

## Unintended Effects of Anonymous Résumés<sup>†</sup>

By LUC BEHAGHEL, BRUNO CRÉPON, AND THOMAS LE BARBANCHON\*

*We evaluate an experimental program in which the French public employment service anonymized résumés for firms that were hiring. Firms were free to participate or not; participating firms were then randomly assigned to receive either anonymous résumés or name-bearing ones. We find that participating firms become less likely to interview and hire minority candidates when receiving anonymous résumés. We show how these unexpected results can be explained by the self-selection of firms into the program and by the fact that anonymization prevents the attenuation of negative signals when the candidate belongs to a minority. (JEL J15, J68, J71)*

Discrimination persists in today's labor markets, a fact that has been exposed in different contexts by correspondence testing, experiments that send out matched pairs of bogus résumés and show that minority candidates receive fewer interview offers (Riach and Rich 2002, Bertrand and Mullainathan 2004, Duguet et al. 2010).<sup>1</sup> Such evidence has led to a renewed interest for “blind” hiring procedures of various forms. In some cases, the full hiring process can take place through a screen: Goldin and Rouse (2000) famously showed that American orchestras conducting blind auditions hired more women. In other cases, only the first stage of the recruitment is made anonymous: this is the case in anonymous application procedures, such

\*Behaghel: Paris School of Economics-Inra, 48, Boulevard Jourdan, 75014 Paris, France, (e-mail: luc.behaghel@ens.fr); Crépon: Crest, 15 Boulevard Gabriel Peri, 92245 Malakoff, France (e-mail: crepon@ensae.fr); Le Barbanchon: Crest, 15 Boulevard Gabriel Peri, 92245 Malakoff, France (e-mail: tlebarbanchon@gmail.com). Address correspondence to Thomas Le Barbanchon. We thank participants at various conferences (Society of Labor Economists (SOLE) 2012, European Society for Population Economics (ESPE) 2012, European Economic Association & Econometric Society (EEA-ESEM) 2012, European Association of Labor Economists (EALE) 2012, Allied Social Science Association (ASSA) 2013, New Opportunities for Research Funding Co-operation Agency in Europe (NORFACE 2013)) and seminar participants at Massachusetts Institute of Technology, Columbia University, Harvard University, Institute for the Study of Labor (IZA), Institute for Employment Research (IAB), Center for Research in Economics and Statistics (CREST), Council for Economic Education (CEE), Cergy University, Institute National D'Études Démographiques (INED), and Paris School of Economics for their helpful comments. We are especially grateful to Kate Antonovics, David Autor, Marianne Bertrand, David Blau, Raj Chetty, Laurent Davezies, Larry Katz, Liam Wren-Lewis, Philip Oreopoulos, Manasa Patnam, Roland Rathelot for comments on earlier drafts of this paper. We thank the French public employment service, Pôle Emploi, for its commitment in implementing this experiment. Pôle Emploi also provided access to data and financial support for this study. We thank Andrea Lepine, Julie Moschion and Pascal Acharid for excellent research assistance, Abderrazak Chebira, Yann Algan, Corinne Prost and Hélène Garner for very helpful discussion on the measurement of applicants' origin.

<sup>†</sup>Go to <http://dx.doi.org/10.1257/app.20140185> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

<sup>1</sup>In what follows, we will describe as “discrimination” situations where recruiters treat candidates differently based on their origin (country of birth or of citizenship, or place of residence), keeping other information on the candidates constant. With this definition, a situation in which changing the applicant's name from a majority-sounding one into a minority-sounding one reduces callback rates is described as “discrimination against the minority.” Similarly, a situation in which hiding minority-sounding and majority-sounding names by anonymizing résumés improves the relative callback rates of candidates with minority-sounding names is described as “discrimination against the minority.”

as the masking of identifying characteristics in résumés (Aslund and Skans 2012; Krause, Rinne, and Zimmermann 2012). Clearly, such procedures may be costly to operate, so an important issue is whether or not to make them mandatory. To our knowledge, no government has passed and enforced laws mandating anonymous résumés. This paper analyzes a middle ground between laissez-faire and mandatory anonymization by evaluating a program in which the French government promoted anonymous résumés with the explicit goal of making blind procedures a “reflex” to employers. The program was implemented in 2010 through Pôle Emploi (the main public employment agency), which offered to send anonymous résumés (instead of name-bearing ones) of prescreened candidates to firms posting jobs.

The public debate about the use of anonymous résumés is often quite contentious. In order to improve the hiring prospects of minority candidates, anonymous résumés should not only impact the screening of résumés, but also hiring decisions once the candidate’s minority status is revealed. Proponents of the measure argue that this will be the case if the nature of discrimination is implicit in the first place: recruiters make quick decisions when screening résumés and may implicitly attribute bad signals to minority workers (Bertrand, Chugh, and Mullainathan 2005); this will not occur during interviews, as they have more time to make decisions. Opponents argue that if employers consider that a majority type is a positive signal or characteristic, anonymous procedures will not prevent them from using that information at the interview stage. They may even interview more candidates when all the résumés they receive are anonymous, in order to make sure to see at least one candidate of their preferred group. In addition, “race-blind” screening processes are not necessarily “race-neutral”: if there is a correlation between minority status and other elements in the résumés, it may not be difficult for recruiters to reconstruct missing information and continue to discriminate.<sup>2</sup>

In this paper, we evaluate an experimental program in which the French public employment agency, Pôle Emploi, offered firms a voluntary anonymous résumé procedure for their recruitments. The design was as follows: when Pôle Emploi identified a suitable vacancy, it asked the recruiter to participate in the program. Firms that refused benefited from the standard service by Pôle Emploi, and received the regular, name-bearing résumés of prescreened candidates. When a firm accepted, Pôle Emploi’s caseworkers first went through the standard prescreening process to come up with a suitable pool of candidates. The matched-up vacancy/candidate pool was then assigned either to a group with a standard recruitment procedure or to a group where all candidates’ résumés were made anonymous (the upper block of the résumés was suppressed: name, address, nationality, ID picture...). Candidates and caseworkers did not know whether résumés would be anonymized or not, guaranteeing that the impact of anonymous résumés on firms’ reactions was not confounded by changes in the pool of candidates.

We analyze the impact of anonymous résumés on the interview and hiring rates of minority candidates—namely residents of deprived neighborhoods, immigrants, or children of immigrants—compared to other (majority) candidates. Drawing on a

<sup>2</sup>See for instance Persico (2009). Similarly, Autor and Scarborough (2008) study the conditions under which “race-blind” job testing are not necessarily “race-neutral.”

sample of about 600 firms participating in the program, we find the surprising result that anonymization leads to a large and significant widening of the gap in interview rates: the interview rate of minority candidates decreases, while that of majority candidates increases. We also find that the hiring rate gap widens, but the effect is smaller than that of interview rates, and only significant at the 10 percent level. This adverse effect on minority candidates is the exact opposite effect to what policymakers had hoped, and a surprising result given existing evidence from correspondence testing in France (Dugué et al. 2010), which shows discrimination against minority candidates for some jobs, no discrimination for others, but never discrimination against majority candidates.<sup>3</sup>

We consider two mechanisms that could explain these results. First, we document firm selection into the program. Among client firms of Pôle Emploi posting suitable jobs, 62 percent accepted the invitation to participate in the program. Participating firms are similar to refusing firms in most observable dimensions, with a significant exception: they tend to interview and hire relatively more minority candidates (when using standard résumés). These facts suggest that the nonmandatory program acted as a separating device which led firms that tend to treat minority candidates less favorably to decline participation, and those that treat them more favorably to accept. Anonymization then prevented selected firms from treating minority candidates more favorably during the experiment.

As a second step, we analyze how participating firms value résumé attributes in the standard and anonymous procedures. In particular, we test the implications of statistical discrimination models à la Aigner and Cain (1977), in which the returns to résumé characteristics may depend on the candidate's minority or majority status. Minority candidates with negative signals (e.g., unemployment spells) can be penalized if anonymous résumés prevent recruiters from interpreting those signals in light of the circumstances faced by the candidates (e.g., adverse labor market conditions faced by minorities). To test this hypothesis, we took all the résumés involved in the experiment, anonymized them, and asked counselors from the public employment agency to rate them in the way recruiters would do. Along with the rating, we also obtained auxiliary information on candidates such as adequate experience and consistency of applicant skills with the position, among others. Consistent with Aigner and Cain (1977), we find that some attributes have differential returns between minority and majority candidates. This is the case for interrupted labor market histories (possibly signaling unemployment spells), which are strongly negatively valued for majority candidates but not for minority candidates. We also find that recruiters examining anonymous résumés update the value they attach to a particular attribute by averaging across the minority and majority groups. As a consequence, interrupted work histories, for example, are now negatively valued for minority candidates under anonymization, and penalize majority candidates less. The change in the valuation of

<sup>3</sup>Importantly, we follow control firms after the program and document that their recruitment behavior is not different during its duration. Thus, the results are not an artifact of the experiment due to a change in the behavior of the control group. Accordingly, had we conducted a correspondence study (in which firms do not know that they are observed) on the firms participating in the program, it is likely that we would have found that, in these firms, minority candidates are more often interviewed than majority ones.

the résumés under treatment and control explains more than half of the change in the interview gap between majority and minority candidates.

The paper unfolds as follows. Section I provides some information about the political context in which the program took place. Section II presents the experimental design, data collection and description. Section III shows impacts on interview and hiring gaps, and on other outcomes related to the hiring process and match quality. Section IV analyzes firms' selection into the program and how participating firms value résumés in the standard and anonymous procedures. Section V concludes.

