

Eriksson, Stefan; Johansson, Per; Langenskiöld, Sophie

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What is the right profile for getting a job? A stated choice experiment of the recruitment process

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A Stated Choice Experiment of the Recruitment Process**

Stefan Eriksson
Per Johansson
Sophie Langenskiöld

June 2012

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Stefan Eriksson

Uppsala University

Per Johansson

*IFAU, Uppsala University
and IZA*

Sophie Langenskiöld

IFAU

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

What is the Right Profile for Getting a Job? A Stated Choice Experiment of the Recruitment Process^{*}

We study the recruitment behavior of Swedish employers using data from a stated choice experiment. In the experiment, the employers are first asked to describe an employee who recently and voluntarily left the firm, and then to choose between two hypothetical applicants to invite to a job interview or to hire as a replacement for their previous employee. The two applicants differ with respect to characteristics such as gender, age, education, experience, ethnicity, religious beliefs, family situation, weight, and health. Our results show that employers discriminate against applicants who are old, non-European, Muslim, Jewish, obese, have several children, or have a history of sickness absence. Moreover, increasing the firms' cost of uncertainty in hiring – through more firm co-payment in the sickness benefit system – may reduce hiring, but does not affect the degree of discrimination. Also, there are only small differences in the degree of discrimination between different types of recruiters and firms. Overall, our results suggest that the discrimination, at least partially, should reflect statistical discrimination.

JEL Classification: J71

Keywords: stated choice experiment, discrimination, gender, age, ethnicity, obesity, sickness absence

Corresponding author:

Per Johansson
IFAU
Box 513
SE-75120 Uppsala
Sweden
E-mail: per.johansson@ifau.uu.se

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1 Introduction

In Sweden, as in most other Western economies, discrimination in the labor market is a major issue in the policy debate. Empirical evidence suggests that observed earnings differentials to a large extent are the result of differences in hiring and promotion.¹ Hence, understanding the sorting of individuals to jobs, through hiring, promotion and potentially also layoffs/firing, seems crucial for understanding the observed differences in labor market outcomes across groups.

Preventing discrimination is often a top priority for the authorities. In Sweden, the Discrimination Act states that employers are not allowed to discriminate based on sex, transgender identity or expression, ethnicity, religion or other belief, disability, sexual orientation or age. Hence, employers are not allowed to make hiring or pay decisions based on these criteria. However, profit-maximizing (or cost minimizing) employers may have incentives to use these easily observed characteristics to sort job applicants if they believe that these factors are correlated with productivity.

Detecting discrimination is, however, a difficult task since it is often possible for employers to defend their behavior by arguing that they have based their hiring decisions on legitimate rather than discriminatory factors. It is, for example, often possible for employers to argue that that the language skills of an immigrant and a native-born person differ.

Many policy attempts have been made to reduce discrimination. For example, anonymous application procedures have been designed where the applicants' gender and ethnicity are hidden from the recruiters, but such policies can only prevent discrimination in the early stages of the hiring process since it is difficult to design and enforce a policy which blindfolds employers throughout the hiring process.² To formulate better policy, more knowledge about discrimination is clearly needed.

In general, it is very difficult to study discrimination using observational data. The

¹ See Edin & Åslund (2001) and the references cited in Section 2.1.

² The most well-known example of an attempt to hide the gender of the applicants throughout the hiring process is Goldin & Rouse (2000), who find that female musicians have a higher probability of getting hired when auditions are made behind a curtain. Åslund and Nordström Skans (2012) study a trial with anonymous applications in Sweden, and find that woman and individuals of non-Western origin have a higher probability of advancing to the interview

reason is that it is often impossible to distinguish between the effects of unobserved characteristics and discrimination. Therefore, many researchers use field experiments in the form of correspondence and audit studies.³ In a typical correspondence study, fictitious job applications which are identical in all aspects except for one (e.g. ethnicity or gender) are sent to employers, the response (callback) rates for the two groups are compared, and any difference is interpreted as evidence of discrimination. In a typical audit study, two auditors, who are trained to appear identical in all aspects except for one, are sent to employers, and the rate of job offers are compared.

Clearly, field experiments offer a better way of studying discrimination than studies using observational data since this approach makes it possible to circumvent the problems with unobserved heterogeneity and thus estimate causal effects. However, this approach also has some weaknesses. In particular, audit studies have been criticized (see e.g. Heckman, 1998, and Heckman & Siegelman, 1993). The concern is that it is very difficult to ensure that the auditors really appear identical to the employers in all other dimensions. The auditors are often aware of the purpose of the experiment, and this may affect their behavior. Moreover, it is not obvious what employers consider as important for productivity. If some important attribute is not averaged out over the groups of auditors, this may affect the estimates of the degree of discrimination.⁴ Correspondence studies are likely to offer a better strategy for identifying discrimination, but even this approach has some limitations. First, it is only possible to study discrimination in the initial stage of the hiring process. Therefore, these studies cannot tell us how discrimination affects the hiring and wage outcome. Second, even in a correspondence study, the estimates of the degree of discrimination may be affected by the choice of the other attributes in the applications.⁵ Third, it is often difficult to separate different types of discrimination in these studies.⁶ Preference-based discrimination is when employers

stage of the hiring process. Edin & Lagerström (2006) analyze a CV database where searchers are allowed to hide their names, and find that a woman has a 15 percent lower callback rate if she reveals her name.

³ See e.g. Riach & Rich (2002) for a review.

⁴ Heckman & Siegelman (1993) and Heckman (1998) show that if the tails of the distribution of an unobserved attribute differs between the groups then, even if the unobserved attribute is uncorrelated with respect to the minority/majority group, the fraction hired can differ in cases when there is no discrimination.

⁵ Both the choice of which attributes to include and the levels these attributes are given may affect the estimates of the degree of discrimination (c.f. Heckman & Siegelman, 1993).

⁶ See Altonji & Blank (1999) and Bertrand (2011) for reviews.

discriminate based on their own (or their employees/customers) preferences (c.f. Becker, 1957). Statistical discrimination is when information-constrained employers discriminate based on differences between groups in average (or dispersion of) productivity (c.f. Aigner & Cain, 1977, and Phelps, 1972). This distinction is important since the different types of discrimination may require very different policy responses; e.g. tighter enforcement of discrimination laws (preference-based discrimination) or training schemes (statistical discrimination). Finally, it may be argued that it is ethically questionable to subject employers to fictitious job search (c.f. Riach and Rich, 2004). Hence, there is a need for new approaches to study discrimination as a complement to the existing methods.

The main purpose of this study is to investigate if employers (recruiters) discriminate based on job applicants' gender, age, ethnicity, religious beliefs, number of children, weight, or history of sickness absence.⁷ Moreover, we try to distinguish between different types of discrimination. We study if the degree of discrimination is affected by the firms' cost of uncertainty in hiring, measured by their co-payment in the sickness benefit system, and if the degree of discrimination differs between different types of recruiters (e.g. male/female and immigrant/native-born) and firms (e.g. sector and size).

To this end, we conduct a stated choice experiment. This approach is often used in other areas of economics, but, as far as we know, our study is the first which uses this approach to study employers' hiring behavior.⁸ In the experiment, the recruiters are first asked to describe an employee who recently and voluntarily left the firm and then to choose between two hypothetical applicants to invite to a job interview or to hire as a replacement for their previous employee. The two applicants differ with respect to four attributes, but are identical to the previous employee in all other dimensions. To measure the marginal value of each attribute (i.e. in monetary terms), the wage is always one of the attributes which are varied. To measure the effect of the cost of uncertainty in hiring, the type of firm co-payment in the sickness benefit system is also always one of the attributes which are varied (we use three well-known alternative

⁷ Throughout the paper, we label different treatment of all these groups as discrimination. However, it should be noted that it is not illegal to use factors such as the applicants' weight or history of sickness absence as sorting criteria.

⁸ This approach is often used in transport, tourism and environmental economics.

schemes). In the first stage (interview), the other attributes are gender, age, education and experience. In the second stage (hiring), the other attributes are gender, ethnicity, religious beliefs, number of children, weight, and history of sickness absence.

The stated choice approach has several important advantages. First, it gives us complete control over the information available to the recruiting employers. This means that we can isolate the effects of each of the applicants' characteristics without having to worry about unobserved heterogeneity. This is similar to correspondence studies. Second, it makes it possible to study many different kinds of discrimination – e.g. gender, age, and ethnicity – simultaneously, which facilitates a comparison of the degrees of discrimination. This is different from traditional correspondence studies. Third, it makes it possible to study applicant characteristics which may be relevant in any of the stages of the hiring process; i.e. both the invitation to a job interview and the hiring decision. This is in contrast to correspondence studies where it is only possible to study the first stage of the hiring process. Fourth, the fact that we include the wage as one of the attributes which we vary makes it possible to calculate the marginal value of each characteristic; i.e. how much the wage must be reduced to make employers indifferent between applicants with and without a particular attribute (all else equal). This is a new way of quantifying the degree of discrimination which is not possible in any of the other approaches, and which may have implications for wage setting. Fifth, the fact that we vary the cost of uncertainty in hiring by considering several firm co-payment schemes in the sickness benefit system, and that we know the exact identity of the recruiter who chooses between the applicants, gives us two new ways of disentangling different types of discrimination: If the degree of discrimination is affected by the extent of firm co-payment, or does not differ between different types of recruiters such as e.g. immigrants and native-born persons, this is an indication of statistical discrimination. There is no reason to expect that the employers' preferences should vary with the extent of firm co-payment, or that different types of recruiters should have similar preferences. This type of analysis is usually not possible in correspondence studies since the identity of the recruiter is rarely known. Sixth, the fact that the employers can only choose between the two hypothetical applicants means that we are more likely to measure the degree of market discrimination than correspondence

studies where the employers often have several other (real) applicants to choose from.⁹ Finally, the fact that all recruiters know that they participate in an experiment, and are allowed to opt-out, means that no ethical concerns arise.

A limitation of the stated choice approach is that it is based on claimed rather than actual behavior. A concern is, therefore, that the employers may try to hide their discriminatory behavior; i.e. a strategic or hypothetical bias may arise. However, the design of the experiment, e.g. the fact that several attributes must be assessed simultaneously, makes it difficult for the employers to answer the questions strategically. In addition, even if the employers try to hide their discriminatory behavior, this should bias the results downwards since the ethically commendable behavior is to not discriminate. Another limitation is the risk of a low response rate in a survey.

Our results show that employers discriminate against applicants who are old, non-European, Muslim, Jewish, obese, have several children, or have a history of sickness absence. The magnitude of the discrimination is substantial, and corresponds to a wage reduction of up to 50 percent. Moreover, more firm co-payment in the sickness benefit system may reduce hiring, but does not affect the degree of discrimination. Finally, we find only small differences in behavior between different types of recruiters and firms.

The rest of the paper is organized as follows. Section 2 gives a background to the empirical analysis. The experimental design is described in Section 3, and Section 4 presents the data. In Section 5, the empirical framework and estimation strategy is discussed. Section 6 contains the results, and Section 7 some concluding remarks.

2 Background

As a background to the empirical analysis, it is instructive to briefly consider the situation for some key groups in the Swedish labor market and to discuss Swedish institutions of relevance for the experiment.

⁹ In correspondence studies, it is difficult to interpret cases when neither of the fictitious applicants is invited to an interview. Since there are other (real) job applicants, an employer may choose one of them and this may affect the magnitude of the estimated degree of discrimination.

