Preventing Discrimination Through Training Measures: An Assessment

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Abstract – In France, training of recruiters is often emphasised as an effective means for combating discrimination and in 2017 it was made compulsory for companies with more than 300 employees. In this study, we assess the effect of a measure similar to this compulsory training by comparing the results of correspondence tests performed before and after implementation of the measure in treated companies and control companies. The results show that the level of discrimination was the same between the two groups of companies prior to implementation of the measures and that the same was true five months later. Double- and triple-difference estimates show no significant impact of these measures on the level of discrimination in access to employment.

JEL: J7, C93
Keywords: discrimination, access to employment, correspondence test, awareness raising measures

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Academic research on discrimination has until now focused mainly on its measurement and interpretation, giving priority to a small number of criteria (gender, ethnic origin or place of residence) and areas (mainly the labour market, and more recently the housing market). It more rarely addresses public policies to combat discrimination. In particular, the contemporary experimental assessment methods which have been disseminated in France since the 2000s have been very seldom applied to anti-discrimination measures.

There are a great variety of measures to combat discrimination and promote diversity: a reminder of the law, awareness raising, specific training courses, information given to those who may be victims of discrimination, signing charters and commitments, equality and diversity labels, etc. Training of recruiters is a key aspect of these measures, since it was made compulsory in France for companies with more than 300 employees, by the Equality and Citizenship Act of 29 January 2017. This type of measure has not yet undergone any rigorous impact assessment, however.

In the field of discrimination prevention measures, research has so far mainly concerned the assessment of policies to change the functioning of the labour market by controlling the information available to the recruiter. The introduction of anonymous CVs is one example of this (Behaghel *et al*., 2015; Krause *et al.*, 2012), while another is the ban on requesting applicants' police records in the United States (Agan & Starr, 2018). This type of policy has so far revealed limited effects or has even proved counter-productive. Some studies have also assessed the effect of incentive policies on discrimination related to place of residence and have shown effects that are positive, although also limited (Chareyron *et al.*, 2022). More coercive measures such as reminders of the law and the threat of legal sanction have also been assessed, mainly in the housing market (Chareyron *et al.*, 2023; Fang *et al*., 2019; Murchie *et al*., 2021).

Given these mixed results, it seems a good idea to examine the effect of measures at company level, particularly in the recruitment process. For example, Berson *et al*. (2020) showed that access to a centralised human resources department in the recruitment process tended to reduce discrimination against applicants of North African origin.

We propose to assess the effects of a recruiter training measure on discrimination based on ethnic origin in access to employment. In its content and intensity, the measure is similar to the compulsory training provided for by the French Labour Code (Art. L1131-2). It was implemented by the Regional Association of Local Youth Employment Centres (*Association régionale des missions locales*) of the Provence-Alpes-Côte d'Azur (PACA) region, which provides access to a one-day training session for a number of volunteer partner companies, whether or not they are concerned by the legal obligation, in order to raise their awareness of the issue of discrimination in recruitment. In this study, we also address a second experiment with similar content that was rolled out in a different region, by the Local Youth Employment Centre (*Mission locale*) of the town of Chambéry. These training courses have legal and practical content, stressing the legal and judicial framework of the prohibition of discrimination and the procedures for organising recruitment in conformance with the principle of equality.

To carry out the assessment, we measure discrimination risk by the correspondence test experimental method. We make a comparison between companies in which the training measure has effectively been implemented and others where it has not been rolled out. The companies in the control group included in the experiment are selected on the basis of their observable characteristics in terms of sector of activity and location. An initial test is performed in the month prior to the training course and a second test within the four months post-training. We test the people who took part in the training directly. A difference-in-differences estimation then enables us to eliminate the time-invariant unobserved differences between the two groups and the variations over time common to both groups.

The results show a substantial and comparable level of discrimination in the companies of the treated and control groups in the period prior to the treatment. In the control group, the applicants whose surname and first name suggest a North African origin have about a 12.5 percentage-point lower probability of receiving a positive response to their application than an applicant whose surname and first name suggest a French origin. This represents a relative difference of nearly 50%. The difference is the same five months later and apparently was not affected by implementation of the measures. The double- and triple-difference results show no significant effect of the training measures on the level of discrimination. This suggests that the public policy consisting of imposing a compulsory training measure of low intensity with mainly legal content is not an adequate response to the challenge of ethno-racial discrimination in the labour market.

Section 1 describes the support measures implemented for companies, while Section 2 describes the experiment protocol for data collection. Section 3 describes the empirical strategy, while Section 4 presents the results of the estimates, before the conclusion.

1. A Training Measure to Combat Discrimination

1.1. Content of the Measure

One of the two training measures assessed in this article was implemented by the Regional Association of Local Youth Employment Centres (*Association régionale des missions locales*) of the PACA region. It consists in proposing to a number of volunteer companies to receive training which involves legally defining non-discrimination and its fields of application based on the law and established legal precedents, providing an objective view of a recruitment process that is formally equal, while outlining the principle of company neutrality. The training lasts one day and is intended for recruiters and the people involved in the recruitment circuits, managers or human resource representatives. The aim is to propose a practical recruitment method to prevent discrimination in the recruitment process from a constructive and non-stigmatising perspective focused on the HR policy of the companies. In-person learning was used throughout the experiment. The trainees are managers or representatives of human resources.