## I. Institutional Background

The first significant mention of anonymous résumés in the French political discourse was in an official report by Claude Bébéar (2004), the CEO of AXA, the main French insurance company, which had for several years been using this procedure for its own recruitment.<sup>4</sup> The recommendation to use anonymous résumés in this report came along with several other suggestions aimed at sensitizing recruiters and developing reporting tools on ethnic diversity within companies, despite the fact that collecting explicit data on race is forbidden by French law (even in surveys, individuals cannot be asked whether they are black or white, Caucasian or African, etc.). At that stage, the purpose of anonymous résumés was to prevent hiring discrimination against women and older workers as much as against ethnic minorities or residents of deprived neighborhoods. Following Bébéar's report, Parliament debated and rejected a legislative amendment put forward by a conservative member that would have made anonymous résumés mandatory for firms with more than 250 workers. Bébéar himself did not support the amendment, arguing that "All the recommendations in my report are based on firms' volunteering.... A law might even be counterproductive."

The debate on anonymous résumés was revived after the riots that took place in the greater Paris area in November 2005. A law passed in April 2006 (the Equality of Chances Act) made anonymous résumés mandatory for firms with more than 50 employees. But the government did not issue the "application decrees" needed to enforce the law.<sup>5</sup> In 2009, it finally decided to launch a program where firms would be invited to adopt an anonymous résumé hiring procedure for their recruitments made through the public employment service. Having the program implemented by Pôle Emploi was in line with its traditionally important role in labor market intermediation: until 2005, Pôle Emploi had had a legal monopoly on the placement of unemployed job seekers and, at least in theory, all vacancies were supposed to be posted by the agency.<sup>6</sup> Furthermore, Pôle Emploi had developed several advanced

<sup>4</sup>In French: [http://www.larevueparlementaire.fr/pages/RP875/RP875\\_AN\\_enbref.htm](http://www.larevueparlementaire.fr/pages/RP875/RP875_AN_enbref.htm).

<sup>5</sup>The law itself specified neither who was supposed to actually anonymize résumés (and what this meant in practice), nor what sanctions would be levied against those who do not comply.

<sup>6</sup>In 2009, 2,438,700 jobs lasting more than one month were posted through Pôle Emploi. This corresponds to 36 percent of hires concerning the same type of jobs. Bessy and Marchal (2007) show that public intermediaries (mostly, Pôle Emploi) are the third channel of recruitment (spontaneous job applications and firm network come first and second), and represent 19 percent of recruitments in indefinite duration contracts. The use of the public employment services (PES) does not differ significantly by industry or firm size; it is however more frequent for firms with a high turnover who set up more standardized recruitment processes and hire at lower wages (close to the minimum wage); it is less frequent for managerial positions.

free services for businesses, such as prescreening candidates before sending their résumés to hiring firms.

## II. Experiment and Data Collection

### A. Program and Experimental Design

In this section, we present the program and the experimental design used to measure the impact of anonymous résumés. The program was conducted over ten months in eight (out of 100) French *départements*, at branches of the public employment service (PES) located in urban areas. It proceeded as follows:

- (i) *Firm entry in the program:* Firms posting vacancies at the PES have the option to ask for a PES agent to make a first screening of applicants based on their résumé. About 50 percent ask for this service, and subsequently receive only selected résumés from Pôle Emploi (from a couple to a dozen, in most cases), instead of résumés sent directly by applicants. This service is free. During the period of the program, all firms with more than 50 employees posting a job lasting at least three months and asking for this service are eligible for the program. They are told that their vacancy will be randomly assigned to the anonymous or standard procedure, with probability 1/2. Firms are free to decline to participate. A given firm enters the program at most once: firms that have already participated are no longer offered the program.
- (ii) *Matching of résumés with vacancies:* The PES posts the vacancy on a variety of media, including a public website asking interested job seekers to apply through the PES branch. On the webpage of the vacancy, one sentence informs job seekers that there is an experiment going on.<sup>7</sup> The PES agent selects résumés from these applicants and from internal databases of job seekers. A first pool of résumés is thus matched with the vacancy.
- (iii) *Randomization and anonymization:* The résumés are sent to research assistants in charge of the randomization at the central PES offices. Vacancies and their first pool of résumés are together randomly assigned (using a random number generator) to treatment or control group, with probability 1/2. If the vacancy is assigned to the treatment group, all the résumés are given a number and anonymized by the research assistant;<sup>8</sup> then they are sent back to the PES agent in charge of the vacancy follow-up.
- (iv) *Selection of résumés by the employer:* The employer selects the résumés of applicants she would like to interview. Control group employers contact

<sup>7</sup>Except for this sentence, the experiment is not advertised among job seekers. Still, it could be that this sentence attracted minority candidates (“calling effect”). We do not find evidence for this: the share of applicants with an African- or Muslim-sounding name is the same in vacancies participating in the experiment as in those not participating.

<sup>8</sup>The degree of anonymization is described below.

the applicants directly; treatment group employers give the PES agent the résumés' numbers so that it is the PES agent who sets up the hiring interviews, in order to maintain applicants' anonymity.<sup>9</sup>

This experimental design calls for a few comments:

*Anonymization Is Limited.*—Anonymization consisted of erasing the top part of the résumé: name, address, gender, nationality, ID picture, age, marital status, and number of children. However, it did not imply any further standardization of the content of the résumé. In particular, information on gender could be gathered from gender-specific terms used in the main part of the résumé, neighborhood of residence and age could be partly inferred from information on where and when the applicant graduated from high school, and ethnicity could be spotted from foreign language skills. To further anonymize résumés would have implied much more complex logistics during the experimental program, and in any case it was felt that such complex procedures could not have been standardized on a large scale.<sup>10</sup>

*Randomization Occurs after Firms Have Agreed to Participate.*—The estimated impact of anonymous résumés only holds for this potentially selected group of firms. But this design also allows us to increase statistical power, as there is perfect compliance in the experimental sample.

*Randomization Occurs at the Vacancy Level.*—For a given vacancy, all résumés sent to recruiters by the PES are treated identically (either anonymous, or standard). This level of randomization ensures that, within the pool of candidates applying through the PES, anonymous résumés do not compete with standard résumés. However, the PES is not the only channel for recruitment: firms may also receive applicants from other sources, and these résumés are not anonymized. We measure below whether firms substitute these other channels for the PES in response to anonymization.

*Randomization Occurs after Matching Résumés to Vacancies.*—Had the pre-screening of résumés by the PES occurred after randomization, the PES agent could have selected different applicants for vacancies with anonymous résumés (consciously or not). This would have affected the comparability of treatment and control applicants. To avoid this, a first pool of résumés was selected before randomization occurred. All analyses below are restricted to this first pool, as it contains résumés that are by construction statistically identical in the control and treatment groups.

During the ten months of the program (November 2009 to September 2010), 1,613 firms were invited to enter the program; 1,005 (62.3 percent) agreed to participate, and the remaining 608 (37.7 percent) declined. Whether participating firms self-selected

<sup>9</sup>If the employer could not fill the position with the first pool of résumés, she requests additional pools. The PES sends a new pool of selected résumés with the same format as for the first pool. Our main analysis excludes the additional pools, as PES counselors knowing the format could have altered their selection.

<sup>10</sup>Online Appendix Figure A-1 provides an example of a (short) anonymous résumé; notice that the gender of the applicant can be inferred from gender-specific terms used; also, given the date of graduation from high school, the person was probably about 25 at the time of the program.



on characteristics related to discrimination is one of the questions of the evaluation, and is analyzed in detail below. In what follows, we first describe the data collection.

### B. Data Collection

We collected both administrative and survey data. The administrative data cover all firms and all job seekers who used the public employment services in the experimental areas during (and after) the program. It includes basic information on the firm (size, sector), on the job position offered (occupation level, type of contract) and limited information on candidates (unless the candidate is filed as unemployed).<sup>11</sup> In what follows, the administrative data are mostly used to characterize the population of firms entering the program, compared with the broader population of firms interacting with the PES.

We conducted telephone interviews with all firms entering the program and with a subsample of firms that declined to participate. We also interviewed subsamples of applicants to these two groups of firms. Lastly, a subsample of applicants to vacancies posted *after the program* by the control group and nonparticipating firms was also interviewed: as detailed below, the goal was to check whether control firms behaved differently because they knew they were in an experiment. The surveys used for applicants (respectively, for firms) were similar across subsamples. We will describe these two surveys briefly here, and present specific questions when they contribute to the subsequent analysis.<sup>12</sup>

As shown in Table 1, there are four groups of sampled firms. There are 385 control and 366 treatment firms that participated in the experimental program and went through the randomization. In addition, 254 firms accepted the program but were not randomly assigned to treatment or control; they canceled or filled the job opening before a first pool of résumés was collected and randomization could take place.<sup>13</sup> All were sampled; response rates are around 60 percent, and the response rate difference between control and treatment firms is not statistically significant. Finally, 608 firms declined to participate in the program and were sampled with lower sampling rates. Response rates are also somewhat lower, as might be expected.

The lower panel of Table 1 details the sample of applicants. Overall, response rates of applicants to participating firms are around 65 percent; even if they are higher in the control group, the difference is not statistically significant (the  $p$ -value is 0.27). The survey thus yields a total sample of 1,268 applicants to participating firms, which constitutes our main sample to analyze the impact of anonymous résumés. This main sample is complemented with 203 applicants to nonparticipating firms, sampled at a significantly lower rate, yielding a secondary sample with

<sup>11</sup> It also provides a follow-up of the recruitment process until the position is filled or the vacancy is withdrawn; however, the quality of that follow-up is weak, and some critical information is missing. (In particular, the data do not show whether the candidate was interviewed before the firm rejected his/her application.)