2.1 Discrimination in the Swedish labor market

In Sweden, there are substantial differences in labor market outcomes between e.g. men and women and native Swedes and immigrants.

The employment rate for women is almost six percentage points lower than for men, and the gender wage gap is around 14 and 20 percent for blue- and white-collar workers, respectively.¹⁰ Differences in observed human capital variables explain some of these differences, but especially for white-collar workers a substantial wage gap remains unexplained (le Grand, 1997, and Albrecht *et al.*, 2003). However, there is substantial occupational segregation based on gender, and if detailed controls for occupations are included much of the remaining wage gap is eliminated (Petersen *et al.*, 2007). There are some recent studies of gender discrimination in hiring. Carlsson (2011), using data from a correspondence study, and Eriksson & Lagerström (2011), using data from an online CV database, find that women in general are not discriminated in hiring.¹¹

The employment rate for immigrants is almost 18 percentage points lower than for native Swedes, and the median disposable income for immigrants is around 75 percent of the corresponding income for native Swedes.¹² Edin & Åslund (2001) show that the average earnings of non-OECD immigrants are around 45 percent of the earnings of similar (on observed characteristics) native Swedes. Roughly a quarter of this difference is due to differences in hourly wages, another quarter to fewer working hours, and the remaining half to a lower employment rate. There are many studies which present evidence that a substantial part of these differences reflects ethnic discrimination. Carlsson & Rooth (2007), using data from a correspondence study, show that applicants with Swedish names receive 50 percent more callbacks than applicants with Arabic names. Similar evidence is found in Eriksson & Lagerström (2011).¹³

¹⁰ According to Statistics Sweden, the employment rate for men and women aged 20-64 in 2011 was 82.8 and 77.2 percent, respectively. The gender wage gap is calculated by the National Institute of Economic Research.

¹¹ Other studies of gender discrimination using the correspondence testing methodology include Riach & Rich (1987, 2006), Neumark (1996), and Weichselbaumer (2004).

¹² According to Statistics Sweden, the employment rate for native Swedes and immigrants aged 20-64 in 2011 was 83.3 and 65.5 percent, respectively.

¹³ Other studies of ethnic discrimination using the correspondence testing methodology include Bertrand & Mullainathan (2004) and the studies cited in Riach & Rich (2002). Drydakis (2010) studies the effects of religious affiliation.

For other types of discrimination, there are fewer studies. For age, Ahmed *et al.* (2012), using data from a correspondence study, show that the callback rate is much lower for older applicants.¹⁴ For weight, Rooth (2009), using data from a correspondence study, show that obese applicants have a lower callback rate.¹⁵

2.2 Swedish institutions

Three institutions which are relevant for the experiment are discrimination legislation, employment protection and pension schemes, and sickness benefits.

Discrimination legislation

In Sweden, reducing discrimination has for a long time been considered an important policy objective. In 1980, legislation regarding gender discrimination in the workplace was enacted. This was followed by legislation regarding discrimination based on ethnic origin, religion and other beliefs, disability, and sexual orientation. In 2009, a new comprehensive Discrimination Act came into force. This act states that it is illegal to discriminate based on sex, transgender identity or expression, ethnicity, religion or other belief, disability, sexual orientation or age. Employers are required to take active steps to prevent discrimination, e.g. by formulating gender equality plans. The enforcement of the act is entrusted to an ombudsman called the Equality Ombudsman. Violations of the act are punishable by compensation payments and fines.

Employment protection and pension schemes

In Sweden, the rules governing who the employer can layoff/fire, and under which circumstances, are in the Employment Protection Act. Layoffs are possible for personal and redundancy reasons. However, the rules for laying off workers for personal reasons are very strict. The rules for layoffs for redundancies state that layoffs should follow the last in first out rule, but this rule can be replaced by agreements between the employer and the union. Also, for a number of years, workers aged over 45 were protected by

¹⁴ Other studies of age discrimination include Bendick *et al.* (1996, 1999) and Albert *et al.* (2011).

¹⁵ Roehling (1999) reviews laboratory experiments on how employers treat obese workers. Cawley (2004) use observational data to study the effects of obesity. Hammermesh & Biddle (1994) and Mobius & Rosenblat (2006) study the relationship between beauty and labor market outcomes.

special rules that made it more difficult to lay them off. Violations against these rules are punishable by compensation payments and fines.

Another institution, which may affect how employers perceive older workers, is that occupational pension schemes for some workers – e.g. state employees and white-collar workers – are calculated in a way so that the employers' costs increase with the employee's age. However, in recent years most of these schemes have been replaced by defined contribution plans where the employers' costs are the same irrespective of the employee's age.

Sickness benefit system

In Sweden, sickness benefits substitutes income for workers who cannot perform their usual work because of temporary illness. All employed persons are covered by the insurance, and unemployed persons are covered if they are registered as unemployed at the Public Employment Service. The first day of sickness absence is not compensated. During the first seven days, it is up to the worker to determine if he or she is too sick to work/search. Thereafter, a doctor's certificate is required.

The employers pay sickness benefits equivalent to approximately 80 percent of the wage during day 2-14 of the period of sickness absence (i.e. 'sick pay').¹⁶ After this period the responsibility for sickness benefits is transferred to the Swedish Social Insurance Agency (SSIA). The SSIA is also responsible for sickness benefits for the unemployed.

Around the time of the experiment, there was a lot of debate about the firms' co-payment in the system. Between 2003 and 2005 employers were responsible for sickness benefits for the first three weeks, but in 2005 this was reduced to two weeks. In 2005, the firms' co-payment was extended by the Social Democratic government. In addition to paying the full sickness benefits for the first two weeks, the employers were also required to pay 15 percent of the sickness benefits for the rest of their employees'

¹⁶ Only income up to a cap of an annual income of SEK 321,000 is used to calculate benefits (7.5 times the 'price base amount').

absence.¹⁷ The objective of the reform was to increase the employers' incentives to rehabilitate long-term sick workers and, hence, to speed up the process of getting these workers back to the workplace. To make the reform cost neutral for the employers, they were compensated by a lower employment tax, but the reform made it more costly to hire workers with a high risk of sickness absence.¹⁸ The reform was heavily criticized by the employers, and was abolished by the new center-conservative government after they won the election in 2006.

Combined these institutions are likely to make employers careful not to hire workers whose productivity, or total labor costs, they are uncertain of, such as workers who are immigrants, belong to ethnic or religious minorities, have many children, are obese or have history of sickness absence. Moreover, some of these institutions may make them reluctant to hire older workers.

3 The experiment

To study discrimination, we conduct a stated choice experiment. This means that the employers are first asked to describe an employee who recently and voluntarily left the firm, and then to choose between two hypothetical applicants to invite to a job interview or to hire as a replacement for their previous employee.¹⁹ The applicants' characteristics are varied in a way that allows us to analyze different kinds of discrimination.

For the experiment to work as intended it is important that the participating recruiters perceive it as realistic; i.e. that it has high face validity (Carson *et al.*, 1994). To achieve this, we started by interviewing a number of personnel managers about their hiring strategies. Then, we designed a pilot survey which we tested in focus groups with personnel managers, and finally we designed a pre-test questionnaire which we sent to 40 employers. Based on the results of these preliminary tests, we designed the final

¹⁷ If the employees returned to part-time work or were enrolled in a rehabilitation program, the employers did not have to pay the extra 15 percent. Also, employees who were judged to be at risk for repeated or long periods of sickness absence were exempted.

¹⁸ This effect is similar to the effect of experience rating. Andersson & Meyer (2000) discuss the effects of experience rating.

¹⁹ The employers were also allowed not to choose between the applicants, i.e. to opt-out. The reason for including an out-out option is both that it is ethically questionable not to include such an option in a survey, and that the stated choice literature stresses the importance of including such an option (c.f. Section 4.3).

questionnaire which we sent to recruiters at 1,000 firms. The experiment was conducted in 2007. In the following sections, we describe the details of the experiment.

3.1 The initial interviews and the pilot survey

To get a better understanding of the recruitment process, we interviewed nine personnel managers. They all had long experience of recruitment and were very familiar with the recruitment process used in their firms. In particular, we discussed which factors they used to screen applications in order to choose whom to invite to job interviews and how they assessed applicants in the interviews. Also, we discussed how familiar they were with the sickness benefit system, including their knowledge of the firms' co-payment. Based on these discussions, it seemed possible to study the effects of the following attributes: Gender, age, education, experience, ethnicity, religious beliefs, number of children, weight, history of sickness absence, wage, and type of firm co-payment in the sickness benefit system.

Then, we designed a pilot survey and tested it in focus groups. In the pilot questionnaire, the respondents were asked to choose between two hypothetical applicants based on the attributes mentioned above. We tested the questionnaire in three focus groups consisting of around nine participants in each group. To ensure that the participants had recent experience of recruitment, we chose to include personnel managers who were responsible for screening and interviewing prospective employees. The participants were first asked to fill out the questionnaire, and then to discuss its design. A researcher always chaired the sessions and another took notes. The researchers could discuss the questions, the response options and how realistic the questions were, but never commented on the participants' responses.

The main conclusions from the interviews in the focus groups were: (i) The recruiters did remember the last employee who left the workplace voluntarily and which qualifications this worker had, (ii) the recruiters indicated that they used signals of productivity to sort workers at the different stages of the recruitment process; typically they distinguished between two stages – whom to invite to a job interview and whom to offer a job – and used different signals at each stage, (iii) the recruiters indicated that the choice between the two hypothetical applicants became difficult if more than four

attributes had to be assessed simultaneously, and (iv) the recruiters indicated that they understood the experiment (e.g. the combinations of attributes and the alternatives).

3.2 The design of the experiment

We decided to study applicant characteristics which should matter in any of the stages of the hiring process; i.e. the invitation to a job interview and the hiring decision. The recruiters' choice always involved a replacement for an employee who had recently and voluntarily left the firm. In the first stage (interview), we decided to include four attributes plus the wage and the type of firm co-payment in the sickness benefit system. The attributes are gender, age, education and experience, which are all typical attributes included in a CV. In the second stage (hiring), we decided to include seven attributes plus the wage and type of firm co-payment in the sickness benefit system. The attributes are gender, ethnicity (country of birth), religious beliefs, number of children, weight, and two measures of the applicant's history of sickness absence (number of sickness spells in the previous year and their length). These are all factors which may be observed or discussed in a job interview, but are usually not included explicitly in a CV. Except for gender, each of the twelve attributes has three possible alternatives. These are discussed in Section 3.3 and are listed in Table A1 in Appendix A.

A problem is that it is not possible to non-parametrically estimate the joint effect of all these attributes. The pilot survey indicated that the respondents could handle four attributes in each game (a game is a choice between two hypothetical applicants), and even if the number of attributes is restricted to four, a full factorial design at three levels would result in a very large survey which is not feasible to implement in practice.²⁰ However, the number of games can be reduced if we focus on estimating a restricted number of effects. Therefore, we decided to focus on estimating the main effects of the attributes and a few interactions. To simplify, we assume that all interactions between the wage and the other attributes are zero. We believe this to be a reasonable assumption since wage setting in Sweden is relatively inflexible: Factors such as union contracts and internal wage hierarchies limit the possibility to set wages individually. Also, we

²⁰ To non-parametrically estimate all effects (including the three- and four-way interaction effects), we would need to include at least 81 (3^4) games for each recruiter.

assume that all three- and four-way interactions are zero (i.e. interactions between more than two attributes). Moreover, we remove all dominated combinations; i.e. combinations (hypothetical employees) that are inferior on all attributes. To determine which alternatives to remove, we used what we learned from the discussions with the recruiters. This means that we systematically considered all possible pairs of hypothetical applicants to identify dominated alternatives. As an example, an obese applicant with the highest wage and prevalence of sickness absence can be assumed to be inferior to all other applicants. For other attributes, the ordering is less clear and then we refrained from removing these alternatives.

