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Job Search and Intermediation under Discrimination: Evidence from Terrorist Attacks in France

Dylan Glover

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Dylan Glover*
INSEAD

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Abstract

Using detailed, high frequency data on potential job matches made through the French Public Employment Service (PES), I present evidence showing that search intensity both by and for minority jobseekers is highly sensitive to a shock that increases bias against their type. In the 10 weeks following the January 2015 "Charlie Hebdo" attacks, employers significantly reduce their search for minorities – jobseekers defined as having a first name of Arabic origin – to fill their vacancies as compared to majority jobseekers – those with classically French sounding first names. Minorities also drastically reduce their job search intensity after the shock. These drops are offset by a substantial increase in matching effort made by job counselors for their minority jobseekers after the shock. This counselor "compensatory effect" is driven by counselors who are themselves minorities and for majority counselors who specialize in getting the most marginalized jobseekers back to work. In addition, these effects are strongest in areas of *low* latent discrimination, proxied for by the local extreme-right vote share. Overall, I find no significant employment effects, but this belies strong heterogeneity: Significant negative employment effects on minorities are observed in micromarkets outside of the job counselors' purview. This suggests that labor market intermediaries can play an important role in mitigating adverse shocks that reduce the efficiency of the labor market matching technology.

Keywords: Labor, Discrimination, Job Search, Public Policy **JEL Codes:** J71, J78, J64

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1 Introduction

In this paper I examine the effects of a shock that potentially dramatically increased labor market uncertainty for a specific group of jobseekers. I exploit the ethno-religious terrorist attacks in and around the Paris region on the Charlie Hebdo satirical newspaper, the police and a Kosher supermarket between the 7th and 9th of January, 2015 as an exogenous shock that may have substantially affected the labor market outcomes of Muslim minorities in France. The focus of the analysis is on the search intensity of three labor market actors following the shock: jobseekers registered with the Public Employment Service (PES), their job counselors and employers. The evidence from this analysis is then used to better understand the employment effects and the value of intermediation in the labor market.

Using a detrended difference-in-differences strategy whereby I control for existing differential seasonal minority and majority group trends, I find that the shock led to a large decline in job search effort both for and by minorities – defined as individuals with Maghreb/Mashriq-sounding first names – compared to majorities – those with French-sounding first names – in the 10 weeks following the terrorist attacks. Search intensity is measured by the average number of personalized job advertisements that employers make to jobseekers registered with the PES for their vacancies, the applications that jobseekers make to vacancies, and the potential matches created by their job counselors. I find that the shock induces employers to reduce their search for minority jobseekers to fill there permanent contract vacancies by 9%, on average, while minority jobseekers reduce their own search intensity by 13%.

Given the large decrease in job search effort both of- and for minorities we might expect minority employment outcomes to significantly deteriorate. This is not the case. I find statistically insignificant changes in employment creation for minorities on aggregate. This finding is not consistent with a job search model in which minorities face a lower job finding probability due to a deterioration of the matching technology or increased employer bias against their type. One response to this puzzle is the important role that intermediaries play in the labor market. I find that job counselors react to the shock by increasing the number of potential matches for their minority jobseekers compared to majorities in the weeks that followed the attack, an increase of 16.5%. Consistent with this explanation, I do find negative employment effects for minorities following the shock in micromarkets (industry x local employment agency) where existing intermediation levels are low: A drop of 8.8% in the minority job finding rate in standard contracts is observed in markets within the bottom half of the pre-exisiting intermediation distribution. Furthermore, this “compensatory effect” is centered on counselors who are themselves minorities, but also on majority counselors whose job entails working with the most difficult casework. I find no compensatory effect for normal majority counselors, suggesting that some intermediaries better perceived the potentially degraded labor market outcomes of their minority jobseekers.