<sup>12</sup> Survey tools are available upon request.

<sup>13</sup> This underscores the fact that many firms actually fill their positions quickly without any help from the PES, a point we will discuss below.

TABLE 1—SAMPLE SIZE AND RESPONSE RATES IN THE APPLICANT AND FIRM SURVEYS

	Population size (a)	Number sampled for survey (b)	Number of respondents (c)	Sampling rate (b/a)	Response rate (c/b)
<i>Panel A. Firms</i>					
Participating	1,005	1,005	575	1.00	0.57
Control	385	385	229	1.00	0.60
Treatment	366	366	212	1.00	0.58
Withdrew before randomization <sup>a</sup>	254	254	134	1.00	0.53
Not participating	608	335	146	0.55	0.44
Total	1,613	1,340	721	0.83	0.54
<i>Panel B. Applicants</i>					
In participating firms	3,965	1,957	1,268	0.49	0.65
Control	2,035	1,003	660	0.49	0.66
Treatment	1,930	954	608	0.49	0.64
In nonparticipating firms	8,583	378	203	0.04	0.54
Total	12,548	2,335	1,471	0.18	0.63

*Notes:* Panel A displays population and sample size among firms invited to participate in the program. They are split into participating and nonparticipating firms; participating firms are then subdivided into firms that receive anonymous résumés (treatment) or not (control) and firms that withdrew their vacancies before randomization. The lower panel displays corresponding numbers for candidates applying to those firms. For participating firms, we restrict the population to the first pool of candidates sent to the recruiter (prescreened before randomization). This cannot be done for nonparticipating firms. As a result, the total number of candidates in participating and nonparticipating firms displayed in the table are not comparable. For the survey of applicants in nonparticipating firms, the sampling was twofold: first we selected a subsample of vacancies among firms that posted vacancies both during and after the experiment (this enabled us to test for a potential John Henry effect below) and then from among those job vacancies, we selected a subsample of applicants.

<sup>a</sup>This corresponds to firms accepting to participate that canceled or filled the opening before a first pool of résumés was collected and randomization could take place. By definition, there are no candidates to survey in the sample of firms withdrawn before randomization.

1,471 observations, which we use to describe the recruitments that were offered to participate in the program.<sup>14</sup>

The firm survey has two main functions. The first is to measure additional characteristics of the vacancy and of the recruiter (characteristics that could be associated with a differential treatment of candidates). Second, the survey includes detailed questions on the result of the recruitment. It measures the time to hiring and the match quality. The first column of Table 2 presents descriptive statistics on firms that were invited to participate in the program.<sup>15</sup> The upper panel uses administrative data, while the bottom one uses information from the firm survey (on the sample of 721 responding firms, see Table 1). With the administrative data, it is possible to compare firms targeted by the program<sup>16</sup> to the broader sample of firms posting jobs at the PES in the experimental areas and around, during the program.<sup>17</sup> Note that only firms with more than 50 employees were invited to participate, resulting in a

<sup>14</sup> 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.

<sup>15</sup> This table pools all firms irrespective of their decision to participate in the program or not. The self-selection of participating firms is analyzed below.

<sup>16</sup> The sample of firms targeted by the program is further disaggregated to compare participating firms with firms that refused to participate: see Section IIIA below.

<sup>17</sup> The available administrative data do not allow us to isolate the geographical area covered by the program. This explains the large number of observations (45,283).



TABLE 2—CHARACTERISTICS OF FIRMS AND VACANCIES

	Offered to participate in the program	Other client firms of the PES
<i>Panel A. Administrative data</i>		
Firm with fewer than 100 employees	0.31	0.64
Firm with 100 to 200 employees	0.16	0.02
Firm with more than 200 employees	0.53	0.34
Nonmarket services	0.24	0.18
Market services	0.57	0.56
Manufacturing	0.13	0.08
Construction	0.03	0.09
Upper occupations	0.07	0.03
Intermediary occupations	0.24	0.11
Skilled white or blue collar	0.59	0.63
Unskilled white or blue collar	0.11	0.23
Indefinite duration contract	0.64	0.51
Temporary contract for more than six months	0.19	0.25
Observations	1,613	45,283
<i>Panel B. Firm survey</i>		
Involves teamwork	0.81	
Frequent customer contact	0.71	
Recruiters' characteristics		
Woman	0.63	
College graduate	0.58	
French as mother tongue	0.96	
Immigrant	0.03	
Child of immigrant	0.08	
At least one friend (in 5) with Muslim or African name	0.26	
At least one colleague (in 5) with Muslim or African name	0.27	
Observations	721	

*Note:* The two columns present mean values of each variable on the population of firms eligible to enter the program and those ineligible.

*Source:* PES administrative file (panel A) and firm survey (panel B). All vacant jobs.

sample skewed toward large firms: half have more than 200 employees, compared to only one third among the rest of PES client firms. Most of them deliver services: 57 percent in the market sector and 24 percent in the nonmarket sector. Most firms in the program offer skilled positions with open-ended contracts.<sup>18</sup>

Table 3 reports some descriptive statistics on candidates, pooling together firms that did and did not participate in the program. We first consider majority and minority candidates together (column a). Forty-six percent of the candidates are women, 21 percent are younger than 26, and 8 percent are over 50.<sup>19</sup> We call “minority” individuals those who reside in deprived neighborhoods or have a foreign background. This is exactly the target group of the anonymization law that was implemented in response to the 2005 riots in Paris. To characterize deprived neighborhoods of residence, we use

<sup>18</sup> Note that firms posting a job lasting under three months were not eligible to the program.

<sup>19</sup> As noted in the above example (online Appendix Figure A-1), indications of the candidate's gender are likely to appear in the résumé even when its upper part is erased. Age can also be guessed—from the date of the last diploma obtained. In this paper we do not consider the effect of anonymization on the differential treatment based on gender or age, but our results are robust to adding these dimensions in the analysis.

TABLE 3—CANDIDATES' CHARACTERISTICS

	All (a)	Majority (b)	Minority (c)	Diff-test <i>p</i> -value (b)–(c)
<i>Panel A. Candidates' survey</i>				
Women	0.46	0.55	0.36	0.01
Under 26 years old	0.21	0.22	0.21	0.72
Over 50 years old	0.08	0.07	0.09	0.57
Deprived neighborhood (1)	0.24		0.50	
Immigrant (2)	0.22		0.45	
Child of immigrant (3)	0.17		0.36	
Minority: (1), (2), or (3)	0.48		1.00	
African- or Muslim-sounding name	0.21	0.02	0.42	0.00
Professional degree	0.27	0.25	0.28	0.63
High school diploma	0.19	0.17	0.21	0.20
Upper education degree	0.47	0.50	0.42	0.19
Relevant experience (years)	4.16	4.57	3.71	0.04
Long-term unemployed	0.42	0.40	0.45	0.40
Reservation wage is min wage	0.50	0.51	0.50	0.93
Observations	1,471	658	813	
<i>Panel B. Coding of résumés</i>				
Interrupted work history	0.33	0.34	0.32	0.70
Adequate skills	0.55	0.53	0.56	0.51
Adequate work experience	0.58	0.61	0.56	0.27
Uncertain rating	0.54	0.51	0.56	0.24
Observations	1,140	504	636	

*Notes:* The first column presents mean values of each variable on the population of candidates applying to vacant jobs eligible to the program (control, treatment, or nonparticipating firms; firms without randomization have no candidates sent through the PES). Columns (b) and (c) compare majority and minority candidates applying to vacancies participating to the program (*p*-value of the test for identity in the last column). Survey sampling weights are used.

*Source:* Candidates' survey

administrative classifications of neighborhoods defined as a means to target subsidies or tax exemptions, since their boundaries closely match socioeconomic geographical disparities.<sup>20</sup> Moreover, one of the alleged perverse effects of such administrative classifications is to create a stigma effect, making them particularly relevant to assess the impact of anonymization. The next issue is to measure discrimination risk associated with foreign origin or ethnicity, given that French law forbids the use of ethnic categories that would label someone as white, black, or African-French, for instance. Instead, we take a two-fold approach. First, in the spirit of correspondence testing studies (Bertrand and Mullainathan 2004), we code whether the applicants' first names have a foreign-sounding origin. Following research by Felouzis (2003), we use the etymology of the name. Muslim first names are identified from a database created by Chebira (2005). Second, we use the place of birth and the citizenship at birth. Immigrants are defined as those born abroad who did not have French citizenship at

<sup>20</sup>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 United States. About 7 percent of the French population lives in ZUS, and 12 percent in ZUS or CUCS.

birth. Children of immigrants are those whose father was born abroad and did not have French citizenship at birth.<sup>21</sup> The two approaches—based upon name or the migration status—are complementary. In some cases, a foreign-sounding name is the only signal that appears on the résumé. But in other cases, immigrants may have a French-sounding first name although their origin can be inferred from other signals on the résumé (for instance, their last name or an ID picture).

In Table 3, roughly one in four applicants lives in a deprived neighborhood; the same proportion has a Muslim- or African-sounding name. One in five is an immigrant, and that proportion rises to two in five if the category is enlarged to be immigrants or children of immigrants.<sup>22</sup> The different measures of origin are correlated. Of particular interest is the correlation between the name and the migration status: clearly, African- or Muslim-sounding names correspond to applicants with a foreign origin (90 percent); however, a significant fraction of migrants (including those from Africa) do not have African- or Muslim-sounding first names (55 percent). The variables based on immigration (as declared during the surveys) may better capture the risk of discrimination when that origin can be inferred from other signals in the résumé. We perform robustness checks with these alternative measures below. In Table 3, we also describe the résumés of the majority and minority candidates.<sup>23</sup> Minority applicants are more frequently men. They have less work experience relevant to the job vacancy. There are no significant differences in terms of age, education, and duration of unemployment.

