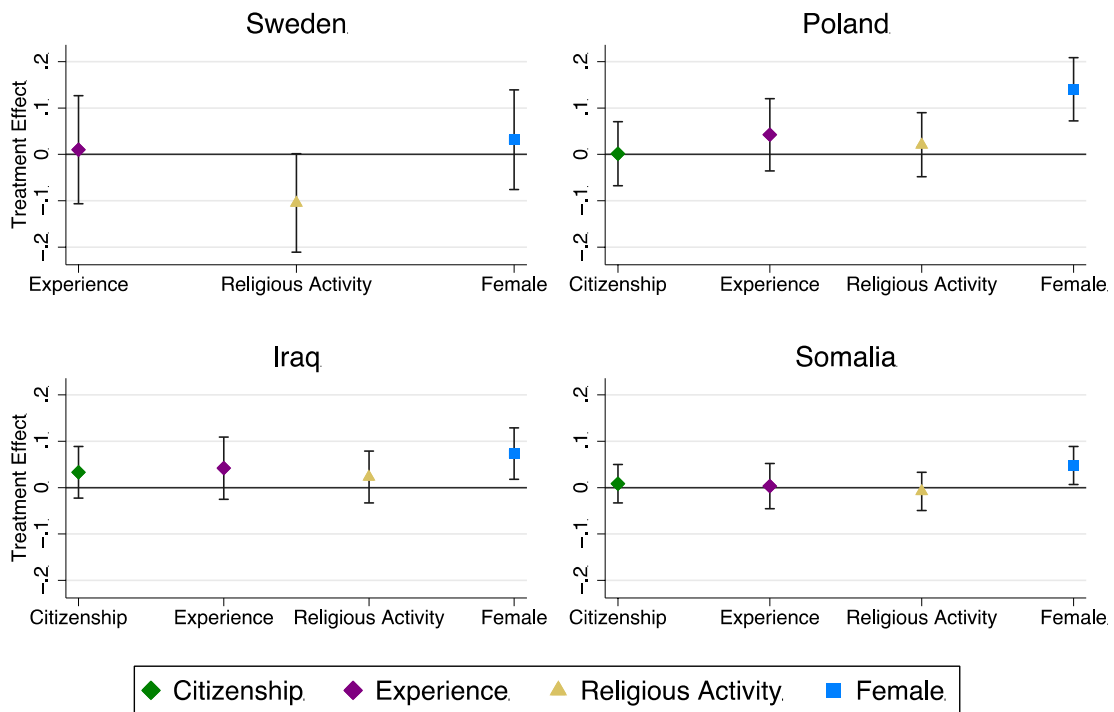
	(1)	(2)	(3)	(4)
<b>Country of birth:</b>				
Poland	-0.031 (0.037)	-0.018 (0.045)	-0.086* (0.048)	-0.079* (0.044)
Iraq	-0.116** (0.048)	-0.082* (0.042)	-0.157*** (0.047)	-0.114*** (0.042)
Somalia	-0.154*** (0.047)	-0.114*** (0.041)	-0.192*** (0.044)	-0.152*** (0.040)
Citizenship (Yes)		0.014 (0.017)	0.014 (0.017)	0.015 (0.017)
Gender (Female)	0.079*** (0.016)	0.078*** (0.016)	0.079*** (0.016)	0.032 (0.055)
Religious Acitivity (Yes)	-0.005 (0.016)	-0.006 (0.016)	-0.105* (0.054)	-0.005 (0.016)
Previous Experience (Yes)	0.026 (0.019)	0.010 (0.059)	0.026 (0.019)	0.027 (0.019)
Job requirement (Waiting Experience)	0.043* (0.024)	0.123* (0.074)	0.045* (0.024)	0.044* (0.024)
<b>Country of birth×Citizenship:</b>				
Poland×Citizenship	0.001 (0.035)			
Iraq×Citizenship	0.032 (0.045)			
Somalia×Citizenship	0.007 (0.041)			
<b>Country of birth×Previous Experience:</b>				
Poland×Previous Experience		0.032 (0.071)		
Iraq×Previous Experience		0.032 (0.068)		
Somalia×Previous Experience		-0.007 (0.064)		
<b>Country of birth×Job Requirement:</b>				
Poland×Job Requirement		-0.084 (0.091)		
Iraq×Job Requirement		-0.101 (0.085)		
Somalia×Job Requirement		-0.099 (0.081)		
<b>Country of birth×Religious Activity:</b>				
Poland×Religious Activity			0.125* (0.065)	
Iraq×Religious Activity			0.128** (0.061)	
Somalia×Religious Activity			0.097* (0.058)	
<b>Country of birth×Gender (Female):</b>				
Poland×Gender(Female)				0.109* (0.065)
Iraq×Gender(Female)				0.042 (0.062)
Somalia×Gender(Female)				0.016 (0.059)
Observations	1,492	1,492	1,492	1,492
R-squared	0.094	0.097	0.098	0.098
City Dummies	Yes	Yes	Yes	Yes
Time-trend	Yes	Yes	Yes	Yes
Adj. R-aquared	0.084	0.084	0.087	0.087

Notes: The dependent variable is a dummy indicating whether applicant was called to an interview (1) or not (0). OLS coefficients; robust standard errors in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1



We next turn to interactions of country of birth and gender (column 4). Recall that the registry analysis showed that among the foreign-born, women are less likely to be employed. However, this outcome could be the result of fewer immigrant women entering the labor force, rather than being due to employer discrimination. The estimates in Column 4 of Table 4 suggest that ethnic penalties are in fact smaller for women than for men. The interaction between country of birth and gender is, however, only statistically significant at conventional levels for the Polish-born.

