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ABSTRACT

Unexplored Dimensions of Discrimination in Europe: Homosexuality and Physical Appearance*

We study labor-market discrimination of individuals with specific characteristics in Italy. We conduct a field experiment in two Italian cities: Rome and Milan, by sending fake CVs to real ads. We find that there is a strong penalty for homosexuals, i.e. about 30% less chance to be called back compared to an heterosexual male and even more so if they are highly skilled. On the other hand, we find no penalty for homosexual females. We also find a beauty premium for females only but this premium is much lower when the "pretty" woman is skilled.

JEL Classification: I10, J16 and J71
Keywords: discrimination, field experiment, gays and lesbians

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1 Introduction

Discrimination in the labor market is maybe one of the most studied topics in economics but certainly the least understood. Indeed, when explaining the adverse labot-market outcomes of certain categories of workers (for example, ethnic minorities or women), it is very difficult to disentangle between discrimination and other (often) unobserved aspects such as low ability or social norms and peer effects. Recently, researchers in economics have been using new methods to tackle this issue: field studies (such as audit studies and correspondence tests) and natural experiments. The results convincingly show that there is discrimination against minority workers and women.[1]

In the present paper, we consider under-investigated categories of individuals that might be discriminated against in the labor market. In particular, we focus on the labor-market outcomes of homosexual and non-attractive individuals.

We first present the theoretical mechanisms underlying discrimination in the labor market and survey the existing related empirical literature. Interestingly, homosexual females do not seem to suffer from discrimination in the labor market and, on the contrary, some studies reveal that they have a "premium" in terms of the probability of finding a job. Using the taste-based and statistical theories of discrimination, we can probably explain why homosexual males have adverse labor-market outcomes but not why we find the opposite result for homosexual females. There is another theory that can explain the latter fact. Becker (1981) has put forward the idea of specialization within families by arguing that heterosexual males specialize in market labor, and heterosexual females in household labor because of comparative advantages caused by biological differences. On the contrary, homosexual households are unable to specialize to the same extent as heterosexual households, because the gains from gender differences between spouses in comparative advantages do not exist. Lesbians who expect to form households will therefore not acquire less market-related human capital than heterosexual females, and will therefore have a higher chance of finding a job. In contrast, gay males are predicted to have worse labor-market outcomes than heterosexual males, because they will invest less in market-related human capital than heterosexual males.

[1] Indeed, economists have accumulated a large body of evidence on the existence of both gender and ethnic/racial discrimination using various empirical approaches ranging from traditional empirical data analysis (Kahn, 1991; Knowles et al. 2001; Altonji and Pierret, 2001) to field experiments (see Riach and Rich, 2002 for an exhaustive survey of field experiments discrimination). Two main procedures of field experiments have been used to carry out tests for the extent of discrimination. A first procedure consists in matching two testers who attend job interviews or buy products, one from the majority group and the other from the minority. These experiments have provided strong evidence of discrimination in different contexts, including housing market (Galster, 1990), sports car market (List, 2004), car sales (Ayres and Siegelman, 1995) or television shows (Levitt, 2004). Another field approach to measure the extent of discrimination at the hiring stage, consists in sending matched CVs that vary in only one variable (for example the name) to employers in response to job advertisements (see for instance, Neumark, 1996; Bertrand and Mullainathan, 2004).

Regarding unattractive individuals, the existing (limited) studies suggest the presence of a penalty, both for males and females, although the magnitudes varies greatly between the different studies. With the exception of some studies based on fields experiments in Sweden and few other studies using special datasets on one country, the evidence on these topics for most European countries is virtually non-existent.

In this paper, we conduct a field experiment in two Italian cities: Rome and Milan to study the relationship between homosexuality or beauty and labor-market outcomes as measured by the difference in the percentage of callback rates between the reference group (homosexuals or "ugly" persons) and the control group (heterosexuals or "pretty" persons). For that, we send "fake" CVs, which clearly indicate the participation to a gay or lesbian organization for homosexuals and different pictures to highlight how handsome or ugly is the candidate. We randomly assigned CVs so that some belong to homosexuals and others to heterosexuals. We use the same procedure for the "beauty" of the person. The design of the experiment allows us to control for all possible nuisances that may bias the assessment of the relationship between sexual orientation in the first case and beauty in the other one.

We find that there is a statistical significant penalty (in terms of callback rates) associated to homosexual males of about 3% whereas homosexual females does not seem to show any significant difference with respect to heterosexual females. To be more precise, since the callback rate for males is 10%, this means that, compared to heterosexual males, homosexuals have 30% less chance to be called back. We also find that this penalty is higher for high-skilled homosexual individuals, with an associated magnitude of more that 8% for homosexuals males. No penalty or premium is instead associated to high-skilled homosexual females, confirming that only males are penalized in the labor market for their homosexuality.

We then investigate differences in response of callback rates by picture beauty. We find that there is a significant premium for attractive females of about 2% and no significant difference between handsome and ugly men. We also investigate whether the beauty premium for women varies by skills. We find that high-skilled attractive women are called back less often than low-skilled attractive women, which may indicate that beauty might not be an advantage for high-skilled women.

The rest of this paper unfolds as follows. In Section 2, we expose the different theories that may explain why individuals with specific observable differences end up with adverse labor-market outcomes. Section 3 gives an overview of the empirical literature. In Section 4, we describe our field experiments and investigate whether there is discrimination in callback rates between homosexuals or pretty individuals and heterosexuals or ugly individuals. Finally, Section 5 concludes.

2 Discrimination and labor-market outcomes: Theoretical mechanisms

2.1 Standard theories of discrimination

The economics literature posits two major sources of discrimination: *taste-based* and *statistical*. The first one is due to the fact that employers dislike some categories of the population while statistical discrimination occurs in an environment of imperfect information where agents form expectations based on limited signals that correlate with some observable characteristics.[2]

To be more precise, *taste-based* models originate from Gary Becker's seminal work (1957). In Becker's model, discrimination in hiring or wages is caused by a "taste for discrimination", that leads the employer to hire or pay higher wages to members of his/her own group. In this approach, discrimination is costly and leads to segregated workplaces. In Becker's model, prejudiced employers, workers or consumers dislike employing, working with, or purchasing from people with observable traits (like e.g. race, gender, beauty, obsesity, homosexuality, etc.).

If the Becker model is correct, the market should relentlessly eliminate discrimination except where it cannot provide sufficient segregation. This is most likely to occur for workers in specialized occupations requiring customer awareness of the characteristic of the worker, where firm entry is limited, where the proportion of discriminated workers in the labor force is large, and where prejudice is widespread.

The second main explanation for discrimination is defined as *statistical discrimination* and is based on incomplete information (Arrow, 1973; Phelps, 1972). According to models of statistical discrimination, employers have incomplete information about the employee's performance and consequently base their hiring/wage setting decisions on (erroneous) stereotypes. In Arrow (1973)'s model and in a similar model developed by Phelps (1972), employers have (erroneous) beliefs that individuals from some particular groups (homosexuals for instance) are less productive and would act accordingly. Models of statistical discrimination differ in the fact that some authors consider that stereotypes are erroneous while others argue that stereotypes may correspond to actual group averages in equilibrium. In the first case, imperfect information would arise because discriminated groups emit noisier signals. Consequently, employers who observe ability with greater error (rationally) discriminate people belonging to discriminated groups (Phelps, 1972; Aigner and Cain, 1977; Cornell, and Welch, 1996). In the second category of statistical discrimination models, negative prior beliefs about members of a particular group may become self-fulfilling in equilibrium (Lundberg

[2]See the overviews by Altonji and Blank (1999), Lang and Lehmann (2011), Kofi Charles and Guryan (2011), and Boeri and van Ours (2012).

and Startz, 1983; Coate and Loury, 1993). This may be the case for instance if individuals of a specific group under-invest in human capital because they anticipate a discriminatory treatment and therefore that they will receive a lower return to education.