As a last source of information, we had access to the résumés of a subsample of candidates associated to firms that participated in the experiment. Specifically, we had access to résumés that were submitted in PDF or doc formats (this excludes a minority of applications that went through an online form on the PES website, to which we did not have access): this amounts to 1,140 résumés out of 1,268 résumés in the main sample. We then asked 16 PES counselors to act as recruiters and to code the résumés, by answering a dozen questions about the adequacy of the qualification, experience and education of the candidates. Finally, the PES counselors were asked to give an overall grade to the candidate and state whether they were certain or not about that grade. In the analysis, we use four binary variables stemming from this coding exercise, indicating whether the candidate has adequate skills, adequate work experience, whether there are unexplained interruptions in his/her work history (possibly signaling unemployment spells), and whether the quality of the application is particularly uncertain.<sup>24</sup> The lower part of Table 3 shows no significant differences in these characteristics. Overall, the table shows limited differences between minority and majority candidates in the experimental sample. One should keep in mind that all this concerns a sample of candidates who have been

<sup>21</sup> 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.

<sup>22</sup> Among immigrants or children of immigrants, 45 percent come from North African or oriental countries, 29 percent from the rest of Africa, 9 percent from Southern Europe, and 6 percent from Eastern Europe. Other zones (Asia, America, Western Europe) are marginally represented (less than 3 percent).

<sup>23</sup> In Table A-2 in the online Appendix, we compare majority and minority candidates applying to vacancies participating in the experiment and to vacancies not participating.

<sup>24</sup> The questions of the grid used to code the résumés are available upon request.

prescreened by PES agents. Differences between the minority and majority may be much larger in the whole population of candidates.

### III. Impact of Anonymous Résumés

Our experimental design allows us to directly estimate the impact of anonymous résumés on recruitment outcomes in participating firms. We focus on the program's impact on the gap between interview (or hiring) rates of minority and majority applicants. Denote  $Y$  the variable measuring the fact that the applicant was received in interview,  $D \in \{0, 1\}$  the indicator variable corresponding to the candidate being from the minority group, and  $An \in \{0, 1\}$  the indicator variable corresponding to candidates applying to a vacancy with anonymous applications (treatment group). The parameter of interest can be written as:

$$(1) \quad \delta = (\bar{Y}^{An=1, D=1} - \bar{Y}^{An=1, D=0}) - (\bar{Y}^{An=0, D=1} - \bar{Y}^{An=0, D=0}),$$

and can be estimated as the coefficient on  $D_i \times An_j$  in a regression of  $Y_{ij}$  on  $D_i$ ,  $An_j$  and  $D_i \times An_j$  (where candidates are indexed by  $i$  and vacancies by  $j$ ).

#### A. Interview Rates

We first contrast the interview rate of candidates applying to control vacancies with that of candidates whose résumés were sent in anonymous form to recruiters. Due to the experimental design, this comparison can be interpreted as causal.<sup>25</sup> Table 4 shows the interview rates for different samples (first row for the control group and second row for the treatment group). The average interview rate in the control group is around 10.5 percent. Recruiters are quite selective when they screen résumés. The average interview rate in the treatment group is 11.3 percent. The difference between the two interview rates is not statistically significant (standard errors are clustered at the vacancy level). This means that the average number of interviews per vacancy does not significantly increase when résumés are anonymous.

In column 2 of Table 4, we report interview rates for the minority group. The interview rate drops from 9.3 percent in the control group to 4.7 percent in the treated group. The difference is statistically significant at the 5 percent level. Minority workers are actually harmed by the anonymization procedure. In column 3, the interview rate of majority workers increases from 11.6 percent to 17.7 percent due to anonymization. This increase is not statistically significant. Both effects imply that the gap in interview rates between majority and minority candidates actually widens by 10.7 percentage points (statistically significant at the 5 percent level, see column 4). This increase in the interview gap against minority candidates is the exact opposite impact to what policymakers intended.<sup>26</sup>

<sup>25</sup> Tables A-3 and A-4 in the online Appendix show that the treatment and control samples are balanced, with two exceptions: candidates in treatment firms have significantly more adequate skills and work experience. We check that our results are robust to controlling for these differences (column 3 of online Appendix Table A-8).

<sup>26</sup> Note that the gap in interview rates in the standard procedure is small (−2.4 percentage points) and not statistically significant. This is qualitatively close to Table 7, even though the sample is slightly different. Again,

TABLE 4—IMPACT OF ANONYMOUS APPLICATIONS ON INTERVIEW AND HIRING RATE GAPS BETWEEN MINORITY AND MAJORITY APPLICANTS

	All	Minority (D)	Majority (ND)	Gap (D–ND)
<i>Panel A. Interview rates</i>				
Standard (c)	0.105 (0.016)	0.093 (0.017)	0.116 (0.026)	–0.024 (0.031)
Anonymous (t)	0.113 (0.017)	0.047 (0.011)	0.177 (0.030)	–0.130*** (0.032)
Effect (t–c)	0.008 (0.023)	–0.046** (0.020)	0.061 (0.040)	–0.107** (0.045)
Number of candidates	1,268	696	572	1,268
Number of vacant jobs	598	418	385	598
<i>Panel B. Hiring rates</i>				
Standard (c)	0.022 (0.006)	0.023 (0.008)	0.021 (0.009)	0.002 (0.012)
Anonymous (t)	0.035 (0.010)	0.017 (0.007)	0.052 (0.017)	–0.035** (0.018)
Effect (t–c)	0.013 (0.011)	–0.006 (0.010)	0.031 (0.019)	–0.037* (0.021)
Number of candidates	1,268	696	572	1,268
Number of vacant jobs	598	418	385	598

Notes: Panel A considers interview rates, panel B considers hiring rates. For panel A, for example, the first column gives the interview rate in standard vacant jobs (first row), in vacant jobs with anonymous applications (second row) and the difference between the two (third row). The second column D does the same computations but for applicants from the minority while the third column ND does it for applicants from the majority. The last column considers the difference between column D and column ND. Survey sampling weights are used. Standard errors are clustered at the vacant job level.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

Source: Candidates' survey

## B. Hiring Rates

Panel B of Table 4 reports hiring rates for minority and majority candidates. The difference between the first and second row can be interpreted as causal, because these are unconditional hiring rates.<sup>27</sup> The hiring rate of minority workers is 2.3 percent in the standard procedure. It decreases to 1.7 percent in the anonymous procedure. The difference is not statistically significant. The hiring rate of majority workers increases from 2.1 percent in the control group to 5.2 percent in the treatment group. Again the difference is not significant. However, the estimate of the program impact on the hiring gap, –3.7 percentage points, is statistically significant at the 10 percent level. This suggests that the negative relative impact of anonymous résumés on

this cannot be interpreted as evidence that there is little discrimination (or of any kind of differential treatment of candidates according to their type), as the effect of the candidate's type may be confounded by differences in credentials as shown on the résumés (observed by the recruiter and not by the econometrician). As a robustness check, we introduce control variables in the analysis below. We further discuss the interpretation of the results with regard to covariates in Section IV.

<sup>27</sup> Difference between hiring rates computed on the population actually interviewed can be biased as anonymization changes the composition of the pool of interviews.

minority candidates persists until the recruitment stage, even though it is less precisely estimated. This finding goes against the argument of opponents to anonymous applications who argue that the practice will only postpone discrimination behavior to later stages of the recruitment, when the applicant's type is revealed.

### *C. Recruitment Success*

The first two panels of Table 5 show the time needed to fill the vacancy, and the share of vacancies that remain unfilled. This may provide indirect evidence on whether anonymous résumés affected the firms' profits. We do not find any effect, however. Four recruitments out of five are successful in both treatment and control groups, and successful recruitment processes lasted an average of 48 days, whatever the procedure.

A related question is whether receiving anonymous résumés from the PES led some recruiters to rely more on other recruitment channels. The middle panel in Table 5 shows that PES candidates only represent one third of hirings, illustrating the importance of other recruitment channels (such as staff referrals, unsolicited applications, etc.). However, that share is not reduced by the introduction of anonymous résumés. As such, there is no evidence of a crowding-out effect (a frequent fear at the beginning of the program).

Lastly, thanks to the recruiters' survey, we have proxy measures for the quality of the candidate eventually hired. Each new contract starts with a trial period (of up to three months). We asked recruiters whether the recruitment was confirmed at the end of the trial period and whether they are satisfied with the performance of the hired candidate over that period. In the bottom panel of Table 5, we report the impact of anonymous résumés on the corresponding variables. Around 80 percent of recruiters hire the candidate past their trial period and recruiters rate candidates on average 7 (on a 1–10 scale). The anonymous procedure does not significantly affect indicators for the quality of the match. The wage paid to the hired candidate is another potential measure of the match quality. Wages paid to new hires do not seem to be affected by anonymous résumés; the same fraction of hired candidates are paid the minimum wage in the treatment and control groups. For those paid above minimum wage, the median wage is around 1,700 euros per month in both groups.

Overall, even though anonymous résumés did change the types of candidates interviewed and also probably the types of candidates hired, we do not find any significant impact on the satisfaction of recruiters, the negotiated wage, or other measures of recruitment success. Even though the estimates are not very precise, they allow us, in a context where the PES was in charge of the anonymization, to reject the claim that this procedure would have a very high cost for firms.

### *D. Robustness Checks*

We perform a variety of tests to check the robustness of our key results to potential threats to the experimental design, as well as to alternative measurement and model specifications.



