Do anonymous resumes make the field more even? Evidence from a randomized field experiment

Luc Behaghel[*], Bruno Crépon[†] and Thomas Le Barbanchon[‡]

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Abstract

This paper provides experimental evidence on the impact of anonymous resumes in France. First, women do benefit from higher callback rates under the anonymous resume procedures; however, the effect is somewhat limited by the extent of labor market segmentation, as half of the job offers in the experimental sample have male applicants, or female applicants only. Second, however, applicants from foreign background or residents in deprived neighborhoods witness a decrease in their relative chances to be interviewed. Third, we find evidence suggesting that anonymous resumes counter homophily in the hiring process: they undo the tendency of female recruiters to select female applicants, and of male recruiters to select male applicants. Interestingly, this effect persists at later stages of the hiring process, so that anonymous resumes in effect equalize the chances of applicants of both genders to be interviewed and finally hired, irrespective of the gender of the recruiter.

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[*] Paris School of Economics - Inra and Crest

[†] Crest

[‡] Crest

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1 Introduction

Differential treatment by race and gender are repeatedly documented as a prominent feature in many labor markets, despite decades of anti-discrimination laws that explicitly prohibit and strongly penalize such firm behavior.[1] This persistence has sparked the debate on alternative (or complementary), non coercitive policies against discrimination. Anonymous referral procedures have received lots of attention. Anonymous resumes are used in Belgium in the federal administration. They have been tested locally in the Netherlands, Sweden and in Switzerland, but the tests have not led to new legislation. In Great Britain, an attempt by Liberal Democrats to impose anonymous resumes for recruitment has been opposed by the government in 2009. In France, which we study here, a law was passed in 2006 that made the use of anonymous resumes mandatory for firms with more than 50 employees; yet, the government did not take the steps to define the conditions under which the law would apply.

In the absence of strong empirical evidence, the confusion and hot debates around anonymous resumes are understandable: there are strong, plausible pros and cons. The theory of statistical discrimination suggests that changing the information set of employers at the beginning of the hiring process may have a strong impact on final decisions, *if* skilled minority applicants who are the victims of the negative signal attached to their group are able to overcome that signal at the interview stage by demonstrating their credentials. Similarly, anonymous resumes may be effective against taste discrimination *if* meeting with the applicant induces the recruiter to overcome his prejudiced views against a group, or simply to give them less weight once they know the individual characteristics of the applicant better. Clearly, little is known on whether these conditions hold in practice. While proponents and opponents of anonymous resumes usually agree that the measure should change the pool of candidates called for an interview, they hold opposite views on whether this change would be sufficient to overcome discrimination in later stages of the hiring process, once the identity of the applicant is revealed to the employer. Additional arguments in the debate concern the cost of the measure: by removing information, the measure can be viewed as increasing matching frictions on the labor market, with ultimately negative welfare impacts on firms and workers.

This paper provides experimental evidence on the impact of anonymous resumes. The

[1] Correspondence testing studies, initially primarily developed in the UK, have been instrumental in providing direct and compelling evidence, even though the exact interpretation in terms of discrimination is still debated (see...). A notable example for race in the US is Bertrand and Mullainathan (2004). Correspondence studies have been increasingly used in France, the country under study here: see in particular Duguet, L'Horty, Meurs and Petit (2010) introducing the special issue of *Annals of Economics and Statistics*, n°99/100, on measuring discrimination.

experiment was implemented in 2010-2011 by the French Public employment service (PES) to help the French government decide on the enforcement conditions of the law passed in 2006. It was felt that a randomized experiment was needed to provide simple and transparent evidence. Firms posting job offers at the PES were asked to participate to an experiment in which they would have one chance out of two to receive anonymous resumes preselected by the PES agents, rather than standard ones. The experiment involved about 1,000 firms in eight local labor markets, and lasted 10 months. Although the experiment was initially designed to mimic the situation that would prevail if the law was finally enforced, compromises had to be struck given the government's reluctance to impose anything on firms at that stage. In particular, participation to the experiment was not mandatory. This is however the first time, to our knowledge, that experimental evidence is brought on the effectiveness of anonymous referral procedures, rather than on the mere existence of discrimination.

The main findings are the following. First, and as expected, women do benefit from higher callback rates under the anonymous resumes procedures; however, the effect is somewhat limited by the extent of labor market segmentation, as half of the job offers in the experimental sample have male applicants, or female applicants only. Second, and in a much less expected way, applicants from foreign background or residents in deprived neighborhoods witness a *decrease* in their relative chances to be interviewed, as compared to the reference group. Third, we find evidence that anonymous resumes counter homophily in the hiring process: they undo the tendency of female recruiters to select female applicants, and of male recruiters to select male applicants. Interestingly, this effect persists at later stages of the hiring process, so that anonymous resumes in effect equalize the chances of applicants of both genders to be interviewed and finally hired, irrespective of the gender of the recruiter. Last, we do not find any evidence that the anonymous procedures increases the firms' direct hiring costs nor the opportunity cost of vacancies.

We also document the representativeness of the sample of firms entering the experiment. Although differences in terms of observable characteristics seem minor, there are indications that firms who accepted the experiment were initially rather favorable to applicants from foreign background or residents in deprived neighborhood. This provides a plausible interpretation to the counter-intuitive impact of anonymous resumes on that group: these self-selected firms may practice "reverse discrimination" (possibly motivated by many reasons, including the possibility to pay minority workers lower wages), and anonymous resumes may prevent that practice. Interestingly, there is no evidence of selection at entry along lines of gender discrimination. Less visible in the French debate, gender discrimination may well be a relevant target for anonymous resumes.

The next section relates our evaluation to the relevant literature. The following sections present the experiment, the data, and the measures used to characterize groups at risk of discrimination. A specific section is devoted to analyzing the representativeness of firms participating to the experiments. The last two sections present the results from the perspective of applicants and firms, respectively.

2 Previous literature

Despite the well-documented widespread discrimination in hiring and the political will to fight against it, there are relatively few evaluations of anonymization during the recruitment process. To our knowledge, two notable exceptions are Goldin and Rouse (2000) and Aslund and Nordstrom Skans (2007). In both evaluations, the introduction of anonymization during the recruitment process is found to increase the hiring rates of women relative to men.

Goldin and Rouse (2000) analyze the introduction of shields in hiring auditions of American Philharmonic orchestras. They identify the effect of shield adoption in a difference and difference framework, assuming that shield adoption is not simultaneous to any changes in other anti-discriminatory practices in the orchestras. Thanks to randomization our evaluation does not rely on such assumptions. They find that women have a higher probability to advance to later stages of the recruitment process when shields are used. Moreover, even if later stages are not anonymous any more, women have a higher probability to be hired if first stages are blind. One possible interpretation of their findings is that, knowing that auditions are blind, more talented women applied to the job opening. In other words, adopting a blind recruitment process sends signals to potential candidates who self select out of the market when the process is nominative. Blind auditions not only change the information set of recruiters in early stage of the recruitment process, but it could also change the composition of the pool of candidates applying. Our experimental design mitigates this 'calling' effect and enables us to estimate the pure information effect.

Aslund and Skans (2007) evaluate the effects of anonymous application forms introduced in the recruitment of 109 public jobs. Those jobs were advertised as anonymous and candidates had to follow a specific application procedure. The evaluation may also estimate the 'calling' effect. The anonymous applications were experimented in two voluntary districts of Goteborg city in 2004-2006. As Goldin and Rouse, they use a difference in difference framework and find that the probability of being interviewed and hired are equalized between male and female candidates when applications are anonymous. They also find that the interview rate is leveled between candidates with foreign origin and natives, but the hiring rate of natives is still higher under anonymous applications. This is first evidence that the efficiency of anonymous procedure is heterogenous, evidence which we also confirm.

Among the wide literature on discrimination, our article contributes to another empirical strand which focuses on what can be called "homophily" or own-group bias. Behind this concept is the simple idea that human beings tend to prefer to interact with people from the same ethnic group, the same gender... This behavior can reveal a true preference, in this case homophily can be associated to taste-based discrimination, or it can be simply rational : obtaining relevant information - extracting a signal - is easier from someone of the same ethnic/gender group (statistical discrimination). Price and Wolfers (2010) find that more personal fouls are awarded against players when they are officiated by an opposite-race officiating crew than when officiated by an own-race refereeing crew. Anwar and Fang (2006) find that troopers from different races are not monolithic in their search behavior. However the authors do not reject the hypothesis that troopers of different races do not exhibit relative racial prejudice. We contribute to this empirical literature by documenting such bias on the labor market and by extending the usual own-ethnic bias analysis to own-gender bias.

3 Experimental design

In this section, we present the experimental design used to measure the impact of anonymous resumes. The experiment was conducted in 8 (out of one hundred) French *départements*, at branches of the public employment service (PES) located in urban areas, during 10 months. It proceeded as follows:

1. **Firm entry in the experiment.** Firms posting job offers at the PES have the option to ask for a PES agent to make a first screening of applicants based on their resume. In that case, the firm receives only selected resumes from the PES (from a couple to a dozen, in most cases), instead of having applicants contact them directly. This service is free. During the time of the experiment, all firms with more than 50 employees posting a job lasting at least 3 months and asking for this service are invited to enter the experiment. They are told that their job offer will be randomly

assigned to the anonymous or standard procedure, with probability 1/2. Firms are free to refuse; however, in order to induce positive responses, participation is presented as the default option. A given plant enters the experiment at most once: plants that have already entered the experiment are no longer asked to participate.

2. **Matching of resumes with job offers.** The job offer is posted by the PES on a variety of supports, including a public website on which interested job seekers are asked to apply through the PES branch. The PES agent selects resumes from these applicants and from internal databases of job seekers. A first lot of resumes is thus matched with the job offer.
3. **Randomization and anonymization.** The resumes are sent to research assistants in charge of the randomization at the central PES offices. Job offers (and their first lot of resumes) are randomly assigned (using a random number generator) to treatment or to control group, with probability 1/2. If the offer is assigned to the treatment group, all the resumes are given a number and anonymized by the research assistant[2]; then, they are sent back to the PES agent in charge of the job offer follow-up.
4. **Selection of resumes by the employer.** The employer selects the resumes of applicants she[3] would like to interview. Control group employers contact the applicants directly, treatment group employers give the PES agent the resumes' numbers so that it is the PES agent who sets up the hiring interviews, in order to maintain the applicant anonymity.
5. **Additional lots of resumes.** If the employer could not fill the position with the first lot of resumes, she requests additional lots. The PES sends a new lot of selected resumes with the same format as for the first lot.