2.2 Discrimination theories more specific to homosexuals

Just as in the case of gender and ethnicity, there might be several possible means through which sexual orientation is independently related to a number of economic outcomes, such as earnings or the probability of finding a job. Two hypotheses have dominated in this literature. The first is the discrimination hypothesis, based on Becker's (1957) taste-based discrimination theory, or Arrow's (1973) and Phelps' (1972) statistical theory of discrimination (both theories have been exposed in the previous section). The second is the hypothesis of *specialization within families*. The predictions are the same: gay males are predicted to experience earnings disadvantages compared with heterosexual males, while lesbians are expected to experience earnings advantages compared with heterosexual females.

According to taste-based discrimination or statistical discrimination (see Section 2.1), employers may act on their bias against homosexuals, which may result in disadvantages on the labor market such as lower earnings or lower chance to find a job. This outcome is, however, more likely for gay males than for lesbians since attitudes towards gay males are much more hostile than are attitudes towards lesbians (Herek 2000; Kite and Whitley 1996). The statistical discrimination model is typically used to make predictions about lesbians. Stereotypes about lesbians, for example that they are more focused on their career, that they are less likely to have children or that they are more masculine, are considered to be an important source of bias. In the statistical discrimination framework, lesbians therefore are predicted to do better than their heterosexual counterparts.

The idea of *specialization within families* was put forward by Becker (1981), who argued that heterosexual males specialize in market labor, and heterosexual females in household labor because of comparative advantages caused by biological differences. Heterosexual females therefore acquire less market-related human capital and more home-related human capital. In contrast, heterosexual males will acquire more market-related human capital. This results in earnings differentials between males and females. On the contrary, homosexual households are unable to specialize to the same extent as heterosexual households, because the gains from gender differences between spouses in comparative advantages do not exist. Lesbians who expect to form households will therefore not acquire less market-related human capital than heterosexual females, and will therefore earn more. In contrast, gay males are predicted to earn less than heterosexual males, because they will invest less in market-related human capital than heterosexual males.

3 Discrimination against homosexuals and physical appearance: Overview of the empirical literature

Let us now review the empirical literature on the relationship between being homosexual or having a specific physical appearance and labor-market outcomes.[3]

3.1 Homosexuality and labor-market outcomes

There is a relative small literature (especially in economics) on discrimination and the labor-market outcomes of being homosexual that we would like to review here. We will first examine if there is evidence of discrimination of homosexuals. Then, we will examine the consequences of being homosexual on the hiring process in the labor market.

3.1.1 Is there discrimination against homosexuals?

During recent decades economists have used field experiments in order to detect discrimination on labor markets, housing markets and product markets in different countries (see Riach and Rich 2002 for an overview). Many of these field experiments have focused on females, on immigrants, on the elderly but less attention has been paid to discrimination against homosexuals.

Psychological and sociological research demonstrates the existence of sexual prejudice. Like other types of prejudice, sexual prejudice is an attitude; it is directed at a social group and its members; and it involves hostility or dislike. There is some literature in psychology furnishing proofs that negative attitudes towards homosexuals do exist (Herek and Capitanio 1996; Yang 1997).

Economic research regarding discrimination against homosexuals has so far primarily made use of register data and econometric methods. Focus has been on differences between homo- and heterosexuals in labor market outcomes.

There are very few experiments trying to test discrimination against homosexuals. In Canada, Adam (1981) established discrimination against male as well as female homosexuals who applied for jobs in Canadian law firms, and Weischselbaumer (2003) found that lesbian females were subject to discrimination when they applied for jobs in Austria.

To the best of our knowledge, Ahmed and Hammarstedt (2009)[4] were the first to have a field experiment studying discrimination against homosexuals on the housing market in Sweden. They conduct a study on the rental housing market using the internet as a research

[3]Even though there is an important literature on earning differences (see, e.g. Ahmed et al., 2011a), we focus here on employment outcomes since this is what we test in our experiments.

[4]See also Ahmed et al. (2012).

platform. Two fictitious couples, one heterosexual and one homosexual, both openly signalling their sexual orientation, apply for vacant rental apartments advertised by landlords on the internet in Sweden. Homosexuals are identified as individuals living with partners of the same sex. The authors explore the incidence of discrimination by observing how landlords e-mail back and invite applicants to further contacts and/or to a showing of the housing unit. Their findings show that homosexual males are discriminated against on the Swedish housing market, since the homosexual couple gets far fewer call-backs and fewer invitations to further contacts and to showings of apartments than the heterosexual couple.

3.1.2 Employment differences between homosexuals and heterosexuals

We would like now to study the difference in the hiring process in the labor market. In this section, we would like to survey the field experiments in this literature.

Interestingly, the European studies (Drydakis, 2009, 2011, for Greece; Weichselbaumer, 2003, for Austria) find strong evidence of discrimination against gays and lesbians in the hiring process while the North American studies (Adam, 1981, for Canada; Hebl et al., 2002 for the US) find no effect. All these studies have some limitations and we would like to expose the recent study by Ahmed et al. (2011b) for Sweden, which is the "cleanest" study in terms of testing this effect. They construct written applications consisting of an application letter that described a fictitious applicant and a résumé suited for applying to ten different occupations. The applications were sent to all employers that were announcing an open vacancy during the period between August 2010 and January 2011. The applicant's gender and sexual orientation were randomly assigned to the application for each employer they contact. Therefore, each potential employer received only one application, which was either from a heterosexual male or female or from a gay or lesbian person. The authors use distinctive male or female names to signal the gender of the applicant and labeled the applicant as a gay, lesbian or heterosexual by revealing the gender of the applicant's spouse and by adding information about voluntary work in a homosexual organization (for gay and lesbian) and a neutral help organization (for heterosexual). Interestingly, this is the first nationwide field experiment, even though gays and lesbians tend to live disproportionately in big cities (Black et al., 2007). Ahmed et al. (2011b) find that there is indeed discrimination in the hiring process in the labor market for both gays and lesbians. Gays are discriminated against in typical male-dominated occupations while lesbians are discriminated against in typical female-dominated occupations. The magnitude of the discrimination varies between different occupations and there is discrimination against gays and lesbians only in the private sector. To be more precise, a heterosexual female applicant received 22 percent more responses from employers than a lesbian applicant while for a heterosexual male this figure was 14 percent.[5]

[5]For Austria (Weichselbaumer, 2003) and Greece (Drydakis, 2011), a heterosexual female applicant re-

So far we have reviewed the empirical literature on homosexuals. Physical appearance is also an aspect that has been studied and which is subject of discrimination. We would like to review the literature on "beauty" and to examine whether people who are not good looking are discriminated against and have adverse labor-market outcomes as compared to better-looking workers.[6]

3.2 Physical appearance and labor-market outcomes

A relatively large body of empirical literature has analyzed the correlation between beauty and labor market outcomes (for a review of this literature, see Hamermesh, 2011). However, the evidence demonstrating a causal relationship is scarce.