Figure 2. Impact of Treatments on Callback Rates by Country of Birth



Note: Capped bars represent 95% confidence intervals.

Finally, the results reported in Table 4 allow us to study whether attributes that immigrants can affect – the adoption of citizenship, the accumulation of skills, and signals of their religious practice – influence their recruitment prospects. Based on the interaction analyses in Table 4, we have calculated the marginal effects of all other treatments conditional on country of birth. For ease of interpretation, we present these conditional treatment effects in Figure 2. Only the ascriptive trait of gender has an effect across countries of birth. In particular, we find that being a woman has a positive and statistically significant effect among all immigrant groups.

These findings conflict with accounts that document the multiple burdens of being a woman and belonging to a minority ethnic group (King 1988) and are more consistent with role theories formulated in social psychology that emphasize that stereotypes about other nationalities are mainly ascribed to males of those nationalities (Eagly and Kite 1987). To the extent that employers hold negative stereotypes of Polish-, Iraqi- and Somali-born immigrants, we should expect males belonging to these groups to receive fewer callbacks than their female compatriots. For the Swedish group of applicants, however, there is little penalty for being male, which in turn could suggest that the gendered nature of callback rates among immigrants is not driven by a general, industry-specific preference for female labor, but rather gendered stereotypes.

### **3 Discussion**

Using a factorial field experiment, our study has gone beyond most existing work in simultaneously testing the impact of a number of factors on discrimination in the hiring process. The results of our experiment paint a bleak picture: We find substantial evidence in support of employer discrimination against immigrants and no evidence that immigrants can reduce this discrimination by acquiring citizenship or investing in job-related experience. Ethnic hierarchies decisively influence immigrants' employment prospects, leaving Iraqis and especially Somalis with much reduced employment chances. Moreover, among these groups, men fare particularly poorly. It bears repeating that Somali men only receive callbacks to 2.5 percent of applications, while the comparable number is 19 percent for Swedish men.

These outcomes are disconcerting not only on normative grounds, but also from a policy perspective. Employers subscribe to an ethnic hierarchy when judging applicants, and their assessments are seemingly impervious to attributes that signal productivity or integration. Fixed traits such as country of birth and gender over which applicants have no control are given much more weight in the hiring process than are characteristics that immigrants and policy measures can actually affect. Though acquiring citizenship and work experience bring benefits, they do not help immigrants obtain jobs in the sector studied here.

At the same time, however, we find that religious activity (vs. religious denomination *per se*) does not appear to harm immigrant groups. It is therefore unlikely that discrimination is only driven by concerns about accommodating religious practice at the workplace. The high degree of rejection of presumptive Muslim applicants is instead likely rooted in taste-based prejudice that is not directly tied to the requirements of their faith but that also reflects ethnic hierarchies (especially given higher levels of discrimination against Somali vs. Iraqi-born applicants). However, since correspondence tests such as ours face limitations in documenting employer beliefs about religious practice, future research should investigate this pathway further. At a minimum, we can state that signals about religious activity among the included immigrant groups does not reduce employer callback rates.

Additionally, future research should replicate our design in other settings. For example, it would be illuminating to test whether ethnic hierarchies influence employer responses in jobs in the restaurant and café sector that do not require interaction with customers (e.g., line cook or cleaning staff). Furthermore, future work can examine whether citizenship, by signaling rootedness and legal status, raises callback rates when employers want to reduce the probability of turnover, or in countries where citizenship is particularly difficult to obtain and therefore communicates a high degree of integration. Finally, it bears repeating that this particular study has focused on charting discrimination in the Hotels and Restaurants sector against young individuals with long residencies in Sweden. We would therefore welcome work that broadens the characteristics of the applicants to include other sectors, and individuals with different age groups with different times of residence. The factorial design used in this paper could be adapted to efficiently study the impact of such characteristics on the rate of discrimination.

## 4 Materials and methods

### 4.1 Empirical strategy for the registry data analysis

*Data.* We use anonymized government registry data that are based on administrative records and are collected by the government statistical agency *Statistics Sweden*. The data include information on all Swedish residents' age, gender, level of education, country of birth, city of residence and annual wage income. The data on foreign-born residents also includes indicators on citizenship adoption and the number of years since immigration to Sweden. We restrict our analysis to members of the working age population (ages 15-64).