This experimental design calls for a few comments:

Plants enter the experiment at most once, either in the treatment or in the control group. The main reason was the fear of the PES that repeated participation to the experiment and the corresponding surveys would have been too much trouble for firms. To maximize positive responses when inviting the firm to participate in the experiment, it was therefore clearly specified that the experiment would only concern one job opening. It could also be argued that having the same hiring officer acting in turn as treatment and as a control individual would have made the results harder to interpret, as this would have increased the risk that her behavior be affected by her previous participation. Possible Hawthorne or Henry effects are discussed in the results section. A drawback of this is

[2]The degree of anonymization is described below.

[3]As shown below, most hiring officers in the experiment are females; we will therefore use feminine pronouns.

that the experiment does not capture learning effects nor the long-term impact of using anonymous resumes.

Anonymization is limited. Anonymization consisted in erasing the top part of the resume: name, address, gender, ID picture, age, marital status and number of children. However, it did not imply any further standardization of the content of the resume. In particular, information on gender could be read from gender-specific terms used in the main part of the CV; neighborhood of residence could be partly inferred from information on where the applicant graduated from high-school; and ethnicity could be spotted from foreign language skills. Going further would have implied much more complex logistics during the experiment, and it was felt that standardization would anyway not have been feasible if anonymous resumes had been made mandatory nationwide.

Randomization occurs at the job offer level. For a given job offer, all resumes transiting by the PES are treated identically (either anonymous, or standard). This level of randomization corresponds to the policy evaluated, that would have all resumes anonymized, instead of some anonymous resumes competing with standard resumes. However, the PES is not the only channel for recruitment: firms may also receive applicants from other sources, whose resumes are not anonymized. We measure below whether firms substitute these other channels to the PES in response to anonymization.

Randomization occurs after matching resumes to job offers. Had the randomization occurred after randomization, the PES agent could have selected different applicants for job offers with anonymous resumes (consciously or not). This would have affected the comparability of treatment and control applicants. To avoid this, a first lot of resume was selected before randomization occurred. Most analyses below are restricted to these first lots, as they contain resumes that are by construction statistically identical in the control and treatment group. We check below whether resumes in subsequent lots sent by the PES agent differ from the first lots.

To summarize, the goal of this experimental design is to mimic as closely as possible what making anonymous resumes mandatory would change for recruitment. By contrast with a law that would have anonymous resumes mandatory, there are however two main caveats: first, only a fraction of the targeted job offers entered the experiment, as the experiment was run in specific urban areas and employers were allowed to opt out; second, only applicants transiting by the PES were concerned, as the firms could keep using their other (non anonymous) recruitment channels. This results from the constraints set by policy makers when launching the experiment, despite our attempts to make participation to the experiment compulsory for all firms using the PES. As detailed below, the data collection strategy was adapted to measure the consequences of these features of the design.

During the ten months of the experiment (November 2009 to September 2010), 1,005 job offers entered the experiment out of total of a bit more than 6,000 eligible offers (each plant counting for one offer). This limited entry into the experiment is due to losses at two steps. First, using administrative data on all job offers posted at the PES, one can check that only 25.5% of the eligible employers were invited to enter the experiment. It should be noted that the experiment took place at a time when inflows of job seekers were extremely large due to the recession, so that PES agents were extremely busy and some of them simply forgot or neglected to invite firms to participate. However, it is also likely that some PES agents preferred not to invite firms that they expected would refuse. Among firms invited to enter the experiment, the take-up rate amounts to 63.3%. Clearly, although only 37.7% of firms formally declined to participate, the representativeness of the experimental sample is an issue, and it is analyzed in depth in section 6.

4 Data

We collect administrative and survey data.[4] The administrative data covers all firms and all job seekers who used the public employment services in the experimental areas during the experiment. It has basic information on the firm (size, sector), the job position offered (occupation level, type of contract), limited information on the job seeker (unless the job seeker has a file as unemployed). It also provides a follow-up of the recruitment process until the position is filled or the job offer is withdrawn; however, the quality of that follow-up is weak, and some critical information is missing (in particular, one does not know whether the candidate was interviewed before the firm rejected his application). In what follows, the administrative data is mostly used to characterize the population of firms entering the experiment, by comparison with the broader population of firms interacting with the PES.

We conducted telephone interviews with all firms entering the experiment, as well as with a subsample of applicants to these firms. The data from these two surveys constitute the core database used in the analysis. In addition, we interviewed a sample of firms that had refused to enter the experiment or that had not been invited by PES agents, despite the fact they were eligible for the experiment: again, the goal is to check whether our core sample is representative of the target population of firms. Last, a subsample of applicants

[4] In addition to these two main sources, information available in the resumes was also coded.

on job offers from control group firms *after the experiment* was also interviewed: as detailed below, the goal is to check whether control firms behaved in a specific way during the experiment. The surveys used for applicants (respectively, for firms) were similar across subsamples. We now present these two surveys briefly; specific questions will be presented when they are used in the analysis, and survey tools (in French) are available on line.[5]

The main goal of the survey of applicants is to provide a reliable measure of whether the applicant was interviewed for the job, and of all his characteristics that could lead to discrimination. We ask in particular for the country of birth and the citizenship at birth, both for the applicant and his parents. There are also questions about the applicant's labor market situation, the recruitment process, as well as subjective questions on self-confidence, perceived discrimination, and perceived labor market prospects.

The firm survey has three main functions. The first one is to measure the result of the recruitment, in particular when the recruitment was abandoned without filling the position, or when the hired candidate came from other channel than the PES (in which case he would not be present in the survey of applicants). Second, the survey includes detailed questions on the hiring process: what were the different steps, how formalized were they, how much time was spent on each of them, who was involved within the firm. Last, the survey tries to characterize the background of the hiring officer who led the hiring process. Just like the applicant, we ask for her country of birth and citizenship at birth, as well as her parents'.[6] In addition, to characterize her social networks and the firm's social composition, we ask for the first names of five friends outside the firm and five colleagues within the firm.

Table 1 details the sample of applicants. The initial population (6742 applicants) is partitioned in two ways: control vs. test; at risk of discrimination vs. other. At that stage, applicants at risk of discrimination are identified from the administrative information as those living in a deprived neighborhood or with an African or Muslim-sounding name. They are given higher sampling weights, in order to maximize statistical power. Overall, response rates are around 65-70%; even though they are lower in the control group, the difference is not statistically significant (the p-value is .27). The survey thus yiels a total sample of 1977 applicants. Among those, 1260 belong to the first lot of resumes matched to a job offer before the randomization took place. As discussed in section 3, these 1260 applicants constitute the cleanest comparison groups; unless otherwise specified, they constitute the sample of analysis.

[5] **Bruno, Thomas, êtes-vous OK pour mettre ces documents en ligne sur la page du projet?**

[6] Special care was devoted to survey the person in charge of the recruitment. All respondents to the firm survey reported being in charge of the selection of resumes; 89% took part to job interviews.

Table 2 presents the sample of firms. There are five separate groups of firms. 385 control and 366 treatment firms accepted the experiment and went through the randomization. 254 firms accepted the experiment but were not randomly assigned to treatment or control: they canceled or filled the job opening before a first lot of resumes was collected and randomization could take place. This underscores the fact that many firms actually fill their positions quickly without any help of the PES. 608 firms refused the experiment, and 4714 were not invited to participate. These last two groups of firms were sampled with lower sampling rates. Their response rates are also somewhat lower, as could be expected. The response rate difference between control and treatment firms is not statistically significant.

5 Measuring applicants' risk of discrimination

The purpose of anonymous resumes is to protect potential victims of discrimination by hiding characteristics that would allow firms to screen them before the interview. Discrimination can however occur along many dimensions: ethnicity and foreign origin, neighborhood of residence, gender, age. This section details how we measure these different dimensions.

Gender, age and neighborhood of residence are available in the administrative data; they are also directly reported on the resume. One issue with age is that it can be inferred fairly easily from the content of the resume (in particular, the year the applicant entered the labor market or finished her education): on the basis of this information, it is possible to predict the applicant's age within a four-year bracket in 60% of applications. Removing the exact age could therefore only matter in so far as employers attach a particular signficance to some age thresholds, for instance, the age of 50. We use the corresponding indicator variable in the analysis, but find little impact on the effect of anonymous resumes. We will therefore not focus on age in the analyses that follow.

Another issue is how to characterize deprived neighborhoods of residence. In the US, Bertrand and Mullainathan (2004) use a variety of criteria based on the fraction of Whites, the fraction of college graduates or the average per capita income. We use administrative classifications of neighborhood defined to target subsidies or tax exemptions[7]: their boundaries closely match socioeconomic geographical disparities; moreover, one of their alleged perverse effect is to create a stigma effect. They are therefore particularly relevant to assess the impact of anonymization.

[7]They are known as "Zones urbaines sensibles" (ZUS) and "quartiers en contrat urbain de cohésion sociale (CUCS)"; these zoning schemes are comparable to "Enterprise zones" in the US.

The main issue is how to measure discrimination risk associated with foreign origin or ethnicity. French law forbids the use of ethnic categories that would label someone as White, Black, or African-French, for instance. Instead, we follow a twofold approach. First, in the spirit of correspondence testing studies (see Bertrand and Mullainathan, 2004), we code whether the applicants' names has a foreign-sounding origin. Following research by Felouzis (2003) and Ores (2007), we use the etymology of the applicant's name: Muslim first names are identified from a database created by Chebira (2005). The second approach uses the place of birth and the citizenship at birth. Immigrants are defined as those born abroad who did not have French citizenship at birth. Children of immigrants are those whose father was born abroad and did not have French citizenship at birth. Specific questions are used for the special case of individuals from former French colonies, who might declare they were French citizens at birth if they were born before independence; they are classified as foreigners if they took the citizenship of their new country at independence. The two approaches – based upon the first name or the migration status – are complementary. In some cases, a foreign-sounding name is the only signal that appears on the resume. But in other cases, immigrants may have a French-sounding name although their origin can be inferred from other signals on the resume (for instance, their last name or an ID picture).[8]

Table 3 compares the different measures of discrimination risk. The sample is balanced between men and women; it is clearly skewed toward young candidates. Roughly one applicant out four lives in a deprived neighborhood; the same proportion has a Muslim or African-sounding name; one out of five is immigrant, and that proportion goes to four out of ten for immigrants or children of immigrants. The different measures of origin are correlated. Of particular interest is the correlation between the name and the migration status, shown in table 4: clearly, African or Muslim-sounding names correspond to applicants with a foreign origin; however, a significant fraction of immigrants (including those from Africa) do not have an African or Muslim-sounding name. The variables based on immigration (as declared during the interviews) may better capture the risk of discrimination, when that origin can be inferred from other signals in the resume. In the analysis, we compare the effects of using these alternative measures.