The assessment also includes a second measure of a similar duration and content implemented by the Chambéry Local Youth Employment Centre, which proposes training and support to persons in charge of human resources in the partner companies, with a view to rethinking the organisation of recruitment based solely on the applicants' abilities, producing job descriptions based on objective criteria and the tasks and competencies required, and avoiding the use of potentially discriminatory characteristics of the applicants, such as the first name, surname or place of residence. The Chambéry scheme consists in supporting the recruiting companies and young people prior to contact with the companies. The support consists of co-designing new recruitment processes.

The measures of the Local Youth Employment Centre include:

- the in-situ design of a job description based on objective criteria and the tasks and competencies required;

- a commitment by the company to receive one or more of the prospective applicants without going through the conventional stages (CV, cover letter);

- no information on potentially discriminatory characteristics of the applicants (first name, surname, place of residence) until the principle of an interview appointment has been agreed on by the company;

- the development of forms of innovative collective recruitment measures (atypical job dating, "improbable" encounters, employment afterwork sessions, recruitments organised in the local centre).

The training measures that we assess are similar in nature and intensity to the compulsory training the principle of which is defined in Act No. 2017-86 of 27 January 2017 on equality and citizenship and as incorporated into the French Labour Code in Article L. 1131-2.[1] This stipulates that "in every company employing at least three hundred employees and in every company specialised in recruitment, the employees in charge of recruitment tasks shall receive training in non-discrimination in recruitment at least once every five years". To comply with this legal obligation, vocational training actors generally propose one-day in-person or e-learning sessions. The content of the training may vary from one service provider to another, but generally includes an introduction to the legal and judicial framework of discrimination and practical advice for organising a recruitment operation complying with the principle of equality. The courses are mostly intended for managers and HR personnel.

1.2. Expected Effects

Traditionally, two economic explanations have been given for discrimination. The first is referred to as taste-based discrimination and was formally defined by Becker (1957). It is the existence of a preference for employment of people from the majority group in the employer's utility function. In this case, the employer may be prepared to recruit a less productive employee and hence accept a reduction in their profit to avoid employing a person from another demographic group. The second explanation relates to the concept of statistical discrimination formulated by Arrow (1974). In this case, faced with imperfect information on the applicants,

1. *A recent DARES research document entitled «* Quelles sont les caractéristiques de l'offre de formation à la non-discrimination à l'embauche ? *» (March 2024) presents an overview of the offer of training in non-discrimination in recruitment since the enactment of the Equality and Citizenship Act of 2017 (Benedetto-Meyer, 2024).*

employers presume that the unobserved characteristics of applicants from another demographic group are less advantageous on average than those of the candidate from the majority group. To maximise their profit, out of applicants having the same observed characteristics, the recruiter will favour the one that belongs to the majority group. More recently, there has been another trend suggesting the explanation that recruiters can have unconscious prejudices, even if they possibly endeavour to correct them consciously. According to this explanation, discrimination may be greater in the case of heavy cognitive loads or inattention to the task (Bertrand *et al*., 2005).

From a theoretical standpoint, a training and awareness raising measure regarding the issue of discrimination in recruitment seems an appropriate solution for combating discrimination. Referring back to the above classification, the aim is mainly to combat implicit discrimination due to the pitfalls of stereotypes. Compulsory training offers an opportunity for professionalisation of the HR function in which diversity is a performance issue: recruiting the person with the required competencies. The "best recruitment practice recommendations" part of the training courses addresses the concept of cognitive biases which can be minimised and thereby reduce implicit discrimination. Given that the training measures consist mainly in a reminder of the legal and judicial framework of discrimination and advice on organising a recruitment operation that complies with the principle of equality, it is unlikely that they would affect direct and rational discrimination. Such discrimination is based on the recruiters' biases and their imperfect knowledge of the applicants' productivity levels. For that, the training measures would have to change the biases and/or reduce information asymmetry during recruitment, which seems unlikely.

2. The Experiment Protocol

2.1. Description of the Protocol

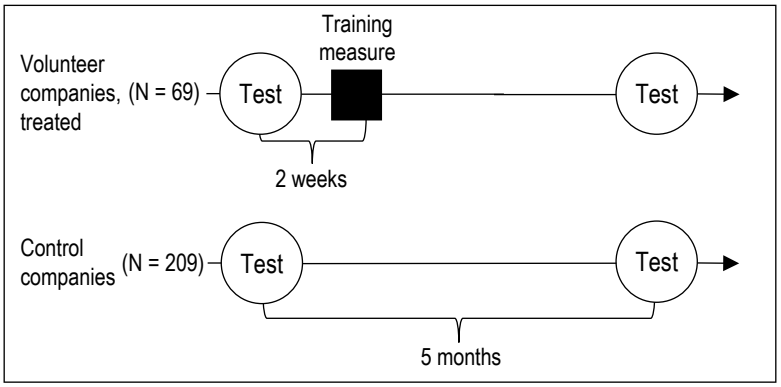
Our approach involves comparing exposure to discrimination in the companies of the treated group before they benefit from the training measure, with a sample of comparable companies which do not benefit from the measure, and then reiterating the comparison after implementation of the training measures. To make these comparisons, we use experimental data collected by the repeated correspondence test method. We accordingly perform two series of tests in each treated company and each control company, the first in the month prior to the measure and the second in the following four months, as described in the diagram below (Figure I).

We created the identities of two fictitious job applicants of the same gender, indicating a French-sounding first name and surname for one and a North African-sounding first name and surname for the other. The first names and surnames were chosen from the most common ones in France based on the official registry files managed by INSEE.