The dependent variable captures whether an individual had any wage income during 2015 (1/0 denotes had/did not have wage income in a given year). For reasons of confidentiality, the "county-of-birth" variable sometimes aggregates several countries that have sent relatively small numbers of migrants to Sweden into a regional category. In our case, this applies to Somalia (but not to Poland or Iraq), which is grouped with Eritrea, Ethiopia, Sudan and Djibouti. Will this grouping compromise our ability to document ethnic hierarchies? First, aggregate statistics indicate that in 2015, the Somali-born made up more than 55% of this group, whereas the second largest sender, Eritrea, made up 26%. Second, like Somalia, Djibouti and Sudan are majority Muslim, whereas Muslims constitute a significant (but not a majority) share of the population in Eritrea and Ethiopia. However, our ability to document ethnic hierarchies is, to some extent, confounded by the conflation of these two countries of origin since aggregate data show that the Eritrean-born are more integrated into the Swedish labor market than are the Somali-born (Heggeman and Gärdqvist 2016). Fortunately, the correspondence study does not face similar methodological problems.

The Swedish citizenship measure relies on a variable that indicates the date on which an individual has changed his or her citizenship, but it does not indicate an individuals' nationality. Given that there is usually a residency requirement for individuals who seek citizenship in a country, we can assume that these changes are from citizenship of another country to Swedish citizenship (for a similar approach, see Engdahl 2014 who also lacks direct measures of citizenship status).

*Oaxaca-Blinder decomposition.* When analyzing immigrant-native employment gaps we employ a modified version of the Oaxaca-Blinder decomposition technique (Blinder

1973; Oaxaca 1973), which has frequently been used to study discrimination in the labor market (e.g. Fortin et al. 2011; Oaxaca and Ransom 1994; Reimers 1983). The decomposition begins with estimating an employment equation of the following form:

$$Y_{ij} = \mathbf{X}_{ij}\boldsymbol{\beta}_j + \varepsilon_{ij} \quad (1)$$

where  $Y_{ij}$  is a dichotomous variable indicating whether individual  $i$  in group  $j$  has had any wage income during 2015. The groups,  $j$ , studied in this paper are the Swedish-, Polish-, Iraqi- and Somali-born and are indexed  $N$ ,  $P$ ,  $I$  and  $S$ , respectively.  $\mathbf{X}_{ij}$  is a vector of individual characteristics. For the Swedish-born group ( $N$ ) this vector includes their level of education, gender, age and age-squared. Moreover, for the Polish-, Iraqi- and Somali-born, an additional set of covariates are included: citizenship, time in Sweden, and time in Sweden squared.  $\boldsymbol{\beta}_j$  is the vector of regression coefficients which are allowed to vary across the country of origin-groups and are therefore indexed by  $j$ . Finally,  $\varepsilon_{ij}$  is the error term. We estimate this using a linear probability model since it eases both the computational burden (we are running decompositions using close to five million observations) and the interpretation of the results.

After having estimated (1) for each group, we decompose the differences in mean employment outcomes between the Swedish-born and the foreign-born groups. In particular, we use the results from the group-wise regressions to decompose the employment gap into two components: (1) The explained part: group-wise differences in characteristics (such as average differences in the level of education across groups) that affect employment probabilities, and (2) the unexplained part: group-wise differences in regression coefficients, which capture differences in returns to characteristics that affect employment probabilities. For instance, the employment gap between Swedish-born (indexed  $N$ ) and Polish-born (indexed  $P$ ) can be written formally as:

$$\bar{Y}^N - \bar{Y}^P = (\bar{X}^N - \bar{X}^P)\boldsymbol{\beta}^* + [\bar{X}^N(\boldsymbol{\beta}^N - \boldsymbol{\beta}^*) - \bar{X}^P(\boldsymbol{\beta}^P - \boldsymbol{\beta}^*)] \quad (2)$$

The term on the left-hand side of equation (2) is the employment gap. The right-hand side expresses this gap as the explained part (the first term) and the unexplained part (the second term). For the explained part,  $\boldsymbol{\beta}^*$ , which is the coefficient vector that would obtain in the absence of discrimination, is multiplied by the difference between the vectors of

group-level averages for the covariates.<sup>22</sup> The second term on the right-hand side is the unexplained part, and it can be interpreted as measuring how much of the employment gap can be attributed to the fact that the groups' coefficient vectors deviate from what we would obtain in the absence of discrimination.

Finally, our Oaxaca-Blinder decomposition departs in one way from traditional applications because we include time of residence and citizenship in the employment equations for the foreign-born groups, but not in the one for the Swedish natives.<sup>23</sup> We follow Aldashev et al.'s (2008) suggestion for ways to modify the decomposition when the sets of covariates differ across groups. Specifically, we first estimate the full regressions for the non-native groups, and then re-estimate them excluding time in country and citizenship and constraining the coefficients for the other covariates to be the same as in the full regression. The effects of average time in Sweden and citizenship is then picked up by the constant in the second step, after which the decomposition can proceed as described above.