Heilman and Saruwatari (1979) provide evidence of a positive correlation between beauty and labor market performance. They asked college students to rate resumes (which included a photograph) of applicants for one of two jobs, a traditionally male managerial job and a traditionally female non-managerial job. Subjects were told that all applicants had recently graduated and had been pre-screened on the basis of educational and background qualifications. An examination of the results showed that attractiveness consistently was an advantage for male applicants but was an advantage only for females seeking traditionally female jobs. Attractive females were perceived as more feminine than unattractive females and were therefore at a disadvantage when seeking a job that traditionally required masculine characteristics. Biddle and Hamermesh (1994) provide further evidence of the beauty premium. Analyzing self-reported data on respondents' appearance and labor market variables, they find that unattractive people earn 5 to 10 percent less than average-looking people, who in turn earn less than the good-looking individuals. Harper (2000) study individuals born in Britain. He finds that the penalty of being unattractive (a self-reported measure) is about 15 percent lower wages for men and 11 percent lower wages for women as compared to the attractive applicants. Fink et al. (2007) find attractiveness to be correlated with the subjects' physical strength. Fletcher (2009) uses longitudinal data on wages from the United States and finds that wage returns to (self-reported) attractiveness are large (5 to 10 percent) relative to the returns to ability (3 to 6 percent).

The works cited are all non-experimental studies. The identification of a causal link between attractiveness and labor market performance is a complex task: one that becomes particularly questionable in non-experimental settings. Different sources of biases, ranging from the selection into occupations/labor market to the potential reverse causality from income to attractiveness, might contaminate the results obtained from non-experimental set-

ceived 31 and 123 percent more responses from employers than a lesbian applicant while for a heterosexual male this figure was 186 percent for Greece since Weichselbaumer (2003) only studied lesbians.

[6]There is also a literature on the adverse-labor market outcomes of obese people that we do not review here since we focus in our experiment on attractive versus non-attractive persons.

tings (Bertrand and Mullainathan 2004). In an experimental setting, Mobius and Rosenblat (2006) revealed that attractive people received higher wages because they were perceived as more able, conditional on productive skills. They find, however, that beauty is not correlated with labor productivity.

Different field experiments have been conducted in different countries. Using recent research in psychology, anthropology, and graphic design technology, people have been using the following methodology (similar to experiments testing discrimination of obese people). One constructs a series of fictitious faces and attach them to fictitious resumes. While ensuring that the resumes are of equal quality (by controlling for their content), researchers made the faces progressively more attractive or unattractive through manipulations by computer. The researchers then submit these fictitious resumes (including photographs) to real job openings and analyze the responses (callbacks).

For instance, in a randomly selected telephone survey in the US (Kuran and McCaffery, 2004), it was found that most of the participants felt that discrimination based on looks exceeded discrimination based on ethnicity or national background.

López Bóo et al. (2012) conduct a randomized field experiment in Buenos Aires, Argentina by providing evidence on the existence of discrimination based on physical appearance in an early stage of the job search process. Although they analyze a different question, their experiment design follows the empirical strategy utilized in Bertrand and Mullainathan (2004). Their results indicate that attractive people receive 36 percent more callbacks than unattractive people. The authors also document that more attractive candidates are not only more likely to be contacted, but that they are contacted sooner than less attractive applicants. Given the experimental setting, the estimated beauty premia can only be attributed to the differences in facial attractiveness of the job candidates. A similar study is that of Ruffle and Shtudiner (2010). These authors analyze the effects of attractiveness on callback rates following a similar experimental strategy but in Israel. They find similar effects.

4 Do employers discriminate against physical appearance and sexual preference? A field experiment

As stated above, estimating the effect of sexual preferences and personal appearances is difficult because it is practically impossible to deal with potential unobserved traits that are likely to be correlated both to sexuality and appearances. First, there is often an *absence of accurate information*. Unlike ethnicity and gender, which are both easily observable, the sexual orientation of individuals is not generally an observable trait and our way of identifying same-sex relationships (by asking to each respondent to identify —among the

member of the household— his or her partner) is not without problems. This clearly biases the results. Second, since *homosexuality is not an observable characteristic*, its exposure can happen either voluntarily or involuntarily. If it occurs voluntarily it is an endogenous action. According to economic theory, rational individuals should experience at least some benefits arising from such an action, which might also bias the results.

To circumvent these problems, we conduct a field experiment that builds on the correspondence testing methodology that has been primarily used in the past to study minority outcomes in the United States, notably by Bertrand and Mullainathan (2004).

We study the effect of perceived homosexuality and of physical appearances in the labor market by sending fictitious résumés to help-wanted ads in Rome and Milan. These are the two Italian cities with the biggest labor markets. We expected it to be easier to find there an on-line help-wanted ad and to receive a callback. As a matter of fact, most of the help-wanted ads in websites commonly used to advertise job vacancies are for jobs in Milan and in Rome. We experimentally manipulate perceived homosexuality by randomly modifying résumés by adding items that reveal sexual preferences. At the same time, we randomly attach to the résumés, pictures that have been previously ranked in terms of beauty to study the effect of appearances on labor market outcomes.

4.1 Description of the experiment

The first step of the experimental design is to restrict the ads to whom résumés were sent. We restricted the field experiment to seven occupations: administrative clerk, bookkeeper, call center operator, receptionist, sales clerk, secretary and shop assistant. These occupations were selected by looking at the distribution of help-wanted ads on specialized web sites. These seven occupations were the most frequent among those not requiring very specific skills for which would have been difficult to create standard profiles of job applicants.

The second step is to create templates for the résumés to be sent. To generate those templates, we collected résumés of actual job seekers by posting ads on the most used Italian websites for job search.[7] From the résumés we received, we extracted information concerning the distribution of educational attainment including names of schools and colleges attended and types of experience including names of previous employers. More specifically, we construct databases containing names of high schools in Milan and in Rome (within an an area of 30 km), names of colleges in Milan and in Rome, names of companies and description of work experiences. These information were then used as a building block for generating the résumés.

Résumés were generated through an ad-hoc software that randomly chose some charac-

[7] In order to avoid generating false hope, candidates were notified by mail that the job vacancy they were applying for had already been filled.

teristics from these ad-hoc datasets.

For each city and occupation, we used six different identities, three males and three females. Some of their characteristics remain constant among résumés for the same city and occupation. First and last names for these identities were selected among the most common Italian names. For each occupation, we associated a specific name-surname pair. By doing so, we could easily understand the occupation and profile considered in case of callbacks. For example, "Giulia Villa" was always an administrative clerk in Milan, while "Francesco Ricci" was always a bookkeeper in Rome. The addresses of residence were chosen to give not any additional information about the socio-economic status of the applicants. The year of birth of the applicants were randomly set between 1977 and 1992. Education and work experience were coherently set with the age of a particular applicant. We used six phone numbers, each of them associated with one name in Milan and one in Rome. This means that each number was associated to 14 different identities, two for each of the seven occupations (then identified by the name-surname pairs).

The résumé items revealing of *homosexual preferences* were periods of internship in pro-gay advocacy groups that are real, well-known by the public at large, city-specific and in any case their names were very explicit about the nature of the group like, for example, "Arcilesbica Roma", "Centro di Iniziativa Gay-Arcigay" or "DGP - Di Gay Project", etc. Applicants in the control group have instead worked as interns for a period of similar length in a non gay/non lesbian cultural association or in a company. In order to better match the occupation, tasks performed during internships were different across applicants. For each city and for each occupation, one of the three types of internship was associated with two identities, one male and one female.

For *physical appearance*, we randomly assign to each résumé a picture chosen among 89 previously collected photos of individuals aged between 20 and 35 years old. The photo was chosen in such a way that it minimized differences between the age declared in the résumé and the real age of the person depicted in the picture.