The experimenter sent the assessor the occupational contact details of the employees who would soon be benefiting from the support measure as they were received. For each treated company one or more control companies were then identified (same sector of activity and same employment zone).[2] We then identified an occupation that is commonly recruited in

2. The control companies were tested in the same time frame as the companies of the test group. We used several websites such as societe.com or infogreffe.fr which list the companies still in activity in a given sector and a given region. In the whole database, we identified 93 different sectors of activity (5-figure NAF activity code), ranging from construction to industry, and including market and non-market services. With regard to geography, insofar as possible we compared the control companies with the companies of the treated group (either in the same municipality, or in a nearby geographic area still remaining within the same department). Information on the size of the companies is not always present on these sites.

Figure I – **Experiment protocol**



the sector of activity of the treated company and which appears among the in-demand occupations (source: INSEE).[3] For this occupation, we adopted the modal gender of the employees based on job survey data. The occupation was changed between the two series of correspondence tests to minimise the risk of detection of the experiment. The number of control companies for each treated company varies (between 1 and 9) depending on the sector of activity of the treated company, and depending on whether it was more or less easy to find control subjects meeting the sector and location criteria. This explains the variation in the number of control companies between tests.

Each of the two fictitious applicants sent the treated company and the corresponding control companies a message requesting information on employment opportunities in the selected occupation. This is therefore a test similar to a spontaneous application (Deuchert & Kauer, 2017). The spontaneous application test is one of the two types of tests most commonly used in the economic literature on discrimination, together with the test in response to job offers (Riach & Rich, 2010).

According to the OFER survey (*Offre d'emploi et recrutement*, Job Offer and Recruitment) survey conducted by the French Ministry of Labour, 21% of recruitments result from a spontaneous application, and in 68% of recruitments, spontaneous applications were examined during the recruitment procedure. Spontaneous applications are therefore a recruitment channel that should not be overlooked. Moreover, certain companies now have a spontaneous application form on their career website, suggesting to candidates to apply in this way.[4] The development of CV libraries also contributes to the role assigned to spontaneous applications as a potential recruitment channel. In this case, however, no CV or cover letter was sent. The test can therefore be considered as a test by information request. Below are examples of messages sent by two fictitious applicants to a given company for a secretarial job.

Hello,

I would like to apply for a job in your company. I am a secretary. Could you provide me with information regarding the opportunities and the person to contact? Thanking you in advance for the information you are able to provide.

Kind regards,
Jamila BELHADJ

Hello, I am looking for a secretarial job. Could you tell me whether there might be opportunities in your company and, if so, who I can send my application to? Thanking you in advance.

Kind regards,
Aurélie Legrand

The order in which the two requests for information were sent to a given company was determined randomly. Two precautions were taken to limit the risk of detection. First, during series 1 and series 2, several days went by between sending the two messages to the same company. Next, the identities of the fictitious applicants were changed regularly. In particular, they were different in series 1 and series 2. The recruiter's response was considered negative if they did not reply to the message requesting information or if they stated explicitly that there was no job opportunity. The response was considered positive if the employer requested additional information (CV) or put the applicant in contact with the HR Department, etc. The responses that the companies gave to the requests from the two fictitious applicants were then compared.

2.2. Sample Size, Attrition and Data Balancing

The data were collected between June 2019 (first tests of series 1) and May 2023 (final tests of series 2). Collection was suspended during the first lockdown (between March and May 2020). 213 companies were included in the treated group and 629 in the control group. The assessment covered the companies that were able to be tested twice, before and after the training measure. If a company in the treated group was unable to be tested twice, all the companies of the control group associated with it were deleted. After removing certain companies in the PACA region, which were due to take part in the training but which did not do so in the end, and the associated companies of the control group, the final sample comprises 202 companies in the treated group (133 for the PACA region and 69 for Chambéry) and 606 companies in the control group (397 for the PACA region and 209 for Chambéry). The treated companies and control companies are identical in series 1 and series 2.

3. We count in all 28 tested occupations which are cross-cutting to most of the sectors, such as administrative employee, accounting employee, non-specialist salesperson, production worker, and unskilled technician.
4. Over our field of study, about 30% of the companies of the control group had this type of form. This proportion is fairly similar for very small companies (34%) and large companies (30%), but lower for companies of unknown size (21%). In this test, the applications were made via this type of form when it existed, or by email when it did not.

The email addresses are usually those of individuals in the companies of the treated group. In the case of Chambéry, for example, we have 42 non-generic email addresses for the 69 test companies, versus 8 non-generic addresses for the 209 control subjects. A non-generic email address assures us that we are testing the same individual during the two series of correspondence tests. This is therefore the case for 60% of the companies of the treated group (versus 4% for the companies of the control group). This difference of proportion is due to the fact that the control companies can mostly be reached by filling in a contact form without knowing beforehand the identity of the person who will actually reply to the request for contact.

2.3. Estimation Strategy

As we selected control companies that were similar in terms of their sector and location for each company in the treated group, the two groups of companies have fairly similar characteristics. However, to avoid companies in the control group receiving the training measures stipulated by law for companies with more than 300 employees during the experimentation period, we were unable to select control group companies of similar size to those of the treated group. In the absence of random selection of the companies of the control group, there could also be unobserved differences in characteristics between the two groups. If those characteristics affect the result variable (i.e. the fact that an applicant receives a positive response to their application), estimating the effect of the treatment by comparing the differences in positive response rates between the applicant presumed of North African origin and the one presumed of French origin in the two groups while controlling for the observable characteristics would lead to biased results. Another possibility would be to observe the variation in differences in the positive response rates between applicants of North African origin and French origin in the treated group before and after treatment. However, this estimate could be biased by any change that is unrelated to the treatment (in particular the economic situation), which could have different effects on the positive response rate for the applicant of North African origin and for the applicant of French origin.