A broad literature on the effects of discrimination on the labor market outcomes of minorities has been expanding since the seminal work of Becker (1957). This work, *The Economics of Discrimination*, viewed the effects of discrimination, defined as differential treatment of equally productive minority and non-minority workers, as a result of the disutility employers experience when employing minorities. Due to this “distaste,” minority workers must compensate the employer’s bias by either being more productive than non-minorities or by accepting inferior wages for equal productivity. This conception of discrimination was followed by Phelps (1972) and Arrow (1973), who tackled discrimination not as problem of taste, but one of imperfect information. Employer’s beliefs or priors about the average productivity of a minority group may lead to the unequal treatment of equally qualified workers. To date, these works, and the research that built on their insights, have focused primarily on the interaction of workers with the employer, either during the hiring process (start with Lang and Lehmann (2012) for a review of the theory literature and Bertrand and Duflo (2017) for applied work) or, more recently, on-the-job (Hjort (2014) and Glover, Pallais, and Pariente (2017)). Topics that have received little attention relate to how discrimination, or the perception of it, might affect job search itself and how labor market intermediaries might internalize it.

Substantial work on the value of intermediation in the labor market has also been undertaken over the past 20 years. Autor (2009) argues that one of the main reasons that intermediaries exist in the labor market is simply because job search is costly. The results in this study show that in the presence of these costs, a group specific shock may drastically reduce the returns, or at least the perception of the returns, to search for some jobseekers. This causes labor market matching efficiency to drop. This situation augments the importance of intermediary intervention because job counselors can compensate for the resulting decline in matching efficiency. Consistent with this idea, Card et al. (2017) have shown that job search assistance programs may be particularly beneficial for disadvantaged populations, particularly because search may be relatively more costly for them (Fougere et al., 2009). Yet there are also contexts in which job search assitance may have little effect (Van den Berg and Van der Klaauw, 2006) or that the matches facilitated by intermediaries may be of lower quality (Crépon et al. (2005); Cottier et al. (2017)). Thus I do not make claims about the quality of the matches facilitated by intermediaries following the shock. The contribution of this paper is to show that job counselors may play an important role in limiting the loss of matching technology efficiency that passes through changes in firms’ search preferences and changes in the volume of discouraged jobseekers (Pissarides, 2000).

I show that it is difficult to disentangle the direct effect of discrimination on employment outcomes due to changes in actual employer preferences from the search intensity of, or for, minorities. If we think of search effort as inputs that improve the efficiency of the matching function in which there are intra- and intergroup congestion externalities, attributing the shock’s impacts on hiring outcomes exclusively to increased employer discrimination would be imprudent. I show that it is important to carefully analyze the relationship between search effort and employment because difference in differences does not allow us to distinguish between a change in employment outcomes due to changes in minority search intensity, from a change in actual employer bias. The fact that we measure actual changes in employer search for minority candidates gives much more reliable evidence that minorities did indeed face more discrimination on the market following the shock.

This paper thus contributes to the literature that concerns itself with how to measure, and indeed, the very existence of discrimination in the labor market.[1]

On a more basic level, this study also adds to the literature on the effects of ethno-religious terrorist attacks on market outcomes for Muslim minorities. Previous results are mixed when it comes to the effects on employment outcomes. Åslund and Rooth (2005) find that attitudes towards Muslim minorities in Sweden changed after the 9-11 terrorist attacks, but find no evidence that this translated into worse employment outcomes for these minorities. Davila and Mora (2005) and Kaushal et al. (2007) find that Arab minorities in the US may have experienced lower wages compared to majorities after 9-11, but no effects on employment. Gautier et al. (2009) find that local housing prices diminish in minority neighborhoods and residential segregation increases following an ethno-religious terrorist attack, reflecting similar results found by Ratcliffe and von Hinke Kessler Scholder (2015) who also show that these types of attacks had a negative effect on employment levels in neighborhoods with a higher percentage of minority residents. Miaari et al. (2012) find that ethno-religious violence increased the separation rates of Arab minorities in Israel. The rich data and analysis in this paper can thus contribute to a better understanding of how to interpret the results on the employment outcomes analyzed in these studies.