## 4.2 Design of the CV study

*Experimental design for the CV study.* In the experiment, we employ a fractional factorial design. More specifically, we are studying four two-level factors and one four-level factor, but not all combinations of factors are used in the CVs. The advantage of factorial designs, such as ours, is that they allow us to study the *main* effects of several factors using the same sample size that would be required for the study of one independent variable without loss of statistical power. Moreover, the factorial design allows one to test for interactions between treatments. Note, however, that when studying interaction effects statistical power will be lower compared to when studying main effects (Collins et al. 2009).

For all job applications, gender (Male/Female), post high-school work-experience (Yes/No) and religious activity (Yes/No) are randomly varied. For the foreign-born group, country of birth (Poland, Somalia, Iraq) and citizenship (Yes/No) are varied.

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<sup>22</sup> How to compute  $\beta^*$  has been discussed extensively in the literature (e.g., Fortin et al. 2011). Sloczynski (2013) depicts this choice as a problem of choosing the right parameter for weighting the regression coefficients for the two groups being compared. For the example of comparing Swedish- and Polish-born we have:

$$\beta^* = \delta\beta^N + (1 - \delta)\beta^P \quad (3)$$

We follow Sloczynski's suggestion and set  $\delta$  equal to the share of the foreign-born group in the total, in which case it can be shown that the unexplained part recovers the average treatment effect (Sloczynski 2013).

<sup>23</sup> Time in Sweden is indistinguishable from age for Swedish-born and citizenship hardly varies.

Including the combination of being Swedish-born but being a foreign citizen in the CV and cover letter was considered problematic since it would entail a slightly different treatment than for the immigrants.<sup>24</sup> The design for immigrants was thus a  $2^4 \times 3$  factorial experiment, whereas the design for natives was a  $2^3$  experiment.<sup>25</sup> All in all, then, 56 different combinations of traits are randomly assigned to the CVs. In addition, during the pilot study (see below) we discovered that many of the job openings required previous experience in the form of cashier and waiting staff experience. For those job candidates who had post high-school work experience, the cover letters and CVs included descriptions of cashier and waiting staff experience. The job openings that were coded as requiring cashier and waiting staff experience only received applications with post high-school work-experience, whereas the ones that listed no such requirement received CVs both with and without post high school work experience.

The design is summarized in Table 5. In Table A3 of the Online Appendix, we show correlations between treatments. While some treatments are correlated by design in a fractional factorial experiment like ours, the other remaining 25 correlations in Table A3 are statistically insignificant with one exception, suggesting that randomization succeeded.

*Occupational Sector, Age and Country of Origin.* To ensure maximum relevancy we chose to focus on the occupational sector in which the highest proportion of immigrants is employed. If we uncover discrimination in such a sector, we can conclude that discrimination has important consequences for the employment prospects of immigrants and, moreover, that our estimates of discrimination potentially represent a lower bound. In the Swedish case, this sector is Hotels and Restaurants. Furthermore, given the focus on young adults (applicants are 21 to 22 years old) and the high level of youth unemployment in Sweden, jobs in the Hotels and Restaurants sector are suitable because they frequently represent entry-level occupations. Finally, we chose to vary the country of birth of immigrant applicants, such that the largest sender country from within Eastern

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<sup>24</sup> The person would be a citizen of some other country, yet living in his/her country of birth (Sweden). Obviously, among the immigrants who are not Swedish citizens, none are living in their country of birth.

<sup>25</sup> Note that our design therefore forces a correlation between citizenship and country of birth. Since these traits are, by design, uncorrelated with the other traits we vary, the only complication this introduces is that we need to include country of birth as a control when estimating the treatment effect of citizenship, and vice versa.

Europe, Asia and Africa (given our age focus), were included (Poland, Iraq and Somalia, respectively).

Table 5. Fractional factorial design for the correspondence study

<b>No Requirement:</b> Ad does not require previous cashier and waiting staff experience				
Factor	Levels			
Country of birth	Sweden (1/7)	Poland (2/7)	Iraq (2/7)	Somalia (2/7)
Swedish citizenship	Yes	Yes/No	Yes/No	Yes/No
Religious activity	Yes/No	Yes/No	Yes/No	Yes/No
Gender	Male/Female	Male/Female	Male/Female	Male/Female
Previous experience	Less/More	Less/More	Less/More	Less/More
<b>Requirement:</b> Ad requires previous cashier and waiting staff experience				
Factor	Levels			
Country of birth	Sweden (1/7)	Poland (2/7)	Iraq (2/7)	Somalia (2/7)
Swedish citizenship	Yes	Yes/No	Yes/No	Yes/No
Religious activity	Yes/No	Yes/No	Yes/No	Yes/No
Gender	Male/Female	Male/Female	Male/Female	Male/Female
Previous experience	More	More	More	More

*Construction of CVs.* The names were chosen based on common names in the fictitious applicant's country of birth. To test the robustness of our results to the specific choice of name we used two female and two male names for each country of birth.<sup>26</sup> Treatment effects did not differ at conventional levels of statistical significance for any of the eight pairs of names.<sup>27</sup> For people born in Poland, Iraq and Somalia, the city of birth was the capital of the country (Warsaw, Baghdad and Mogadishu, respectively). For people born in Sweden, the city of birth was one of the following (depending on which city the job opening was located in): Stockholm, Malmö, Gothenburg, Örebro, Västerås, Uppsala or Linköping. The previous residential area where the applicant went to primary and upper

<sup>26</sup> Swedish names: Malin Svensson, Emma Lindberg, Erik Svensson and Filip Lindberg. Polish names: Julia Kowalski, Maria Nowak, Antoni Kowalski and Mateusz Nowak. Iraqi names: Sarah Amari, Zainab Hadad, Ahmed Amari and Ali Hadad. Somali names: Awa Zakaria, Fatima Mohamed, Khalid Zakaria and Abdi Mohamed.