We therefore adopted a difference-in-differences (or triple differences) estimation strategy. By comparing variations in differences in the positive response rates between applicants of North African origin and applicants of French origin in the treated group and the control group between the period prior to treatment and the period following it, we eliminate the time-invariant unobserved differences between the two groups and the variations over time common to both groups. This triple difference is estimated using the following model:

$$\begin{aligned} REP_{iet} = {} & \alpha + \beta\, NAfr_i + \varphi\, Post_t + \gamma\, T_e \times NAfr_i + \tau\, T_e \\ & \times Post_t + \omega\, NAfr_i \times Post_t + \delta\, T_e \times NAfr_i \\ & \times Post_t + \pi\, X_{iet} + \mu_t + \phi_e + \varepsilon_{iet} \end{aligned} \quad (1)$$

where REP_{iet} is a dichotomous variable indicating whether company e responds positively to applicant i at date t. $NAfr_i$ is a dichotomous variable indicating whether the applicant is of North African origin. T_e is a dichotomous variable taking value 1 if the company belongs to the treated group and 0 otherwise. $Post_t$ is a dichotomous variable taking value 1 in the post-treatment period and 0 in the period prior to treatment.[5] X_{iet} corresponds to the order in which applications i are sent to company e at date t. μ_t are the fixed effects related to the date of sending the application (month × year and day) and ϕ_e are the company fixed effects. The associated coefficient δ associated with variable $T \times NAfr \times Post$ captures the effect of the training measures.

This strategy is based on the assumption that the variations in differences in the positive response rates according to ethnic origin (i.e. discrimination against the applicant of North African origin) would have been similar in the companies of the treated group and the control group in the absence of treatment (common trend assumption). We cannot observe the trends prior to treatment in order to confirm this assumption. Moreover, inclusion in the treatment is not random and mainly concerns companies that are part of the local youth employment centre's network and are volunteers to take part in the measure, so they are possibly already more aware of discrimination and the need for a change of practices.

However, several factors support the idea that this assumption is complied with. Firstly, while the companies do volunteer to take part in this support, the recruiters who have undergone training as part of this support were generally not the decision-makers on this. From the viewpoint of a recruiter in a treated company, the training is imposed by the company and not a matter on which they are free to make their own decision. Secondly, compliance with this assumption is confirmed by the selection in the

5. *Since the treatment was administered as the occasion arose, it does not coincide with a particular date.*

control group of companies similar to those in the treated group from the sector and location viewpoints. Moreover, as may be noted below, the companies in the two groups have similar discriminatory behaviours in series 1, even in terms of level. They are therefore unlikely to have seen variations in the level of discrimination that were not related to the support measures in the five months following the first test.

Despite this, a shock that might affect the treatment of applicants of French origin differently to applicants of North African origin in the treated companies and in the control companies could create a bias in the estimate. Given that the second series of tests occurred after the COVID pandemic, it is possible that a shock could have had different effects on the positive response rates of the treated companies and control companies, e.g. because their average size is not the same. On the other hand, it is less likely that this same shock could have affected differently the positive response rate received by the applicant of French origin compared with that obtained by the applicant of North African origin. The latter type of shock would, in theory, be due rather to a political event or to (domestic or external) conflicts liable to alter the recruiters' preferences. This type of shock apparently did not occur between June 2019 and May 2023.

One important point to watch is the possibility that companies may have benefited from other training measures in addition to that provided for in the experiment, especially because of the requirement instituted by the Act of the 2017 that companies with more than 300 employees must propose training on non-discrimination in recruitment for the employees in charge of recruitment tasks.

We cannot know whether the companies in the treated and control groups had implemented the compulsory training at the time of the experiments. However, since the control group largely consists of companies with less than 300 employees, it is unlikely that they received this training during this period.

Moreover, the recruiters of the treated companies may have possibly already taken part in a training course since the introduction of compulsory training in 2017, but it is unlikely that a training course of this type was implemented in addition to the training assessed during the study period, given the frequency stipulated by law. However, we are unable to determine whether there is an equivalence or a complementarity between the compulsory training and the training under the experiments.

Given that the number of companies selected in the control group may vary for each treated company, in the estimates we weight the control group observations by the inverse of the number of control subjects selected for each treated company. Furthermore, the standard errors are clustered at the company level.

3. Results

3.1. Comparison of the Treated and Control Groups Prior io Treatment

In Table 1 we compare the characteristics of the companies in the treated group and the control group for the PACA region and Chambéry territories separately. The differences are generally slight, except on the size of the companies. More specifically, the companies in the treated group are large significantly more often than those in the control group. This under-representation of large companies in the control group is due to the fact that we gave priority to selecting companies with less than 300 employees in the control group to avoid those subject to the training obligation by the terms of the Act of 2017.[6]

Generally speaking, less than a quarter of the companies are in the public sector in Chambéry and about 13% in the PACA region. A large number of sectors of activity are represented. Of the sectors of activity, it may be noted that jobs in general government, the building sector and retailing account for a fairly large proportion of the offers. In the PACA region, temporary work agencies and domestic help companies are also very largely represented. For the measure implemented in Chambéry, 90% of the companies are located in the Savoie department and for the measure implemented in the PACA region, about 30% of the companies are located in Marseille. Apart from the size of the companies, no significant difference of characteristics can be observed between the companies in the treated group and the control group.