Table 6 shows no significant observable differences between control and treatment applicants, in the first lot of resumes (selected by the PES before randomization).[9]

[8] Alternative measures of origin include the applicant's patronyme and his mother tongue. Measures using the applicant's patronyme were hard to implement and did not seem, by cursory look at the resumes, to improve on the information yielded by the first name and the migration status. Moreover, in the French context, the mother tongue does not allow to capture immigrants well: according to Simon (1998), only 13% of 2nd-generation Algerian youth declare their parent's language as their mother tongue.

[9] We also tested whether differential selection by the PES agent introduces systematic differences

Last, table 5 displays the average credentials of the different groups of applicants. Specifically, each line corresponds to the regression of a given characteristics (e.g. years of education) on four indicator variables characterizing the applicant's gender, neighborhood of residence, and migration status (distinguishing immigrants and children of immigrants).[10] Applicants from potentially discriminated groups do differ by some observables from the reference candidate (a male who is not an immigrant nor the son of an immigrant, and who does not reside in a deprived neighborhood). Overall, people at risk of discrimination in the sample are younger, have less work experience (in particular in the type of job they are applying for), and tend to have a lower reservation wage. Women and immigrants are more educated, whereas children of immigrants and residents in deprived neighborhood are less educated; driving licenses are less frequents except for residents of deprived neighborhoods. Overall, this does not suggest that applicants from groups at risk of discrimination have significantly lower credentials. This feature of our sample may of course result from the screening of applicants by the PES.

6 Representativeness of firms entering the experiment

Before analyzing the impact of anonymous resumes, it is important to check whether firms entering the experiment are representative of firms targeted by the law. Indeed, as noted above, the experimental design allowed firms to refuse to participate, and a significant fraction (around 38%) did so; moreover, a large share of firms eligible for the experiment were not invited to participate. An obvious question is therefore whether firms that entered the experiment were more or less prone to discriminate than other firms. Different hypotheses are possible. One may suspect that firms that do not discriminate are more likely to accept the experiment. In that case, the evaluation would yield the impact of anonymous resumes on "well-behaved" firms, and would not say anything of their impact on firms that do discriminate. But the opposite may be true. There is anecdotal evidence of firms with a strong policy against discrimination that refused to participate, claiming that anonymous resumes are a heavy procedure and unnecessary

between applicants in the treatment and control groups for lots of resumes that were selected after randomization (as would be the case if the agent decided to over-select applicants at risk of discrimination for the anonymous procedure, for instance). There is however no evidence of this: control and treatment applicants remain comparable. More precisely, one does indeed note that resumes from the first lot differ from resumes of the subsequent lots, but the difference is the same for treatment and control job offers. (Results omitted here.)

[10]This additive specification turns out to be a convenient summary. Other descriptive approaches lead to similar main facts.

procedure, possibly even counter-productive by preventing the firm to take into account the disadvantaged background of applicants when assessing their credentials. Moreover, firms that discriminate may choose to participate to the experiment, by fear of raising suspicions if they did not participate. In that case, the evaluation would estimate the local effect on anonymous resumes on more discriminatory firms (possibly overestimating the average impact on the overall firm population).

To address this question, we take a twofold approach. First, we look for observable differences between firms in and outside of the experiment. Table 7 shows that firms participating to the experiment indeed display some specific features, although the differences are not massive. The first two columns describe firms in the control and treatment groups, respectively. As expected with random assignment, differences are small and only one is statistically significant (significant differences are signalled in columns 6 to 9). The third column describes firms that withdrew their offer before randomization could take place. Column 4 (respectively, 5) displays firms that refused to participate (respectively, were not invited to participate). The size and industry of firms that refused to participate are close to those of control firms. But firms that were less frequently invited to participate are concentrated in the non-merchant service sector.[11] Firms refusing to enter the experiment are less frequently firms offering skilled jobs. Similarly, firms that are not invited to participate are often offering indefinite duration contracts.

Tables 8 and 9 complement table 7 using the richer information provider by the firm survey (at the cost of reduced sample size, which reduces the likelihood of detecting statistically significant differences). Firms refusing the experiment or not invited to participate less often declare to have mobilization actions against discriminations. Firms that refuse also more often declare having difficulties to fill a vacancy: this may be one reason for not participating, by fear of jeopardizing a difficult recruitment process. More surprisingly, firms that refuse the experiment are also more often frequent users of the public employment services.

All these differences are suggestive of selective entry in the experiment. However, there is no evidence that this selection is correlated with discriminatory practices. In particular, taste-based models of discrimination emphasize prejudiced "tastes" of customers, coworkers, or employers. However, the fact that the position offered implies frequent customer contact or teamwork with coworkers does not correlate with the firm's decision to enter the experiment. Observing employer's tastes is hard, but one can use the detailed infor-

[11] One likely explanation for that is that subsidized jobs were excluded from the experiment while these jobs are more frequent in the non-merchant service sector; even though we exclude the corresponding job offers from the table, we were told that some PES agents misunderstood the rule and did not propose the experiment to any firm from that sector, even when the job was not subsidized.

mation on the hiring officer. Her origin and migration status, her professional or personal networks do not correlate with the entry of the firm in the experiment. In particular, there is no evidence that the composition of the firm or the personal network of the hiring officer – as measured by the presence of African or Muslim sounding names – are different in firms that do not participate to the experiment. Overall, there is evidence that firms entering the experiment are specific, but it is hard to say whether these specificities are linked to discriminatory practices.

The second approach to assess selective entry in the experiment is to look directly at the firms' record in selecting applicants. Unfortunately, interview rates of different groups of applicants are not well measured in administrative sources. Our approach is therefore to extend the survey of applicants – initially designed for applicants to experimental job offers – to a subsample of applicants on job offers that did not enter the experiment (either because the firm was not invited to participate, or because it refused to participate). This allows us to measure interview rates across different types of applicants, and to compare these differences across firms inside and outside the experiment, when using standard (nominative) resumes. The goal is to check whether minority candidates tend to be in a better or worse relative position in firms that entered the experiment. As discussed further in the next section, a parsimonious model to answer that question is:

$$Y_{ij} = a_0 + a_1 M_i + a_2 F_i + d_0 P_j + d_1 P_j \times M_i + d_2 P_j \times F_i + e_{ij}, \tag{1}$$

where Y_{ij} is an indicator variable equal to 1 if applicant i on job offer j is interviewed (or is hired), M is an indicator for being in the group of immigrants, children of immigrants and/or residents of deprived neighborhoods, F is an indicator for female applicants, and P is the indicator variable for the firms participation in the experiment. Testing $d_1 = d_2 = 0$ amounts to testing whether the relative chances of potentially discriminated applicants (defined by their migration status, their residence or their gender) are specific in firms that entered the experiment. Table 10 shows the estimation results, using different set of control variables. Although the probability to be interviewed is the same for applicants to participating and non-participating firms (first column), there are significant differences across applicants' types: firms that accept to enter the experiment tend to more frequently call minority applicants for interviews, and to less frequently interview women. These differences are only marginally significant when using no controls; the significance further decreases when controlling for applicants' and firms' characteristics. Introducing job offer fixed effects has two consequences: first, the fixed effect absorbs differences across firms that are not related to the applicant's type; second, only firms with mixed pools of applicants play a role in identifying d_1 and d_2. This has little impact on the estimation of

d_1. It does however lower the point estimate for d_2, suggesting that, if one restricts the comparison to firms with men and women in their applicant pools, there is no significant difference along gender lines between firms inside and outside the experiment. Results on hiring decisions (on the right hand side panel) yield a similar picture.

Overall, these results do suggest some differences: with standard nominative resumes, the chances of minority candidates tend to be higher in firms participating to the experiment; women's chances would instead be lower. Note that these differences may still be due to unobserved firm and applicant heterogeneity, rather than to difference in discrimination behavior. However, these results do call for a note of caution, as the population of firms entering the experiment is not representative of the overall population of firms: simple correlations suggest that they represent firms that are rather more favorable to minority applicants. Interestingly, they do not seem to be more favorable to women (if anything, they are actually less favorable). This echoes the findings of qualitative analyses of the experiment: some hiring officers said that they first perceived this experiment as concerning candidates of foreign origin or residing in deprived neighborhoods, but that participating to the experiment made them more aware of gender issues as well. It is seems therefore possible that firms self-selected themselves more according to their behavior concerning ethnic minorities and residents in deprived neighborhood, rather than according to their treatment of female applicants.

7 Impact of anonymous resumes on applicants

7.1 Overall impact

We start by analyzing the average impact of anonymous resumes on different groups of applicants, all firms taken together. In the next subsection, we investigate the heterogeneity of these effects according to the firms' characteristics.