In the questionnaire, we decided to include twelve games for each respondent: Four games with respect to the invitation to a job interview and eight games with respect to the job offer. In each game, the recruiter is asked to choose between two hypothetical applicants with different attributes. Each applicant has four attributes, and, in most cases, each attribute can take on three levels.

To estimate the marginal value of each attribute (i.e. the wage reduction needed to make employers indifferent between applicants with and without a particular attribute), we always included the wage as one of the attributes in all games. Moreover, since we are interested in varying the firms' cost of uncertainty in hiring (i.e. the extent of firm co-payment in the sickness benefit system), the type of firm co-payment was included in all games. The other two variables in each game were chosen as follows: Gender and age (games 1 and 2), education and experience (games 3 and 4), ethnicity and religious beliefs (games 5 and 6), gender and number of children (games 7 and 8), gender and weight (games 9 and 10), and frequency and intensity of sickness absence (games 11 and 12). We strived to include the combinations of attributes which are most likely to have important interaction effects. This design allows us to estimate the main effect of each attribute as well as the interaction effects between the attributes within each game; i.e. the interaction effects between all applicant characteristics and the type of sickness benefit system, between gender and age, education and experience, etc. To estimate

these effects, we need to include 156 hypothetical applicants.²¹ This can be achieved by having 13 versions of the questionnaire with 12 games in each questionnaire. The application sent to each employer was randomly drawn from these 13 versions.

In addition to the games, the questionnaire also included detailed questions about the last employee who left the firm voluntarily, the recruiter, and the firm for which the recruiter worked (see Section 4.2 for descriptive statistics). An example of the games in the questionnaire is presented in Appendix B.

3.3 The attributes and their levels

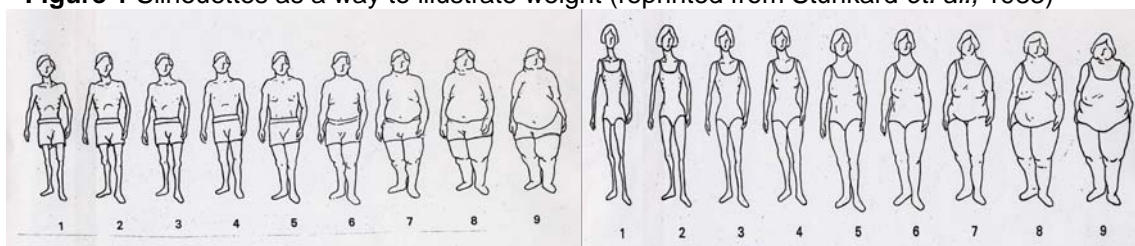
When we chose the attributes and their levels, our objective was to include information which are typically mentioned in a CV, or are typically observed or discussed in a job interview. Moreover, we wanted to choose levels of the attributes which appeared realistic to the employers. Most of the attributes/alternatives are very straightforward, and are summarized in Table A1 in Appendix A.

For gender, we used male and female. For age, we used 29 years or younger, 30-55 years and 56 years or older. For education, the pilot survey indicated that it was best to relate this attribute to what was required for the job in question. Therefore, we used the lowest possible education (lowest quartile), the average education (middle quartiles), and the highest education for the job in question (highest quartile). For experience, we used 4 years or less, 5-7 years and 8 years or more. For ethnicity, we used born in the Nordic countries, born in Europe except the Nordic countries, and born in Africa, the Middle Eastern countries or South America. We chose this division since there is ample empirical evidence indicating that ethnic discrimination mainly affects immigrants from developing countries outside Europe. For religious beliefs, we used Christian, Jewish and Muslim. The majority population in Sweden is Christian and the two other groups are important minorities. For the number of children, we used no children, one child and two children or more. For weight, we used the silhouettes originally developed by Stunkard *et al.* (1983).²² The silhouettes for men and women are illustrated in Figure 1.

²¹ The calculation of the number of hypothetical applicants required to estimate the relevant effects is based on standard statistical tables for fractional factorial experiments (c.f. Hahn and Shapiro, 1966).

²² These silhouettes have previously been used in several studies (e.g. Berkey *et al.*, 1999, Munoz *et al.*, 1996), and have also been linked to body mass index (BMI) in Bulik *et al.* (2001).

Figure 1 Silhouettes as a way to illustrate weight (reprinted from Stunkard *et. al.*, 1983)



Silhouettes 1/2 represent underweight, silhouettes 3/4 normal weight, silhouettes 5/6 overweight and silhouettes 7/8/9 obese. We used normal weight, overweight and obese. For the two measures of sickness absence, we used 1-2, 3-5, and 6 or more times in the last year for frequency, and 7 or less, 8-14, and 15 or more days each time for intensity. For the wage, we decided to relate it to the wage of the previous employee. We used the alternatives the same wage, a ten percent lower wage, and a ten percent higher wage than the previous employee.²³ For the extent of firm co-payment in the sickness benefit system, we used two weeks of full firm payment, three weeks of full firm payment, and two weeks of full firm payment plus 15 percent co-payment for the rest of the sickness absence. The firms' cost of uncertainty should be lowest in the first and highest in the last alternative. All these alternatives have been discussed in the policy debate, and were well-known and understood by the recruiters taking part in the pilot survey.

4 Data

In this section, we describe the sample selection and present some descriptive statistics for the experiment.

4.1 Sample selection

We decided to focus on medium- and large-sized workplaces in Stockholm County, which is the largest Swedish county in terms of inhabitants. In this county, there are 2,048 workplaces with one location and 20 or more employees. From this population, we drew a sample of 1,000 workplaces to which we sent the questionnaire. Since we

²³ In the pre-test, we used a five percent lower/higher wage, but the results convinced us that we should increase the variation to potentially increase the efficiency in the estimation of the marginal values.

want to study potential differences in discrimination between different types of workplaces, we made a stratified sampling where the strata were based on the size of the workplace, the gender composition of the workplace, and the sector to which the workplace belonged.²⁴ The survey was administered by Statistics Sweden and was sent to the employers by postal mail. An accompanying letter stated that the purpose of the study was to investigate the recruitment behavior of firms. The participants received no compensation for their participation and were asked to return the questionnaire by postal mail. Two reminders were sent to non-respondents.

The response rate was around 46 percent. It was somewhat higher in the public sector than in the private sector.²⁵ A separate analysis of the non-respondents shows that the main reason why they did not participate seemed to be a lack of time rather than a reluctance to participate in a study of recruitment behavior.²⁶ In total, 426 employers (recruiters) are included in the analysis (this corresponds to 4,895 observations).²⁷

4.2 Descriptive statistics

In this section, we describe the workplaces that participated in the experiment and the recruiters who responded to the questions. Then, we describe the characteristics of the last employee who left the firms voluntarily; i.e. the reference person in the experiment. Finally, we discuss to what extent employers try to gather information about their applicants' health and history of sickness absence. More detailed descriptive statistics are presented in Appendix A.

²⁴ For sector, we used the strata: (i) the private sector, and (ii) the public sector. For size, we used the strata: (i) 20-49 employees, (ii) 50-99 employees, (iii) 100-199 employees, (iv) 200-499 employees, and (v) 500 or more employees. For gender distribution, we used the strata: (i) less than 40 percent women, (ii) between 40 and 60 percent women, and (iii) more than 60 percent women.

²⁵ The response rate in the private sector was 44 percent and in the public sector 50 percent. For the different size, categories the response rates were 43, 48, 56, 47 and 60 percent. For the gender distribution categories, the response rates were 46, 44 and 47 percent.

²⁶ In the drop-out analysis, we contacted 33 employers who did not return their questionnaire. In this group, 21 stated that they were unwilling to answer the questionnaire. Eleven of them stated a reason: eight said that the reason was a lack of time and one said that the reason was the purpose of the study.

²⁷ Respondents who reported that no employee had left the firm are excluded from the analysis.

The workplaces and the recruiters

Table A2 presents descriptive statistics for the workplaces.²⁸ The workplaces are quite diverse: Nearly two thirds are in the private sector, one third in the public sector, and almost half have less than 50 employees.

Table A3 presents descriptive statistics for the recruiters who responded to our questions. Around one third of the respondents were managing directors, one third personnel managers, and the rest held other positions tasked with personnel issues. Most of them worked with recruiting, personnel policy and rehabilitation, and almost all of them had worked with these issues for a number of years. Around three quarters were aged 30-55, around one quarter aged over 55, most were born in Sweden, almost two thirds were women, and most had several children. In general, they were highly educated with nearly 80 percent having a university education. They considered themselves to be Christians in three quarters of the cases and atheist/agnostic in most of the remaining cases. A majority considered themselves to be overweight or obese, and most of them had only limited sickness absence.

The previous employee

Nearly all respondents stated that at least one employee had left their workplace voluntarily in the two years preceding the experiment, and the majority stated that this employee had left within the last six months. Therefore, we find it reasonable to assume that the respondents remembered the most recent employee who left the workplace voluntarily.

In the questionnaire, the respondents were asked to describe this employee. Table A4 presents some descriptive statistics. The employees were in the majority of the cases men (51%), aged 30-55 (69%), born in Sweden (84%), and had secondary (39%) or university (53%) education. Their qualifications for the positions they held were in the middle two or highest quartiles in 47 and 41 percent of the cases, respectively. Mostly, they had eight years or more of experience (52%), but had only spent part of this time in their current position. The clear majority was believed to be Christian, but in around a

²⁸ Around 90 percent of the workplaces had a plan against gender, sexual, ethnic or religious discrimination, and 60 percent had a plan against age discrimination.

quarter of the cases their religious beliefs were unknown to the employer. Most of them had only been absent from work due to sickness on a few short occasions, and around 40 percent were judged to be overweight or obese. Their mean wage was SEK 26,800 (€3,000), and their median wage was SEK 25,000 (€2,800).

Health and history of sickness absence

An important issue is if it is reasonable to assume that recruiting employers try to gather information about their job applicants' health and history of sickness absence in the recruitment process. Table A5 presents some descriptive statistics. We find that around half of the recruiters claim that they try to gather information about their applicants' health (44%) or history of sickness absence (41%). They try to get information about health by asking the applicants or their references, asking about leisure activities, requiring health examinations, asking about smoking habits, evaluating physical appearance, and asking about previous occupational injuries. They try to get information about their applicants' history of sickness absence by asking the applicants or their references, and requesting the applicants to provide an excerpt from the SSIA. Based on this evidence, we find it reasonable to assume that the recruiters often have some information about their applicants' health and history of sickness absence.

5 Theoretical framework and estimation

This section sketches a simple theoretical model of the employers' recruitment decision, discusses the empirical modeling and the validity of the estimation procedure.