The gross rates of response before and after treatment are presented separately in Table 2 for the group of treated companies and the group of control companies. The positive response rates are fairly high by comparison with those of studies using spontaneous application tests (Chareyron *et al*., 2024) and reach comparable,

6. Less than 5% of the companies in the control group have a size exceeding 300 employees. When it was hard to find a company in the same sector and the same location, a company with more than 300 employees was selected. It is important to note that we do not know the size of all the companies. However, even among those for which we know the size, less than 10% have more than 300 employees. Moreover, it is likely that the companies whose size was unable to be determined are mostly companies with less than 300 employees.

Table 1 – **Comparison of the characteristics of companies in the treated group and the control group**

	Chambéry			PACA		
	Control	Treated	Difference	Control	Treated	Difference
	%	%		%	%	
Public-sector companies	21.5	15.9	−5.6	13.1	12.0	−1.1
Size:						
Large	4.8	15.9	11.1***	4.0	11.3	7.3***
Small or medium	25.8	14.5	−11.3**	24.9	33.1	8.2***
Very small	27.8	17.4	−10.4**	29.0	16.5	−12.5***
Unknown	41.6	52.2	10.6**	42.1	39.1	−3
Sector of activity:						
Temporary work agency	0.0	0.0	0	17.1	12.0	−5.1
Domestic help	1.4	1.4	0	7.6	7.5	−0.1
Public administration	9.6	7.2	−2.4	9.1	7.5	−1.6
Business support	1.9	1.4	−0.5	1.5	1.5	0
Childcare	6.2	4.3	−1.9	3.8	5.3	1.5
Medical accommodation	5.7	4.3	−1.4	5.5	7.5	2
Building	11.5	11.6	0.1	5.0	5.3	0.3
Trades and crafts	8.1	10.1	2	1.0	1.5	0.5
Restaurants and catering	3.8	2.9	−0.9	1.5	0.8	−0.7
Retailing	14.8	18.8	4	10.1	8.3	−1.8
Tourist accommodation	0.0	0.0	0	3.8	3.0	−0.8
Social welfare	3.3	2.9	−0.4	1.8	3.0	1.2
Training	5.3	4.3	−1	4.5	3.0	−1.5
Other	28.2	30.4	2.2	27.7	33.8	6.1*
Location:						
Savoie	90.9	92.8	1.9	0.0	0.0	0
Marseille	0.5	1.4	0.9	33.5	34.6	1.1
Observations	418	138		794	266	

Note: *** p<0.01; ** p<0.05; * p<0.1. The p-values are obtained from equality of proportion tests. The information on the company's size is collected using several website such as societe.com and infogreffe.fr. This information is not always present on these sites.
Source: Diademe testing, TEPP-CNRS.

or even higher levels than those observed in certain studies using applications in response to an offer (Edo & Jacquemet, 2013). However, they are slightly lower in the control group than in the treated group. Before and after treatment, the difference in the positive response rates between applicants of North African origin and those of French origin is significant at the 5% threshold in both the treated and control groups.[7] In both groups, discrimination does not vary significantly between the two periods. The before-after variation in discrimination between the two groups, presented in the last column of the table, is not significant either.

Tables A1 and A2 in Appendix show these same results separately for Chambéry and PACA. The results are generally similar in both regions. However, we can see a significant reduction in discrimination in the control group, even though this does not result in a significant difference in the variation between the two groups.

Figure II presents the estimated levels of discrimination for the companies of the treated group and the control group in the PACA region, in Chambéry and for both the regions in the period prior to the start of treatment. The differences in the positive response rates between the applicant of North African origin and the applicant of French origin are significantly different from 0 at the 5% threshold in each of the regions and

7. The positive response rates generally show a declining trend between the two periods. This decline is significant at the 10% threshold for the applicants of North African origin in the treated group and at the 5% threshold for the applicants of French origin in the control group. However, these variations do not significantly change the differences in the positive response rates between applicants.

Table 2 – **Positive response rates before and after treatment**

	Before treatment	After treatment	Difference	Difference-in-differences
Treated group				
French origin	45.54	41.09	−4.45	
North African origin	34.65	26.73	−7.92*	
Difference	−10.89**	−14.36***	−3.47	
Control group				−9.07
French origin	33.66	27.72	−5.94**	
North African origin	19.97	19.64	−0.33	
Difference	−13.69***	−8.08***	5.61	

Note: *** p<0.01; ** p<0.05; * p<0.1. The p-values are obtained from equality of proportion tests. The last column of the table shows the difference in variation of discrimination between the treated group and the control group and between the two periods (−3.47−5.61).
Source: Diademe testing, TEPP-CNRS.