I refine the analysis using a measure of existing, or latent discrimination at the local level. I use the municipality level vote share for the extreme-right political party in France, the Front National (FN), in the 2012 French presidential elections. These data are used to proxy for the existing discrimination that minority jobseekers may face in their job search. Consistent with the idea that the terrorist attacks can instrument a change in the bias faced by Muslim minorities in France, I find strong heterogeneous effects of the shock on jobseeker and counselor search intensity across this dimension. While we see a negative effect on a minority's own search effort in both high and low FN areas, the impact is almost four times as large in *low* extreme-right areas. Likewise, the counselor compensatory effect is also only observed in areas of low latent discrimination. These, perhaps, striking results can be rationalized by modeling the marginal effect of discrimination on minority job search as diminishing. Thus the marginal effect of a large shock to bias should be larger in areas with relatively low initial levels of discrimination.

To support the use of the extreme-right vote share as a proxy for existing discrimination I make use of Google search trends data, first looking at search trends in the year before the shock, disaggregated by French region. I find that the FN vote share is indeed strongly correlated with Google searches that connote the prevalence of discrimination and discriminatory animus towards the minority group. I then measure the change in search trends around the date of the January attacks. We wee that the volume for these negative search terms sharply increases due to the shock and continues to be highly positively correlated with the FN vote share. Yet we see that Google

[1]Correspondence studies (see Riach and Rich (2002) for an introduction) generally find large discrepancies in call back rates for minorities with identical resumés as compared to majorities (see Bertrand and Mullainathan (2004) in the US and , Petit et al. (2011), Adida et al. (2016) and Behaghel et al. (2015) for the French context). Yet Heckman (1998) has made the point that the results of correspondence studies can be hard to interpret as indicating the presence of discrimination in the labor market because they are based on the premise that workers apply randomly to jobs.

searches for these terms did not disproportionately rise in regions with low extreme-right vote shares suggesting that the functional form relating minority job search intensity and discrimination drives the heterogeneous results.

The paper is organized as follows: Section 2 describes the data sources and provides a description of the sample. Section 3 illustrates how we identify the effect and presents the baseline specification. This is followed by section 4 in which I present the main results on the shock's impact on search intensity, hires and provide an interpretation of the hiring effects. Section 5 attempts to add clarity to our understanding of these effects by exploiting the large heterogeneity in effects we see across latent levels of discrimination, counselor type and intermediation levels. Section 6 concludes.

2 Data

Administrative data

We have access to rich historical administrative data at the daily level for just under 4.4 million jobseekers registered with the PES over the 10 weeks before and after the shock. I isolate these jobeekers because I can categorize them as either majority or minority using their first names (we explain this procedure below). We have their personal characteristics, their hires and the potential matches made by them, to them or on their behalf. These three matching channels are defined as follows: (1) Jobseeker initiated: Jobseekers apply directly to vacancies posted with PES. (2) Employer initiated: Employers search for jobseekers in the PES "CV Bank" that correspond to their hiring needs and make a personal job advertisement to the jobseeker, encouraging them to apply. (3) Counselor initiated: They propose a vacancy to a jobseeker then verify the fit and interest of the jobseeker. Once verified they generate an official document "obliging" the jobseeker to apply. This official document takes the form of a letter sent to the jobseeker by mail, email or to their PES personal webspace in which there is a code and link to apply to the vacancy accompanied by the job description.[2]

Of course, these potential matches made through the PES are not the only way candidates and employers match in the French labor market. That said, in addition to the vacancies posted directly with the PES it claims to make available, i.e. duplicate, roughly 70% of vacancies posted on other job search platforms. Thus the PES data provide a considerable slice of the search and matching that happens in the French labor market. I exploit the date, channel and group status of the jobseeker concerned by the potential match to create the key series for the analysis: each labor market actor's search intensity over time.