For each city and for each occupation, we created 1,200 fake résumés. To understand how the program generates a fake résumé, consider a specific identity. Name, surname, home address, phone number, e-mail address, date and place of birth, and type of internship are fixed. First of all, the program randomly chooses the year of birth from 1977 to 1992. Then, depending on the age, it picks a picture. The program associates pictures to identities who have 2 years from the age of the person in the picture. Secondly, the program randomly chooses the highest level of education attained. If it has selected a degree, then it randomly chooses a senior high school from the second dataset. Thirdly, the program fixes the length of the work period, accordingly to the following formula: length of work period = age − 19 − years of the degree − 6 months of internship. Then, on the basis of the years of work, the program assigns one, two or three work experiences, accordingly to the following rules:

- work period $\leq$ 3 years: one work experience
- work period $>$ 3 years: two work experiences (each experience lasts "work period/2")
- work period $>$ 10 years: three work experiences (each experience lasts "work period/3")

Lastly, the program chooses a level of language skills and a level of computer skills. If the program has selected a senior high school or a degree specializing in modern languages, it then assigns to the résumé the knowledge of two foreign languages, one of which with excellent proficiency. For the computer skills, there are no constraints.

The beauty of the person portrayed in the picture was assessed by an independent panel formed by 24 people. Members of this panel had never seen in real life the persons in the picture. Some of the members of the panel were employees of an employment agency that specialize in matching résumés to open vacancies.

The field experiment started on January 17th, 2012 and ended on February 21st, 2012. During this period, for each city and occupation, we selected the most recent employment ads published in two websites: Job Rapido and Monster. They are the most popular websites among actual jobseekers. We answered to 531 ads, 336 in Milan and 195 in Rome. We typically sent four résumés in response to each ad: two from the treatment group and two from the control group. In total, we sent $2,320$ résumés. Our sample size is relatively low when compared to other field experiments consisting in sending fake CVs to potential employers. For instance, the total number of application sent by Bertrand and Mullainathan (2004) is 4,870. Although we would have preferred sending out a larger number of résumés, ethical considerations prevented us of doing so. Given the limited size of the two Italian markets considered, sending a larger number of fake CVs could have resulted in a lower call back rates for the real job applicants.

4.2 Results

Table 1 reports the recall rates by sex and cities. The overall response rate was about 11%, with a minor difference between males and females (10.83% and 11.24%, respectively). Looking at the percentages by city, the response rate is higher in Rome (about 16%), where males were more likely to be called back than females (17.48% versus 14.96%). On the contrary, in Milan, the overall response is roughly divided by two (about 8%) and males were less likely to be called back than females (7.19% versus 9.10%).[8]

[Insert Table 1 here]

[8]These call back rates are in line with those of other fields experiments that consist in sending matched CVs to employers. For instance, in their experiment, Bertrand and Mullainathan (2004) obtain a total call back rate of 8.05%.

Table 2 shows that "call center operator" type of job was the occupation which received the highest rate of callbacks (also by gender), followed by "receptionist" and "sales clerk". Interestingly, people who sent their CVs to a secretary or a shop assistant job got a very low callback rate, i.e. 4.66% and 2.95%, respectively.

[Insert Table 2 here]

We continue our data analysis by showing some evidence on the validity of our experiments for answering our main research question. Table 3 shows the association between our treatment variables and characteristics of the résumés and the ads (treatment variable "homosexual" and "beauty scores" in panel (a) and (b), respectively). We provide this table as a randomization check to examine whether random assignment has succeeded, thus enabling us to assess the internal validity of the experiment results. As expected, the beauty score is associated with a "gay" internship and with age. As a result, it is important in our following regression analysis to control for picture beauty and age when assessing the premium/penalty of being homosexual in the labor market. Notice that we also control for other covariates as their inclusion is well known to improve efficiency and thus increase the precision of the estimates. In the Appendix, we demonstrate formally this claim.

[Insert Table 3 here]

We start with Table 4 to investigate the relationship between response rate (callback) and homosexual preferences in panel (a) and between response rate and picture beauty in panel (b), with basic controls and distinguishing between males and females. We use linear probability models where the dependent variable takes value 1 if the identity of the person is called back (denoted "callback" in the table).[9] Our target variable in panel (a) is a dummy taking value 1 if the identity is associated with periods of internship in pro-gay advocacy groups and 0 otherwise (denoted by "homosexual"). In panel (b), it is a variable containing the beauty scores received by the picture associated with the identity (denoted by "beauty score"), as assessed by the jury panel. The scores range between zero and 10, where 10 indicates the most attractive individual. Figure 1 shows the distribution of beauty scores in our sample of job applicants. The distribution is broken into five intervals, each containing approximately 20% of the sample.

[Insert Figure 1 here]

Results in panel (a) reveal a statistical significant penalty (in terms of callback rates) associated to homosexual males of about 3% whereas homosexual females does not seem

[9] Although we only report results for the linear probability model, the findings in this section are qualitatively and quantitatively similar when using a saturated model in which age and beauty score are discretized.

to show a significant difference in callback rate with respect to heterosexual females, even though the sign is positive. Observe that the 3% penalty for homosexual males is quite high since the callback rate for males is 10% (see Table 1), which means that compared to heterosexual males, they have 30% less chance to be called back. This confirms previous studies on the relationship between homosexuality and labor-market outcomes (see Section 3.1.2) where there is strong evidence of discrimination against gays in the hiring process. For example, Ahmed et al. (2011b) for Sweden, which also construct "fake" CVs, find that a heterosexual male applicant received 14% more responses from employers than a homosexual applicant (see also Weichselbaumer, 2003, for Austria, and Drydakis, 2011, for Greece).

[Insert Table 4 here]

When we instead look at differences in response rates by picture beauty, panel (b) shows a significant premium for attractive females of about 2% and no significance difference between handsome and ugly men. This is line with the beauty premium highlighted in Section 3.2. Panel (c) shows that these results remain unchanged when both our variables of interest (homosexuality and beauty) are included as regressors.

In Table 5, panel (a) we use the same specification as in Table 4 panel (c) and add as controls all the other characteristics that were collected in our experiment. They include type of secondary schools (for example, schools specialized in math or in literature, etc.), the years of experience of the candidate, whether he/she has college degree or not, and characteristics of the ads, namely if the ad was targeting a specific gender, if the ad was mentioning the equal opportunity (non discriminatory) act, if the picture was required, if a cover letter was required (if it was required we sent a cover letter), if the ad was posted by an employment agency or a directly by a company, if the knowledge of one or more foreign languages was required and if a computer knowledge was specified by the ad. The results remain unchanged. In Table 5, panel (b), we instead investigate possible non-linearities in the relationship between response rate and picture beauty by coding the beauty scores with a set of dummy variables capturing various beauty ranges. These ranges are those plotted in Figure 1. The main results remain mainly unchanged, i.e. attractive females are the group that seem to receive a premium whereas attractive males do not. However, these results show that this premium for females seem to be mainly driven by highly attractive women (in the extreme upper tail of the beauty score distribution) that appear to receive a premium of almost 8%.

[Insert Table 5 here]

We continue our analysis in Table 6 by using the same specification as in Table 5, panel (b) and add an interaction term between our dummy capturing homosexual preferences ("homosexual") and a dummy variable ("good CV") taking value 1 if the individual is depicted

in the résumé as having a college degree with excellent English proficiency and excellent computer skills and zero otherwise. Our aim is to investigate if the penalty associated with homosexual preferences is mitigated for high-skilled individuals. Interestingly, we find the opposite result. Indeed, Table 6 reveals that the penalty seems to be actually higher for high-skilled homosexual individuals, with an associated magnitude of more that 8% for homosexuals males. No penalty or premium is instead associated to high-skilled homosexual females, confirming that only males are penalized in the labor market for their homosexuality. This suggests that high-skilled homosexuals are more discriminated against than low-skilled homosexuals, although our experiments only considers jobs with low-skilled profiles.