<sup>27</sup> Results are available from the authors.



secondary school (the latter with a social science program) and current address were chosen with regard to average income and demographics,<sup>28</sup> and we selected an existing public upper secondary school (with a social science program) in the previous residential area. The resume contained no description of a specific primary school but was described as follows: “Degree primary school, [previous residential area]”. All persons (including the Swedish-born applicants) were described as having Swedish, English and some German language skills.<sup>29</sup> Applicants who were born abroad indicated an additional language based on their country of birth.<sup>30</sup>

Three fictitious workplaces were used in the CVs. All applicants had worked at a café during upper secondary school. Their resumes also stated an internship at a cinema. Half of the applicants had additional work experience as a waiter at a restaurant since high school (these are considered as having more experience).<sup>31</sup> The CV also included associational memberships. It stated that the applicant was member of football club in his/her residential area, and the club was therefore selected separately for each city.<sup>32</sup>

Additionally, half of the CVs included membership in a religious congregation. Like in many previous correspondence studies on the impact of religious affiliation on hiring discrimination, we signal religious affiliation by having applicants report activities in religious groups (Drydakis 2010; Pierné 2013; Wright et al. 2013; Wallace et al. 2014).<sup>33</sup> We selected the denomination and associated congregation on the basis of the dominant religion in the country of birth.<sup>34</sup> The religious congregation was selected separately for each city.<sup>35</sup> **Figure A1** and **Figure A2** in the Online Appendix present translated CVs of

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<sup>28</sup> For applicants in the Stockholm labor market, the previous residential area was Solna. The previous residential areas for the Gothenburg and Malmö labor markets were Mölndal and Oxie, respectively.

<sup>29</sup> In Swedish elementary schools, it is mandatory to study English and a third language (typically German, French or Spanish).

<sup>30</sup> Spoken language in a country can in some cases vary depending on city or region. In these cases, the language skills were chosen based on the city/region where the person was born.

<sup>31</sup> The café and restaurant are fictitious. Before sending out applications, we ensured that no restaurants or cafés with these names already existed in Stockholm, Malmö or Gothenburg. In the choice of cinema we aimed for a large cinema with reasonable distance to where the applicant grew up.

<sup>32</sup> The football club was chosen based on the following criteria: a reasonable distance from home for practices, existing women’s and men’s team, as well as an active team in fourth league.

<sup>33</sup> The only exceptions to this strategy that we know of are Adida et al. (2010) and Banerjee et al. (2009) who work in contexts where variations in last names can be used to signal religious denomination, something that would have been impossible in our case, especially given our choice of countries of origin.

<sup>34</sup> In cases where the dominant religion varies regionally, the choice of religion was based on the dominant religion in the applicant’s region of birth.

<sup>35</sup> The selection of the congregation was based on the following criteria: distance from home and that the congregation had an active youth section. Swedish applicants were active in a Protestant congregation, whereas Polish applicants were active in a Catholic congregation. Somalis and Iraqis were active in Muslim congregations.

a male Iraqi-born Swedish citizen who is active in a mosque in Stockholm (with more or less previous work experience, respectively).

*Cover Letters.* All cover letters began with a self-description of the applicant as “cooperative, ambitious and resourceful” followed by a description of his or her commitment to football, with the focal point on the positive qualities which this commitment generated, and previous work experience. To increase the response rate, the work experience matched the qualities that were commonly mentioned in work ads. For example, resumés with experience from before graduation at a café, stated cashier and barista skills. Resumés with work experience gained after graduation at a restaurant, stated skills such as table service and management. **Figure A 3** and **Figure A 4** in the Online Appendix present translated cover letters of a male Iraqi-born Swedish citizen who is active in a mosque in Stockholm (with more or less previous work experience, respectively).

*Geographical Boundaries.* We selected workplaces based on commuting times, and, using a city’s internet-based travel planner, set the maximum commute time to 50 minutes. An alternative strategy could have been to only send applications to jobs within the municipality, but this would have excluded workplaces that are close to the applicant’s home but outside of the municipality.

*Types of Jobs.* Job applications were sent to ads for job openings announced on the Swedish Public Employment Agency’s website *Platsbanken*. In particular, we applied for jobs within the category Hotels and Restaurants whose qualifications were consistent with the work experience in the resumé. We excluded jobs that required photos of the applicant, or where applications had to be submitted through means other than email. Specifically, we excluded website-based applications because they would have constrained our ability to vary relevant applicant characteristics and to design the layout. Moreover, these sites normally require social security numbers. Recruitment meetings or telephone applications were ruled out because these applications require some form of acting. The exclusion of jobs where the application was submitted through an application-website (where each application has to follow a very specific format) could potentially limit our ability to generalize our findings to large fast-food chains which typically recruit in this way.