5.1 Theoretical framework

It is reasonable to assume that most recruiting employers have access to only limited information about their job applicants' productivity – e.g. skills and turnover propensity – prior to hiring. In such cases, employers may find it optimal to base their hiring decisions on easily observed characteristics – e.g. gender or ethnicity – which they believe are correlated with productivity. Then, statistical discrimination may arise. However, employers, co-workers or customers may also have preferences regarding

different groups which affect the hiring decisions. Then, preference-based discrimination may arise.

To capture the possibility of both types of discrimination, we can state the employer's maximization problem in the following way: The employer faces different applicants with observed characteristics given by the vector \mathbf{x} . The employer's utility depends on his or her expectations regarding the profits associated with the observed characteristics \mathbf{x} and his or her preferences regarding these characteristics. Therefore, the employer maximizes:

$$U(\mathbf{z}) = pf(\mathbf{x}) - [w(\mathbf{x}) + d(\mathbf{x})], \quad (1)$$

where $U(\mathbf{z})$ is the employer's utility function, $\mathbf{z} = (\mathbf{x}, w(\mathbf{x}))$, p is the price of the product the firm produces, $f(\mathbf{x})$ is the production function, $w(\mathbf{x})$ is the wage function, and $d(\mathbf{x})$ is the (dis)taste function, with $d(\mathbf{x}) \geq 0$. If an employer has negative preferences regarding some component of the vector \mathbf{x} , then the employer will only hire the applicant if his or her marginal productivity is higher than the wage.

5.2 Empirical modeling

Let us now consider an employer who participates in the experiment. A previous employee with observed characteristics (and known productivity) $\mathbf{z}^0 = (\mathbf{x}^0, w^0)$ is supposed to be replaced by one of two hypothetical applicants with observed characteristics $\mathbf{z}_1 = (\mathbf{x}_1, w_1, I_1)$ and $\mathbf{z}_2 = (\mathbf{x}_2, w_2, I_2)$. \mathbf{x}_1 and \mathbf{x}_2 are a subset of \mathbf{x}^0 , which varies between the games, and I_i , $i = 1, 2$, are the type of firm co-payment in the sickness benefit system.

If we expand the utility associated with hiring applicant i in game g with observed characteristics \mathbf{z}_{ig} around the previous employee's set of observed characteristics, we get:

$$U_e(\mathbf{z}_{ig}) = \alpha_{eg} + \mathbf{x}_{ig}\alpha + I_{ig}\gamma + (I_{ig}\mathbf{x}_{ig})\boldsymbol{\delta} - w_{ig}\beta + \eta_{ige}, \quad (2)$$

where e denotes the employer and g the game. In this equation, we allow for a (linear) fixed employer effect α_{eg} to take into account that the production, wage and taste functions are likely to differ between workplaces. Since the attributes \mathbf{z}_{ig} differ between

each game, it is a fixed effect for each employer in each game. Note that the equation include all relevant interaction effects, and that the wage, by design, is made orthogonal to the attributes \mathbf{x}_{ig} and I_{ig} .

In the choice between the two hypothetical applicants i and j , applicant i is invited to an interview or hired if:

$$U_e(\mathbf{z}_{ig}) - U_e(\mathbf{z}_{jg}) > 0 \text{ and } \partial U_e(\mathbf{z}_{ig}) / \partial \mathbf{z}_{ig} > 0. \quad (3)$$

We estimate $\theta = (\boldsymbol{\alpha}', \gamma, \boldsymbol{\delta}', \beta)'$, where the vector $\boldsymbol{\alpha}$ measures the average degree of discrimination (both statistical and preference-based) with respect to the applicant characteristics.

From this specification, the marginal value of attribute x_k can be calculated as the ratio of parameters, hence:

$$\frac{\partial w}{\partial x_k} = \frac{\partial U_e(\mathbf{z}_{ig}) / \partial x_{igk}}{\partial U_e(\mathbf{z}_{ig}) / \partial w_{ig}} = \frac{\alpha_k + I_{ig} \delta_k}{-\beta}. \quad (4)$$

Note that if $\delta_k = 0$ we get the marginal value of attribute x_k when there is no interaction effect between attribute x_{igk} and I_{ig} . Henceforth, we refer to the case where all interaction effects are zero as the baseline model.²⁹

We estimate the model parameters using the ordinary least square estimator. Thus, we are estimating (for applicants 1 and 2):

$$y_{1eg} - y_{2eg} = (\mathbf{x}_{1g} - \mathbf{x}_{2g})\boldsymbol{\alpha} + (I_{1g} - I_{2g})\gamma + (I_{1g}\mathbf{x}_{1g} - I_{2g}\mathbf{x}_{2g})\boldsymbol{\delta} - (w_{1g} - w_{2g})\beta + \eta_{1eg} - \eta_{2eg}, \quad (5)$$

where y is the outcome variable, e denotes the employer and g the game.

In each game, the employer (recruiter) is asked to choose one of the two applicants or to opt-out.³⁰ If the respondent chooses an applicant, the dependent variable is either 1 or -1 if the first or the second applicant is chosen. We allow for heteroskedasticity by estimating the standard errors using a robust covariance matrix. We prefer to use the linear model rather than the logit model since the design is orthogonal for the linear

²⁹ I.e. the interaction effects between attribute k and the type of firm co-payment as well as all other applicant attributes are set to zero.

³⁰ In each of the games, around 30 percent of the respondents opted out. The results are not affected if we include these cases in the estimation.

model only. We have, however, also estimated fixed effect logit models and, qualitatively, the results are the same.

5.3 Validity

A potential concern with stated choice experiments is that the elicited preferences, or marginal values, may differ from what would be the case in real-world situations. This problem is known as strategic or hypothetical bias in the literature. The related method of contingent valuation has been severely criticized based on these arguments (see e.g. Carson *et al.*, 1996). However, a number of methods for reducing this bias have been suggested in the literature (see e.g. Murphy *et al.*, 2004 for a meta-analysis, and List, 2001), and several recent studies show that stated and revealed preferences often coincide (e.g. Jacquemet *et al.*, 2011 and Murphy *et al.*, 2010). The results in these studies suggest that the importance of hypothetical bias depends on the experimental setting (c.f. Taylor *et al.*, 2001, and Ajzen *et al.*, 2004).³¹ Overall, the studies indicate that the hypothetical valuation of a good is likely to exceed the actual valuation of the same good in situations which involve an important perceived ethical dimension, and where a high value is considered ethically commendable, but not in other situations.

The stated choice experiment approach differs from the ‘all or nothing’ dichotomous contingent valuation approach by adding realism to the experiment as the respondents are asked to choose between alternatives with different attributes in situations closely resembling individual purchasing – or as in our case hiring – decisions. In such experiments, several recent studies show that it is not possible to reject the hypothesis of equal valuation of the attributes in stated choice experiments and the real world (see e.g. Carlsson and Martinson, 2001, Cameron *et al.*, 2002, Backhaus *et al.*, 2005, Lusk & Schroder, 2004, and List *et al.*, 2006).

The results in these studies suggest that there are ways to minimize the problem with hypothetical bias in stated choice experiments. First, the respondents should be given

³¹ Ajzen *et al.* (2004) argue that the activation of positive attitudes may result in a discrepancy between intentions and actions in cases where the experiment involves the selling of desirable public goods (c.f. Lusk and Norwood 2009). Guzman and Kolstad (2007) argue that the problem of hypothetical bias may be related to the respondents’ reluctance to invest in costly information acquisition in a hypothetical experiment. These arguments are based on the idea that people want to improve or maintain their self-image, but that it is more costly to do so when real money is involved.

sufficient information about the good that is to be valued. Second, the respondents should be provided with a known reference which they can compare the alternatives against. Third, the respondents should be allowed to make any choice between the suggested alternatives or to opt-out (c.f. Hensher, 2010).

In this study, we incorporate all these features. All the respondents handle personnel issues on a day-to-day basis, they are asked to consider well-defined replacements for a well-known previous employee, and they are allowed to choose or not to choose (i.e. to opt-out) between the suggested applicants in each game. In addition, since the ethically commendable behavior is to not discriminate, we expect any remaining hypothetical bias to affect the results downwards (i.e. reduce the estimates of discrimination).

6 Results

In this section, we study the prevalence of discrimination. First, we estimate the effects of each of the attributes included in the experiment; i.e. we estimate the baseline model with no interaction effects. We measure the degree of discrimination in terms of the probability of being invited to a job interview (the callback rate) and being offered a job (the job offer rate) as well as in terms of the marginal value of each attribute (the wage reduction needed to make the employers indifferent between applicants with and without a particular attribute). Second, we investigate if the degree of discrimination is affected by the firms' cost of uncertainty in hiring (the extent of firm co-payment in the sickness benefit system), i.e. we estimate the interaction effects. Third, we study if the degree of discrimination varies with the type of recruiter and firm; i.e. we estimate the model separately for different subgroups.

6.1 The degree of discrimination

Invitations to job interviews

Table 1 presents the estimates of the probability of being invited to a job interview (the callback rate).

The idea that people derive value from the mere expression of attitudes and values that correspond to what they consider to be 'good and worthy' actions is however not new (c.f. Katz, 1960, and Andreoni, 1989, 1990).

Table 1 Callback rate to a job interview

<i>Gender (ref: male):</i>	
Female	-0.01 (0.04)
<i>Age (ref: 29 years or younger):</i>	
30-55 years	0.12** (0.05)
>55 years	-0.64*** (0.05)
<i>Education (ref: lowest quartile):</i>	
Middle quartiles	0.63*** (0.06)
Highest quartile	0.82*** (0.05)
<i>Experience (ref: <5 years):</i>	
5-7 years	0.13** (0.06)
>7 years	0.12** (0.06)
<i>Wage:</i>	
Wage	-0.01*** (0.00)
<i>Sickness benefits (ref: 3 weeks full firm payment):</i>	
2 weeks full firm payment	0.09** (0.04)
2 weeks full firm payment plus 15% co-payment	-0.07** (0.04)

Notes: Estimated with ordinary least squares. The dependent variable is the probability of being invited to a job interview (the callback rate). Also included are a constant and dummy variables for the industry the workplace belongs to. Robust standard errors are in parentheses. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

All estimates of the effects of the applicants' attributes on the callback rate are statistically significant, except for gender. The most striking result is the very large negative effect for applicants over 55 years old: The callback rate for such an applicant is 64 percentage points lower than the callback rate for an applicant who is less than 30 years old. The callback rate for a 30-55 year old applicant is 12 percentage points higher than for an applicant who is less than 30 years old. In contrast, there is no gender difference in the callback rate. Education and experience have the expected effects, i.e. a higher callback rate for applicants with more education or experience. In particular, education has a strong effect: An applicant with the highest education relevant for the job in question has an 82 percentage point higher callback rate than an applicant with the lowest education. The wage has a negative effect on the callback rate. Finally, the type of firm co-payment in the sickness benefit system has a clear effect on the callback

rate: Reducing the time the firms pay sickness benefits from three to two weeks would increase the callback rate with 9 percentage points whereas combining the same reduction with a 15 percent employer co-payment for the complete sickness spell would decrease the callback rate with 7 percentage points. These results are as expected since more firm co-payment in the sickness benefit system (for a given wage) implies higher costs for the firms and, therefore, less hiring.