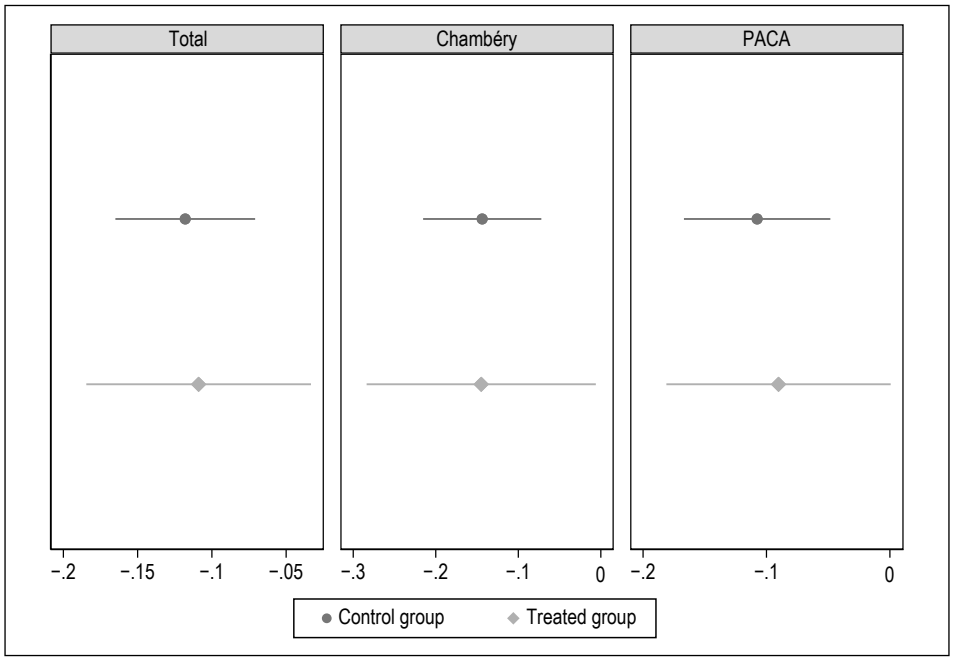
each of the groups except in the treated group for the PACA region where the difference is significant at the 10% threshold. However, the levels of discrimination are not significantly different between the companies of the treated group and the control group.

This lack of any significant difference in the level of discrimination between the two groups suggests that, although they are of different sizes, the companies in the treated group and the control group share similar discriminatory characteristics and behaviours. A comparison of the differences in the positive response rates between the applicant of North African origin and the applicant of French origin after the treatment occurred could therefore give unbiased results. However, it seems wiser to eliminate any unobserved differences by comparing the variations in differences in the positive response rates.

3.2. Difference-in-Differences

Table 2 also gives an initial general view of the variations in positive response rates obtained

Figure II – **Comparison of discrimination levels between the treated group and the control group prior to treatment**



Note: The 95% confidence intervals are presented. The standard errors used to calculate the confidence intervals are clustered at the company level. The control group observations are weighted by the inverse of the number of control subjects selected for each company treated. The estimated $\hat{\beta}$ coefficients are obtained based on ordinary least squares estimates of the model $REP_{iet} = \alpha + \beta\ NAfr_i + \varepsilon_{iet}$ during the period prior to treatment.
Source: Diademe testing, TEPP-CNRS.

by the two applicants between the period prior to treatment and the post-treatment period. The positive response rate for the applicant of French origin decreases significantly between the two series in the control group but not in the treated group. The positive response rate for the applicant of North African origin decreases, although not very significantly, in the treated group but not in the control group. These facts suggest no substantial reduction in discrimination in the treated group by comparison with the control group.

Table 3 presents the ordinary least squares estimates of equation (1). Columns (1) and (2) present the estimates performed on the whole sample, columns (3) and (4) on the observations relating to the Chambéry region and columns (5) and (6) on the observations relating to the PACA region. In columns (2), (4) and (6), the order in which the requests for information are sent is included in the checks without any major change in the results.[8]

The results show the existence of significant discrimination in the control group in the period prior to the treatment. The applicant whose surname and first name evoke a North African origin has a 12.5 percentage-point lower probability of obtaining a positive response than the applicant presumed of French origin, in the first test in the control group. Since the positive response rate is about 27% in the control group in the period prior to the treatment, in relative terms this corresponds to a penalty of slightly less than 50% to the disadvantage of the applicant of North African origin. This penalty is of the same order of magnitude, although at the high end of the range, as the results obtained by prior studies examining discrimination related to ethnic origin in access to employment. In France, recent studies show that the penalty adversely affecting applicants of North African origin is around 40% (Chareyron *et al.*, 2022). In the United States, Bertrand & Mullainathan (2004) found a 33% difference in the response rate between a white applicant and an Afro-American candidate. In Belgium, Baert *et al.* (2015) obtained 31% fewer responses for the Turkish applicant than for the Flemish applicant. This suggests that the use of requests for information in a correspondence test gives similar results to those obtained with responses to job offers.

However, no significant variation in the level of discrimination can be seen post-treatment or between the control group and the treated group, at the 5% threshold. In particular, the estimated coefficient associated with the variable $T \times NAfr \times Post$, which captures the effect of the training measure, is not significant at the 5% threshold.

However, the lack of significance of the estimated coefficient could be due to a lack of statistical power rather than a real lack of effect of the treatment. Consequently, we now endeavour to determine whether these results can be interpreted as the lack of effect of the training measures. To do so, we calculate the 95% confidence intervals. The results are presented in the second part of Table 3. While it seems unlikely that the effect of the training could be negative, i.e. that it could increase the level of discrimination, considering the upper bound of the confidence interval, we can reasonably rule out an effect of more than 13.3 percentage points. With a 13.7 percentage-point difference in the positive response rates in the control group, this corresponds to a maximum reduction in discrimination of around 100% in relative terms. The estimated effect of the measures is therefore imprecise and it is not possible to rule out a non-negligible effect of training on discrimination.

As mentioned earlier, to take part in the controlled experiment we selected, for each company of the treated group, control companies that are similar in terms of their observed characteristics. The two groups therefore share similar discriminatory characteristics and behaviours prior to the occurrence of treatment. One alternative to the triple differences estimation strategy could therefore be to compare the differences in the positive response rates between the applicant of North African origin and the applicant of French origin after the treatment occurred. Similarly, insofar as it seems unlikely that a shock may have affected the positive response rates of the applicants of North African and French origin differently between the two periods, another possibility could be to compare the change in differences in the response rates for the two applicants between the period prior to the training measure and the period following it. While it does have advantages in terms of identification, triple differences estimation has the disadvantage of having less statistical power.