The hiring data is a near exhaustive measure of job creation flows in the French market. These hiring declarations, required by French law, are called *Déclaration préalable à l'embauche* and firms are required to submit them before, or shortly after, the contract start date. Thus we have a

[2]Unfortunately we do not have information on whether the jobseeker actually follows up nor on the sanctions the jobseeker faces if they do not follow through on the act of intermediation. Though, in theory, refusing three "reasonable" job offers can result in penalties on unemployment insurance benefits.

reliable measure of job creation for the entire population of jobseekers.[3]

Using the hiring declarations, I extract the contract type, its start- and end-dates (for fixed-term contracts) and the personal identifier to link the hire to jobseekers on the PES roster. Using the start and end dates for fixed-term contracts we calculate the number of workdays created for each contract. I do this because these declarations are contract flows and thus are not directly a measure of employment. For example, a week of one-day (Monday to Saturday) hires for the same individual would be counted as 6 fixed-term contract flows, but as only one contract if it were a fixed-term contract that ran for the week. By calculating workdays joint with permanent contract flows and temp hires, it allows us to have a measure of total employment creation.

Names

To assign minority status we use the first names of jobseekers available in the PES administrative data. We do this because it is illegal to collect data on ethnicity in France. The first name data are the same used in Algan, Mayer, Thoenig et al. (2013) and Behaghel, Crépon, and Le Barbanchon (2015) who, like in this study, use it as a proxy for the origin or ethnic background of an individual.[4] This data set links 23,388 first names to nine etymological origins: French, Maghreb/Mashriq, sub-Saharan African, Asian, British, Germanic, Jewish, Southern European and Eastern European. The categorization was compiled using register data on birth names given to French babies from 2003-2007 by Algan et al. (2013). We define majority status as those jobseekers with French first names and minority status as those with Maghreb/Mashriq first names and will also exploit British and Southern European names as a robustness check.

A measure of existing discrimination

I contrast the results through a local measure of latent bias towards minorities using the vote share for the Front National (FN) party in the first round of the 2012 French presidential elections. This is the major extreme-right party in France. We'll use the 36,565 commune-level, or municipality-level, vote shares for the FN. Each commune is administratively attached to a PES agency. I aggregate the total vote share for the FN in all municipalities in the agency's purview and assign it this score. We thus have local-level variation in a proxy for existing discrimination.[5]

Data structure

We take PES administrative data on search intensity and hires at the daily-jobseeker level, combined with names and vote data, and aggregate it to the week-agency-group level for the 10 weeks before and 10 weeks after the shock.[6] We thus have two observations per week for each local

[3]Exceptions to the requirement for this hiring declaration concern internships and volunteer contracts, the recruitment made by private individuals and some public sector jobs.

[4]Glover, Pallais, and Pariente (2017) also use names to identify minority status in France. For a discussion about how naming is also related to preferences for social integration and cultural transmission, start with Algan et al. (2013) who estimate the economic penalties associated with having a first name of Maghreb/Mashriq origin.

[5]We will discuss this at length and provide empirical evidence on the link between the FN vote share and discriminatory attitudes in the sections below.

[6]We aggregate because it is computationally more efficient given that the shock is aggregate and, at most, the

employment agency, one for the minority and one for the majority population in the agency. In total we follow 810 local employment agencies throughout mainland France and Corsica over the 20 weeks spanning the attack. This gives 68,400 minority/majority-agency-week observations.

Google Trends

Finally, to determine whether the shock and FN vote are correlated with discriminatory attitudes, we use Google trends data on search volume over time for all of France and comparatively across French regions. Specifically, we look at key search terms before and after the date of the shock that may be correlated with the prevalence of discrimination and animus against minorities as well as search terms that connote social cohesion. We then look at the correlation of these search scores and the extreme-right vote share at the regional level to understand what our local measure of latent bias proxies for.

2.1 Distribution statistics

Table 1 displays distribution statistics for jobseekers at the employment agency level in the 10-week pre-shock period. Column 1 shows the overall average proportion per agency while columns 2 and 3 show the relative proportion within majority and minority populations, respectively. In examining the typology of jobseekers registered with the PES we see that around 71% are currently unemployed and looking for full time work in a permanent contract. Comparing across groups, minorities are about 8 percentage points more likely to be in this category than majorities. The next most frequent type of jobseeker are those looking for part-time work followed by individuals looking for fixed-term, temp or seasonal work and those engaging in on-the-job search. Minorities are relatively less likely to fall into these last two categories.