In general, these results confirm the results in previous correspondence studies. As mentioned above, Ahmed *et al.* (2012) find strong evidence of age discrimination, and Carlsson (2011) find no evidence of gender discrimination. Also, the results suggest that more firm co-payment in the sickness benefit system may affect hiring negatively.

Job offers

We now turn to the firms' hiring decision. In Table 2, we present the estimated effects of the applicants' attributes on the probability of offering a job (first column) and the marginal value of each attribute (second column).

For the job offer rate, we again find that most of the effects are statistically significant. A first striking result is the strong effects of ethnicity and religious beliefs. The job offer rate is similar for applicants born in Europe, while applicants born in Africa, the Middle Eastern countries and South America face a much lower job offer rate (minus 28 percentage points). Applicants who are Muslim or Jewish also have a much lower job offer rate than applicants who are Christian (minus 30 and 26 percentage points). A second striking result is the very large negative effect for obese applicants: Being obese decreases the job offer rate by 83 percentage points compared to having normal weight. Moreover, applicants with two or more children have a 25 percentage points lower job offer rate. There is also, as expected, a lower job offer rate for workers with a history of sickness absence, especially for those with many spells and long durations. Also, the wage has a negative effect. Finally, the type of firm co-payment in the sickness benefit system has the expected effect; i.e. if the employers' costs increase the job offer rate decreases.

In the second column, we have the corresponding estimates in terms of the marginal value of each attribute. These estimates can be interpreted as the wage reduction needed

Table 2 Job offer rate and the marginal value of each attribute

	Job offer rate	Marginal value
<i>Gender (ref: male):</i>		
Female	0.00 (0.03)	0.10 (1.62)
<i>Ethnicity (ref: Nordic):</i>		
Other European	-0.02 (0.06)	-0.94 (3.34)
Africa, Middle East, South America	-0.28*** (0.06)	-16.24*** (3,67)
<i>Religious belief (ref: Christian):</i>		
Jewish	-0.26*** (0.06)	-15.02*** (3.61)
Muslim	-0.30*** (0.06)	-17.19*** (3.78)
<i>Children (ref: no children):</i>		
1 child	-0.09 (0.06)	-5.06 (3.32)
2 or more children	-0.25*** (0.06)	-14.58*** (3.40)
<i>Weight (ref: normal weight):</i>		
Overweight	-0.13** (0.06)	-7.68** (3.29)
Obese	-0.83*** (0.05)	-48.08*** (4.91)
<i>Wage:</i>		
Wage	-0.02*** (0.00)	-
<i>Intensity of sickness absence (ref: 1-2 times per year):</i>		
3-5 times per year	-0.41*** (0.05)	-23.85*** (3.33)
6 or more times per year	-0.83*** (0.05)	-48.03*** (4.57)
<i>Frequency of sickness absence (ref: 7 or less days):</i>		
8-14 days each time	-0.29*** (0.04)	-16.57*** (2.94)
15 or more days each time	-0.55*** (0.05)	-31.69*** (3.63)
<i>Sickness benefit (ref: 3 weeks full firm payment):</i>		
2 weeks full firm payment	0.13*** (0.03)	7.24*** (1.69)
2 weeks of full firm payment plus 15% copayment	-0.09*** (0.03)	-4.97*** (1.53)

Notes: The estimates in the first column are estimated with ordinary least squares, and the dependent variable is the probability of offering a job. Also included are a constant and dummy variables for the industry the workplace belongs to. In the second column are the implied marginal values of the attributes in terms of the wage. They are calculated as the ratio of the coefficient for the attribute and the coefficient for the wage. Robust standard errors are in parentheses (in the second column calculated using the delta method). ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

to make employers indifferent between applicants with and without a particular attribute (all else equal). The results suggest that to eliminate discrimination, an applicant who is

born in Africa, the Middle Eastern countries or South America would need to get a wage which is around 16 percent lower than an applicant who is born in the Nordic countries, an applicant who is Muslim (Jewish) would need to get a wage which is 17 (15) percent lower than an applicant who is Christian, an overweight (obese) applicant would need to get a wage which is 8 (48) percent lower than a normal weight applicant, and an applicant with a history of sickness absence would need to get up to a 48 percent lower wage. Since the mean of the previous employees' monthly wage is SEK 26,800 (€3,000), a 15 percent reduction would correspond to a SEK 4,000 (€440) lower wage, and a 48 percent reduction to a SEK 12,900 (€1,400) lower wage. The exact numbers should be interpreted with caution, but they indicate that the wage reductions needed to make employers indifferent between applicants with and without some of the attributes are substantial.

In general, the results confirm the results from correspondence studies of the early stages of the hiring process, but also extend these results by considering attributes which have not been studied before. It is widely documented that non-European immigrants and Muslims face widespread discrimination in the Swedish labor market (c.f. Carlsson & Rooth, 2007). The negative effect for Jewish applicants is somewhat more surprising since this group has been in Sweden for a long time and is considered as well-established. However, there has been some debate recently about discrimination against Jews.³² Also, it may be that in many real-world hiring situations employers do not know that an applicant is a Jew. It is also striking that applicants who are obese, have a history of sickness absence and/or have several children seem to face significant discrimination. Moreover, the estimates of the size of the wage reductions needed to make employers indifferent between applicants with and without these attributes are substantial. The discrimination we find evidence of may reflect both preference-based and statistical discrimination. For example, discrimination against ethnic and religious minorities may reflect both types of discrimination, while discrimination against workers with several children or a history of sickness absence is likely to reflect

³² See e.g. the Telegraph 21 Feb 2010, <http://www.telegraph.co.uk/news/worldnews/europe/sweden/7278532/Jews-leave-Swedish-city-after-sharp-rise-in-anti-Semitic-hate-crimes.html>. Discrimination against Jews in Sweden is also discussed in e.g. U.S. Department of State (2005) and Bachner & Ring (2005).

statistical discrimination. Moreover, the results again suggest that more firm co-payment in the sickness benefit system may affect hiring negatively. Finally, it should be noted that these estimates probably should be seen as a lower bound on discrimination. As was discussed above, the design of the experiment makes it difficult for employers to hide their discriminatory behavior. However, as discrimination is both morally questionable and in most cases illegal, employers may try to hide their discriminatory behavior in a hypothetical experiment like this, and this may bias the estimates downwards.

6.2 Discrimination and the cost of uncertainty in hiring

We now turn to the analysis of what happens with the degree of discrimination when we vary the extent of firm co-payment in the sickness benefit system. In the experiment, we have three sickness benefit schemes; two weeks of full firm payment, three weeks of full firm payment, and two weeks of full firm payment followed by 15 percent firm co-payment. In the previous section, we saw that more co-payment resulted in lower callback and job offer rates. Now we want to investigate if different groups are affected differently when the extent of firm co-payment is changed.

The idea is simply that, if employers are statistically discriminating (i.e. ‘cream skimming’), then the degree of this behavior should be correlated with the extent of firm co-payment. Hence, if employers are cream skimming, we should expect them to be less likely to hire applicants with attributes which signals a high risk of sickness absence when the extent of firm co-payment increases. In addition, more co-payment may also affect how employers perceive other attributes which signals uncertainty about total labor costs. The reason is that more co-payment implies that the employers’ uncertainty about total labor costs increases. If employers are risk adverse, more uncertainty would make them less willing to hire applicants they perceive as risky.

To estimate these effects, we interact the sickness benefit scheme variables (type of firm co-payment) with the attributes signaling a higher risk of sickness absence (e.g.

overweight/obese and a history of sickness absence) or uncertainty in the hiring decision in a more general sense (e.g. ethnic and religious minorities).³³

Tables 3 and 4 present the estimates of the interaction effects for the two stages of the hiring process; the invitation to a job interview (the callback rate) and the job offer (the marginal value in terms of the wage).

Table 3 Change in the callback rate to a job interview from changing the firm co-payment in the sickness benefit system (reference 3 weeks of full firm payment)

	2 weeks	2 weeks plus 15%
<i>Gender (ref: male):</i>		
Female	0.02 (0.06)	-0.05 (0.05)
<i>Age (ref: 29 years or younger):</i>		
30-55 years	-0.14** (0.07)	0.04 (0.07)
>55 years	-0.08 (0.08)	-0.03 (0.07)
<i>Education (ref: lowest quartile):</i>		
Middle quartiles	0.08 (0.11)	0.08 (0.10)
Highest quartile	0.12 (0.09)	0.23*** (0.09)
<i>Experience (ref: <5 years):</i>		
5-7 years	0.02 (0.10)	0.28** (0.12)
>7 years	0.00 (0.11)	0.18* (0.10)

Notes: Estimated with ordinary least squares. The table presents the interaction effects between the attributes and the type of firm co-payment in the sickness benefit system. The regression also includes all applicant attributes in Table 1, all other interaction effects, a constant and dummy variables for the industry the workplace belongs to. Robust standard errors are in parentheses. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

In general, we find little evidence of any change in the degree of discrimination when the extent of firm co-payment is changed. In Table 3, only four interaction effects are statistically significant: There is a negative interaction effect between 30-55 years of age and two weeks of full firm payment, and positive interaction effects between the highest education, 5-7 years of experience, more than 7 years of experience and two weeks of full firm payment followed by 15 percent firm co-payment. The last three effects are

³³ In the regressions, we include all interactions described in Section 3, but only report the interactions between the sickness benefit schemes and the applicant characteristics. Most other interactions are not statistically significant. These estimates are available upon request.

reasonable; i.e. education and experience are considered as more valuable in a system with more co-payment. In Table 4, there are some statistically significant interaction effects, but most of them are difficult to interpret. For example, the estimates imply that applicants who are Muslims fare better both in a system with less co-payment (two weeks) and in a system with more co-payment (two weeks followed by 15 percent).

Table 4 Change in the job offer rate from changing the firm co-payment in the sickness benefit system (reference 3 weeks of full firm payment)

	2 weeks	2 weeks plus 15%
<i>Gender (ref: male):</i>		
Female	-5.52** (2.63)	2.19 (2.28)
<i>Ethnicity (ref: Nordic):</i>		
Other European	-8.31 (5.46)	-5.56 (4.93)
Africa, Middle East, South America	6.52 (5.34)	6.60 (4.89)
<i>Religious belief (ref: Christian):</i>		
Jewish	11.67** (5.43)	-0.27 (4.78)
Muslim	18.99*** (6.53)	14.82*** (5.47)
<i>Children (ref: no children):</i>		
1 child	-3.53 (4.70)	-4.18 (4.77)
2 or more children	4.27 (4.53)	0.85 (4.66)
<i>Weight (ref: normal weight):</i>		
Overweight	-8.38* (5.05)	-1.89 (4.36)
Obese	-8.71* (4.65)	2.75 (4.07)
<i>Intensity of sickness absence (ref: 1-2 times per year):</i>		
3-5 times per year	3.99 (4.28)	2.80 (3.93)
6 or more times per year	-5.42 (3.98)	-7.69** (3.77)
<i>Frequency of sickness absence (ref: 7 or less days):</i>		
8-14 days each time	-0.13 (3.98)	-2.33 (3.77)
15 or more days each time	-2.83 (4.50)	-1.92 (3.92)

Notes: The table presents the implied marginal values in terms of the wage for the interaction terms in the job offer regression; i.e. a regression with all applicant characteristics in Table 2, all interaction effects, a constant and dummy variables for the industry the workplace belongs to (the calculation of the marginal effects is described in the notes to Table 2). The standard errors are calculated using the delta method and the heteroskedastic robust covariance matrix. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

In general, the results show that there is little evidence of any systematic relationship between the degree of discrimination and the extent of firm co-payment in the sickness benefit system. This may be interpreted as evidence against statistical discrimination, but there is an alternative explanation. It may be that the firms' total costs associated with an employee's absence are high in all three sickness benefit schemes. For most firms, the total costs when an employee is absent include not only the costs of their co-payment, but also many other costs associated with the disruption in production the absence may cause. Therefore, it may be that the (realistic) changes in the co-payment scheme we study are simply too small to affect the employers' hiring decisions.