*Job Advertisements.* Of the 56 possible CVs, we randomly selected only one to be sent to each job opening. By contrast, many other correspondence studies send two or more applications to each opening. We do so because, first, we were concerned that sending several CVs to the same job opening would increase the risk of detection and, second, we worried about ethical implications. Specifically, we wanted to minimize the time spent by employers on fictitious applicants and to avoid the risk that employers would overestimate the labor supply, which could in turn affect their hiring and treatment of future employees.

*Replies.* We registered replies (which came in via email or phone voice message) in two ways. Either the applicant got a positive response (coded 1). Positive responses most often came in the form of an invitation to interview, but they could also consist of requests for more information (e.g. “Are you still interested in coming to work for us?”) or direct offers of work. Our applicants also often received replies in which they were told that the employer had chosen to move forward with another candidate (coded 0). (Automatic reply messages that confirmed receipt of the application were not registered as invitations to interview.) Whenever employers sent a positive response, we quickly answered and courteously declined, explaining that the applicant had already accepted another job offer. In case the employer did not respond, this was also coded as 0.

*Pilot Study.* Between January and February in 2016, we carried out a pilot study in the Stockholm labor market. We sent applications to 90 job openings announced on the Swedish Public Employment Agency’s website *Platsbanken*. Since the callback rate was low (5 invitations), we implemented a number of changes to make our fictitious candidates more attractive to employers. First, we improved the layout of the CVs. Second, we enhanced the cover letters and CVs, adding more detailed information about skills that are generally relevant to working in the category of jobs studied.<sup>36</sup> These two changes were implemented in an identical fashion in all cover letters and CVs, and thus raised the general quality of all our candidates. Third, in the pilot study we had an additional factor to the ones listed above, namely whether the applicant had relevant work experience during high school. To increase the response rate in the main study, we decided that all resumé would include this experience.

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<sup>36</sup> To do so, we examined the 90 job ads from the pilot study for the skills that employers list as desirable.

*Econometric model.* To ease the interpretation of our results, we consistently use a linear probability model to estimate the effects of the various treatments. All the results, however, remain very similar when using logistic regression; see Tables A1 and A2 in the Online Appendix. In our regression analyses, we have included both a lowess-smoothed time-trend with a bandwidth of 5% to account for seasonal variations and dummy variables for each of the seven cities included in our study. We analyze main effect and two-way interaction effects. The reason for not studying more complex interactions is the above-mentioned power considerations.

## Appendix

Table A1. CV Experiment Results. DV: Invited to interview. Logit estimates

	(1)	(2)
<b>Country of birth:</b>		
Poland	-0.142 (0.238)	-0.126 (0.242)
Iraq	-0.763*** (0.262)	-0.766*** (0.268)
Somalia	-1.549*** (0.302)	-1.578*** (0.304)
Citizenship (Yes)	0.184 (0.189)	0.195 (0.191)
Gender (Female)		0.852*** (0.178)
Religious Acitivity (Yes)		-0.031 (0.170)
Previous Experience (More)		0.345* (0.203)
Job requirement (Waiting experience)		0.315 (0.219)
Observations	1,492	1,492
Pseudo-R-squared	0.097	0.130
City Dummies	Yes	Yes
Time-trend	Yes	Yes

*Notes:* The dependent variable is a dummy indicating whether applicant was called to an interview (1) or not (0). Logit coefficients; robust standard errors in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table A2. Heterogeneous effects of country of birth. DV: Invited to interview. Logit estimates.

	(1)	(2)	(3)	(4)
<b>Country of birth:</b>				
Poland	-0.031 (0.528)	-0.098 (0.367)	-0.562* (0.323)	-0.665* (0.365)
Iraq	-0.863 (0.554)	-0.832** (0.404)	-1.259*** (0.378)	-1.137*** (0.404)
Somalia	-1.568*** (0.355)	-1.489*** (0.469)	-1.861*** (0.414)	-2.213*** (0.538)
Citizenship (Yes)		0.200 (0.191)	0.191 (0.192)	0.198 (0.193)
Gender (Female)	0.850*** (0.178)	0.845*** (0.178)	0.868*** (0.180)	0.215 (0.362)
Religious Aciticity (Yes)	-0.034 (0.170)	-0.038 (0.171)	-0.727* (0.376)	-0.020 (0.171)
Previous Experience (Yes)	0.344* (0.203)	0.164 (0.447)	0.338* (0.202)	0.337* (0.204)
Job requirement (Waiting Experience)	0.319 (0.219)	0.626 (0.455)	0.339 (0.220)	0.352 (0.221)
<b>Country of birth×Citizenship:</b>				
Poland×Citizenship	0.049 (0.267)			
Iraq×Citizenship	0.410 (0.340)			
Somalia×Citizenship	0.215 (0.459)			
<b>Country of birth×Previous Experience:</b>				
Poland×Previous Experience		0.193 (0.543)		
Iraq×Previous Experience		0.388 (0.600)		
Somalia×Previous Experience		0.041 (0.718)		
<b>Country of birth×Job Requirement:</b>				
Poland×Job Requirement		-0.420 (0.575)		
Iraq×Job Requirement		-0.455 (0.619)		
Somalia×Job Requirement		-0.297 (0.744)		
<b>Country of birth×Religious Activity:</b>				
Poland×Religious Activity			0.922** (0.464)	
Iraq×Religious Activity			1.017**	