TABLE 5—IMPACT ON THE RECRUITMENT PROCESS AND MATCH QUALITY  
(From the recruiters' point of view)

	Standard application	Treatment effect	Number of vacant jobs
<i>Panel A. Recruitment process status</i>			
Recruitment canceled	0.140*** (0.023)	−0.022 (0.032)	441
Recruitment in progress	0.083*** (0.018)	0.030 (0.028)	441
Recruitment completed	0.777*** (0.028)	−0.013 (0.040)	441
<i>Panel B. Time to hiring</i>			
Mean in days	48.1*** (4.4)	−1.7 (5.6)	307
First quartile (in days)	19.0*** (2.0)	1.0 (2.9)	307
Third quartile (in days)	76.0*** (6.6)	−13.0 (8.2)	307
<i>Panel C. Substitution between PES or non-PES candidates</i>			
Share of PES candidates among applications	0.563*** (0.025)	0.001 (0.038)	354
Share of PES candidates among interviews	0.230*** (0.023)	0.056 (0.037)	341
Share of PES candidates among hirings	0.348*** (0.036)	0.010 (0.052)	340
<i>Panel D. Match quality</i>			
Successful trial period	0.82*** (0.03)	−0.02 (0.05)	240
Recruiter's satisfaction (1–10 scale) about early tasks	7.32*** (0.16)	0.06 (0.22)	220
Hired candidate paid the minimum wage	0.22*** (0.04)	0.01 (0.05)	268
Median wage (except min wage earners)	1.715*** (57)	−15 (78)	177

Notes: Panels A and B of the table provide information about the recruitment status and its duration. Panel C provides information about the share of candidates applying through the public employment service at the application stage, the interview stage and hiring stage. Panel D provides information about the characteristic of the match on completed recruitment. In each row, we regress one dependent variable in the recruiter survey on the treatment dummy. Thus, the first column gives the mean of vacant jobs with standard applications while the second column gives the difference between vacant jobs with anonymous applications compared to vacant jobs with standard applications. The number of observations in panel D can be lower than the recruitment success rate times the number of total vacant jobs, because of partial nonresponse.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

Source: Firm survey

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 for control firms: they know they were observed; they also know the identity of the applicants. They may therefore artificially select more minority 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

TABLE 6—INTERVIEW RATES IN CONTROL FIRMS DURING AND AFTER THE EXPERIMENT

	Minority (D)	Majority (ND)	Gap (D-ND)
During the experiment (c)	0.123 (0.028)	0.137 (0.029)	−0.014 (0.039)
After the experiment (a)	0.166 (0.033)	0.145 (0.036)	0.021 (0.051)
Difference (a−c)	−0.043 (0.044)	−0.008 (0.048)	−0.035 (0.065)
Number of candidates	442	365	807
Number of vacant jobs	220	218	296
Number of firms	134	132	146

*Notes:* The table has the same difference-in-differences structure as Table 4. It only involves control firms which received standard résumés during the experiment. It compares interview rates for vacant jobs during the experiment, first row (i.e., the experimental vacant jobs analyzed in Table 4) and vacant jobs posted later (second row). Only control firms that resubmitted a vacant job to the public employment services in the months following the experiment are included (explaining the minor difference with Table 4). Survey sampling weights are used. Standard errors are clustered at the vacant job level. More details on the sample characteristics can be found in online supplementary Appendix Table A-1.

*Source:* Candidates' survey

extra efforts to perform well. Such an effect could explain why treated firms appear less favorable to minority applicants: the negative difference would not be due to a negative impact of anonymous résumés on treatment firms, but to the positive impact of monitoring firms in the control group.

To test for the presence of such an effect, we ask whether control firms changed behavior *after* the experiment, once they believe they are no longer observed. To do so, we surveyed a subsample of applicants to 148 vacancies that were posted by control firms after the experiment.<sup>28</sup>

Table 6 shows no evidence of a John Henry effect. If anything, control firms were more, rather than less, favorable to minority applicants 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 subside. This is not the case. It is also possible that control firms that hire minority workers because they are observed during the experiment continue to do so afterwards because they have learned that minorities are not such bad hires after all; although that would imply a large change in beliefs, we cannot fully rule out that interpretation.<sup>29</sup>

Another issue with the program is imperfect anonymization. The treatment consists of removing the upper part of the résumé, leaving its main part unaltered. As a consequence, one may spot minority candidates from three characteristics on the résumés: language skills (in particular Arabic), foreign education, or work

<sup>28</sup>Note that relying on applicants for information on interview and hiring decisions made by the firm removes any concern of firms becoming aware that we continue to observe them.

<sup>29</sup>Instead of focusing on vacancies posted by control firms after the experiment, one could have looked at vacancies posted before the experiment that would be fully exempt from any John Henry effect. However, this turns out not to be feasible. Indeed, administrative information being insufficient, we would need to run a survey of applicants on these past vacancies. 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.

experience abroad. Imperfect anonymization may result in attenuation bias; but it may also result in artificially large effects if firms feel more free to discriminate against minority candidates when this status is imperfectly hidden, as they could claim that they did not know the identity of the candidate. In practice, we do not find that the impact of anonymous résumés on minority candidates depends on whether the minority status could be identified from elements of the résumé's main part.<sup>30</sup>

In our main analysis, the applicant's background enters in a quite specific way, exerting the same effect for being of foreign background (immigrant or child of immigrant), residing in deprived neighborhoods, and cumulating the two characteristics. We decomposed our minority population into three subpopulations: being of foreign background and residing in a deprived neighborhood, being of foreign background and not residing in a deprived neighborhood, and residing in a deprived neighborhood but not being of foreign background. The estimated impacts on both interview and hiring gaps for these three populations were found homogeneous.<sup>31</sup>

As discussed above, there are also several ways to measure foreign background. We considered three alternative measures—being an immigrant, being the child of an immigrant, or having a Muslim- or African-sounding first name. They 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 a Muslim- or African-sounding name.<sup>32</sup>

We checked that results are robust to alternative methods of inference, in particular by conducting permutation tests, drawing placebo treatment statuses at the job posting level. Interestingly, the estimated changes in interview gaps and in hiring gaps become significant at the 1 percent and 5 percent levels, respectively, when permutation tests are used (detailed results available upon request). Lastly, results are robust overall to specification checks with covariates, weights, and functional form.<sup>33</sup>

#### IV. Mechanisms

The results so far may seem at odds with existing evidence on firms' hiring practices, in France and elsewhere. Indeed, even though correspondence studies are less frequent in France than in the United States or Canada, existing ones often find statistically larger callback rates for majority candidates, and we are not aware of any finding of a statistically larger callback rate for minority candidates.<sup>34</sup> In this

<sup>30</sup> See supplementary Table A-5 in the online Appendix.

<sup>31</sup> Results are shown in the supplementary Table A-6 of the online Appendix.

<sup>32</sup> Results are shown in the supplementary Table A-7 of the online Appendix.

<sup>33</sup> Specifically, supplementary Table A-8 in the online Appendix shows that estimated impacts on interview rates and hiring rates are robust to controlling for the whole set of administrative and survey variables listed in Tables 2 and 3, as well as for covariates coded from their résumés. Adding vacancy fixed effects does not change the estimates but reduces precision: the coefficient on  $An \times D$  remains stable but it is no longer statistically significant ( $p$ -value: 0.20). We suspect that in the presence of vacancy fixed effects, robust standard errors clustered at the vacancy level may be overly conservative. Indeed, with permutation tests as an alternative inference method (drawing placebo treatment status at the vacancy level), the  $p$ -value falls to 0.05. Removing sampling weights makes the coefficient become smaller and marginally significant only, which underscores the fact that treatment effects are smaller among applicants with a Muslim- or African-sounding name, who have been oversampled (see online supplementary Appendix Table A-7).

<sup>34</sup> See the special issue of *Annals of Economics and Statistics* (Duguet et al. 2010).

section, we analyze what can explain this discrepancy. We first show suggestive evidence that the voluntary nature of the program has led to a specific population of firms—potentially an important difference with correspondence studies. We then analyze how anonymization changes the way these firms screen résumés.

### A. Firms' Participation Decision

As explained above, targeted firms were free to participate or not in the program, and about 40 percent of them declined to participate. In this subsection, we document the firms' self-selection and assess whether this can explain the estimates. It should be stressed that this self-selection comes as a second layer of selection, as the program was only offered to a specific pool of large firms, located in urban labor markets and used to relying on the public employment services to screen candidates. The specific features of firms targeted by the program have been documented above, with the limited available data. What follows only considers factors associated with the firm's decision to participate in the program, when offered to do so. It uses the much more detailed data that were collected through surveys.

Table 7 shows that nonparticipating firms strongly differ from participating ones by interviewing and hiring significantly fewer minority candidates. Specifically, the table compares interview and hiring rates of minority and majority candidates applying to vacancies posted by firms that accepted to participate in the program but received name-bearing résumés (i.e., control firms) with the rates of those applying to firms that declined to participate. This allows us to assess whether interview and hiring gaps differ in the two groups of firms, when anonymous résumés are not put in place.