Perhaps unsurprisingly, we see that while 98% of jobseekers with French sounding first names are French nationals only 62% of jobseekers with Maghreb/Mashriq first names are. We also see that we categorize zero jobseekers who are born in the Maghreb as majorities while 30% of our minority jobseekers are Moroccan, Algerian or Tunisian nationals.

Turning to demographic characteristics, we see that minority jobseekers are more likely to be male, less likely to have a college degree or be categorized as skilled labor (high qualification), but are about 18 percentage points more likely to live in a Sensitive Urban Zone (*Zone Urbaine Sensible* or ZUS). These are residential zones that have been prioritized by the French government to receive additional resources and funding because they exhibit significantly higher levels of unemployment and poverty.[7]

We also see significant differences between groups in the type of professions jobseekers are looking to work in, suggesting that there is large occupational segregation in the French context.[8]

variation in discrimination we will be utilizing is at the agency level i.e variation in the FN vote share.

[7]In 2014, the *Observatoire National des Zones Urbaines Sensibles* recorded a total of 751 ZUS in France in which 4.4 million people live. See `http://publications.onzus.fr/rapport_2014` for more information.

[8]See Aeberhardt et al. (2010) for a discussion on the types of jobs taken-up by minorities and its relationship with wage differentials between groups.

Minorities are less likely to search in commerce and sales, IT, accounting, human resources and secretarial work, while they are more likely to look for jobs in construction, transport and "personal services" which may include child care services or living assistance for the elderly.

In examining jobseeker applications, or potential matches, we also see stark contrasts between groups. The average jobseeker application rate (number of applications divided by number of jobseekers) is over 20 percentage points higher for minorities compared to majority jobseekers (+80%). The average rate for counselor initiated matches is also four points higher for minorities (+22%). It thus appears that minority jobseekers may be more reliant on the PES for their job search. The differential in counselor initiated matches is also intriguing and could be linked to the fact that skills searched for in vacancies posted with the PES are, on average, better matched to the skills of minority jobseekers.[9] Yet we cannot exclude the hypothesis that counselors treat minorities differently, a point we will we come back to in the discussion on the impact of the shock. Finally, perhaps surprisingly, we see very little pre-shock difference between groups in the average rate of potential matches initiated by employers.

For hires we see no difference across groups in the rate of being hired in the standard and most prized type of contract, a permanent contract, but see large differences for the other types of contracts: -13 points for fixed-term contracts and +12 points for temp, or interim, contracts for minorities compared to majorities. These last statistics are also interesting when we keep in mind the level of pre-shock discrimination in the market. Fixed-term contracts are very hard to break legally before the end-date by either the employer or the employee unless a better contract has been found in the mean time, while temp work requires no contractual agreement between the individual and the employer, only between the temp agency and the employer. Thus personnel can be easily changed at the request of the employer. Unfortunately, these hiring dynamics are beyond the scope of this paper. In terms of how people find these jobs, the PES conducts representative surveys of jobseekers leaving their rosters. In 2014, 13.2% of jobseekers found their job through a PES counselor. This ranks third in the way people find jobs, lagging behind applying on one's own (25.9%) and personal networks (21%).[10]

Finally in describing the sample, we look at the average number of jobseekers registered per local employment agency. On average there are 972 minorities per 5428 registered jobseekers. And looking at what will be our latent bias proxy, we see that the proportion is lower for agencies that have below-the-median vote shares for the FN, but not dramatically so.

[9] These baseline differences are interesting and may speak to the underlying differences in demographic characteristics between majority and minority jobseekers. But it may also be evidence of pre-existing discrimination in the market, i.e. more search effort is required for minorities to find a job, on average, a point made by many correspondence studies.

[10] Unfortunately, these statistics are not available by minority status. See Pôle emploi's *Enquête Sortant 2014* at *www.pole-emploi.org* for more information.

3 Identification and empirical specification

We will assume that the terrorist attacks were completely unforeseen by the French population, thus we will not concern ourselves with labor market actors modifying their behavior in anticipation of the shock. But since the shock could have had aggregate economic impacts, we want to control for this. A difference in differences, DD, specification is attractive because of this.