	(1)	(2)	(3)	(4)
			(0.512)	
Somalia×Religious Activity			0.620	
			(0.594)	
<b>Country of birth×Gender (Female):</b>				
Poland×Gender(Female)				0.916*
				(0.469)
Iraq×Gender(Female)				0.654
				(0.504)
Somalia×Gender(Female)				1.024
				(0.650)
Observations	1,492	1,492	1,492	1,492
Pseudo-R-squared	0.131	0.135	0.134	0.131
City Dummies	Yes	Yes	Yes	Yes
Time-trend	Yes	Yes	Yes	Yes

*Notes:* The dependent variable is a dummy indicating whether applicants called to an interview (1) or not (0). Logit coefficients; robust standard errors in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \*p<0.1

Table A 3. Correlations between treatments

	Citizenship (Yes)	Religious Acitivity (Yes)	Previous Experience (Yes)	Sweden	Poland	Iraq	Somalia	Gender (Female)	Job requi- rement
Citizenship (Yes)	1.00								
Religious Acitivity (Yes)	-0.01 (0.69)	1.00							
Previous Experience (Yes)	-0.00 (0.95)	-0.00 (0.99)	1.00						
Sweden	0.36 (0.00)	-0.02 (0.55)	0.02 (0.41)	1.00					
Poland	-0.08 (0.00)	0.00 (0.95)	0.01 (0.68)	-0.26 (0.00)	1.00				
Iraq	-0.10 (0.00)	0.02 (0.49)	-0.03 (0.31)	-0.26 (0.00)	-0.40 (0.00)	1.00			
Somalia	-0.10 (0.00)	-0.01 (0.78)	-0.00 (0.97)	-0.26 (0.00)	-0.40 (0.00)	-0.40 (0.00)	1.00		
Gender (Female)	-0.01 (0.70)	-0.02 (0.50)	-0.02 (0.50)	-0.01 (0.83)	0.01 (0.59)	-0.01 (0.83)	-0.00 (0.88)	1.00	
Job requirement	0.04 (0.17)	-0.04 (0.15)	0.53 (0.00)	0.03 (0.20)	-0.05 (0.06)	0.03 (0.31)	-0.00 (0.88)	0.02 (0.35)	1.00

Notes: Pearson's r. Entries in parentheses are p-values. Shaded areas show treatments that are correlated by design.



Table A 4. Main treatment effects and differences in treatment effects across countries of origin using Benjamini-Hochberg method to control for false discovery rate

	Main effects	Differences in treatment effects across countries of origin					
		Sweden-Poland	Sweden-Iraq	Sweden-Somalia	Poland-Iraq	Poland-Somalia	Iraq-Somalia
Poland	-0.024 (0.470)						
Iraq	-0.094# (0.004)						
Somalia	-0.144# (0.000)						
Citizenship (Yes)	0.014 (0.390)				-0.032 (0.484)	-0.007 (0.864)	0.025 (0.484)
Previous Experience (Yes)	0.026 (0.167)	-0.032 (0.651)	-0.032 (0.640)	0.007 (0.915)	0.000 (0.995)	0.039 (0.402)	0.039 (0.357)
Job Requirement	0.043 (0.078)	0.084 (0.354)	0.101 (0.237)	0.099 (0.221)	0.017 (0.807)	0.015 (0.810)	-0.002 (0.974)
Religious Activity (Yes)	-0.005 (0.759)	-0.125 (0.052)	-0.128 (0.037)	-0.097 (0.095)	-0.002 (0.959)	0.029 (0.483)	0.031 (0.378)
Gender (Female)	0.079# (0.000)	-0.109 (0.094)	-0.042 (0.498)	-0.016 (0.782)	0.067 (0.135)	0.093 (0.022)	0.026 (0.467)

Notes: Entries are differences in treatment effects based on Table 3 and Table 4 in main text. Entries in parentheses are p-values. # significant when false discovery rate is set to 0.05

Figure A1. Example of a CV with less previous experience

# Ahmed Amari

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Prästgårdsgatan 45, 172 32 Sundbyberg, Stockholm | Telephone:  
0739691921 | ahmed.amari776@gmail.com

## About me

I was born in 1995 in Baghdad, but came to Sweden as a 6-year-old and became a Swedish citizen two years ago. I am at the moment living in Sundbyberg. In my spare time I like to spend time with my family and friends. Football is also a great interest of mine. Through it, I have developed my ability to work with different types of people, which allows me to easily adapt to new contexts. I'm sure I will fit in well in your workplace.