Due to the experimental design, the impact of anonymous resumes on any sub-population is immediately identified as the difference in mean outcomes between control and treatment individuals, within this subpopulation. However, the result of the policy is better defined as a relative impact: do anonymous resumes reduce the gap between applicants at risk of discrimination and other applicants? This question implies to start by defining a group of reference (presumably not victim of discrimination), and one or several groups that are potentially discriminated. Clearly, there is a trade-off between the advantage of looking at narrowly, well-defined groups, and the statistical precision allowed by the sample size. We conducted a variety of statistical tests (described in the appendix) to detect along which lines anonymous resumes have heterogeneous effects. We considered four dimensions along which anonymous resumes may have a differential impact: the applicant's gender, age, place of residence and migration status. Interacting these four dimensions yields 16 different groups, with 0, 1, 2, 3 or 4 potential stigmas. Anonymous resumes do not seem to impact applicants of different ages differently – perhaps simply because age can easily be derived from the work experience detailed in the resume. Finally, it turns out that the impact of anonymous resumes is well summarized by a parsimonious model:

$$Y_{ij} = \alpha_0 + \alpha_1 M_i + \alpha_2 F_i + \delta_0 T_j + \delta_1 T_j \times M_i + \delta_2 T_j \times F_i + \epsilon_{ij}, \tag{2}$$

where Y_{ij} is an indicator variable equal to 1 if applicant i on job offer j is interviewed (or is hired), M is an indicator for being in the group of immigrants, children of immigrants and/or residents of deprived neighborhoods, F is an indicator for female applicants, and T is the indicator variable for the use of anonymous resumes on the job offer. Equation 2 is estimated by OLS, accounting for correlations between applicants on the same job offers using robust standard errors, clustered at the job offer level. We use sampling weights to account for the fact that some applicants were oversampled in the survey. Unless otherwise specified, the model is estimated only on applicants whose resumes were preselected by the PES before randomization, so as to ensure the comparability of applicants under the standard and nominative procedures. Among firms that entered the experiment, compliance to random assignment is nearly perfect[12], so that δ_0 is directly interpreted as the impact of anonymous resumes on the reference group (males who are neither immigrants, sons of immigrants nor residents in deprived neighborhoods), and δ_1 and δ_2 give the additional impact for immigrants, sons of immigrants or residents in deprived neighborhoods, on the one hand[13], and for women, on the other hand. In other words, δ_1 and δ_2 summarize how the gap between potentially discriminated applicants and other applicants is impacted by anonymous resumes.[14]

Table 11 gives a first pass on three questions: (i) Do anonymous resumes induce firms to interview more applicants, in order to compensate for the loss of information on applicants' type? (ii) Do anonymous resumes improve the relative changes of potentially discriminated applicants to be selected for a job interview? (iii) Do the effects of making applications anonymous persist after the applicant's type is revealed by the interview, so that hiring decisions are impacted?

[12] 17 firms (13 treatment firms and 4 control firms) exited the experiment after the random assignment and therefore received standard resumes. Applicants to these firms are interviewed, and analyzed according to the initial random assignment.

[13] Disaggregated results for that composite group are discussed below.

[14] Note that α_1 and α_2 do not have a causal interpretation, as they may capture unobserved differences in applicants' productivity that are correlated with applicants' type.

The left panel displays the impact of anonymous resumes on the probability that a given applicants is interviewed. There is no evidence that firms reacted to anonymous resumes by selecting a larger pool of applicants for interview (column 1). But, as shown in column 2 (which corresponds to model 2), this overall stability hides lower callback rates for applicants of foreign origin and/or residents of deprived neighborhood, and higher callback rates for other applicants. As a result, the gap between majority candidates and candidates at risk of discrimination due to their migration status or their place of residence, which is small and not significant with standard resumes, *increases* significantly when anonymous resumes are used. The effect is large (a 10 percentage point increase, to be compared to average interview rates around 11%). This is the first key, counter-intuitive result of the experiment: overall, anonymous resumes redistribute chances to be called for a job interview, but this redistribution occurs at the expense of those that the law expected to benefit. By contrast, anonymous resumes do not significantly impact the relative chances of women to be interviewed (the point estimate is positive, but far from conventional significance levels). As expected with random assignment, these results are robust to the addition of control variabls (column 3). However, adding job offer fixed effects does change the picture. The negative impact on candidates of foreign origin or residents in deprived neighborhoods is confirmed (though it is not statistically significant anymore due to a loss of precision), but anonymous resumes now seem to improve the relative chances of women: the point estimate is large, so that the effect, even if it is unprecisely estimated, is significant at the 10% level. To interpret this pattern, note that fixed effects restrict the source of variation identifying δ_2 to comparisons of male and female applicants in a given job offer – this is perhaps the most natural way to analyze the impact of anonymous resumes: how does it change the relative chances of candidates when they are competing on the same offers? In the survey of applicants we use, however, only 117 job offers (out of 598) have applicants of both genders. Part of this is due to sampling (there are applicants of both genders, but all were not interviewed); part of it, however, reflects the fact that the PES preselected only men for 31% of the experimental job offers, and only women for 17% of the others. On these job offers, anonymous resumes should not impact the relative chances of women, except if firms react to uncertainty by interviewing more candidates, and if they do so differently depending on whether they have male or female applicants. In other words, the estimate of δ_2 in columns 2 and 3 is the mean of the (presumably null) impact of anonymous resumes on job offers where the two genders do not compete, and of its impact on job offers where they indeed compete. The estimates in column 4 suggest that, in the latter job offers, woman have lower interview rates than men when resumes are nominative (a -11.1 pp difference, not statistically significant), and that anonymous resumes are effective in closing the gap. Table 20 checks this interpretation by estimating equation 11 only on job offers with candidates of both genders[15]: this allows us to estimate the effect of anonymous resumes using variations in 311 rather than 117 firms. The estimated δ_2 is still positive, statistically significant, even though slightly smaller (around 12 percentage points). [16]

The right panel of table 11 addresses the third question: do these effects persist one the firm knows the applicant's identity? Unfortunately, the statistical power to detect an impact of anonymous resumes on hiring decision is limited: the probability of being hired is small; only very large changes (when expressed in percents of the initial hiring probability) can be detected. Column 6 does suggest that the lower interview rates of applicants of foreign origin and/or residing in deprived neighborhood translate into lower chances to be hired (the relative impact is negative, but significant at 10%). This however is not robust to the inclusion of controls.

To summarize, table 11 yields four main key results:

1. Anonymous resumes do not induce firms to call more applicants for interviews.
2. Anonymous resumes decrease the relative chances of applicants of foreign origin and/or residing in deprived neighborhood to be interviewed for a job.
3. Anonymous resumes increase the relative chances of women on job offers for which they are competing with men; however, there is only half of such offers in our sample, so that this translates only in a modest and non significant change in the overall population.
4. Evidence on whether these effects on the selection of resumes by firms translate into hiring decisions is not conclusive.

Robustness checks

We performed a variety of tests to check the robustness of these key facts to potential threats to the experimental design, as well as to alternative measurements and model specifications.

A John Henry effect?

[15] As characterized by the total pool of candidates from the administrative data, rather than the sample from the survey of applicants.

[16] We explore other dimensions of effect heterogeneity in the next subsection. The distinction between job offers with or without a mixed pools of applicants matters less when it comes to migration status or place of residence. Indeed, in our sample, 72% of job offers had applicants both from the potentially discriminated group and from the rest of the population. Restricting the estimation to these job offers does not alter the picture much (see table 21).

As noted below, a possible issue with the experimental design is the fact that participating firms knew that they were part of an experiment. This in itself could affect their behavior. The risk is particularly acute control firms: they know they were observed, they also know the identity of applicants, and are therefore directly confronted to the question of calling for interviews potentially discriminated applicants. They may therefore artificially select more of these applicants in order to signal to the PES that they do not discriminate. This type of effect is known as a "John Henry" effect, by which the control group makes extra effort to perform well. Such effect could explain why treated firms appear less favorable to applicants of foreign origin and/or residing in deprived neighborhood: the negative difference would not be due to a negative impact of anonymous resumes on treatment firms, but to the positive impact of monitoring firms in the control group.

Our strategy to test for the presence of such an effect is to look at control firm hiring behavior *after* the experiment. The idea is the following: randomization ensures that control and treatment firms are comparable. But *during the experiment*, control firms change their behavior, so that they are not a valid counterfactual. However, unless being part once of an experiment (without being treated) has surprisingly lasting effects, control firm behavior should not be distorted anymore after the experiment. We therefore ran the survey of applicants on a subsample of applicants to 148 job offers that were posted by control firms after the experiment.[17] The test for the presence of a John Henry effect is very simple: we ask whether interview and hiring gaps between applicants of different groups were different before and after the experiment. Formally, we use the same type of model as above:

$$Y_{ij} = \alpha_0 + \alpha_1 M_i + \alpha_2 F_i + \delta_0 EXP_j + \delta_1 EXP_j \times M_i + \delta_2 EXP_j \times F_i + \epsilon_{ij}, \qquad (3)$$

where Y_{ij} is an indicator variable equal to 1 if applicant i on job offer j is interviewed (or is hired), M is an indicator for being in the group of immigrants, children of immigrants and/or residents of deprived neighborhoods, F is an indicator for female applicants; last EXP is the indicator variable for job offers that were part of the experiment. The model is estimated on 807 applicants, who applied to 148 pairs of job offers posted by control firms, one job offer being included in the experiment, the other being excluded. δ_0, δ_1 and δ_2 summarize how interview and hiring rates evolved for different group of applicants in the same firms, before and after the experiment.

Table 12 shows no evidence of an Henry effect. Interaction coefficients are small, and

[17] Note that relying on applicants for information on interview and hiring decisions made by the firm removes any concern that firms become aware that we continue observing them.

far from being significant. If anything, control firms were more rather than less favorable to applicants of foreign origin and/or residing in a deprived neighborhood when the experiment stopped. Of course, a John Henry effect may persist over time (once firms know they have been observed, they are durably more cautious); but one would expect the effect to decay. This is not the case.[18]

Insufficient anonymization?

As noted above, anonymization in the experiment is imperfect, as the content of resumes is not standardized. In particular, foreign background can be inferred from languages skills: can this explain why anonymous resumes did not have more favorable effects on applicants of foreign origin?

In our sample, 90% of the resumes have been processed manually. For those resumes we have information about the language skills of the candidate; in particular, we know whether the applicants speaks Arabic, or any other foreign language than those typically taught in French schools (English, German and Spanish). Let us define as "foreign" any other language than French, English, German or Spanish. When recruiters read in a candidate's resume that she has foreign language skills, they can infer that she is immigrant or child of immigrant: this will be a bad guess in only 20% of the cases. Observing no language skills in the resume is less informative: among the candidates who do not state any foreign language skills, around one third are immigrants or children of immigrants. All in all, using the languages skills as a proxy for foreign background is a successful strategy in 70% of the resumes. Focusing on Arabic, language is even a better proxy for foreign background: when recruiters read in a candidate's resume that she has Arabic language skills, they can infer that she is immigrant or child of immigrant from the Maghreb, and this will be a bad guess in only 7% of cases. Again, observing no Arabic language skills in the resume is less informative. Among the candidates who do not state Arabic language skills, 13% are immigrants or children of immigrants. All in all, Arabic language skills is a good proxy for foreign origin in 87% of resumes.