The upper panel displays interview rates. Participating and nonparticipating firms interview the same proportion of candidates (about 14 percent on average).<sup>35</sup> However this hides considerable difference between the interview rates of minority and majority candidates. Define the raw interview gap between minority and majority candidates in participating firms as  $\bar{Y}^{P=1,D=1} - \bar{Y}^{P=1,D=0}$ , with similar notations as in the previous section and  $P = 1$  for participating firms. This gap is small (0.6 percentage points) and nonsignificant. By contrast, minority candidates are much less likely to be interviewed than majority candidates in nonparticipating firms (7.3 percent versus 21 percent). The resulting interview gap,  $\bar{Y}^{P=0,D=1} - \bar{Y}^{P=0,D=0}$ , is large in absolute terms (−13.7 percentage points, significant at the 5 percent level). Finally, the difference in interview gaps across the two types of firms,  $(\bar{Y}^{P=1,D=1} - \bar{Y}^{P=1,D=0}) - (\bar{Y}^{P=0,D=1} - \bar{Y}^{P=0,D=0})$ , is large and significant (14.3 percentage points, significant at the 5 percent level). The lower panel displays similar results for hiring rates: hiring rates of minority and majority candidates do not differ in control firms, but they are significantly lower for minority candidates in nonparticipating firms (a 9.5 percentage point difference), so that the

<sup>35</sup> The sample in Table 7 only includes control firms and nonparticipating firms that posted at least a second vacancy in the six months that followed the experimental program. This is due to the fact that the sample was initially used to analyze the firms' behavior after the experiment was over (see the analysis of Table 6). As shown above, the behaviors during and after the program are similar; Table 7 therefore pools vacant jobs during the program and vacant jobs after the program, in order to increase statistical power. Using only vacant jobs during the program yields similar results.

TABLE 7—INTERVIEW AND HIRING RATES IN FIRMS ACCEPTING TO PARTICIPATE  
(and Randomized in the Control Group) AND IN REFUSING FIRMS

	All	Minority (D)	Majority (ND)	Gap (D–ND)
<i>Panel A. Interview rates</i>				
Participating (only controls) firms (c)	0.143 (0.015)	0.146 (0.021)	0.140 (0.022)	0.006 (0.031)
Nonparticipating firms (r)	0.146 (0.038)	0.073 (0.028)	0.210 (0.059)	–0.137** (0.063)
Difference (c–r)	–0.004 (0.041)	0.073** (0.035)	–0.070 (0.063)	0.143** (0.070)
Number of candidates	1,378	759	619	1,378
Number of vacant jobs	507	374	376	507
<i>Panel B. Hiring rates</i>				
Participating (only controls) firms (c)	0.042 (0.009)	0.038 (0.012)	0.046 (0.012)	–0.007 (0.018)
Nonparticipating firms (r)	0.085 (0.025)	0.034 (0.018)	0.129 (0.042)	–0.095** (0.046)
Difference (c–r)	–0.043 (0.027)	0.004 (0.022)	–0.083* (0.044)	0.087* (0.049)
Number of candidates	1,378	759	619	1,378
Number of vacant jobs	507	374	376	507

*Notes:* This table displays interview (panel A) and hiring (panel B) rates of candidates applying to control firms and to firms that declined to participate in the program. The difference in interview rates between the two types of firms is in the third row. In the first column minority and majority candidates are pooled together, while the second column restricts to minority candidates (D) and the third to majority candidates (ND). In the last column, we compute the difference between minority and majority candidate. Thus, the difference in the minority/majority gap between firms participating and not participating can be found in the third row, last column. Survey sampling weights are used. Standard errors are clustered at the vacant job level. We pool candidates applying during and after the experiment to different vacant jobs posted by eligible firms. Candidates to nonparticipating firms were sampled among firms that posted vacancies during and after the experiment (this allows us to conduct the analysis of Table 6). Similarly, only control firms that also posted vacancies after the experiment are included here. For control firms, we pool candidates applying before or after randomization on the same vacant job. For those reasons, the pool of candidates analyzed on control firms is different from that of our main sample of analysis. However, the minority/majority gap in interview rates is the same as in our main sample of analysis. More details on the sample characteristics can be found in online supplementary Appendix Table A-1.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

*Source:* Candidates' survey

hiring gap difference amounts to 8.7 percentage points (significant at the 5 percent level).<sup>36</sup>

Table 7 is not sufficient to conclude that participating firms self-selected from among those that prefer minority candidates as such (or have a weaker preference for majority ones): the large difference in interview and hiring gaps between participating and nonparticipating firms may be due to characteristics of the candidates that are observed by the recruiter and not by the analyst.<sup>37</sup> However, combined with the

<sup>36</sup> Online supplementary Appendix Table A-9 provides robustness checks. In particular, the results are robust to controlling for résumé attributes.

<sup>37</sup> Online supplementary Appendix Table A-10 shows that firms' participation is not correlated with the frequency of customer contact or the prevalence of teamwork in the job, aspects which are often thought to be respectively associated with taste-based customer discrimination and taste-based employee discrimination. The background of the recruiter (education and foreign origin) also does not predict firm's participation, with the exception perhaps

experimental results in the previous section, they suggest a consistent story in which firms participating in the experiment had a positive valuation of minority candidates' résumés, so that anonymous résumés made their decision become less favorable to those candidates. The next subsection further documents these mechanisms.

### B. *Résumé Valuation by Participating Firms*

In this subsection, we provide additional evidence on two questions, using detailed information on candidates (coded from their résumés). First, how do participating employers rate candidates in the standard procedure? Second, what are the exact mechanisms through which anonymous résumés change the interview decisions of these employers? In particular, we test whether a model of statistical discrimination à la Aigner and Cain (1977) can explain the results. In such a model, recruiters may interpret résumés' characteristics differently for minority and majority candidates. They may for instance penalize candidates with unemployment spells less when they are from the minority, as those candidates face more adverse labor market conditions. In such a case, anonymous résumés can harm minority candidates with negative signals as they prevent recruiters from interpreting those signals in the light of the adverse circumstances faced by the candidates.

We start from a simple framework to analyze employer interview decisions under the standard and anonymous procedures. We denote by  $X$  the information set available to the recruiter in the résumé, besides minority status (which we denote by the indicator  $D$ , equal to 1 for minority candidates).

Consider the standard procedure first. The recruiter values résumés based on  $(X, D)$ . Assume that the potential valuation of a candidate  $V^{St}(X, D)$  can be written as:

$$(2) \quad V^{St}(X, D) = (1 - D) \times X\beta_0^{St} + D \times X\beta_1^{St} + \alpha D,$$

where  $\beta_0^{St}$  and  $\beta_1^{St}$  are the two vectors of returns to signals  $X$  for majority and minority candidates.<sup>38</sup>

Equation 2 covers different standard cases of interest. If  $\beta_0^{St} = \beta_1^{St}$  and  $\alpha = 0$ , participating recruiters do not value the résumés of minority and majority candidates differently and there is no discrimination in the standard procedure. In a second case,  $\beta_0^{St} = \beta_1^{St}$  and  $\alpha > 0$ : recruiters tend to value minority candidates more highly, even after controlling for all the information available in the résumés, but they interpret signals  $X$  similarly for both types of candidates. This occurs in models of taste-based discrimination where the preference for minority candidates applies uniformly to all minority candidates, irrespective of their other observable characteristics. It can also arise in some models of statistical discrimination, in which minority and majority candidates are known (or believed) to have different average productivity.

---

of a negative, marginally significant effect of being a woman. Lastly, the recruiter was asked to name five friends as a way to assess his social network, and then to name five colleagues as a way to measure the ethnic composition of the firm. The corresponding variables do not have a significant effect. Interestingly, even though the variables of Table A-10 constitute an unusually rich set of information on firms, none allows us to interpret the differences in recruitment behavior observed in Table 7.

<sup>38</sup>To simplify notations, we normalize  $V^{St}(0, 0) = 0$ . Estimations below include an intercept, though.



The third and last case of interest is  $\beta_0^{St} \neq \beta_1^{St}$ . This occurs in particular in models of statistical discrimination with heterogeneous signal quality, such as Aigner and Cain (1977) and Cornell and Welch (1996), where signals are perceived to be more or less informative depending on the candidate's type.<sup>39</sup>

Estimates of the different parameters in equation 2 can be used to understand the valuation of résumés under the anonymous procedure. In that procedure, recruiters do not have direct information on the minority status of the candidate,  $D$ . Through Bayesian updating, however, they can infer the expected value of a candidate characterized by  $X$  based on their prior on the conditional distribution of  $D$  given  $X$ :

$$(3) \quad \begin{aligned} V^{An}(X) &= \Pr(D = 1|X)V^{St}(X, 1) + \Pr(D = 0|X)V^{St}(X, 0) \\ &= p(X) \times (X\beta_1^{St} + \alpha) + (1 - p(X)) \times X\beta_0^{St}, \end{aligned}$$

where  $p(X) \equiv \Pr(D = 1|X)$  reflects the recruiter's prior on the probability that a candidate with characteristics  $X$  is from the minority. When recruiters believe that a given characteristic  $X_k$  is evenly distributed among minority and majority candidates, equation 3 implies that the return to  $X_k$  is a weighted average of the returns in the standard and anonymous cases. This is no longer the case when observing  $X_k$  induces the recruiter to update her prior on whether the candidate is from the minority.<sup>40</sup>

The different parameters are estimated assuming linear probability models:

$$(4) \quad E(I_{ij}^{St} | X_i, D_i, j) = (1 - D_i) \times X_i\beta_0^{St} + D_i \times X_i\beta_1^{St} + \alpha D_i + c_j^{St}$$

$$(5) \quad E(I_{ij}^{An} | X_i, D_i, j) = X_i\beta^{An} + c_j^{An},$$

where  $I_{ij}^s$  is an indicator variable equal to 1 if candidate  $i$  is interviewed by firm  $j$ , and  $s$  denotes the recruitment procedure type ( $s = St$  or  $s = An$ ).  $\beta^{An}$  is the return to  $X$  under the anonymous procedure. With Bayesian updating,  $X\beta^{An}$  is a linear approximation of  $p(X) \times (X\beta_1^{St} + \alpha) + (1 - p(X)) \times X\beta_0^{St}$ . Consistent estimates of 4 and 5 require accounting for firms' and candidates' specific heterogeneity. Unobserved firm heterogeneity is captured by the job posting effects  $c_j^s$ , which can control for the fact that minority and majority candidates may apply to different types of firms. Note that the  $c_j^s$  are allowed to have different distributions in the anonymous and standard procedures, as firms may change their recruitment strategy in response to anonymization. Accounting for candidate specific heterogeneity requires that the vector  $X$ , included in the equation, contains all the relevant information available to the recruiters. To ensure this, we collected all the candidate information from the résumés so as to match the exact information set available to recruiters. As

<sup>39</sup> Clearly, this corresponds to pure cases, and may not hold in more general models; for instance, if the magnitude of taste-based discrimination varies across groups of candidates, then taste-based discrimination corresponds to the third case.