[Insert Table 6 here]

In Table 7, we then investigate whether the beauty premium for women varies by skills. We find that high-skilled attractive women are called back less often than low-skilled attractive women. This may indicate that beauty might not be an advantage for high-skilled women.

[Insert Table 7 here]

A possible concern, however, can be that employers are discriminating between internships in cultural associations and in companies, as the control group includes identities reporting internships both in non gay/non lesbian cultural associations and companies. If employers prefer to employ people that had experience in companies rather than in cultural associations, then the penalty associated with pro-gay/lesbian cultural associations can simply capture the penalty associated with experience in a cultural association rather than in a company.

We investigate this issue in Table 8. In panel (a), we display the results when excluding individuals from the control group having internships in companies. Our target variable is thus now a dummy taking value 1 if the résumé reports an internship in a pro-gay/lesbian cultural association and 0 if the intership is a non pro-gay/lesbian cultural association. One can see that the results are virtually the same as those reported in Table 4, with even a slightly higher penalty for homosexual males (about 3.5% in Table 8 versus about 3% in Table 4). As a further robustness check, we investigate in Table 8, panel (b) if employers actually discriminate between internships in cultural associations and in companies. The results show that this is not the case. When excluding from our sample individuals with homosexual preferences and coding our target as 1 if the internship was in cultural associations and 0 if instead it was in a company, Table 8 shows no statistically differences in response rates between the two groups. This further robustness check increases our confidence in the estimated penalty of 3% for homesexuals in the labor market and the fact that employers tend to discriminate against homosexuals in the labor market.

[Insert Table 8 here]

5 Concluding remarks

In this paper, we contribute to the political debate on discrimination in Europe by considering unexplored dimensions of discrimination. For that, we conduct a field experiment in Rome and Milan and find that there is a strong penalty for homosexuals. Indeed, homosexuals have 30% less chance to be called back compared to an heterosexuals and even more so if they are highly skilled. On the contrary, no penalty exists for homosexual females. We also found a beauty premium for females only but this premium is much lower when the "pretty" woman is skilled.[10]

Different economic arguments can be considered to explain our results. Following Becker (1957), employers may dislike the lifestyle of gay men but not of lesbians and then act on this bias. Research in social psychology has indeed shown that the attitudes towards gay men are much more hostile than the attitudes towards lesbians (see, e.g. Kite and Whitley, 1996; Herek, 2000). The results could also be explained by an argument based on statistical discrimination (Arrow, 1973; Phelps 1972). For gay men, an often-mentioned reason for statistical discrimination is HIV/AIDS since this is often the source of negative attitudes towards gay men (Badgett, 2001; Elmslie and Tebaldi, 2007). Statistical discrimination may then occur if employers believe that HIV/AIDS infection decreases workers' productivity and increases absenteeism. In contrast to gay men, most people think that lesbians are more focused on their careers, not on husbands or children, and that they have a strong aggressive style (Peplau and Fingerhut 2004).

We can also apply the standard economic theories of discrimination (tasted base and statistical) to explain the negative impact on the labor-market outcomes of less-attractive individuals. Since previous experimental research indicates that beauty is not correlated with labor productivity (Mobius and Rosenblat, 2006), our finding may suggests the existence of labor market discrimination against the less attractive workers, especially women. The fact that high-skilled pretty women obtain less beauty premium than low-skilled pretty women may indicate the fear of competition with these women for certain types of jobs. Importantly, we show that the beauty results are particularly relevant for occupations requiring

[10] One could argue that what is relevant from an economic point of view is not whether gays are called back at lower rate, but whether they are hired with lower probability. This is a common shortcoming of fake CVs studies as they can—by design—only provide answers to the question whether certain groups are discriminated in call backs and not whether they are then actually discriminated in the hiring decision. Yet, we believe that discrimination in the call back decision is still discrimination as it limits opportunity. This is especially relevant here as the magnitude of the penalties we found are very large.

the interaction with customers as secretaries, receptionists and general customer service.

Finally, the sexual orientation and beauty results as gender differences may be driven by occupational segregation in some of the vacancy profiles targeted in the experiment.

All these issues are really complex and the mechanisms behind them difficult to identify. More work should be done, both from a theoretical and empirical viewpoint, and we leave that for future research.

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APPENDIX

Let y_i denote our variable of interest, that is, y_i denotes whether individual i has been recalled for an interview. Let D_i be an indicator denoting whether individual i is gay ($D_i = 1$) or not ($D_i = 0$) and X_i the beauty score index. Finally, Z_i is a vector of other variables such as education level, age, gender and other characteristics of the fictitious individual i.

The objective is to estimate whether y_i is affected by D_i and X_i. The starting point would be consider the effect of D_i and X_i on i independently. That is, considering

$$\Delta_D = \Pr(y_i|D_i = 0) - \Pr(y_i|D_i = 1) \tag{1}$$

and

$$\Delta_x(x_\delta) = \left.\frac{\partial \Pr(y_i|X_i = x)}{\partial x}\right|_{x=x_\delta}. \tag{2}$$

An alternative estimator is

$$\Delta_x = \int \Delta_x(u)dF(u),$$

where $F(u)$ is the probability function. Following this notation, Δ_D is the difference between the probability of being recall if the individual is gay and the probability of being recalled if the individual is not gay. A negative $\Delta_D < 0$ is indication of discrimination. Similarly, $\Delta_x(x_\delta)$ is the marginal effect of beauty on the probability of being called at $x = x_\delta$. Δ_x is the average marginal effect of beauty.

The quantity Δ_D can be estimated by running a OLS of y_i on D_i, that is,

$$y_i = \beta_0 + \beta_1 D_i + u_i. \tag{3}$$

and $\widehat{\Delta_D} = \hat{\beta}_1$. Under what conditions $\widehat{\Delta_D} \xrightarrow{p} \Delta_D$? The condition is that

$$Cov\,(D_i, u_i) = 0.$$

In other words, the gay assignment should be uncorrelated to all the other factors that are, in turn, correlated with the recall, that is u_i.

Similarly, $\Delta_x(x_\delta)$ and Δ_x, can be estimated by a probit model

$$\Pr(y_i|X_i = x) = \Phi(\gamma_0 + \gamma_1 X_i)$$

and

$$\widehat{\Delta_x}(x_\delta) = \phi(\hat{\gamma}_0 + \hat{\gamma}_1 x_\delta)\hat{\gamma}_1,$$

and

$$\widehat{\Delta_x} = \frac{1}{n}\sum_{i=1}^{n} \phi(\hat{\gamma}_0 + \hat{\gamma}_1 x_i)\hat{\gamma}_1.$$

Once again, the condition for consistently estimating $\Delta_x(x_\delta)$ (and/or Δ_x) is that X_i is independent of all the other factors that have an effect on the probability of recall. This can be done easily if instead of using a probit model we use a Linear Probability Model. In this case,

$$y_i = \xi_0 + \xi_1 X_i + u_i.$$

In this case, the OLS estimator of ξ_1 is consistent for Δ_x is $Cov(X_i, u_i) = 0$.