Foreign language skills are therefore a strong signal of foreign background. One may therefore suspect that the impact of anonymous resumes is lower on applicants with such

[18]Instead of focusing on job offers posted by control firms after the experiment, one could have looked at job offers posted before the experiment, that would be fully exempt from any John Henry effect. However, this turns out not feasible. Indeed, administrative information being insufficient, we would need to run survey of applicants on these past job offers. By construction, these surveys would often occur with a significant delay – to identify control firms, one needs to wait for them to enter into the experiment! – which would create memory bias among respondents.

skills on their resumes. We estimate the following heterogeneous treatment effect model:

$$Y_{ij} = \alpha_0 + \alpha_1 D_i \times (1 - L_i) + \alpha_2 D_i \times L_i + \delta_0 T_j + \delta_1 D_i \times (1 - L_i) \times T_j + \delta_2 D_i \times L_i \times T_j,$$

where L indicates whether the candidate states a foreign language skill, D indicates whether the candidate is potentially discriminated on basis of her foreign background and T indicates that the job offer was processed with anonymous resumes.

Results on the interview rate are displayed in table 13. In the first two columns, potentially discriminated candidates are defined in the usual way : they have foreign background or they live in a deprived neighborhood. In the third column, foreign background is restricted to immigrants or children of immigrants from the Maghreb. In the first column, foreign language is defined broadly (any language different from English, German or Spanish). In the last two, it is restricted to Arabic language. According to the first column, being potentially discriminated or speaking a foreign language does not affect the interview rate when resumes are nominative. Anonymization has no significant effect on non discriminated candidates (δ_0). The typical negative relative effect of anonymization on potentially discriminated candidates is estimated for both groups of candidates (δ_1 and δ_2). Effects are not heterogeneous depending on language skills: the difference between the two coefficients (around 3 percentage points) is not statistically significant. The second and third columns confirm the absence of heterogeneous effects. The analysis of hiring rates leads to the same conclusion. We discuss possible interpretations of this finding below.

Alternative measures of applicants' background

The applicant's background enters model 2 in a quite specific way, imposing the same effect for being of foreign background (immigrant or child of immigrant, denoted by the indicator variable I), residing in deprived neighborhood (denoted by the indicator variable Z), and cumulating the two characteristics. Other models are possible: for instance, the impact of potentially discriminated characteristics may cumulate (implying an additive model, with I and Z entering separately); they may reinforce each other (implying a model with I, Z and $Z \times I$), etc. The correct specification is an empirical question. Moreover, it is not obvious how foreign background should be measured. Names are directly impacted by anonymous resumes, so that this may be the relevant measures. However, coding whether family names denote a foreign background is not immediate. Moreover, even if first names and surnames do not denote a foreign background, a picture ID might. In that case, measuring foreign background with the applicant's migration status may be more relevant. Again, the appropriate measure is an empirical question.

Table 22 displays alternative possible specifications. Looking at coefficients on $T \times I$, $T \times Z$ and $T \times I \times Z$ in columns 2, 4, 6 and 8 shows that the effects of I and Z do not cumulate: basically, having only one of the two characteristics or the two of them does not modify the (negative) impact of T. This is why our preferred specification characterizes applicants as potentially discriminated due to their background when they are either of foreign background, or residing in deprived neighborhood, or both. In columns 3 to 8, three possible alternative measures of foreign background – being an immigrant, or being the child of an immigrant, or having a Muslim or African-sounding first name – yield similar results to our preferred measure (which groups immigrants and children of immigrants). Point estimates, however, tend to be lower, and the effect is no longer statistically significant when considering only Muslim or African-sounding name. A plausible explanation for that is attenuation bias due to measurement error. For instance, we know from table 4 that about 40% of applicants with a foreign background are not signalled by a Muslim or African-sounding name. If they are actually detected by firms, this contaminates the group of reference, creating a downward bias on the coefficient of interest. Overall, table 22 justifies model 2 as a parsimonious but appropriate to model the differential impact of anonymous resumes.

Other specification issues

Table 23 displays additional robustness checks. First, we check whether sampling weights make a difference. The coefficient on $T \times M$ becomes smaller and marginally significant only. This may be due the fact that, among applicants from a foreign background, applicants with a Muslim or African-sounding name have been oversampled (this was the only information on foreign background available at the time of sampling). The lower point estimate suggests that the negative effect of anonymous resumes could be smaller on that group. The difference, however, is far from significant. In column 3, we check whether expanding the sample to applicants whose resumes were pre-selected by the PES agents after randomization makes a difference. Again, the coefficient on $T \times M$ is smaller. There remains however a suspicion that the pools of candidates in the treatment and control group are no longer comparable. Last, we check that using a logit model rather than a linear probability specification does not affect the results.

7.2 Heterogeneous effects

An important question is whether the main effects summarized in table 11 apply generally, or whether anonymous resumes impact the gap between different groups differently on different subpopulations of jobs, applicants or firms.

The obvious problem here is the curse of dimensionality (what we want to analyze here is a difference in differences in differences: anonymous vs standard resumes, for applicants with or without potentially discriminated characteristics, in subpopulations A and B), and the corresponding risk is data mining. We did however replicate the analysis of table 11 on different subsamples defined by the job skill level, the industry, the applicant's education level, whether the firm reported HR policies against discriminations or not, etc. No systematic and significant differences appeared. A better approach is certainly to start from priors on dimensions of heterogeneity that should matter, from a theoretical perspective. We consider two of them.

Labor market segmentation by gender

The first dimension to consider relates to a labor market segmentation hypothesis: if there are men jobs, women jobs, and jobs for men and women, one should not expect anonymous resumes to impact these jobs similarly. The most likely predictions is that anonymous resumes will not change the prospects of women for women jobs (that they will get anyway), nor for men jobs (which they will not get), but that they may improve their chances on jobs for which men and women are competing. As discussed above, the contrast between columns 3 and 4 of table 11 tends to confirm this hypothesis. More precisely, tables 11 and 20 show that anonymous resumes have no impact on job offers for which PES agents select only men or only women, but that anonymous resumes positively impact women when the PES agents preselect a mixed pool of applications. The question that remains open is whether the PES agents' pre-selection reflect a feature of the labor market (segmentation). To check this, we analyze the share of female job-seekers by type of job thought.[19] The distribution of the share of female job-seekers across jobs is displayed on figure 2. We define 3 types of job sought:

1. Male dominated jobs: when the share of female unemployed seeking this type of job is less than 25% (one example of position is security guard))
2. Women dominated jobs : when the share of female unemployed seeking this type of job is more than 75%(for instance, secretary)
3. Mixed jobs (the complement)

Among the stock of registered unemployed, those jobs represent respectively 36%, 14% and 50%. This indicator of gender segregation is a good predictor of the segregation observed in our sample displayed on figure 1: the coefficient of correlation is 0.72. 60% of

[19]This analysis uses an additional data source, the administrative files kept by the PES on all registered job-seekers.

jobs offers have both indicator consistent. For example, 65% of the job offers predicted as mixed by the external segregation measure are indeed mixed in our sample.

Overall, the analysis confirms that anonymous resumes improve the chances of women to be interviewed for a job on jobs for which there is labor supply is mixed. However, the French labor market features persistent segmentation, so that some positions only attract women's applications, whereas other only attract men's applications. As expected, we see no impact of anonymous resumes on these segments. One possibility though is that anonymous resumes could, in the long run, have a "calling" effect: if women feel they now have a fair chance to get positions that used to be "men's jobs" thanks to anonymous resumes, they may start competing for these positions too. Such effect is absent from our evaluation, where applicants were most likely not aware of the use anonymous resumes.

Homophily

The second hypothesis is known in the literature as the homophily hypothesis: in our setting, individuals would tend to discriminate against members who do not belong to their own group. With this hypothesis in mind, we made specific effort to characterize the group of the recruiters in experimental firms (see the data question). Table 14 (respectively,15) estimates equation 2 after stratifying the sample of recruiter by gender (respectively, according to her network).

Table 14 shows a pattern that is consistent with the homophily hypothesis. Male recruiters tend to select fewer women for interview, and to hire fewer of them, while female recruiters tend to select fewer men (as shown by columns 3 and 6, the differences are significant at the 5% level). Of course, alternative interpretations are possible, as the recruiter gender may be correlated with other characteristics of the firm.[20]. Turning to interaction effects, we find that anonymous resumes undo this differential treatment: the interaction coefficient on T $\times$ woman is positive when the hiring recruiter is a man, negative when it is a woman. This difference is strongly significant. In other words, anonymous resumes counteract the tendency of hiring officers to select applicants of their own gender: it therefore equalizes the chances of men and women, independently from the gender of the recruiter. Most interestingly, this has consequences on the final recruitment decision, after the hiring officer has actually met the candidate.

Table 15 looks for a similar pattern for applicants of foreign background: are they treated differently depending on the background of the recruiter? There are unfortunately very few recruiters with a foreign background in our sample. A more useful measure is provided by asking the recruiter about the first names of her friends: this allows to identify

[20]We do not however find that it is correlated with the fact that the applicants' pool is mixed or not

recruiters who cite at least one African or Muslim-sounding name among three friends. We do not find evidence of differential treatment with standard resumes; correspondingly, we do not find that anonymous resumes affect applicants with a foreign background differently depending on the identity of the hiring officer.

8 Impact of anonymous resumes from the recruiter perspective

We now evaluate the effects of anonymous resumes on the costs of the recruitment process.[21] During the experiment, the direct costs of anonymization have been paid by the Public Employment Agency. We thus focus on more indirect but no less important costs, such as the number of interviews, the time to recruit... We expect those costs to increase with anonymization. By reducing the level of information in the first stage of the recruitment process, firms may report their selections to further stages, and increase the number of interviews or tests, which are typically more costly. Those modifications of the recruitment process may also delay the hiring date, increasing the opportunity cost of keeping a job unfilled.

A particular concern is whether anonymization affects match quality, as measured by wages or output. A direct measure of output is not available, but we take as a proxy whether the trial period was successful, which should reflect that output is above a minimum threshold. We also estimate the effect of anonymization on the hiring wage. Note however that hiring wages do not only reflect productivity, but also the outside labor market options of the candidate. Assume that anonymization does not affect the productivity of the hired candidate but that hired candidates are more often from the discriminated minority group. Wages may still decrease as a result of the candidate's lower bargaining power.

8.1 Crowding out effects

Before performing the cost benefit analysis from the recruiter perspective, we estimate possible crowding out effects of candidates from the Public Employment Service. As a

[21] We also considered whether the costs and the nature of the recruitment process differs between firms that entered the experiment and firms that did not enter. Except if noted otherwise, we found no significant difference (results omitted).

response to the lower level of information on candidates sent by the PES, firms may activate other more costly channels to meet candidates.