## Education

- 2011-2014 Social Science Programme, Solna Upper secondary school, Solna
- 2011 Degree Primary school, Solna

## Work experience

### RESTAURANT ASSISTANT/BARISTA | KANELBULLENS CAFÉ | 2011-2014

- The tasks included making simple meals and drinks, cash assistant, dishwashing and cleaning.

### INTERNSHIP | KANELBULLENS CAFÉ | 2011

- The tasks included cleaning and making simple meals and drinks.

### INTERNSHIP | SF FILMSTADEN KISTA | 2010

- The tasks included cleaning of movie theaters and ticketing.

## Other associations

SPÅNGA IS FOTBOLL

STOCKHOLMS MOSKÉ

## Other qualifications

DRIVER'S LICENCE

LANGUAGES:

- English, Swedish, Arabic and some German

Figure A2. Example of a CV with more previous experience

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# Ahmed Amari

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Prästgårdsgatan 45, 172 32 Sundbyberg, Stockholm | Telephone:  
0739691921 | ahmed.amari776@gmail.com

**About me**

I was born in 1995 in Baghdad, but came to Sweden as a 6-year-old and became a Swedish citizen two years ago. I am at the moment living in Sundbyberg. In my spare time I like to spend time with my family and friends. Football is also a great interest of mine. Through it, I have developed my ability to work with different types of people, which allows me to easily adapt to new contexts. I'm sure I will fit in well in your workplace.

**Education**

- 2011-2014 Social Science Programme, Solna Upper secondary school, Solna
- 2011 Degree Primary school, Solna

**Work experience**

**WAITER | BISTRO LA MÉDITERRANÉE | 2014-**

- The tasks included table service and management of the restaurant's reservations.

**RESTAURANT ASSISTANT/BARISTA | KANELBULLENS CAFÉ | 2011-2014**

- The tasks included making simple meals and drinks, cash assistant, dishwashing and cleaning.

**INTERNSHIP | KANELBULLENS CAFÉ | 2011**

- The tasks included cleaning and making simple meals and drinks.

**INTERNSHIP | SF FILMSTADEN KISTA | 2010**

- The tasks included cleaning of movie theaters and ticketing.

**Other associations**

**SPÅNGA IS FOTBOLL**

**STOCKHOLMS MOSKÉ**

**Other qualifications**

**DRIVER'S LICENCE**

**LANGUAGES:**

- English, Swedish, Arabic and some German

Figure A 3. Example of a cover letter with less previous experience

## Ahmed Amari

Prästgårdsgatan 45, 172 32 Sundbyberg, Stockholm | Telephone:  
0739691921 | ahmed.amari776@gmail.com

### Cover letter

I quickly realized after finding your work ad at Platsbanken that this job suits me perfectly.

I am a cooperative, ambitious and resourceful person. People around me perceive me as a very positive and energetic person. I have a great interest in sport, especially football. Through my involvement in football, I have developed a great ability to cooperate, which I think will be a great advantage in this type of work. It is also by experience in team sports I have developed my ambitious side. I have a habit of always striving for the best possible results and my ability to take initiatives is often an advantage in these situations.

With my previous work experience, I believe I have a good foundation to stand on. Through my experience of working as a restaurant assistant at Kanelbullens café, I have developed my social skills that can come in handy in many situations. I have also developed the ability to adapt to different situations that may occur. I also have good skills as a cashier and basic barista skills. I like working with people and always strive to provide a good service.

I am applying for this job because I am searching for new challenges. I am convinced that I will learn a lot but also contribute much. I will tell you more about myself during a possible interview!

Greetings,  
Ahmed

Figure A 4. Example of a cover letter with more previous experience

## Ahmed Amari

Prästgårdsgatan 45, 172 32 Sundbyberg, Stockholm | Telephone:  
0739691921 | ahmed.amari776@gmail.com

### Cover letter

I quickly realized after finding your work ad at platsbanken that this job suits me perfectly.

I am a cooperative, ambitious and resourceful person. People around me perceive me as a very positive and energetic person. I have a great interest in sport, especially football. Through my involvement in football, I have developed a great ability to cooperate, which I think will be a great advantage in this type of work. It is also by experience in team sports I have developed my ambitious side. I have a habit of always striving for the best possible results and my ability to take initiatives is often an advantage in these situations.

With my previous work experience, I believe I have a good foundation to stand on. Through my experience of working as a restaurant assistant at Kanelbullens café, I have developed my social skills that can come in handy in many situations. I have also developed the ability to adapt to different situations that may occur. I also have good skills as a cashier and basic barista skills.

I like working with people and always strive to provide a good service. In my previous work as a waiter at Bistro la Méditerranée, I have had the opportunity to further develop my social skills and also learn to handle stressful situations. I am also very familiar with the skills required to be a waiter, for example serving dishes at tables according to order and cash handling and credit card payment procedures.

I am applying for this job because I am searching for new challenges. I am convinced that I will learn a lot but also contribute much. I will tell you more about myself during a possible interview!

Greetings,  
Ahmed

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