We have access to the preceding year to judge whether this approach is appropriate in our context because the PES began collecting the job search data by channel in January 2013. Appendix Figure A.1 plots the evolution of the key search outcome variables: the average number of potential matches made by jobseekers, counselors and employers for our time period of interest, but in the previous year: 2013-2014. Outcomes are binned at the weekly level for majority and minority populations and these points are fitted using a regression with a polynomial time trend of order 3. The vertical line indicates where the shock would take place in the following year. In this "placebo year" we see that the "pre-shock" weeks exhibit mostly parallel trends with some significant differential changes around the holiday weeks. In the "post-shock" period we see even starker diverging trends between groups. This suggests that there may be a strong seasonal effect that impacts minorities differentially to majorities in the beginning of the year regardless of the presence of any discrimination shock. In Table 1, we highlighted large differences in key characteristics between majority and minority populations of jobseekers as well as significant levels of occupational segregation. Hence it may be that job search and employment exhibit differential time effects between groups that are correlated with these observable characteristics, but also other, potentially important, unobservables. Thus it appears that the implementation of a standard DD approach may be hazardous because it will confound any impacts due to a discrimination shock with seasonal, group-specific variation around the beginning of the year.[11]

Because we have this previous year of data, we are able to identify causal effects of the shock given that the effect of the shock β_1 can be formulated by the following expectation function:

$$E(y_1|m,t,T) = E(y_0|m,t,T) + \beta_1 = \psi_m + \gamma_{mtT} + \beta_1$$

where y_0 is the outcome in absence of the shock and ψ the fixed group effect: $m = 1$ for minority and $m = 0$ majority. The term γ captures the group-specific time effect for the pre- or post-shock periods, $t \in \{0,1\}$. And $T = 1$ indicates that we are in the year where the shock takes place and $T = 0$ the preceding "placebo" year.

In appendix section A.1 I provide an in-depth exploration of this approach and demonstrate that, similar to a standard DD approach, identification is achieved when the *de-trended* (DT)

[11] Conditioning parametrically using pre-shock outcomes and characteristics in order to improve the credibility of a counterfactual parallel trends assumption is only a partial solution due to the problem of unobservables. Furthermore, since we observe relatively strong parallel trends in the expectation function in the pre-shock period, it is not readily apparent how we might gauge the validity of controlling parametrically for group differences that are correlated with the time effect.

group-specific time effects would have been the same absence the shock:

$$\gamma^{DT}_{m=1,t=1} - \gamma^{DT}_{m=1,t=0} = \gamma^{DT}_{m=0,t=1} - \gamma^{DT}_{m=0,t=0} \tag{1}$$

where $\gamma^{DT}_{m,t} = \gamma_{m,t,T=1} - \gamma_{m,t,T=0}$. Hence, even if trends would have differed naturally in the year of the shock ($T = 1$), we can still achieve identification if the *de-trended (DT)* evolution between groups would have been similar across periods in absence of the shock. Put another way, if the trends in detrended time effects are constant, a detrended difference differences DDD parameter identifies the shock's effect. And similar to the typical DD identifying assumption, we do not need the detrended levels in the time effect to be equal between groups, only the difference must be constant moving from period $t = 0$ to $t = 1$. This gives a new formalization to what Bell, Blundell, and Van Reenen (1999) and later Blundell and Dias (2009) coin as the "Differential trend adjusted difference-in-differences."[12] More recently, Draca et al. (2011) use a similar methodology to account for strong seasonality in their outcomes.[13] We are fortunate to observe multiple weeks before the shock, thus our main identifying test will involve examining the evolution of detrended outcomes in period $t = 0$ to give credence to the assumption that the difference would have stayed the same in absence of the shock.