6.3 Discrimination and the type of recruiters and firms

We now turn to the issue if the degree of discrimination is similar in all firms or if it differs depending on the type of the recruiter and/or firm. To investigate this, we estimate the models separately for different subgroups with respect to the characteristics of the recruiters and the workplaces. We focus on the effects on the hiring decision.³⁴ As before, all regressions include controls for the industry the firms belong to.

The characteristics of the recruiters

The characteristics of the recruiters which we consider are gender, age, education, experience, ethnicity, religious beliefs, children, weight, and history of sickness absence. Some of the results are presented in Table 5.

Comparing male and female recruiters, there are only three statistically significant differences.³⁵ First, female recruiters, on average, are more reluctant than male recruiters to offer a job to an applicant who is obese or have a history of sickness absence of more than six spells per year. Second, female recruiters respond more to a change in the extent of firm co-payment in the sickness benefit system from three to two weeks than male recruiters. One way of interpreting these results is that female recruiters, on average, are more risk averse than male recruiters.³⁶

³⁴ The results for the callback rates are available upon request.

³⁵ Hensvik (2011) reports evidence that Swedish male and female employers do not use different hiring strategies.

³⁶ Although we control for the industry the firms belong to, we cannot exclude the possibility that these differences are explained by other systematic differences in the characteristics of the firms male and female recruiters work in.

For age, we have estimated the model separately for recruiters who are less than 30 years, 30-55 years and over 55 years. We find three statistically significant differences, all of which between the oldest and youngest recruiters. The two groups respond differently to a change in the extent of firm co-payment in the sickness benefit system and to applicants who have a history of more than six spells of sickness absence.

For education, we have estimated the model separately for recruiters with university, secondary and primary education. We only find two statistically significant differences; recruiters with secondary and university education differ in their treatment of applicants born in Europe outside the Nordic countries and applicants with one child. An analysis of recruiters with different lengths of experience yields no important differences. For ethnicity, we have estimated the model separately for recruiters born in Sweden and recruiters born in other countries.³⁷ We find that both types of recruiters treat applicants in the same way in most cases. The only statistically significant difference is that recruiters who are immigrants are less likely to discriminate against applicants who are Jewish. There is a similar tendency for applicants who are Muslims, but this difference is not statistically significant. However, there is no difference in the recruiters' treatment of applicants born inside and outside Europe. Some of these results may be interpreted as suggestive of preference-based discrimination. However, the results should be interpreted with caution, especially since most immigrant recruiters are born in other Nordic and European countries.

For religious beliefs, we have estimated the model separately for recruiters who state that they have a religious belief (mostly Christian) and recruiters who do not state that they have a religious belief (i.e. recruiters who answered atheist, agnostic or do not know). We find some statistically significant differences; 'religious recruiters' treat applicants who have two or more children less favorably and women more favorably. There is also a tendency that 'religious recruiters' view applicants who are Muslim (statistically significant at the ten percent level) or Jewish (not statistically significant) less favorably.

³⁷ Åslund *et al.* (2009) report evidence that Swedish employers tend to hire employees with a similar ethnic background as their own.

For the recruiters' family situation, we have estimated the model separately for recruiters who have no children, one child and two or more children. We find only two statistically significant differences; recruiters with two or more children treat applicants with one child more favorably than recruiters with no children. Moreover, recruiters with two or more children are more reluctant to hire applicants who are obese or have a history of many sickness spells in the past.

For weight, we have estimated the model separately for recruiters who report that they are underweight, normal weight, overweight, and obese. We find some evidence that obese recruiters treat applicants who are obese more favorably than other recruiters. Moreover, overweight recruiters seem to value European applicants lower and female applicants higher than normal weight recruiters. We have also run separate regressions based on the recruiters' history of sickness absence, but find no important differences.

In general, the results indicate that the different types of recruiters treat applicants with different attributes quite similarly. This is more supportive of statistical discrimination than preference-based discrimination since, if preference-based discrimination is important, we would expect to find clear differences in recruitment behavior between different types of recruiters. However, some of the results are consistent with preference-based discrimination.

Table 5 The marginal value of the applicants' attributes, subgroups recruiters

	Gender		Age			Education		Ethnicity		Religious beliefs	
	Male	Female	<30 years	30-55 years	>55 years	Secondary	University	Native Swede	Immigrant	Non- religious	Religious
Female	-0.93 (1.91)	1.01 (2.65)	1.07 (4.31)	-1.17 (1.62)	6.43 (7.51)	-5.09 (4.18)	1.16 (1.78)	0.91 (1.71)	-7.11 (4.96)	-5.55* (3.02)	1.96 (1.91)
Other European	-1.40 (3.93)	1.84 (5.47)	-1.93 (10.78)	-0.95 (3.41)	-2.68 (14.16)	-22.00** (9.90)	2.43 (3.66)	0.41 (3.51)	-11.79 (10.64)	-5.69 (5.83)	0.84 (3.99)
Non-European	-11.04*** 4.20	-20.03*** (6.21)	13.33 (15.73)	-11.66*** (3.60)	-59.44** (25.29)	-25.31*** (9.58)	-14.33*** (4.06)	-15.96*** (3.86)	-23.02* (11.99)	-13.11* (6.95)	-17.10*** (4.26)
Jewish	-15.17*** (4.14)	-13.95** (6.03)	1.41 (9.42)	-14.17*** (3.64)	-15.04 (15.63)	-6.91 (8.53)	-17.99*** (4.15)	-17.67*** (3.86)	7.14 (10.74)	-6.30 (6.33)	-17.73*** (4.30)
Muslim	-16.95*** (4.32)	-16.26*** (6.27)	20.37 (19.38)	-15.23*** (3.80)	-27.06 (17.12)	-18.64*** (9.86)	-18.93*** (4.23)	-19.00*** (4.04)	-4.10 (11.08)	-5.44 (6.64)	-19.79*** (4.52)
1 child	-2.38 (3.76)	-9.20 (5.65)	-5.91 (8.03)	-3.15 (3.32)	-20.13 (16.30)	15.50* (9.43)	-8.19 (3.62)	-4.40 (3.50)	-10.45 (9.79)	-7.15 (6.28)	-5.15 (3.83)
2 or more	-10.01** (3.94)	-19.09*** (5.71)	-2.18 (7.61)	-9.91*** (3.36)	-49.35** (21.23)	-10.57 (8.37)	-15.47** (3.78)	-13.50*** (3.55)	-19.84* (10.84)	-3.58 (5.92)	-17.68*** (4.04)
Overweight	-0.54 (3.98)	-12.83** (5.42)	-0.82 (7.10)	-6.18* (3.36)	-23.40 (16.03)	-8.19 (8.02)	-7.04** (3.62)	-7.01** (3.46)	-8.86 (9.75)	-4.28 (6.14)	-8.83** (3.83)
Obese	-33.23*** (4.85)	-61.77*** (9.36)	-31.42*** (10.19)	-40.11*** (4.46)	-109.76*** (40.11)	-65.23*** (15.25)	-44.17*** (5.12)	-47.81*** (5.14)	-42.38*** (13.97)	-42.05*** (8.41)	-48.93*** (5.79)
3-5 times per year	-19.31*** (3.94)	-30.05*** (5.76)	-5.02 (7.83)	-20.92*** (3.20)	-49.89** (21.17)	-30.59*** (9.51)	-22.53*** (3.56)	-22.79*** (3.45)	-31.57*** (11.11)	-15.90*** (5.70)	-25.39*** (3.95)
6 or more times per year	-34.20*** (4.52)	-63.06*** (9.00)	-16.30** (7.98)	-42.15*** (4.29)	-97.44*** (34.15)	-44.94*** (11.69)	-48.78*** (4.99)	-47.81*** (4.78)	-49.07*** (14.62)	-45.56*** (8.03)	-47.73*** (5.35)
8-14 days each time	-14.28*** (3.44)	-19.26*** (4.92)	-11.88 (8.88)	-13.70*** (2.82)	-30.21* (15.95)	-17.68** (7.77)	-16.24*** (3.18)	-15.95*** (3.08)	-17.73** (8.72)	-14.91*** (5.53)	-16.50*** (3.39)
15 or more days each time	-25.85*** (4.09)	-37.66*** (6.39)	-26.16** (10.17)	-28.36*** (3.51)	-59.43** (23.08)	-36.73*** (9.81)	-30.09*** (3.90)	-32.30*** (3.81)	-20.10** (10.14)	-28.54*** (6.29)	-32.08*** (4.31)
2 weeks full firm payment	3.34* (1.92)	12.09*** (3.00)	-4.09 (4.24)	7.13*** (1.71)	12.29 (8.14)	13.12*** (4.82)	6.17*** (1.82)	6.98*** (1.77)	8.58 (5.34)	3.34 (3.02)	8.52*** (2.01)
2 weeks of full firm payment plus 15%	-2.28 (1.83)	-7.50*** (2.53)	-15.71*** (5.10)	-4.30*** (1.53)	-6.94 (6.83)	1.79 (3.88)	-5.68*** (1.70)	-4.75*** (1.60)	-7.16 (4.87)	-6.04** (2.83)	-4.37** (1.77)

Notes: The table reports the implied marginal values of the attributes in terms of the wage. They are calculated as the ratio of the coefficient for the attribute and the coefficient for the wage from regressions of the job offer rate (see the notes to Table 2 for the details). The reference categories are the same as in Table 2. The standard errors are calculated using the delta method and the heteroskedastic robust covariance matrix. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

The characteristics of the workplaces

The characteristics of the workplaces which we consider are the sector the workplace belongs to, the size of the workplace, and the gender composition of the workplace. Some of the results are presented in Table 6.

For the sector the workplaces belong to, we have estimated the model separately for workplaces in the private and public sector, but find essentially no differences, even though there is some tendency that employers in the public sector treat applicants with attributes associated with risk more unfavorably.

For the size of the workplaces, we have estimated the model separately for four different categories: 20-49, 50-99, 100-249, and >249 employees. Overall, we find that large workplaces are less likely to discriminate than small workplaces. For example, we find that workplaces with more than 249 employees do not discriminate against applicants who are born outside Europe, whereas workplaces with 20-49 and 50-99 employees attach a negative value to this characteristic. Large workplaces also consider applicants who have two or more children or who are overweight more favorably than smaller firms.

For the gender composition of the workplaces, we have estimated the model separately for workplaces with less than 40 percent women, 40-60 percent women and more than 60 percent women. However, we find no clear pattern or any statistically significant differences in the response to different attributes between the groups.

In general, we find that different types of workplaces treat applicants with different attributes quite similarly. The only difference we find is that large firms are less likely to discriminate than small firms. This may be interpreted as evidence of statistical discrimination since it is likely that the consequences for a small firm of hiring the 'wrong' worker are more substantial than for a large firm. However, the scope for preference-based discrimination may be bigger in small firms where the recruiter and the employees interact more closely in the day-to-day operations.