This is not our case though. First, D_i and X_i are both correlated with the *Age* structure. Furthermore, D_i and X_i are correlated with each other. Thus, if beauty has an effect on recall rates, then this will render inconsistent the estimate of the effect of gayness using regression (3).

The effect of the correlation between D_i and X_i and *age* and that of D_i with X_i can be easily fixed by including *age* and X_i in the regression. Specifically,

$$y_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + Z_i'\zeta + \eta_i.$$

Now, β_1 is the average effect of being gay, β_2 is the average effect of beauty on recall rates. These effects are easily estimated using OLS (they can also be estimated using probit/logit. This does not change much).

The fact that X_i and D_i and *age* are correlated is not a problem. Since they are observed we can control them out—so this is not a problem at all. What happens if we introduced correlation between D_i and unobservables? Since they are unobservables, we cannot control them out. The best answer is here the following: there are not many unobservable — since the CV are standard and we collect all the information on the CVs, the unobservables are really not present.

This first part basically shows that we have to (at least) run

$$y_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + Age_i'\zeta + \eta_i.$$

Should we include other variables? Yes. For two reasons. The first is that potentially other observables could correlated with D_i and X_i. Controlling for all the Z_i could correct other possible sources of bias. Second, even if the variables in Z_i are not correlated with u_i, as long as they influence y_i they reduce the variance of the estimators. The reason for this is that, in the regression

$$y_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + Z_i'\zeta + \eta_i.$$

the variance of $\hat{\beta}_1$ is, letting $W_i = (1, D_i, X_i, Z_i')$,

$$Var(\hat{\beta}_1) = \frac{\sigma_\eta^2}{n} \left(EW_i'W_i\right)^{-1}_{33}.$$

Let $G = (1, D_i, X_i, Age_i)$. The idea is that if the other variables in Z_i are uncorrelated with D_i and X_i then $(EW_i'W_i)^{-1}_{33} = (EG_i'G_i)^{-1}_{33}$, but the variance of the residuals with the larger set of regressors will be smaller, thus the variance of $\hat{\beta}_1$ will be smaller.

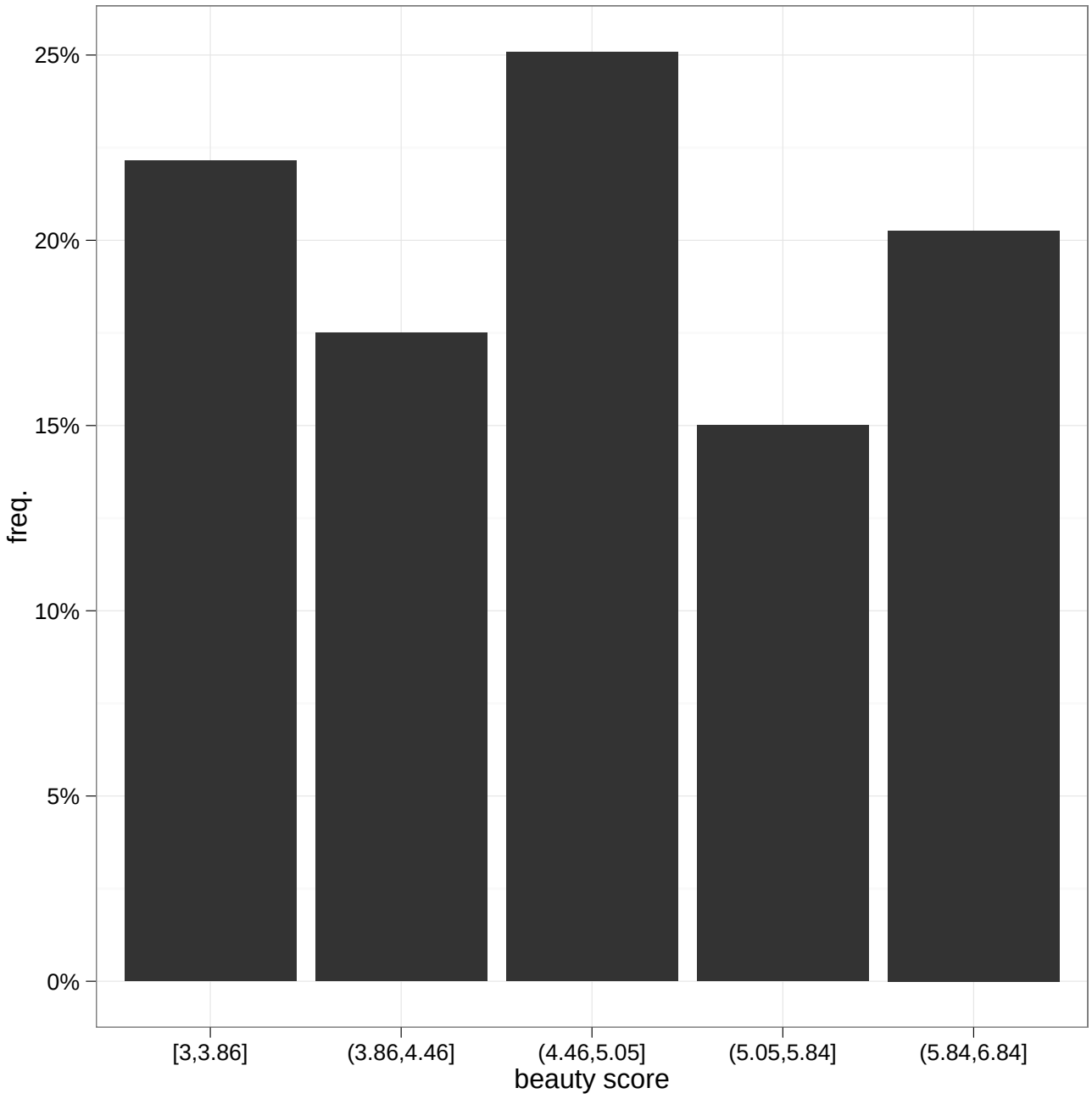

Figure 1: Distribution of beauty scores into intervals. Each interval contains approximately 20% of the sample of job applicants.

		All sample		Females		Males	
	call back	count	freq.	count	freq.	count	freq.
All	yes	256	**11.03%**	130	**11.24%**	126	**10.83%**
	no	2,064	88.97%	1,027	88.76%	1,037	89.17%
Rome	yes	135	**16.21%**	63	**14.96%**	72	**17.48%**
	no	698	83.79%	358	85.04%	340	82.52%
Milan	yes	121	**8.14%**	67	**9.10%**	54	**7.19%**
	no	1,366	91.85%	669	90.90%	697	92.81%

Table 1: Call back rates by city and gender.

			call back			
	yes	**no**	**no**	**yes**	**no**	**yes**
occupation	**All sample**		**Females**		**Males**	
administrative clerk	19	284	12	134	7	150
	6.27%	93.73%	8.22%	91.68%	4.46%	95.54%
bookkeeper	20	287	10	144	10	143
	6.51%	93.49%	6.49%	93.51%	6.54%	93.46%
call center	120	248	51	131	69	117
	32.61%	67.39%	28.02%	71.98%	37.10%	62.90%
receptionist	32	256	18	125	14	131
	11.11%	88.89%	12.59%	87.41%	9.66%	90.34%
sales clerk	39	320	24	166	15	154
	10.86%	89.14%	12.63%	87.37%	8.88%	91.12%
secretary	15	307	10	151	5	156
	4.66%	95.34%	6.21%	93.79%	3.11%	96.89%
shop assistant	11	362	5	176	6	186
	2.95%	97.05%	2.76%	97.24%	3.12%	96.88%

Table 2: Call back rates by type of job.