The empirical model to estimate β_1 is thus,

$$y_{imtT} = \beta_0 + \beta_1(m * t * T) + \beta_2(t * T) + \beta_3(m * t) + \beta_4(m * T) + \beta_5 t + \beta_6 m + \beta_7 T + e_{imtT} \tag{2}$$

Observations are each population m in agency i at period t in year T. The parameter of interest is β_1. It captures a detrended difference in differences DDD parameter and can be described as the impact of the shock on minorities as compared to majorities on outcome y, controlling for potential differences in trends that may be present in period t regardless of the shock. The constant is β_0 and e is a normally distributed, zero mean error term.[14]

Finally, because the dependent variables are population averages (the number of potential matches or hiring outcomes divided by the number of registered jobseekers), we weight the regression equations by $\sqrt{n_{imtT}}$ where n_{imtT} is the number of jobseekers contributing to the observation's average.[15] Standard errors are clustered at the agency level to account for correlation in agency outcomes overtime and for correlation between minority and majority outcomes within agencies

[12] This methodology was also recently explored by Lee (2016) as "Generalized differences-in-differences."

[13] They exploit an ethno-religious terrorist attack in London to estimate the impacts of policing on crime.

[14] The advantages of this identification strategy are easily demonstrated in a simple DD setup. It is clear that if equation 2 is the true population model and we estimate a simple DD through

$$y_{it} = \alpha_0 + \alpha_1(m * t)_{it} + \alpha_2 t_t + \alpha_3 m_i + \epsilon_{it}, \tag{3}$$

we estimate $\widehat{DD} = \hat{\alpha}_1 + \beta_3$, where β_3 is the change in minority outcomes compared to majorities that happens in period $t = 1$ regardless of any discrimination shock.

[15] See section A.2 in the appendix for a discussion on the motivation for using WLS as opposed to OLS. We show later in the appendix that OLS regressions on unweighted data and also Poisson regressions on the pure count data give consistent results.

(Bertrand, Duflo, and Mullainathan, 2004).[16]

4 Impacts

4.1 A first stage

To illustrate the link between the terrorist attacks and a potential change of context in which Muslim minorities search for jobs, we refer to Figure 1 which plots Google search interest in France for the word "islamophobie," the equivalent of islamophobia in English, over the 18 months spanning the January 2015 attacks.[17] We see that the search interest is close to null until the week of the January attacks to which it quickly jumps to 100 in the week of the attack, the date by which all other points are normalized. And though we are not able to ascertain exactly why nor who searches for this term, the figure indicates that bias against Muslim minorities came prominently into the public consciousness in the weeks that followed the attack. We also see a second spike that corresponds to the November 13th attacks in Paris. This spike is about half the size of the first attack's even though it was much more deadly. Though labor market actors reactions before and after the second attack are endogenous to the January attacks, it may be the case that the first attack had a much more dramatic effect on the context in which minorities search for jobs in France. The weeks and months following the January attacks saw the spawning of numerous public debates in the media concerning the integration of French Muslims, such as the debate "Je suis Charlie" (I am Charlie) versus "Je ne suis pas Charlie" (I am not Charlie).[18] It would then follow that the the main change in the perception of islamophobia in France was due much more to the first attack. This conclusion is supported by data on real islamophic acts which we explore next.

We should now ask ourselves if this change in Google trends is indicative of an increase in realized manifestations of bias against Muslim minorities. Figure 2a shows that islamophobic acts for which law enforcement investigated (grey bars) increased by +223% in the year of the attack from the previous year, 2014. These include cases of violence, vandalism, threats, etc. The black bars also provide evidence that online hate speech recorded by the Ministry of the Interior ballooned by 121% in 2015 compared to 2014.

Unfortunately, more disaggregated data are not publicly available from the Ministry of the Interior. Thus to get a sense of the within-year timing of realized acts we refer to Figure 2b which plots the increase in islamophobic acts by month compared to the 2014 monthly average. These

[16] Clustering at higher levels of aggregation such as the employment zone or even at the regional level, to account for correlation in outcomes across larger markets, provides very similar standard errors and does not change inference on the results.

[17] Search interest is calculated as (number of searches for term) / (total Google searches). The search interest score is then normalized to the date with the highest search interest. To understand the relative volume of the search for islamophobie we refer to Figure A.2 in the appendix which plots search interest in islamophobie compared to the search interest for "trouver un emploi", translated as "find a job" and to Christine and the Queens, a popular French pop star who's debut album was released in early 2015.