Around one out of two applications received by the recruiter come from the PES and one out of four interviewed candidates are sent by the PES (line 1 and 1 ,column 1 in table 16). Last, one out of three hired candidates are sent by the PES. This highlights the fact that recruiters do not rely exclusively on the PES to drain candidates. However small, the share PES candidates does not decrease with anonymous resumes: there is no evidence that anonymous resumes in the PES leads to a crowding out of the candidates it sends.

8.2 Costs

Anonymous resumes have not altered the probability of successful recruitment. Around four out of five hirings were completed at the time of the survey (see line 3 column 1 of table 17) and the difference between control and test (column 2) is small and not significant. Anonymous resumes have not altered the probability that the recruitment had been stopped without any hiring (line 1, column 1 and 2). The mean time to hiring is 49 days in the control group. The first and third quarters of the distribution are 20 and 72 days. Anonymous resumes do not alter that distribution.

Overall, these findings suggest that anonymous resumes do not increase the costs associated with foregone output due to unsuccessful or delayed hiring.

We now turn to the hiring process itself. Half of the recruiters in the control group receive at most 12 applications and interview at most 6 candidates (line 1 and 2, column 1 in table 18). The median numbers of applications and interviews is not affected by the use of anonymous resumes.

Recruiters select candidates thanks to various tools : phone interviews, collective, individual interviews and tests (in situ). Individual interviews are conducted by four out of five recruiters, phone interviews by two out of five recruiters, tests by one out of five recruiters (line 3, 4 and 5; column 1 in table 18). Collective interviews are relatively marginal. Anonymous resumes do not lead recruiters to change their mix of selection tools (column 2). The mean number of tools used is 1.6 in both control and test group (line 6).[22]

[22]Recruiters who withdraw before randomization tend to have a significantly larger selection toolkit (1.8 mean number of tools). Again this shows that their recruitment process is more intensive, leading presumably to a faster recruitment (results not presented here).

We find no increase in the number of recruiters or the total working time devoted to the recruitment. In the control group, around 2 recruiters take part to the process and half of the job offers are filled in less than 8 hours and a half.

8.3 Benefits

Even in the absence of cost increase, it is relevant from the recruiters' point of view to estimate potential benefits associated with anonymous resumes. In table 19, we analyze hired candidates as described by the recruiters in the firm survey. Note that most of the hired candidates were not addressed by the PES: in this subsection we analyze a broader population than in the previous section on candidates. Indeed, from the recruiter perspective, this global effect is the relevant one.

Four hired candidates out of five successfully complete their trial period. Recruiters are generally satisfied with the first tasks performed by the hired candidate or more generally with his/her adequation to the job. Moreover, match quality as measured by successful trial period or recruiters' subjective satisfaction is not affected by the use of anonymous resumes. One hired candidate out of five is paid the minimum wage. Half of the workers who are paid more than the minimum wage earn more than 1 715 euros per month (gross wage without any bonuses). The wage distribution is concentrated just above the minimum wage (1 350 euros). The first and third quartile are respectively 1.1 and 1.63 of the minimum wage. Anonymous resumes do not affect the share of hired candidates paid the minimum wage, nor the median or first quartile of the wage distribution. The third quartile is significantly lower by 250 euros. This latter result, however, is not robust to the addition of controls; moreover, it is not clear whether such an effect should be interpreted in terms of productivity or bargaining power.

Overall, we find no evidence that anonymous resumes change hiring costs, labor costs and match quality. Two caveats must be kept in mind, though. First, the PES took in charge the anonymization procedure itself and these costs are not included here; second, we only test for short-term effects for filling one position: anonymous resumes may, in the long run, lead firms to more substantially modify their hiring process.

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Table 1: Sample size and response rates in the applicant survey

	Population size (a)	# sampled for survey (b)	# of respondents (c)	Sampling rate (b/a)	Response rate (c/b)	Information rate (c/a)
Control	3443	1520	1012	0.44	0.67	0.29
Treatment	3299	1464	945	0.44	0.65	0.29
At risk of discrimination	2312	1369	900	0.59	0.66	0.39
Other applicants	4430	1615	1057	0.36	0.65	0.24

Note : This table displays population and sample size among applicants to job offers entering the experiment. The first two lines distinguish applicants according to whether resumes were anonymous (treatment) or not (control); the last two lines distinguish applicants who have an African / Muslim-sounding name or live in deprived neighborhoods from other applicants.

Table 2: Sample size and response rates in the firm survey

	Population size (a)	# sampled for survey (b)	# of respondents (c)	Sampling rate (b/a)	Response rate (c/b)	Information rate (c/a)
Control	385	385	229	100.0	59.5	59.5
Treatment	366	366	212	100.0	57.9	57.9
Withdrew before randomization	254	254	134	100.0	52.8	52.8
Refused the experiment	608	335	146	55.1	43.6	24.0
Not invited	4714	542	281	11.5	51.8	6.0

Note: This table displays population and sample size among job offers eligible for the experiment (one job offer per plant). The first two lines display job offers handled with standard resumes (control job offers) or anonymous resumes (treatment job offers). The third line corresponds to firms who accepted to enter the experiment but withdrew of filled their position before the PES had provided a first lot of resumes and randomization could take place. The last two lines correspond to plants that refused the experiments or that were not invited to participate, despite the fact they were eligible.

Table 3: Measures of risk of discrimination

Discriminatory characteristic	Mean	Correlation with							
		(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
Woman (a)	49	1							
Age below 30 (b)	48	0.15	1						
Age above 50 (c)	11	-0.07		1					
Deprived neighborhood (d)	24	-0.09	0.09	0.09	1				
African or Muslim-sounding name (e)	23	-0.12	0.05	-0.02	0.15	1			
Immigrant (f)	22	-0.10	-0.03	0.00	0.14	0.38	1		
Son of immigrant (g)	16	-0.02	0.14	-0.10	0.11	0.36	-0.24	1	
Immigrant or son of immigrant (h)	38	-0.10	0.08	-0.07	0.20	0.59	0.67	0.56	1

Source : Applicants' main sample; 1260 observations.

Table 4: Immigrants and applicants with African or Muslim-sounding name

	Immigrant or son of immigrant	Other applicant	Total
African or Muslim-sounding name	21%	2%	23%
Other name	17%	60%	77%
Total	38%	62%	100%

Note : This table compares the risk of discrimination due to migratory origin based on two indicators: having an African or Muslim-sounding name, or being an immigrant or the child of an immigrant. Each cell gives the frequency within the experimental population (using sampling weights).

Table 5: Applicants' characteristics according to gender, neighborhood of residence and migration status

	Intercept	Women	Deprived neighborhood	Immigrant	Child of immigrant
Years of education	12.684***	.418**	-.518***	.653***	-.170
	(.144)	(.169)	(.195)	(.245)	(.209)
Age	35.263***	-2.542***	-1.387***	.283	-3.505***
	(.410)	(.454)	(.505)	(.553)	(.559)
Work experience (in years)	4.633***	-.614***	-.417**	-.381*	-.467**
	(.150)	(.169)	(.183)	(.204)	(.219)
Experience on a similar job (in years)	2.265***	-.555***	-.013	-.807***	-.789***
	(.185)	(.187)	(.229)	(.220)	(.234)
Has been looking for a job for more than a year	.136***	-.035**	.027	-.013	-.026
	(.013)	(.015)	(.017)	(.019)	(.019)
Reservation wage is minimum wage	.422***	.155***	.102***	.041	.063**
	(.019)	(.022)	(.025)	(.028)	(.029)
Reservation wage (euros)	1841.086***	-203.178***	-134.363***	-30.362	-93.493***
	(26.912)	(25.787)	(25.786)	(31.380)	(35.377)
Has a driving license	.700***	-.130***	-.011	-.153***	-.095***
	(.020)	(.023)	(.025)	(.029)	(.031)
Speaks a non-European language	.039***	.008	-.019	.306***	.214***
	(.011)	(.016)	(.018)	(.024)	(.024)

Note : Each line corresponds to an OLS regression of an applicant credential on indicator variables for women, residents in deprived neighborhood, immigrants and children of immigrants. Standard errors in brackets. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. Sample: all applicants from applicant survey (1,957 observations). Source: applicant survey, except for driving license and language skills, directly coded from resumes.

Table 6: Balancing between control and treated candidates

	Test	Control
Candidates' characteristics		
Women	0.51	0.47
Less than 26	0.29	0.29
More than 50	**0.13**	0.09
Deprived neighborhood (1)	0.23	0.25
Immigrant (2)	0.22	**0.23**
Child of immigrant (3)	0.18	0.15
(1), (2) or (3)	0.50	0.49
African or Muslim-sounding name (4)	0.24	0.24
More than 12 years of education	0.72	0.75
Number of candidates sent to recruiter		
candidates (1) or (4)	2.08	2.05
other candidates	3.45	3.42

Source : Candidates' survey

Table 7: Comparison of firms and positions offered according to participation in the experiment

	Control (a)	Treatment (b)	Withdrew before randomization (c)	Refused to participate (d)	Not invited to participate (e)	Test of difference (b) vs (a)	(c) vs (a)	(d) vs (a)	(e) vs (a)
Firm with less than 100 employees	34.4	32.4	34.6	35.8	30.4				
Firm with 100 to 200 employees	16.8	17.4	18.4	18.8	17.8				
Firm with more than 200 employees	48.8	50.2	47	45.4	51.8				
Non-merchant services	24.7	23.2	28	21.5	30.2				**
Merchant services	47	47	49.2	49.8	47.3				
Manufacturing	13.8	16.9	9.8	11.5	8.5				***
Construction	3.4	3.6	2.4	2.8	3.4				
Upper occupations	9.9	6.3	5.5	4.9	5.4	*	**	***	***
Intermediary occupations	24.4	26	27.6	20.9	21.1				
Skilled white or blue collar	55.3	58.7	52.4	63.7	58.8			***	
Unskilled white or blue collar	10.4	9	14.6	10.5	14.7				***
Indefinite duration contract	66.5	62.6	63.4	62.7	59.5				***
Contract for more than 6 months	86	82.2	79.1	82.9	83.3				
Number of observations	385	366	254	608	4719				

Note : Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. **Source:** PES administrative data (SAGE, TCD-banque de CV, MER). All eligible job offers.