	Panel (a) Dep. var: homosexual			Panel (b) Dep. var: beauty score		
	All sample	**Females**	**Males**	**All sample**	**Females**	**Males**
beauty score	0.13***	0.12*	0.11*			
	(0.04)	(0.06)	(0.06)			
homosexual				0.12***	0.10*	0.10*
				(0.04)	(0.05)	(0.06)
bookkeeper	0.01	0.10	-0.08	0.06	-0.05	0.16
	(0.16)	(0.23)	(0.23)	(0.08)	(0.11)	(0.11)
call center	0.13	0.20	0.07	0.02	-0.03	0.08
	(0.16)	(0.22)	(0.22)	(0.07)	(0.10)	(0.11)
receptionist	0.06	0.14	-0.01	-0.06	-0.08	-0.01
	(0.17)	(0.23)	(0.24)	(0.08)	(0.10)	(0.12)
sales clerk	0.03	0.19	-0.15	-0.01	0.02	-0.05
	(0.16)	(0.22)	(0.22)	(0.07)	(0.10)	(0.11)
secretary	0.03	0.10	-0.06	-0.02	-0.14	0.08
	(0.16)	(0.23)	(0.23)	(0.08)	(0.10)	(0.11)
shop assistant	0.00	0.06	-0.07	0.07	0.02	0.10
	(0.16)	(0.22)	(0.23)	(0.07)	(0.10)	(0.11)
rome	0.06	0.10	0.02	0.00	-0.03	0.03
	(0.09)	(0.12)	(0.13)	(0.04)	(0.06)	(0.06)
age	0.02**	0.01	0.04***	-0.05***	-0.08***	-0.02***
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
degree	0.14	0.19	0.13	0.10*	0.16*	0.06
	(0.12)	(0.17)	(0.17)	(0.06)	(0.08)	(0.08)
sex	-0.12			0.69***		
	(0.90)			(0.03)		
Constant	-1.44***	-1.09*	-1.81***	5.73***	6.53***	5.63***
	(0.39)	(0.59)	(0.54)	(0.14)	(0.18)	(0.20)
N	2320	1163	1157	2320	1163	1157

Table 3: Panel (a) reports Logit regression results, where the dependent variable is "homosexual". Panel (b) reports OLS regression results, where the dependent variable is "beauty score".

Dependent:	**Panel (a)**			**Panel (b)**			**Panel (c)**		
call back	**All sample**	**Females**	**Males**	**All sample**	**Females**	**Males**	**All sample**	**Females**	**Males**
homosexual	-0.008	0.015	-0.031*				-0.009	0.014	-0.030*
	(0.012)	(0.018)	(0.017)				(0.012)	(0.018)	(0.017)
beauty score				0.005	0.018**	-0.008	0.005	0.018*	-0.007
				(0.006)	(0.009)	(0.009)	(0.006)	(0.009)	(0.009)
bookkeeper	0.004	-0.014	0.021	0.004	-0.018	0.020	0.004	-0.018	0.021
	(0.020)	(0.030)	(0.025)	(0.020)	(0.030)	(0.025)	(0.020)	(0.030)	(0.025)
call center	0.255***	0.192***	0.316***	0.255***	0.190***	0.314***	0.255***	0.190***	0.315***
	(0.028)	(0.041)	(0.038)	(0.028)	(0.040)	(0.038)	(0.028)	(0.040)	(0.038)
receptionist	0.051**	0.047	0.054*	0.051**	0.047	0.053*	0.051**	0.046	0.054*
	(0.023)	(0.036)	(0.029)	(0.023)	(0.036)	(0.029)	(0.023)	(0.036)	(0.029)
sales clerk	0.042*	0.043	0.038	0.042*	0.042	0.036	0.042*	0.043	0.038
	(0.022)	(0.034)	(0.028)	(0.022)	(0.034)	(0.028)	(0.022)	(0.034)	(0.028)
secretary	-0.020	-0.020	-0.023	-0.020	-0.022	-0.025	-0.020	-0.022	-0.024
	(0.018)	(0.030)	(0.022)	(0.018)	(0.030)	(0.021)	(0.018)	(0.030)	(0.022)
shop assistant	-0.040**	-0.058**	-0.024	-0.040**	-0.060**	-0.024	-0.040**	-0.060**	-0.024
	(0.017)	(0.026)	(0.021)	(0.017)	(0.026)	(0.021)	(0.017)	(0.026)	(0.021)
age	0.039**	0.028	0.044	0.038*	0.023	0.038	0.039**	0.020	0.040
	(0.020)	(0.029)	(0.027)	(0.020)	(0.029)	(0.027)	(0.020)	(0.029)	(0.027)
age2	-0.001**	-0.001	-0.001*	-0.001**	0.000	-0.001	-0.001**	0.000	-0.001
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
rome	0.066***	0.048**	0.085***	0.066***	0.048**	0.084***	0.066***	0.048**	0.085***
	(0.014)	(0.020)	(0.020)	(0.014)	(0.020)	(0.020)	(0.014)	(0.020)	(0.020)
Constant	-0.443*	-0.305	-0.505	-0.461*	-0.330	-0.392	-0.475*	-0.293	-0.408
	(0.267)	(0.394)	(0.368)	(0.269)	(0.391)	(0.377)	(0.269)	(0.394)	(0.376)
R^2	0.11	0.07	0.17	0.11	0.07	0.16	0.11	0.07	0.17
adj. R^2	0.11	0.06	0.16	0.11	0.07	0.16	0.11	0.07	0.16
N	2320	1157	1163	2320	1157	1163	2320	1157	1163

Table 4: OLS regression results. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	Panel (a)			Panel (b)		
call back	**All sample**	**Females**	**Males**	**All sample**	**Females**	**Males**
homosexual	-0.008	0.014	-0.028*	-0.008	0.014	-0.029*
	(0.012)	(0.018)	(0.017)	(0.012)	(0.019)	(0.017)
beauty score	0.007	0.019**	-0.006			
	(0.006)	(0.009)	(0.009)			
beauty score ∈ (3.86,4.46]				0.019	0.043	0.016
				(0.019)	(0.033)	(0.025)
beauty score ∈(4.46,5.05]				0.027	0.052*	0.017
				(0.018)	(0.030)	(0.023)
beauty score ∈(5.05,5.84]				0.008	0.036	-0.009
				(0.019)	(0.029)	(0.034)
beauty score ∈(5.84,6.84]				0.021	0.077**	-0.036
				(0.020)	(0.032)	(0.026)
bookkeeper	0.012	-0.008	0.028	0.013	-0.007	0.029
	(0.020)	(0.031)	(0.026)	(0.020)	(0.031)	(0.027)
call center	0.256***	0.193***	0.317***	0.255***	0.192***	0.318***
	(0.030)	(0.045)	(0.040)	(0.030)	(0.045)	(0.040)
receptionist	0.040	0.037	0.045	0.040	0.038	0.046
	(0.026)	(0.041)	(0.035)	(0.026)	(0.041)	(0.035)
sales clerk	0.050**	0.049	0.051	0.049**	0.047	0.049
	(0.024)	(0.036)	(0.031)	(0.024)	(0.037)	(0.031)
secretary	-0.010	-0.020	-0.002	-0.010	-0.020	-0.001
	(0.021)	(0.034)	(0.027)	(0.021)	(0.034)	(0.027)
shop assistant	-0.041**	-0.069**	-0.014	-0.041**	-0.069**	-0.015
	(0.019)	(0.030)	(0.025)	(0.019)	(0.031)	(0.025)
age	0.069**	0.038	0.077**	0.072**	0.038	0.078**
	(0.028)	(0.041)	(0.038)	(0.028)	(0.040)	(0.038)
age2	-0.001**	-0.001	-0.001**	-0.001***	-0.001	-0.001**
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
rome	0.052***	0.040*	0.062***	0.052***	0.040*	0.062***
	(0.015)	(0.022)	(0.021)	(0.015)	(0.022)	(0.021)
Constant	-0.943**	-0.574	-0.959	-0.963**	-0.511	-1.006*
	(0.451)	(0.641)	(0.609)	(0.446)	(0.631)	(0.607)
R^2	0.13	0.10	0.20	0.13	0.10	0.20
adj. R^2	0.12	0.07	0.17	0.12	0.07	0.18
N	2320	1157	1163	2320	1157	1163
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes

Table 5: Panel (a) shows the OLS regression results of the same specification in Table 4 Panel c) plus additional controls describing the applicant's characteristics and the requirements of the job posting. Control of the first kind are: years of working experience, dummies for type of secondary school attended, dummies for foreign languages proficiency, and dummies for different types of computer skills. Controls of the second kind are dummies for: whether the posting required a picture and/or a cover letter, and whether the posting targeted applicants of a given gender and/or xs good looking. In Panel (b), besides including these additional controls, we code beauty score using the quantiles of its empirical distribution. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Dep. var: call back	**All sample**	**Females**	Males
homosexual	-0.001	0.020	-0.019
	(0.013)	(0.020)	(0.018)
homosexual × good cv	-0.059*	-0.057	-0.081*
	(0.036)	(0.056)	(0.047)
good cv	0.044	0.073	0.012
	(0.034)	(0.050)	(0.048)
beauty ∈ (3.86,4.46]	0.019	0.046	0.015
	(0.019)	(0.033)	(0.025)
beauty ∈ (4.46,5.05]	0.027	0.052*	0.016
	(0.018)	(0.030)	(0.023)
beauty ∈ (5.05,5.84]	0.008	0.036	-0.008
	(0.019)	(0.029)	(0.035)
beauty ∈ (5.84,6.84]	0.021	0.078**	-0.037
	(0.020)	(0.032)	(0.026)
bookkeeper	0.012	-0.009	0.029
	(0.020)	(0.031)	(0.027)
call center	0.256***	0.191***	0.319***
	(0.030)	(0.045)	(0.040)
receptionist	0.040	0.039	0.046
	(0.026)	(0.042)	(0.035)
sales clerk	0.050**	0.046	0.050
	(0.024)	(0.037)	(0.031)
secretary	-0.009	-0.020	0.000
	(0.021)	(0.034)	(0.026)
shop assistant	-0.041**	-0.069**	-0.015
	(0.019)	(0.031)	(0.025)
age	0.073**	0.037	0.079**
	(0.028)	(0.040)	(0.038)
age^2	-0.001***	-0.001	-0.001**
	(0.000)	(0.001)	(0.000)
rome	0.052***	0.039*	0.061***
	(0.015)	(0.022)	(0.021)
Constant	-0.980**	-0.506	-1.022*
	(0.447)	(0.631)	(0.611)
R^2	0.13	0.10	0.20
adj. R^2	0.12	0.07	0.18
N	2320	1157	1163
Additional controls	Yes	Yes	Yes

Table 6: OLS estimation results. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Dep. var: call back	**All sample**	**Females**	Males
beauty	0.01	0.03***	0.00
	(0.01)	(0.01)	(0.01)
beauty × good cv	-0.03**	-0.08***	-0.02
	(0.02)	(0.03)	(0.02)
good cv	0.16*	0.42**	0.09
	(0.09)	(0.17)	(0.11)
homosexual	-0.01	0.01	-0.03*
	(0.01)	(0.02)	(0.02)
bookkeeper	0.01	-0.01	0.03
	(0.02)	(0.03)	(0.03)
call center	0.25***	0.18***	0.31***
	(0.03)	(0.04)	(0.04)
receptionist	0.04	0.03	0.04
	(0.03)	(0.04)	(0.03)
sales clerk	0.04*	0.05	0.04
	(0.02)	(0.04)	(0.03)
secretary	-0.02	-0.03	-0.01
	(0.02)	(0.03)	(0.02)
shop assistant	-0.05**	-0.08***	-0.02
	(0.02)	(0.03)	(0.02)
age	0.03	0.03	0.04
	(0.02)	(0.04)	(0.03)
age2	0.00**	0.00	0.00*
	(0.00)	(0.00)	(0.00)
rome	0.06***	0.04**	0.08***
	(0.01)	(0.02)	(0.02)
Constant	-0.30	-0.49	-0.34
	(0.28)	(0.63)	(0.40)
R^2	0.12	0.10	0.18
adj. R^2	0.12	0.08	0.16
N	2320	1157	1163

Table 7: OLS estimation results. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Dep. var:	**Gay/Lesbians vs general associations**			**General association vs companies**		
call back	**All sample**	**Females**	**Males**	**All sample**	**Females**	**Males**
homosexual	-0.009	0.015	-0.035*	-0.003	-0.009	0.001
	(0.015)	(0.021)	(0.021)	(0.017)	(0.024)	(0.024)
beauty score	0.009	0.028**	-0.016	-0.001	0.008	-0.009
	(0.007)	(0.011)	(0.010)	(0.008)	(0.012)	(0.014)
bookkeeper	0.003	-0.002	0.000	0.037	0.006	0.074*
	(0.024)	(0.039)	(0.029)	(0.025)	(0.034)	(0.038)
call center	0.236***	0.192***	0.281***	0.307***	0.237***	0.393***
	(0.034)	(0.051)	(0.047)	(0.040)	(0.056)	(0.055)
receptionist	0.031	0.027	0.035	0.083**	0.098*	0.096**
	(0.031)	(0.050)	(0.040)	(0.034)	(0.053)	(0.047)
sales clerk	0.048*	0.034	0.062*	0.052*	0.055	0.057
	(0.028)	(0.043)	(0.037)	(0.029)	(0.043)	(0.040)
secretary	-0.016	-0.032	-0.002	0.020	0.027	0.027
	(0.025)	(0.039)	(0.031)	(0.026)	(0.042)	(0.032)
shop assistant	-0.044*	-0.091**	-0.004	-0.018	-0.031	0.009
	(0.023)	(0.035)	(0.031)	(0.024)	(0.039)	(0.030)
age	0.096***	0.078	0.091**	0.042	0.026	0.039
	(0.034)	(0.048)	(0.046)	(0.038)	(0.055)	(0.052)
age2	-0.001***	-0.001	-0.001**	-0.001	-0.001	0.000
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
rome	0.046**	0.042	0.046*	0.063***	0.044	0.081***
	(0.018)	(0.027)	(0.024)	(0.020)	(0.027)	(0.030)
Constant	-1.379***	-1.297*	-1.111	-0.493	-0.260	-0.475
	(0.533)	(0.779)	(0.712)	(0.598)	(0.842)	(0.843)
R^2	0.12	0.11	0.18	0.15	0.13	0.23
adj. R^2	0.11	0.07	0.15	0.13	0.08	0.20
N	1684	840	844	1272	640	632

Table 8: OLS regression results. The first panel displays the results when excluding individuals from the control group having internships in companies. The second panel excludes instead individuals having internship in pro-gay/lesbian cultural association. Although not reported, all specifications include as regressors: age, age^2, dummies for type of job, and city dummy. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1