<sup>40</sup> Formally,  $\frac{\partial V^{An}(X)}{\partial X_k} = p\beta_{1k}^{St} + (1 - p)\beta_{0k}^{St}$  if  $p(X) = p$ . But if  $p$  varies with  $X$ , then  $\frac{\partial V^{An}(X)}{\partial X_k}$  also depends on  $\frac{\partial p(X)}{\partial X_k}$ . Online Appendix Figure A-2 illustrates that  $\frac{\partial V^{An}(X)}{\partial X_k}$  can be negative even when  $\beta_{1k}^{St}$  and  $\beta_{0k}^{St}$  are both positive. We are grateful to an anonymous referee for suggesting this illustration.

detailed in Section IIB, 1,140 résumés were made anonymous and coded by PES counselors.  $X$  includes almost all “intermediate” items introduced in the grid used to code résumés (adequate skills, adequate work experience, interrupted work history, information content of the résumé).<sup>41</sup>

*Résumés’ Valuation under the Standard Procedure: Where Do the High Interview Rates of Minority Candidates Come From?*—The first three columns of Table 8 present the estimation results of model 4, in the standard procedure. The third column presents the  $p$ -values for the test that the coefficients of columns 1 and 2 are equal.<sup>42</sup>

Overall, estimated effects tend to go in the expected direction. In particular, interview rates increase when the candidate has adequate skills to perform tasks advertised in the vacant job. The increase is large in magnitude and significant at the 5 percent level for majority candidates: having adequate skills increases the interview rate by 14.4 percentage points (this is the largest point estimate in the model). The return is smaller and not statistically significant for minority candidates. The effect of unexplained interruptions in the candidate’s labor market history—the fact that the résumé shows gaps other than those explained by education or maternity leave—is statistically insignificant in both the majority and minority groups. However, the parameter estimate is negative for the majority group and positive for the minority group, so that their difference is statistically significant (at the 10 percent level). The larger returns for majority candidates are suggestive evidence in favor of the statistical discrimination model with heterogeneous signal quality (Aigner and Cain 1977). The nonsignificant effect of interrupted labor market history for minority candidates also corroborates the findings by Kroft, Lange, and Notowidigdo (2013) that recruiters attach less importance to unemployment spells when they can be explained by adverse labor market conditions (in our case, high unemployment rates in deprived neighborhoods). Lastly, job counselors were asked whether they were certain/confident in their rating of each candidate. Their answer is a proxy for the level of information conveyed by the résumés. They were told to declare their rating “uncertain” when they found they had insufficient information to unambiguously rate the candidate. We find no statistically significant effect of the “uncertainty” variable in the minority and majority groups. The estimates are not significantly different.

*Résumés’ Valuation under the Anonymous Procedure: How Do Participating Recruiters Extract Information?*—The fourth column of Table 8 presents the estimation results of model 5, in the anonymous procedure. As discussed above, the relationship with the returns in the standard procedure (columns 1–2) depends on whether recruiters believe that the candidates’ characteristics ( $X$ ) help predict their minority status ( $\Pr(D = 1 | X)$  varies with  $X$ ). Table 3 shows no significant correlation between  $X$  and  $D$ . If there is Bayesian updating and recruiters’ priors are consistent with the observed pool of candidates, we therefore expect the return

<sup>41</sup> We restrict the number of covariates to avoid potential multicollinearity problems. As a robustness check, we include the overall rating made by the caseworkers evaluating the résumés. Doing so allows us to capture all relevant information missing in our grid. Supplementary Table A-11 in the online Appendix shows that this does not qualitatively affect the main results.

<sup>42</sup> Note that the coefficients of columns 1 and 2 come from the same regression.

TABLE 8—EFFECTS OF DIFFERENT ELEMENTS OF THE RÉSUMÉ ON THE INTERVIEW DECISION

	Standard application		Test <i>p</i> -value $\beta^0(St) = \beta^1(St)$	Anonymous All candidates $\beta(An)$	Test <i>p</i> -value $\beta^0(St) = \beta^1(St) = \beta(An)$
	Majority $\beta^0(St)$ (1)	Minority $\beta^1(St)$ (2)			
Minority effect ( $\alpha$ )		−0.019 (0.087)			
Has interrupted work history	−0.094 (0.061)	0.028 (0.045)	0.088*	−0.045 (0.040)	0.198
Adequate skills	0.144** (0.058)	0.073 (0.048)	0.360	0.054 (0.037)	0.419
Adequate work experience	−0.034 (0.051)	0.028 (0.046)	0.367	0.034 (0.035)	0.524
High uncertainty	0.040 (0.052)	0.069 (0.048)	0.678	−0.106** (0.050)	0.030**
Number of candidates	252	334		554	
Number of vacant jobs		283		270	

Notes: We estimate the effects of résumés’ signals on the interview rate in a model with vacant job fixed effects. Standard errors are clustered at the vacant job level. Columns 1 and 2 show returns to signals  $X$  when résumés bear names (estimation of  $I_{ij} = (1 - D_i) \times X_{ij}\beta^0(St) + \alpha D_i + D_i \times X_{ij}\beta^1(St) + c_j + \nu_{ij}$ , where  $i$  indicates candidates,  $j$  indicates vacant jobs and  $D$  indicates minority status), and column 4 when résumés are anonymous (estimation of  $I_{ij} = X_{ij}\beta(An) + c_j + \nu_{ij}$ ). Column 1 concerns majority candidates (results for  $\beta^0(St)$ ), column 2 minority candidates (results for  $\alpha$  and  $\beta^1(St)$ ). In column 3, we report the  $p$ -value of the test of equality in returns between columns 1 and 2. In column 5, we report the  $p$ -value of the test of equality between columns 1, 2, and 4. For example, when nominative résumés show unexplained interruption of labor market history, the interview rate of majority candidates decreases by 9.4 points and that of minority candidate increases by 2.8 points.

\*\*\*Significant at the 1 percent level.  
\*\*Significant at the 5 percent level.  
\*Significant at the 10 percent level.

Source: Candidates’ survey and résumés’ coding

to each characteristic under the anonymous procedure to be close to the average of the returns for minority and majority candidates in the standard procedure. This prediction holds concerning the return to unexplained interruption in labor market history. The average of the returns between minority and majority candidates is around  $-0.03 (= -0.09 \times 0.5 + 0.03 \times 0.5$ , where 0.5 is the actual fraction of minority candidates), which is close to the actual return estimated in the anonymous sample ( $-0.045$  in the column 4). Some minority candidates are actually harmed by anonymization, because they have an interrupted labor market history. However, the Bayesian updating model is rejected in the case of “uncertainty,” which has a significantly negative impact in the anonymous procedure, while coefficients were insignificant and positive in the standard procedure, both for minority and majority candidates. In that dimension, it seems that when the amount of information conveyed in the anonymous résumé is low, recruiters are “scared” and reject the candidates.

*A Decomposition of Anonymous Résumés’ Impact.*—We have shown that minority candidates are adversely affected by anonymization because some negative signals in their résumés, such as interrupted work history, are not attenuated as they are in the standard procedure. Minority candidates can also be adversely affected because

recruiters reject candidates with low-information résumés when résumés are anonymous. If the résumés of minority candidates are less informative than those of the majority candidates,<sup>43</sup> the former would be more adversely affected by anonymization than the latter. To quantify these effects, we perform a decomposition exercise. We use estimates reported in the previous table to decompose the change in the interview gap. Let us consider first anonymous résumés. Using model 5, the gap in interview rates can be expressed as:

$$\begin{aligned} \text{Gap}(An) &= E(I_{ij}|An = 1, D = 1) - E(I_{ij}|An = 1, D = 0) \\ &= (E(X_i|An = 1, D = 1) - E(X_i|An = 1, D = 0))\beta^{An} \\ &\quad + E(c_j|An = 1, D = 1) - E(c_j|An = 1, D = 0) \\ &\quad + E(v_{ij}|An = 1, D = 1) - E(v_{ij}|An = 1, D = 0). \end{aligned}$$

There are therefore three components. The first is the effect of differences in endowments of characteristics  $X$ , valued using the estimated  $\beta$ 's. The second is the vacancy effect: job seekers from the minority may apply to vacant jobs that in average receive more (or less) candidates than vacant jobs to which candidates from the majority are applying. The last component is the contribution of the residuals ( $v_{ij}$ ): there may be characteristics missing that are correlated with minority.

A similar decomposition can be produced for the standard procedure:

$$\begin{aligned} \text{Gap}(St) &= E(I_{ij}|An = 0, D = 1) - E(I_{ij}|An = 0, D = 0) \\ &= \alpha + (E(X_i|An = 0, D = 1)\beta_1^{St} - E(X_i|An = 0, D = 0)\beta_0^{St}) \\ &\quad + E(c_j|An = 1, D = 1) - E(c_j|An = 1, D = 0). \end{aligned}$$

There are several differences in this decomposition compared to the previous one. First, the effect of belonging to the minority ( $\alpha$ ) directly enters the decomposition; second, the returns to each item are allowed to differ between minority and majority candidates; and last, there is no residual effect. All these changes reflect the fact that the recruiter knows the minority/majority type of the candidate.