Table 8: Comparison of firms and positions offered according to participation in the experiment (2)

	Control (a)	Treatment (b)	Withdrew before randomization (c)	Refused to participate (d)	Not invited to participate (e)	Test of difference (b) vs (a)	(c) vs (a)	(d) vs (a)	(e) vs (a)
Firm's characteristics									
International group	33.2	40.3	25.0	34.2	31.4				
Firm has led actions against discriminations	58.0	48.7	50.0	47.5	47.7	*		*	**
Firm has an employee in charge of fighting discriminations	28.2	24.1	21.9	24.5	28.7				
Characteristics of a typical recruitment									
Easy	68.4	63.3	59.2	57.5	62.5		*	**	
Through the PES	79.4	80.5	84.6	89.0	77.1			**	
Uses PES selection of resume	47.0	45.0	41.8	46.1	55.3				*

Note : Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. **Source:** Firm survey.

Table 9: Comparison of firms and positions offered according to participation in the experiment (3)

	Control (a)	Treatment (b)	Withdrew before randomization (c)	Refused to participate (d)	Not invited to participate (e)	Test of difference (b) vs (a)	(c) vs (a)	(d) vs (a)	(e) vs (a)
Job characteristics									
Involves teamwork	85.2	75.4	81.2	83.0	84.2	**			
Frequent customer contact	71.9	67.9	75.2	70.4	75.7				
Hiring officer characteristics									
Woman	63.8	57.8	63.6	65.8	56.4				*
College graduate	59.0	62.1	62.9	53.4	58.6				
Age	40.6	39.5	39.8	40.5	41.2				
Firm tenure (in years)	9.1	8.0	6.9	8.0	8.3		**		
Experience in hiring (in years)	8.5	9.2	9.6	9.2	10.4				***
French as mother tongue	97.8	97.6	91.7	96.5	95.2		**		
Immigrant	2.6	2.4	4.5	3.4	2.9				
Immigrant or daughter of immigrant	11.4	10.0	10.6	11.6	11.8				
At least one friend (out of 5) with Muslim or Afr. name	24.6	22.0	30.2	26.5	24.7				
At least one colleague (out of 5) with Muslim or Afr. name	27.5	27.0	27.0	26.6	29.4				

Note : Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. **Source:** Firm survey.

Table 10: Comparison of interview and hiring rates in firms entering the experiment (with anonymous resumes) and non-participating firms

	interview				Recruitment			
Intercept	0.132***	0.103***	0.097***		0.059***	0.038**	0.034**	
	(0.025)	(0.031)	(0.032)		(0.015)	(0.015)	(0.016)	
Deprived neighborhood		-0.077**	-0.070*	-0.064*		-0.035	-0.034	-0.055**
or foreign origin		(0.038)	(0.037)	(0.036)		(0.027)	(0.027)	(0.027)
Woman		0.122***	0.116***	0.022		0.070***	0.068***	0.011
		(0.041)	(0.040)	(0.036)		(0.026)	(0.025)	(0.028)
Entered the experiment (P)	0.011	0.030	0.044		-0.017	0.002	0.009	
	(0.030)	(0.042)	(0.046)		(0.018)	(0.023)	(0.025)	
P × deprived neighborhood		0.083*	0.071	0.072		0.027	0.025	0.052
or foreign origin		(0.048)	(0.050)	(0.050)		(0.032)	(0.033)	(0.035)
P × woman		-0.105**	-0.096*	-0.042		-0.058*	-0.052*	-0.012
		(0.052)	(0.051)	(0.062)		(0.031)	(0.031)	(0.050)
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Job offer effects	No	No	No	Yes	No	No	No	Yes
Observations	1,688	1,688	1,688	1,688	1,688	1,688	1,688	1,688
Job offers	631	631	631	631	631	631	631	631
Plants	365	365	365	365	365	365	365	365

Note : each column corresponds to one regression. Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. **Source:** survey of applicants.

Table 11: Impact of anonymous resumes

	Interview				Recruitment			
Intercept	0.106***	0.131***	0.129***		0.022***	0.016**	0.022**	
	(0.016)	(0.035)	(0.034)		(0.006)	(0.007)	(0.009)	
Deprived neighborhood		-0.028	-0.010	0.023		0.002	0.001	0.016
or foreign origin		(0.032)	(0.032)	(0.050)		(0.012)	(0.013)	(0.025)
Woman		-0.022	-0.042	-0.111		0.011	-0.002	0.003
		(0.033)	(0.035)	(0.086)		(0.012)	(0.014)	(0.044)
Anonymous resume (T)	0.005	0.042	0.037		0.013	0.025*	0.023*	
	(0.023)	(0.047)	(0.044)		(0.011)	(0.015)	(0.014)	
T × deprived neighborhood		-0.100**	-0.090**	-0.117		-0.035*	-0.025	-0.046
or foreign origin		(0.044)	(0.044)	(0.074)		(0.020)	(0.020)	(0.035)
T × woman		0.028	0.039	0.201*		0.009	0.006	-0.001
		(0.045)	(0.043)	(0.109)		(0.021)	(0.020)	(0.045)
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Job offer effects	No	No	No	Yes	No	No	No	Yes
Observations	1,260	1,260	1,260	1,260	1,260	1,260	1,260	1,260
Job offers	598	598	598	598	598	598	598	598
R-squared	0.109	0.128	0.173	0.657	0.030	0.037	0.082	0.582

Note : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. **Source:** survey of applicants.

Table 12: Interview and recruitment rates in control firms before and after the experiment: test of a John Henry effect

	Interview			Recruitment		
Intercept	0.156***	0.141***	0.102*	0.053***	0.042	0.010
	(0.025)	(0.045)	(0.060)	(0.015)	(0.030)	(0.034)
Deprived neighborhood		0.020	0.017		-0.002	-0.006
or foreign origine		(0.051)	(0.052)		(0.030)	(0.031)
Woman		0.010	0.012		0.026	0.032
		(0.051)	(0.052)		(0.031)	(0.033)
Experimental job offer (EXP)	-0.025	-0.019	0.083	-0.020	0.004	0.078
	(0.033)	(0.055)	(0.113)	(0.019)	(0.035)	(0.054)
EXP × deprived neighborhood		-0.031	-0.038		-0.023	-0.009
or foreign origin		(0.064)	(0.067)		(0.034)	(0.035)
EXP × woman		0.020	0.021		-0.032	-0.034
		(0.070)	(0.070)		(0.038)	(0.036)
Observations	807	807	807	807	807	807
Job offers	296	296	296	296	296	296
R-squared	0.147	0.148	0.168	0.047	0.051	0.077

Note : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. **Source:** survey of applicants.

Table 13: Heterogenous effect on the interview rates : Insufficient anonymization ?

Origin	Foreign	Foreign	Maghreb
Language	Foreign	Arabic	Arabic
	(1)	(2)	(3)
Intercept	.127***	.127***	.124***
	(.029)	(.029)	(.025)
Deprived neighborhood or foreign origin	-.033	-.030	-.032
No foreign language skills in the resumes	(.036)	(.035)	(.033)
Deprived neighborhood or foreign origin	-.039	-.058	-.065
Foreign language skills in the resumes	(.044)	(.049)	(.046)
Anonymous Resumes (T)	.051	.051	.024
	(.044)	(.044)	(.036)
T x deprived neighborhood or foreign origin	-.090*	-.097**	-.064
No foreign language skills in the resumes	(.050)	(.049)	(.044)
T x deprived neighborhood or foreign origin	-.126**	-.106*	-.069
Foreign language skills in the resumes	(.055)	(.060)	(.054)

Source: survey of applicants. Linear probability model with sampling weights. No controls. No fixed effects. **Note** : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***.

Table 14: Homophily in gender

	Interview			Recruitment		
	Male recruiter	Female recruiter	test	Male recruiter	Female recruiter	test
Intercept	0.225**	0.064**		0.025	0.023*	
	(0.097)	(0.027)		(0.019)	(0.013)	
Deprived neighborhood	-0.062	-0.019		0.010	-0.028	
or foreign origin	(0.089)	(0.034)		(0.025)	(0.022)	
Woman	-0.155**	0.050	**	-0.024	0.040*	**
	(0.073)	(0.037)		(0.020)	(0.021)	
Anonymous resumes (T)	-0.032	0.207***	**	0.025	0.018	
	(0.112)	(0.073)		(0.031)	(0.030)	
T x deprived neighborhood	-0.126	-0.123*		-0.065*	0.030	*
or foreign origin	(0.105)	(0.070)		(0.037)	(0.033)	
T x woman	0.220**	-0.175**	***	0.063*	-0.076**	***
	(0.092)	(0.075)		(0.034)	(0.033)	
Observations	289	436		289	436	
R-squared	0.193	0.145		0.054	0.047	

Source: survey of applicants. Linear probability model. No controls. No fixed effects. **Note** : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***.

Table 15: Homophily in origin

	Interview			Recruitment		
	Has the recruiter foreign friends ?			Has the recruiter foreign friends ?		
	No	Yes	test	No	Yes	test
Intercept	0.134**	0.093*		0.037**	0.027	
	(0.058)	(0.054)		(0.016)	(0.028)	
Deprived neighborhood	-0.035	0.009		-0.027	-0.026	
or foreign origin	(0.054)	(0.059)		(0.027)	(0.032)	
Woman	-0.005	-0.046		0.022	0.024	
	(0.058)	(0.064)		(0.029)	(0.028)	
Anonymous resumes (T)	0.069	0.167		-0.005	0.005	
	(0.081)	(0.122)		(0.024)	(0.036)	
T x deprived neighborhood	-0.123	-0.236**		0.025	-0.023	
or foreign origin	(0.079)	(0.108)		(0.033)	(0.047)	
T x woman	0.007	0.001		-0.039	0.022	
	(0.080)	(0.108)		(0.034)	(0.043)	
Observations	425	159		425	159	
R-squared	0.148	0.168		0.037	0.061	

Source: survey of applicants. Linear probability model. No controls. No fixed effects. **Note** : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***.

Table 16: Crowding out effects

	Intercept (nominative)	Anonymous resume
Share of candidates sent by the PES	.563***	.0001
	(.025)	(.038)
Share of interviewed candidates sent by the PES	.230***	.056
	(.023)	(.037)
Among successful recruitment		
Hiring from the PES	.348***	.010
	(.036)	(.052)
Nb. of successful recruitment observed	178	162

Surveyed recruiters with successful or stopped recruitment. Results are from median regressions on line 1 and 2, and from mean regressions on line 3 and 4. For the median regression, standard errors are bootstrapped.