Table 6 The marginal value of the applicants' attributes, subgroups firms

	Sector		Size				Percentage women		
	Private	Public	20-49	50-99	100-249	>249	<40%	40-60%	>60%
Female	-1.59 (1.75)	5.47 (4.65)	-2.89 (2.34)	0.37 (5.22)	1.22 (3.39)	4.96 (3.40)	-1.09 (2.49)	2.89 (3.01)	-0.23 (3.27)
Other European	-2.77 (3.59)	3.04 (9.44)	-8.59* (4.64)	6.45 (11.41)	17.27** (8.50)	5.17 (7.60)	-0.91 (5.11)	-2.19 (6.19)	0.82 (6.50)
Africa, Middle East, South America	-18.53*** (3.96)	-6.81 (10.82)	-22.22*** (5.38)	-26.16** (12.22)	-2.93 (7.42)	3.19 (9.13)	-18.32*** (5.75)	-15.31** (6.75)	-14.97** (7.39)
Jewish	-12.39*** (3.71)	-33.00** (12.96)	-18.54*** (5.07)	-10.23 (11.02)	2.95 (8.45)	-13.83* (8.20)	-13.81** (5.46)	-10.49* (6.24)	-24.00*** (8.02)
Muslim	-14.40*** (3.98)	-28.95** (12.35)	-18.06*** (5.20)	-3.16 (12.47)	-7.85 (7.61)	-20.93** (9.91)	-16.92*** (5.96)	-17.35** (7.63)	-20.56*** (7.33)
1 child	-8.94** (3.67)	9.96 (9.44)	2.21 (4.62)	-17.79 (11.56)	-12.69* (6.94)	-3.34 (7.44)	-6.12 (5.04)	-10.18 (6.36)	2.69 (6.68)
2 or more children	-14.59*** (3.68)	-20.47** (9.85)	-16.50*** (4.82)	-22.16** (11.09)	-12.19* (7.24)	-1.65 (7.02)	-18.14*** (5.40)	-14.10** (5.96)	-9.83 (6.67)
Overweight	-9.04** (3.61)	-4.54 (8.85)	-8.12* (4.84)	-14.11 (10.55)	10.77 (6.70)	-18.35** (7.35)	-11.48** (5.17)	-7.94 (5.78)	-3.48 (6.61)
Obese	-42.22*** (4.98)	-78.18*** (18.85)	-49.81*** (7.39)	-86.14*** (22.75)	-31.08*** (7.86)	-34.56*** (8.37)	-51.35*** (7.94)	-38.58*** (7.68)	-54.06*** (10.69)
3-5 times per year	-20.51*** (3.53)	-34.32*** (10.44)	-25.80*** (5.02)	-30.62*** (11.24)	-20.99*** (6.58)	-17.27*** (6.19)	-22.87*** (5.46)	-21.18*** (5.64)	-27.54*** (6.70)
6 or more times per year	-40.83*** (4.49)	-76.74*** (17.78)	-43.70*** (6.31)	-69.25*** (18.04)	-44.41*** (9.58)	-38.35*** (8.05)	-47.78*** (7.24)	-40.16*** (7.36)	-56.02*** (9.94)
8-14 days each time	-16.15*** (3.30)	-20.97*** (8.13)	-17.03*** (4.21)	-38.43*** (12.54)	-13.72** (5.80)	-2.57 (5.52)	-13.72*** (4.62)	-18.99*** (5.42)	-16.69*** (5.63)
15 or more days each time	-31.87*** (4.19)	-38.69*** (10.74)	-32.85*** (5.33)	-52.18*** (14.29)	-17.73** (7.54)	-22.72*** (6.67)	-30.08*** (5.78)	-27.77*** (6.30)	-36.22*** (7.35)
2 weeks full firm payment	5.63*** (1.80)	14.14*** (5.30)	8.64*** (2.46)	9.12* (5.37)	3.65 (3.56)	4.58 (3.54)	7.84*** (2.68)	8.69*** (3.07)	5.69* (3.27)
2 weeks of full firm payment plus 15%	-3.99** (1.64)	-7.13 (4.40)	-4.44** (2.17)	-11.52** (5.38)	-1.76 (3.22)	-6.39** (3.24)	-3.28 (2.41)	-3.35 (2.73)	-7.93*** (3.06)

Notes: The table reports the implied marginal values of the attributes in terms of the wage. They are calculated as the ratio of the coefficient for the attribute and the coefficient for the wage from regressions of the job offer rate (see the notes to Table 2 for the details). The reference categories are the same as in Table 2. The standard errors are calculated using the delta method and the heteroskedastic robust covariance matrix. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively.

7 Conclusions

Labor market discrimination is a major issue in the policy debate both in Sweden and many other Western economies. Many policy attempts have been tried to reduce discrimination, but it has proven to be difficult to find effective policy measures. One interpretation of this is that policymakers are not properly informed about the degree and nature of the existing discrimination. In recent years, field experiments have expanded our knowledge about discrimination, but this approach has some inherent weaknesses; e.g. focusing on only the initial stage of the hiring process. Hence, there is a need for new approaches that can help us get a better understanding of discrimination.

In this paper, we study if Swedish employers (recruiters) discriminate based on the applicants' gender, age, ethnicity, religious beliefs, number of children, weight, or history of sickness absence by conducting a stated choice experiment. In the experiment, the recruiters are first asked to describe an employee who recently and voluntarily left the firm, and then to choose between two hypothetical applicants to invite to a job interview or to hire as a replacement for their previous employee. The applicants differ with respect to four attributes (of which one is always the wage), but are identical to the previous employee in all other dimensions.

Our results show that the recruiters discriminate against applicants who are old, non-European, Muslim, Jewish, obese, have several children, or have a history of sickness absence. Many of these results confirm what we know from previous observational and correspondence studies of discrimination. However, our results also extend the existing literature in several ways. We show that discrimination is a major issue *both* in the invitation to an interview phase and the hiring phase of the recruitment process. Moreover, we can *quantify* the degree of discrimination in a new way by calculating the wage reduction needed to make employers indifferent between applicants with and without a particular attribute (all else equal). These calculations should be interpreted with caution, but indicate that the wage reductions needed are substantial: In several cases, they correspond to a wage reduction of 10-50 percent. It should also be noted that our estimates probably should be seen as a lower bound of the actual degree of

discrimination since the employers may have attempted to hide their discriminatory behavior. From a policy perspective, it is difficult to see how so much wage flexibility could be implemented in practice or the consequences to society if it were.

The discrimination we find evidence of may reflect both preference-based and statistical discrimination: Discrimination based on e.g. ethnicity and religious beliefs may reflect both types of discrimination, while discrimination based on e.g. the number of children and previous sickness absence are likely to reflect statistical discrimination. In reality, both types of discrimination are likely to co-exist. However, policymakers need to be informed about their relative importance since the policies needed to prevent discrimination are very different depending on the type of discrimination. We used two methods to learn more about this. First, we analyzed if the degree of discrimination was affected by the firms' cost of uncertainty in hiring as measured by the extent of firm co-payment in the sickness benefit system. The idea being that, if the primary reason for discrimination is statistical, then we would expect to find evidence of more discrimination when we increase the cost of uncertainty in hiring. The results show that the degree of discrimination is *not* affected by changing the cost of uncertainty. This may be interpreted as evidence against statistical discrimination, but may also reflect that the costs to firms of recruiting the 'wrong' worker are substantial in all the (realistic) sickness benefit schemes we analyzed and, therefore, did not affect their recruitment behavior. Second, we analyzed if the degree of discrimination differed between different types of recruiters and firms. The idea being that, if the degree of discrimination is the same for different types of recruiters, then it is likely to reflect statistical discrimination. The results show that different types of recruiters – e.g. native Swedes and immigrants – respond in a quite similar way to most of the applicants' attributes. This may be interpreted as supportive of statistical discrimination since it is unlikely that different types of recruiters should have the same preferences regarding the attributes we consider. Moreover, we find that large firms discriminate less than small firms. This is also supportive of statistical discrimination since the consequences of hiring the 'wrong' worker should be more substantial for small firms. Overall, the results concerning the nature of the discrimination indicate that both preference-based

and statistical discrimination is prevalent, but that statistical discrimination may be more important.

Our results may also have implications for the design of firm co-payment schemes in social insurance systems, such as sickness benefits (c.f. experience rating). Our results show that more firm co-payment in the sickness benefit system reduces the callback and job offer rates, but that changing the extent of co-payment does not affect the degree of discrimination. These results suggest that policymakers should consider the effects on hiring carefully before introducing more firm co-payment, but that they may not need to worry that such changes will affect vulnerable groups more than other groups.

Our study demonstrates that stated choice experiments may be a valuable complement to the existing approaches to study discrimination. This approach has some weaknesses, such as being based on claimed rather than actual behavior, but also offers clear advantages by allowing us to study applicant characteristics which may be relevant in any of the stages of the hiring process and by giving us a new way of quantifying the degree of discrimination. In addition, our study illustrates that a potential way to distinguish between different types of discrimination is to use policy changes which involve changes in the firms' costs of hiring the 'wrong' worker. In our case, the variation may not have been sufficiently large to find any effects, but future studies may be able to introduce more variation between the alternatives. However, it is important that the alternatives are perceived as realistic by the firms, and this – as in our case – may put restrictions on the alternatives which can be analyzed.

In summary, our results show that discrimination is prevalent in the Swedish labor market, that the magnitude of this discrimination in terms of the wage reductions needed to make the employers indifferent between applicants with and without some attributes are substantial, and that the discrimination, at least partially, is likely to reflect statistical discrimination.

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Appendix A: Descriptive statistics

Table A1 The levels of the attributes

Gender	Female Male
Age	29 years or younger 30-55 years 56 years or older
Education	Lowest possible education for the job in question (lowest quartile) Average education for the job in question (middle quartiles) Highest education for the job in question (highest quartile)
Experience	4 years or less 5-7 years 8 years or more
Origin of birth	Nordic Countries Europe, except Nordic countries Africa, Middle East, South America
Religious belief	Christian Jewish Muslim
Children	No child 1 child 2 children or more
Weight	Normal weight Overweight Obese
Frequency of sickness absence	1-2 times per year 3-5 times per year 6 or more times per year
Intensity of sickness absence	7 or less days each time 8-14 days each time 15 or more days each time
Wage	10% lower wage than the previous employee Same wage as the previous employee 10% higher wage than the previous employee
Firm co-payment in the sickness benefit system	3 weeks of full firm payment 2 weeks of full firm payment 2 weeks of full firm payment followed by 15% co-payment

Notes: The levels used as a reference level in the estimation are bold.