The results obtained in column (2) of Table 1 are therefore compared with those obtained by double differences on a cross section (post-treatment treated/control comparison) and double differences in time (treated group before/after comparison). Figure III shows the

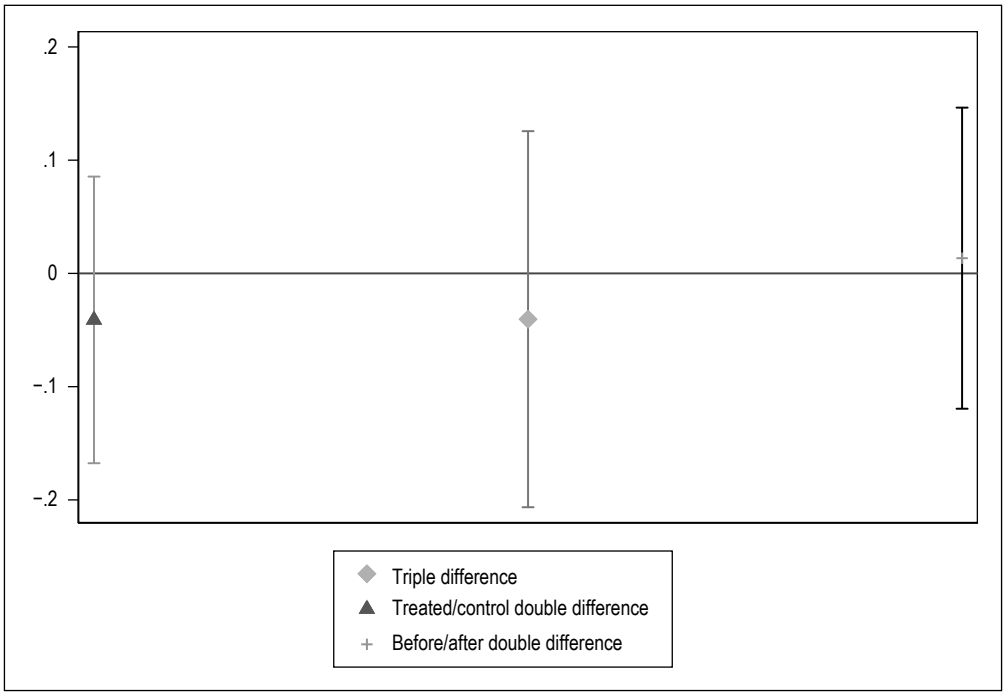
8. Deleting from the sample the few companies of the control group with more than 300 employees does not change the results (see Table A3 in Appendix).

Table 3 – **Effect of the training measures**

			Chambéry		PACA	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
NAfr	−0.126***	−0.125***	−0.167**	−0.166**	−0.077	−0.089*
	(0.039)	(0.039)	(0.073)	(0.073)	(0.053)	(0.053)
T × NAfr	0.019	0.022	0.047	0.045	−0.031	−0.008
	(0.058)	(0.059)	(0.114)	(0.113)	(0.084)	(0.083)
Post	0.173	−0.064	−0.265	−0.290	0.098	−0.964***
	(0.183)	(0.227)	(0.299)	(0.361)	(0.262)	(0.340)
NAfr × Post	0.042	0.043	0.064	0.064	−0.025	0.003
	(0.053)	(0.053)	(0.087)	(0.086)	(0.071)	(0.072)
T × Post	0.006	−0.008	0.120	0.118	−0.056	−0.106
	(0.062)	(0.061)	(0.113)	(0.112)	(0.085)	(0.086)
T × NAfr × Post	−0.033	−0.040	−0.078	−0.078	0.065	0.032
	(0.084)	(0.085)	(0.146)	(0.146)	(0.110)	(0.109)
Lower CI	−0.198	−0.207	−0.364	−0.364	−0.151	−0.182
Upper CI	0.132	0.127	0.208	0.208	0.281	0.246
Max. relative effect	0.96	0.92	1.50	1.50	2.06	1.81
Date fixed effects	X	X	X	X	X	X
Company fixed effects	X	X	X	X	X	X
Order of sending		X		X		X
Observations	3,232	3,232	1,112	1,112	2,120	2,120
R^2	0.162	0.166	0.293	0.293	0.177	0.195

Note: *** p<0.01; ** p<0.05; * p<0.1. The coefficients estimated by the ordinary least squares method are presented in the table. The standard errors clustered at the company level are shown in brackets. The control group observations are weighted by the inverse of the number of control subjects selected for each company treated. The maximum relative effect is obtained by dividing the upper bound of the confidence interval by the level of discrimination in the control group in the period prior to treatment.
Source: Diademe testing, TEPP-CNRS.

Figure III – **Comparison of the estimated coefficients obtained based on double- and triple-difference estimates**



Note: Triple difference corresponds to the estimated coefficient associated with the variable $T \times NAfr \times Post$ of equation (1). Treated/control comparison corresponds to the estimated coefficient associated with the variable $T \times NAfr$ of equation (1), estimated on the sample of companies tested post-treatment. Before/after comparison corresponds to the estimated coefficient associated with the variable $NAfr \times Post$ of equation (1), estimated on the sample of companies belonging to the treated group.

estimated effects of treatment based on these three different strategies. In the three cases, the estimated effect is not significant at the 5% threshold and the estimated coefficients are generally very close to 0. However, although the estimation by comparison of the level of the treated group with the control group is slightly more precise than the others, the upper bounds of the confidence intervals mean that we cannot rule out the measure having an effect of less than 10 percentage points.