Results are reported in Table 9. The first column reports elements of the decomposition of the interview gap under the anonymous procedure, the second presents the decomposition of the interview gap under the standard procedure, and the third gives the decomposition of the change in gap. For each variable, as well as for the vacant job fixed effects, we compute a net effect by subtracting column 1 from column 2.

The first line provides the two interview gaps as well as the difference between them. Closely related to the previous results presented in Table 4, the change-in-gap

<sup>43</sup>Table 3 shows a small difference in that direction, even though not statistically significant.

TABLE 9—DECOMPOSITION OF INTERVIEW GAPS BETWEEN MINORITY AND MAJORITY CANDIDATES

	Anonymous (a)	Standard (n)	Difference-in-differences (a)–(n)
Raw interview gap	–0.098*** (0.033)	–0.028 (0.029)	–0.070 (0.043)
<i>Signals/characteristics (X):</i>			
Interrupted work history	–0.000 (0.002)	0.037* (0.022)	–0.037* (0.023)
Adequate skills	–0.001 (0.003)	–0.034 (0.040)	0.033 (0.040)
Adequate work experience	–0.000 (0.002)	0.035 (0.038)	–0.035 (0.038)
High uncertainty	–0.001 (0.005)	0.018 (0.036)	–0.019 (0.036)
Minority effect (constant)	0.000 (0.000)	–0.019 (0.087)	0.019 (0.087)
Subtotal over X (including constant)	–0.003 (0.007)	0.036 (0.037)	–0.039 (0.038)
Vacancy fixed effects	–0.069*** (0.026)	–0.065** (0.030)	–0.005 (0.038)
Residuals	–0.026 (0.018)	–0.000 (0.000)	–0.026 (0.018)

*Notes:* We report the minority/majority gap in interview rates when résumés are anonymous in column 1 and when résumés are nominative in column 2. In column 3, we compute the change in minority/majority gap due to anonymization. The raw gap in interview rates (line 1) is decomposed between contributions due to difference in signal valuations (from line 2 to 6), difference in type of vacant jobs and unexplained difference (line 8 and last line). Contributions are obtained from estimates in Table 8. To obtain standard errors, we bootstrap the decomposition 500 times clustering at the vacant job level.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

*Source:* Candidates' survey and résumés' coding

to explain is a widening of 7 points.<sup>44</sup> The contributions of the different signals to the minority/majority gap in the anonymous sample are low (column 1). They are all negative and small, so that their sum amounts to –0.3 percentage points. This is not surprising, as there are few differences in the signals' distribution between minority and majority candidates (see Table 3). The contribution of vacant job fixed effects is larger, almost 7 percentage points. This may reflect the self-selection of minority candidates (who tend to apply to job offers where interview rates are low) or selection by the PES counselors in charge of the initial matching of vacant jobs with candidates. Lastly, the contribution of residuals is lower, but still important: –2.6 percentage points. Even if PES counselors coded the résumés that were sent to recruiters, some signals correlated with the minority status are still missing from our analysis.

<sup>44</sup> The difference between our estimate in the previous section and this estimate is due to the restriction of the sample from 1,268 to 1,140 individuals. We exclude candidates who did not send their résumés as PDFs or docs, but went through an online application form on the website.

In contrast to the decomposition in the anonymous sample, the contributions of the different signals in the nominative sample (column 2) are sizable: the total amounts to 3.6 percentage points (to be compared to an average interview rate of about 10 percent).<sup>45</sup> This mainly reflects the fact that returns to signals differ between the majority and minority candidates. For example, the attenuation of the negative signal conveyed by an interrupted work history leads to an increase by 3.7 points in the interview rates of minority candidates. Vacancy fixed effects have the same contribution in the nominative sample than in the anonymous sample, and the contribution of residuals is by construction equal to 0 in the nominative sample.

As a consequence, it appears in column 3 that the main component of our change-in-gap estimate is due to signal extraction, more precisely to the uniformization of returns between majority and minority groups following anonymization. Out of the  $-7$  points of our change-in-gap estimate,  $-3.9$  can be explained by difference in signal valuation. The contribution of vacant job fixed effects vanishes as it is the same in both nominative and anonymous samples. Finally, a relatively small fraction of our change-in-gap estimate is left unexplained ( $-2.6$ ); this may reflect remaining unobserved heterogeneity.

## V. Conclusion

We have analyzed the first large-scale randomized experiment on the effects of anonymous applications. The program was implemented among client firms of the French public employment agency, which were free to participate or not. Among 600 participating firms, 50 percent received résumés where the upper block was suppressed. In anonymous résumés, recruiters could not read names, addresses, nationality, or other identifying items. Results show that minority candidates are actually harmed by anonymous applications. The gap in interview rate between minority and majority candidates worsens by around 10 percentage points when résumés are made anonymous. The effect seems to persist beyond résumés' screening, as the hiring gap also widens by 4 points (only significant at the 10 percent level). Interestingly, anonymous résumés do not make recruiters interview more candidates, and the overall quality of the job match does not seem to be affected. The findings of the experiment led the PES to stop the program. However, the government went one step further: it also abandoned the idea of making anonymous résumés mandatory, an option that had not been evaluated. Our results concern volunteer firms and should not be extrapolated to all firms, as we show that a significant number that usually interview relatively few minority workers decided not to participate. This raises the suspicion that the sample of participating firms did not include those less favorable to minority candidates—providing a plausible reason why a policy unable to reach these firms failed to help minority candidates.

Our results provide insights on the way recruiters in participating firms evaluate the candidates' credentials in résumés, and how they extract information when receiving anonymous résumés. In particular, we find evidence in favor of statistical

<sup>45</sup> As noted above,  $\alpha$  is nothing else than the return to minority status for a reference individual. Its contribution is therefore included in this subtotal.



discrimination models with heterogeneous signal quality à la Aigner and Cain (1977): unexplained interruptions in work histories cause less penalization to minority candidates, as should be the case if such interruptions are more frequent in that group, and therefore less informative on the skills of a particular candidate. We also find evidence of Bayesian updating when résumés are made anonymous. However, the finding that uncertainty on the candidate's skills is strongly penalized when résumés are anonymous is harder to reconcile with statistical discrimination and Bayesian updating. Confirming and combining these findings on recruiters' behavior is a stimulating avenue for further research.

## REFERENCES

- Aigner, Dennis J., and Glen G. Cain. 1977. "Statistical Theories of Discrimination in Labor Markets." *Industrial and Labor Relations Review* 30 (2): 175–87.
- Åslund, Olof, and Oskar Nordström Skans. 2012. "Do Anonymous Job Application Procedures Level the Playing Field?" *Industrial and Labor Relations Review* 65 (1): 82–107.
- Autor, David H., and David Scarborough. 2008. "Does Job Testing Harm Minority Workers? Evidence from Retail Establishments." *Quarterly Journal of Economics* 123 (1): 219–77.
- Bébéar, Claude. 2004. *Des entreprises aux couleurs de la France*. Rapport au Premier ministre.
- Behaghel, Luc, Bruno Crépon, and Thomas Le Barbanchon. 2015. "Unintended Effects of Anonymous Résumés: Dataset." *American Economic Journal: Applied Economics*. <http://dx.doi.org/10.1257/app.20140185>.
- Bertrand, Marianne, Dolly Chugh, and Sendhil Mullainathan. 2005. "Implicit Discrimination." *American Economic Review* 95 (2): 94–98.
- Bertrand, Marianne, and Sendhil Mullainathan. 2004. "Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination." *American Economic Review* 94 (4): 991–1013.
- Bessy, Christian, and Emmanuelle Marchal. 2007. "L'usage des canaux de recrutement par les entreprises." Centre d'Études de l'Emploi Working Paper 89.
- Chebira, Abderrazak. 2005. *L'indispensable manuel orthographique des prénoms français d'origine arabe et musulmane*. Éd. APIC. <http://lesfiguresdeladomination.org/index.php?id=230>.
- Cornell, Bradford, and Ivo Welch. 1996. "Culture, Information, and Screening Discrimination." *Journal of Political Economy* 104 (3): 542–71.
- Duguet, Emmanuel, Yannick L'Horty, Dominique Meurs, and Pascal Petit. 2010. "Measuring Discriminations: An Introduction." *Annals of Economics and Statistics* 99–100: 5–14.
- Felouzis, Georges. 2003. "La segregation ethnique au college et ses consequences." *Revue Francaise de Sociologie* 44 (3): 413–48.
- Goldin, Claudia, and Cecilia Rouse. 2000. "Orchestrating Impartiality: The Impact of 'Blind' Auditions on Female Musicians." *American Economic Review* 90 (4): 715–41.
- Krause, Annabelle, Ulf Rinne, and Klaus F. Zimmermann. 2012. "Anonymous Job Applications of Fresh Ph.D. Economists." *Economics Letters* 117 (2): 441–44.
- Kroft, Kory, Fabian Lange, and Matthew J. Notowidigdo. 2013. "Duration Dependence and Labor Market Conditions: Theory and Evidence from a Field Experiment." *Quarterly Journal of Economics* 128 (3): 1123–67.
- Persico, Nicola. 2009. "Racial Profiling? Detecting Bias Using Statistical Evidence." *Annual Review of Economics* 1: 229–54.
- Riach, P. A., and J. Rich. 2002. "Field Experiments of Discrimination in the Market Place." *Economic Journal* 112 (483): F480–518.