Table A2 The workplaces in the experiment

	Fraction of workplaces
<i>Sector:</i>	
Private	65%
Public	28%
Other	7%
<i>Number of employees:</i>	
20-49	47%
50-99	25%
100-249	14%
>249	14%
<i>Percentage women:</i>	
1-40%	38%
40-60%	24%
60-100%	38%

Table A3 The characteristics of the recruiters

Personal characteristics		Education and experience		History of sickness absence and health	
<i>Gender:</i>		<i>Education:</i>		<i>Sickness absence last 12 months:</i>	
Female	64%	Primary	3%	Yes	36%
Male	36%	Secondary	18%	No	64%
		University	79%		
<i>Age:</i>		<i>Position:</i>		<i>Frequency of sickness absence:</i>	
<30 years	3%	General manager	29%	1-2 times per year	94%
30-55	73%	Personnel manager	31%	3-5 times per year	6%
>55 years	24%	Other	40%	6 or more times per year	0%
<i>Number of children:</i>		<i>Tasks:</i>		<i>Intensity of sickness absence:</i>	
No children	18%	Recruitment	90%	7 or less days each time	93%
One child	14%	Personnel policy	82%	8-14 days each time	3%
Two or more children	68%	Rehabilitation	71%	15 or more days each time	4%
<i>Country of birth:</i>		<i>Experience working with these issues:</i>		<i>Weight for men:</i>	
Sweden	90%	At least 4 years	80%	Underweight	3%
Other Nordic countries	7%	Less than 4 years	20%	Normal weight	26%
Other European countries	2%			Overweight	57%
Outside Europe	1%			Obese	14%
<i>Religious beliefs:</i>				<i>Weight for women:</i>	
Christian	76%			Underweight	6%
Jewish	0%			Normal weight	44%
Muslim	0%			Overweight	43%
Other	1%			Obese	7%
Atheist/agnostic	17%				
Don't know	6%				

Table A4 The characteristics of the last employee

Personal characteristics		Education and experience		History of sickness absence and health	
<i>Gender:</i>		<i>Education:</i>		<i>Sickness absence last 12 months:</i>	
Female	49%	Primary	8%	Yes	32%
Male	51%	Secondary	39%	No	66%
		University	53%	Don't know	2%
<i>Age:</i>		<i>Qualifications:</i>		<i>Frequency of sickness absence</i>	
<30 years	22%	Unqualified/lowest quartile	8%	1-2 times per year	46%
30-55	69%	Middle quartiles	47%	3-5 times per year	33%
>55 years	9%	Highest quartile/overqualified	41%	6 or more times per year	16%
		Don't know	4%	Don't know	5%
<i>Number of children:</i>		<i>Years of experience:</i>		<i>Intensity of sickness absence:</i>	
No children	37%	4 years or less	24%	7 or less days each time	80%
One child	15%	5-7 years	20%	8-14 days each time	7%
Two or more children	39%	8 years or more	52%	15 or more days each time	11%
Don't know	9%	Don't know	4%	Don't know	2%
<i>Country of birth:</i>		<i>Tenure in the firm:</i>		<i>Weight for men:</i>	
Sweden	84%	4 years or less	56%	Underweight	19%
Other Nordic countries	3%	5-7 years	25%	Normal weight	40%
Other European countries	3%	8 years or more	19%	Overweight	32%
Outside Europe	5%			Obese	9%
Don't know	5%				
<i>Religious beliefs:</i>		<i>Wage:</i>		<i>Weight for women:</i>	
Christian	65%	Mean	26,800	Underweight	26%
Jewish	0%	Median	25,000	Normal weight	38%
Muslim	2%			Overweight	28%
Other	0%			Obese	8%
Atheist/agnostic	5%				
Don't know	27%				

Table A5 Information about the job applicants' history of sickness absence and health

<i>Try to gather information about history of sickness absence:</i>	
Yes	41%
No	56%
Don't know	3%
 <i>Method to get information about history of sickness absence:</i>	
Ask the applicant	88%
Ask a reference	90%
Ask the applicant to provide an excerpt from the SSIA	10%
 <i>Try to gather information about health:</i>	
Yes	44%
No	50%
Don't know	6%
 <i>Method to get information about health:</i>	
Ask the applicant	93%
Ask a reference	57%
Ask about previous occupation injuries	25%
Health examination	34%
Ask about leisure activities	46%
Ask about smoking habits	30%
Evaluate physical appearance	29%

Appendix B: The questionnaire

Part 2: Who do you invite to a job interview as a replacement?

Suppose that your company *must* fill the position that became vacant when the employee you described in Part 1 left the company and that your firm has only two applicants for the position. This means that in questions 22-25 you *must* invite one of the two applicants to an interview. You should choose the applicant who is the most suitable – or least unsuitable – of the two applicants for the position.

Suppose further that the two applicants are identical to the individual you described in Part 1 in all dimensions except the ones stated in the questions.

Note that COFIN means that your company, in addition to paying the sick-wage for the days specified, would pay 15% of the sickness benefits for the rest of the sickness spell. However, your company would not have to pay costs less than SEK 12,000 or more than 4% of the company's total wage costs. Also, COFIN means that your company would pay a 0.25% lower employment tax.

22 Who do you invite to an interview if you must choose one of the following two applicants?

The only differences between the two applicants and the employee you described in Part 1 are summarized in the table.

APPLICANT A:		APPLICANT B
Woman	Gender	Man
29 years or younger	Age	56 years or older
10% lower wage than the employee	Wage (non-negotiable)	10% higher wage than the employee
Day 2-21	Sickness insurance	Day 2-14

1 Invite A

2 A and B similar

3 Invite B

23 Who do you invite to an interview if you must choose one of the following two applicants?
The only differences between the two applicants and the employee you described in Part 1 are summarized in the table.

APPLICANT A:		APPLICANT B
Man	Gender	Woman
30-55 years	Age	29 years or younger
Same wage as the employee	Wage (non-negotiable)	10% lower wage than the employee
Day 2-21	Sickness insurance	Day 2-14
1 <input type="checkbox"/> Invite A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Invite B

24 Who do you invite to an interview if you must choose one of the following two applicants?
The only differences between the two applicants and the employee you described in Part 1 are summarized in the table. The education quartile is described in question 10.

APPLICANT A:		APPLICANT B
The middle quartiles	Education quartile	The lowest quartile
5-7 years	Experience	8 years or more
Same wage as the employee	Wage (non-negotiable)	10% lower wage than the employee
Day 2-14 + COFIN	Sickness insurance	Day 2-14
1 <input type="checkbox"/> Invite A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Invite B

25 Who do you invite to an interview if you must choose one of the following two applicants?
The only differences between the two applicants and the employee you described in Part 1 are summarized in the table. The education quartile is described in question 10.

APPLICANT A:		APPLICANT B
The middle quartiles	Education quartile	The lowest quartile
5-7 years	Experience	8 years or more
10% higher wage than the employee	Wage (non-negotiable)	Same wage as the employee
Day 2-14	Sickness insurance	Day 2-21
1 <input type="checkbox"/> Invite A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Invite B

Part 3: Who do you hire as a replacement?

Suppose that your company *must* fill the position that became vacant when the employee you described in Part 1 left the company and that your firm has only two applicants for the position. This means that in questions 26-33 you *must* hire one of the two applicants. You should choose the applicant who is the most suitable – or least unsuitable – of the two applicants for the position.

Suppose further that the two applicants are identical to the person you described in Part 1 in all dimensions except the ones stated in the questions.

COFIN is explained in the introduction to Part 2.

<p>26 Who do you hire if you must hire one of the following two applicants? <i>The only differences between the two applicants and the employee you described in Part 1 are summarized in the table.</i></p>		
APPLICANT A:		APPLICANT B
Europe excluding the Nordic countries	Country of birth	Africa/Middle East/South America
Christian	Religion	Muslim
Same wage as the employee	Wage (non-negotiable)	10% lower wage than the employee
Day 2-21	Sickness insurance	Day 2-14 + COFIN
1 <input type="checkbox"/> Hire A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Hire B
<p>27 Who do you hire if you must hire one of the following two applicants? <i>The only differences between the two applicants and the employee you described in Part 1 are summarized in the table.</i></p>		
APPLICANT A:		APPLICANT B
Europe excluding the Nordic countries	Country of birth	Africa/Middle East/South America
Muslim	Religion	Jewish
Same wage as the employee	Wage (non-negotiable)	10% lower wage than the employee
Day 2-14	Sickness insurance	Day 2-21
1 <input type="checkbox"/> Hire A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Hire B

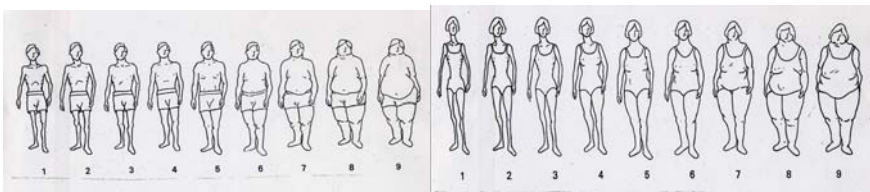
28 Who do you hire if you must hire one of the following two applicants?
The only differences between the two applicants and the employee you described in Part 1 are summarized in the table.

APPLICANT A:		APPLICANT B
Woman	Gender	Man
2 or more children	Children	2 or more children
10% lower wage than the employee	Wage (non-negotiable)	10% higher wage than the employee
Day 2-14 + COFIN	Sickness insurance	Day 2-21
1 <input type="checkbox"/> Hire A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Hire B

29 Who do you hire if you must hire one of the following two applicants?
The only differences between the two applicants and the employee you described in Part 1 are summarized in the table.

APPLICANT A		APPLICANT B
Man	Gender	Woman
2 or more children	Children	No children
Same wage as the employee	Wage (non-negotiable)	10% lower wage than the employee
Day 2-14	Sickness insurance	Dag 2-14 + COFIN
1 <input type="checkbox"/> Hire A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Hire B

30 Who do you hire if you must hire one of the following two applicants?
The only differences between the two applicants and the employee you described in Part 1 are summarized in the table. The weight scale is given below:



APPLICANT A:		APPLICANT B
Woman	Gender	Woman
7-9	Weight	7-9
Same wage as the employee	Wage (non-negotiable)	10% lower wage than the employee
Day 2-21	Sickness insurance	Day 2-14 + COFIN
1 <input type="checkbox"/> Hire A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Hire B

31 Who do you hire if you must hire one of the following two applicants?

The only differences between the two applicants and the employee you described in Part 1 are summarized in the table. The weight scale is given in question 30.

APPLICANT A:		APPLICANT B
Man	Gender	Woman
3-4	Weight	5-6
Same wage as the employee	Wage (non-negotiable)	10% lower wage than the employee
Day 2-21q	Sickness insurance	Day 2-14
1 <input type="checkbox"/> Hire A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Hire B

32 Who do you hire if you must hire one of the following two applicants?

The only differences between the two applicants and the employee you described in Part 1 are summarized in the table.

APPLICANT A		APPLICANT B
1-2 times per year	Sickness absence, times per year	3-5 times per year
15 or more days each time	Sickness absence, days each time	8-14 days each time
10% higher wage than the employee	Wage (non-negotiable)	Same wage as the employee
Day 2-14	Sickness insurance	Day 2-21
1 <input type="checkbox"/> Hire A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Hire B

33 Who do you hire if you must hire one of the following two applicants?

The only differences between the two applicants and the employee you described in Part 1 are summarized in the table.

APPLICANT A:		APPLICANT B
3-5 times per year	Sickness absence, times per year	6 or more times per year
8-14 days each time	Sickness absence, days each time	7 or less days each time
Same wage as the employee	Wage (non-negotiable)	10% lower wage than the employee
Day 2-14 + COFIN	Sickness insurance	Day 2-14
1 <input type="checkbox"/> Hire A	2 <input type="checkbox"/> A and B similar	3 <input type="checkbox"/> Hire B