* *
*

Recruiter training is the main public measure used in France to combat discrimination in access to employment. Moreover, it was made compulsory for companies with more than 300 employees by the Equality and Citizenship Act of 29 January 2017. In this study, we assess the effect of measures which are similar in content and intensity to this compulsory training. But the results obtained from difference-in-differences estimation on experimental data do not confirm their effectiveness. Discrimination against the applicant of North African origin existed before the treatment and was not significantly reduced by the training measures four months after their implementation. This result suggests that the public policy consisting of imposing a compulsory training measure of low intensity with mainly legal content is not an adequate response to the challenge of ethno-racial discrimination in the labour market. It would be advisable either to bolster the intensity of the training measures, by increasing the duration or frequency of the sessions, or to change the nature of these measures so that they may effectively change behaviours, or else add complementary measures. This also suggests that other measures taken at the company level, such as centralisation of the human resources department, could be more effective than training (Berson *et al*., 2020) in reducing discrimination.

However, given the size of the sample and in particular the limited number of companies present in the treated group, we cannot be certain of detecting a small or even a major effect of these measures. This is the main limitation of this study. It would therefore be interesting to reproduce this type of assessment, if possible on a larger scale, to see whether an effect can be detected. Each test has a certain probability of detection of the effect, which may be low, especially if the actual effect is small. Moreover, difference-in-differences estimation reduces the statistical power compared with a mere comparison of proportions between two groups. Ideally, therefore, the companies which will receive the training should be selected randomly. That would also eliminate the risks of bias due to any unobserved characteristics varying over time between the groups.

The second limitation is due to the fact that the results are potentially influenced by the time allowed between execution of the training measures and the second series of tests. The five-month time horizon covered by this study could be too long to detect any very-short-term effect of the measures.

Lastly, this assessment only examines whether recruiters respond in the same way to requests for information on job opportunities depending on the presumed ethnic origin of an applicant. It cannot account for discrimination in the subsequent stages of the recruitment process. The identification of discrimination at the stage of requests for information is, however, widespread and used in other studies (Anne *et al.,* 2022 on the labour market; Bunel *et al.*, 2021 and Le Gallo *et al*., 2020 on the housing market). □

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APPENDIX

Table A1 – **Positive response rates before and after treatment (Chambéry)**

	Before treatment	After treatment	Difference	Difference-in-differences
Treated group				
French origin	44.93	39.13	−5.80	
North African origin	30.43	23.19	−7.24	
Difference	−14.5*	−15.94**	−1.44	
Control group				−0.49
French origin	35.41	33.49	−1.92	
North African origin	21.53	18.66	−2.87	
Difference	−13.88***	−14.83***	−0.95	

Note: *** p<0.01; ** p<0.05; * p<0.1. The p-values are obtained from equality of proportion tests. The last column of the table shows the difference in variation of discrimination between the treated group and the control group and between the two periods (−1.44−(−0.95)).
Source: Diademe testing, TEPP-CNRS.

Table A2 – **Positive response rates before and after treatment (PACA)**

	Before treatment	After treatment	Difference	Difference-in-differences
Treated group				
French origin	45.86	42.11	−3.75	
North African origin	36.84	28.57	−8.27	
Difference	−9.02	−13.54**	−4.52	
Control group				−13.59
French origin	32.75	24.69	−8.06**	
North African origin	19.14	20.15	1.01	
Difference	−13.61***	−4.54	9.07**	

Note: *** p<0.01; ** p<0.05; * p<0.1. The p-values are obtained from equality of proportion tests. The last column of the table shows the difference in variation of discrimination between the treated group and the control group and between the two periods (−4.52−9.07).
Source: Diademe testing, TEPP-CNRS.

Table A3 – **Effect of the training measures (without companies in the control group with more than 300 employees)**

			Chambéry		PACA	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
NAfr	−0.122***	−0.120***	−0.165**	−0.164**	−0.067	−0.079
	(0.040)	(0.040)	(0.074)	(0.075)	(0.055)	(0.055)
T × NAfr	0.014	0.017	0.045	0.044	−0.042	−0.019
	(0.059)	(0.060)	(0.115)	(0.114)	(0.086)	(0.085)
Post	0.169	−0.067	−0.250	−0.273	0.114	−0.944***
	(0.184)	(0.228)	(0.303)	(0.364)	(0.265)	(0.342)
NAfr × Post	0.039	0.041	0.056	0.056	−0.035	−0.004
	(0.055)	(0.055)	(0.088)	(0.088)	(0.075)	(0.076)
T × Post	0.011	−0.003	0.116	0.114	−0.051	−0.100
	(0.064)	(0.063)	(0.115)	(0.114)	(0.088)	(0.089)
T × NAfr × Post	−0.029	−0.036	−0.069	−0.069	0.076	0.039
	(0.086)	(0.086)	(0.148)	(0.148)	(0.114)	(0.113)
Date fixed effects	X	X	X	X	X	X
Company fixed effects	X	X	X	X	X	X
Order of sending		X		X		X
Observations	3,132	3,132	1,072	1,072	2,060	2,060
R^2	0.163	0.167	0.294	0.294	0.178	0.196

Note: *** p<0.01; ** p<0.05; * p<0.1. The coefficients estimated by the ordinary least squares method are presented in the table. The standard errors clustered at the company level are shown in brackets. The control group observations are weighted by the inverse of the number of control subjects selected for each company treated.
Source: Diademe testing, TEPP-CNRS.