Table 17: Recruitment Issue

	Intercept (nominative)	Anonymous resume
Recruitment stopped	.140***	-.022
	(.023)	(.032)
Recruitment in progress	.083***	.030
	(.018)	(.028)
Successful recruitment	.777***	-.013
	(.028)	(.040)
	Time to hiring...	
Mean in days	48.510***	-.968
	(2.792)	(4.086)
First quartile (in days)	20.000***	0.000
	(1.980)	(3.317)
Third quartile (in days)	72.000***	-9.000
	(6.253)	(8.292)
Nb. of successful recruitment observed	178	162

Recruiters responding to the survey. Linear probability model.

Table 18: The recruitment process in details

	Intercept (nominative)	Anonymous resume
Number of candidates (median)	12.000***	0.000
	(2.107)	(2.857)
Number of interviews (median)	4.000***	0.000
	(.597)	(.785)
Share of recruitment with ...		
Phone interviews	.399***	.044
	(.034)	(.051)
Collective interviews	.067***	.014
	(.017)	(.026)
Individual interviews	.857***	-.028
	(.024)	(.037)
Tests	.224***	.011
	(.029)	(.042)
Number of selection methods	1.581***	.031
	(.059)	(.092)
Number of recruiters involved	1.897***	.059
	(.051)	(.083)
Total time recruiters have spent for...		
Phone interviews (median in minutes)	90.000***	0.000
	(11.703)	(24.329)
Individual interviews (median in minutes)	300.000***	-60.000
	(29.821)	(42.032)
Tests (median in minutes)	120.000***	0.000
	(33.596)	(69.353)
Total (median in hours)	8.500***	.500
	(.820)	(1.108)

Table 19: Match quality

	Intercept (nominative)	Anonymous resume
Successful trial period	.818***	-.016
	(.033)	(.048)
Recruiter's satisfaction (1-10 scale) about...		
early tasks	7.320***	.058
	(.159)	(.224)
Wage (monthly gross wage, without bonuses)		
Hired candidate paid the minimum wage	.220***	.008
	(.035)	(.051)
Median (except min wage earners)	1715.000***	-15.000
	(57.268)	(77.766)
First quartile (except min wage earners)	1500.000***	-50.000
	(24.865)	(49.592)
Third quartile (except min wage earners)	2200.000***	-250.000**
	(106.982)	(122.712)
Nb. of observations	141	127

Source : successful recruitment in the recruiters' survey for which the recruiter accept to communicate information on the hired candidate.

Appendix tables

Table 20: Impact of anonymous resumes: job offers with male and female applicants

	Interview			Recruitment		
Intercept	0.120***	0.185***	0.180***	0.026***	0.016	0.014
	(0.021)	(0.054)	(0.047)	(0.009)	(0.011)	(0.012)
Deprived neighborhood		-0.035	-0.013		0.007	0.007
or foreign origin		(0.043)	(0.042)		(0.017)	(0.017)
Woman		-0.086*	-0.094**		0.012	0.011
		(0.047)	(0.046)		(0.017)	(0.017)
Anonymous resumes (T)	-0.042	-0.076	-0.069	-0.013	0.001	0.012
	(0.029)	(0.065)	(0.060)	(0.010)	(0.013)	(0.015)
T × deprived neighborhood		-0.060	-0.062		-0.011	-0.008
or foreign origin		(0.057)	(0.057)		(0.019)	(0.020)
T × woman		0.119**	0.125**		-0.015	-0.025
		(0.057)	(0.055)		(0.019)	(0.022)
Observations	714	714	714	714	714	714
Job offers	311	311	311	311	311	311
R-squared	0.105	0.129	0.208	0.022	0.024	0.060

Note : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. **Source:** survey of applicants.

Table 21: Impact of anonymous resumes: job offers with applicants of foreign origin or deprived neighborhood and other applicants

	Interview			Recruitment		
Intercept	0.087***	0.092***	0.097***	0.022***	0.015*	0.026**
	(0.015)	(0.034)	(0.031)	(0.007)	(0.009)	(0.011)
Deprived neighborhood		0.010	0.016		0.003	-0.001
or foreign origin		(0.031)	(0.031)		(0.013)	(0.015)
Woman		-0.023	-0.045		0.011	-0.007
		(0.031)	(0.033)		(0.013)	(0.016)
Anonymous resumes (T)	-0.001	0.050	0.044	0.006	0.024	0.020
	(0.021)	(0.048)	(0.043)	(0.012)	(0.018)	(0.017)
T × deprived neighborhood		-0.101**	-0.089**		-0.026	-0.018
or foreign origin		(0.044)	(0.044)		(0.022)	(0.023)
T × woman		0.010	0.028		-0.009	-0.004
		(0.042)	(0.041)		(0.022)	(0.022)
Observations	1,005	1,005	1,005	1,005	1,005	1,005
Job offers	439	439	439	439	439	439
R-squared	0.086	0.098	0.138	0.025	0.028	0.068

Note : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***. **Source:** survey of applicants.

Table 22: Robustness analysis: impact of anonymous resume by origin and neighborhood of residence // with different measures of foreign origin

	Foreign origin measured as							
	Immigrant or child of immigrant		Child of an immigrant		Immigrant		Muslim or African-sounding name	
C	0.119***	0.131***	0.111***	0.121***	0.113***	0.124***	0.119***	0.132***
	(0.026)	(0.035)	(0.022)	(0.030)	(0.023)	(0.032)	(0.023)	(0.034)
Deprived neighborhood or foreign background	-0.026		-0.015		-0.018		-0.031	
	(0.031)		(0.030)		(0.030)		(0.030)	
Deprived neighborhood		0.014		-0.011		0.010		-0.009
		(0.051)		(0.036)		(0.039)		(0.040)
Foreign background		-0.040		-0.029		-0.035		-0.062*
		(0.036)		(0.042)		(0.039)		(0.037)
Deprived neighborhood and foreign background		-0.013		0.035		-0.025		0.037
		(0.063)		(0.071)		(0.059)		(0.062)
Woman		-0.023		-0.020		-0.022		-0.026
		(0.033)		(0.032)		(0.033)		(0.034)
Anonymous resume (T)	0.057	0.039	0.034	0.014	0.033	0.013	0.027	0.004
	(0.040)	(0.047)	(0.032)	(0.039)	(0.034)	(0.042)	(0.034)	(0.043)
T × deprived neighborhood or foreign background	-0.103**		-0.084**		-0.072*		-0.060	
	(0.044)		(0.039)		(0.041)		(0.041)	
T × deprived neighborhood		-0.144**		-0.061		-0.111**		-0.068
		(0.061)		(0.047)		(0.048)		(0.051)
T × foreign background		-0.102**		-0.101**		-0.063		-0.040
		(0.048)		(0.049)		(0.051)		(0.050)
T × deprived neighborhood and foreign background		0.193**		0.065		0.199**		0.058
		(0.079)		(0.082)		(0.086)		(0.081)
T × woman		0.033		0.038		0.036		0.042
		(0.045)		(0.046)		(0.046)		(0.047)
Observations	1,260	1,260	1,260	1,260	1,260	1,260	1,260	1,260
R-squared	0.128	0.130	0.118	0.120	0.117	0.121	0.118	0.120

Source: survey of applicants. Linear probability model with sampling weights. No controls. No fixed effects. **Note** : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***.

Table 23: Robustness to different samples /specifications

	Interview				Recruitment			
	benchmark	unweighted	before-after	logit	benchmark	unweighted	before-after	logit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	.131***	.117***	.125***	-1.885***	.016**	.028**	.032***	-4.127***
	(.035)	(.023)	(.027)	(.329)	(.007)	(.012)	(.010)	(.430)
Deprived neighborhood	-.028	-.027	-.022	-.301	.002	-.002	-.015	.115
or foreign origin	(.032)	(.026)	(.027)	(.328)	(.012)	(.014)	(.011)	(.537)
woman	-.022	.042	-.0002	-.234	.011	.015	.006	.521
	(.033)	(.028)	(.027)	(.344)	(.012)	(.014)	(.012)	(.540)
Anonymous resumes (T)	.042	.080**	-.007	.300	.025*	.017	-.0009	.823
	(.047)	(.038)	(.033)	(.412)	(.015)	(.018)	(.013)	(.569)
T x deprived neighborhood	-.100**	-.075*	-.041	-1.172***	-.035*	-.025	-.011	-1.194*
or foreign origin	(.044)	(.039)	(.034)	(.450)	(.020)	(.021)	(.015)	(.714)
T x woman	.028	-.055	.013	.298	.009	.008	.014	.108
	(.045)	(.040)	(.035)	(.474)	(.021)	(.022)	(.017)	(.745)

Source: survey of applicants. Linear probability model. No controls. No fixed effects. **Note** : Robust standard errors in parentheses. Significant differences at 10%, 5% and 1% are denoted by *, **, and ***.

Figure 1: Share of women among job-seekers, by position sought

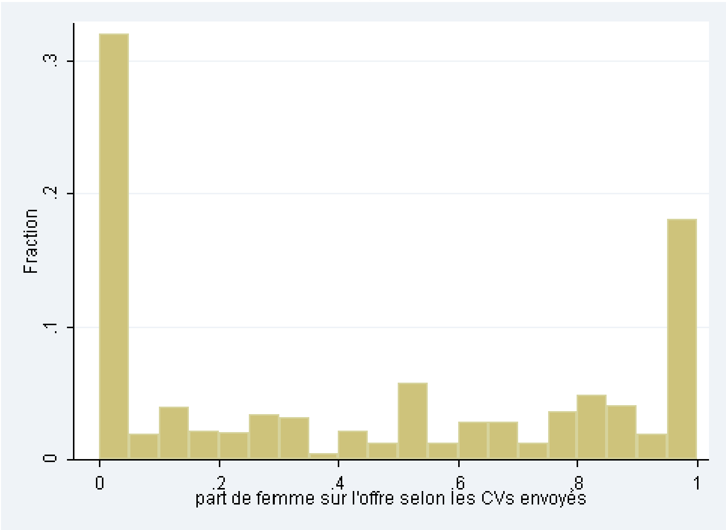

Figure 2: Share of women among resumes preselected by the PES, experimental offers