[18] See Todd (2015) for an interesting discussion of these topics.

data come from the *Collectif contre l'islamophobie* (CCIF), an NGO dedicated to documenting discrimination against Muslim minorities in France. We see a huge spike, +154%, in islamophobic acts in the month of the attack and much smaller and inconsistently signed changes off of the 2014 monthly average in the following months.[19] The timing of the increase in realized islamophobic acts actually appears to be quite punctual in nature and consistent with the Google search data. Interestingly, these data also provide evidence that the November 13th attacks may have indeed had a much smaller impact on the discriminatory climate faced by Muslim minorities in France and suggests that changes in anti-muslim sentiment from subsequent attacks were strongly dependent on the Charlie Hebdo attack.

To be clear, I do not make the claim that these Google search trends and realized acts map completely to equally dramatic increases in the actual bias that minorities face on the French labor market. I simply argue that the January 2015 shock provides an exogenous change to the context in which labor market actors interact.

4.2 Search effort

We now turn to the main results to see whether the shock actually degraded minority jobseekers' employment prospects. Figure 3 plots a weekly DDD estimate in the observation period compared to a reference week in the $t = 0$ period (see the figure notes for the specification). The dependent variable is the average directed advertising effort that jobseekers receive from employers for their permanent contract vacancies. I divide the dependent variable by its pre-shock standard deviation; this will allow us to gauge effect sizes and compare across search effort channels. We discuss absolute effects below. Coefficients for each week are connected by a line with 95% confidence intervals denoted by vertical grey lines.

This figure also provides formal evidence to test the identifying condition presented in equation 1. We see strong strongly support for it. The pre-shock weekly coefficients show small and insignificant differences in employer search effort between minority and majority groups in the pre-shock period, the weeks up to- and including the vertical line. After the shock we see an immediate drop in the personalized advertising that firms make to minority jobseekers, a drop of about 0.3 standard deviations on average compared to the reference week and 0.24 standard deviations when taken over the first 4 weeks following the attacks. This suggests that the attacks reduced employer willingness to engage in a potential matching process with minority candidates. We cannot say why this happens, but it does provide strong evidence that the employment prospects of minorities has indeed been degraded by the shock.

I now add the homologous results for jobseekers and counselors in Figure 4. For jobseekers and counselors we see significantly more variation in pre-shock trends, but still strong supporting evidence for the parallel "detrended trends" condition with small and mostly insignificant differences in search effort between minorities and majorities compared to the reference week. We do see

[19] Unfortunately, the actual monthly numbers for 2014 are not presented in their report, only the total over the whole year.

significant differences for search effort of minority jobseeker in the two weeks during the holiday season suggesting that the detrending did not fully absorb the seasonal differentials. We discuss this more below when we examine the raw data. Out of a total of 27 estimated pre-shock week coefficients for the three actors, we find three that are statistically different from zero at the 5% level. Even though this is above what we would expect from natural randomness, the differences seen after the shock are orders of magnitude larger. Focusing now on jobseekers, we see a sharp drop in the job search intensity of minority jobseekers two weeks after the attack. This impact stays relatively constant for about a month before it starts to progress back towards majority levels of search effort in the very last week of observation. The magnitude of the impact is also striking: minority search effort drops by over 0.4 standard deviations in the fourth week after the shock compared to the reference week. Indeed, we note that employers appear to react more quickly to the discrimination shock, while the effect on jobseekers does not "kick-in" right away. This may suggest that the way in which labor market actors internalized the shock may vary considerably. It may take time for supply-side actors to realize that they are now actually facing degraded labor market prospects. This evidence is consistent with the existence of positive trading externalities between supply and demand side actors (Pissarides, 2000). Simply put, it implies that an increase (decrease) in the frequency with which actors are willing to trade on one side of the market, induces an increase (decrease) in the frequency with which actors on the other side of the market are willing to trade. In addition, if search effort is simply endogenous whereby minorities optimize effort by equating the marginal cost of search to the marginal change in the job finding rate, these results are entirely rational. Thus the effect we see on minority jobseekers could be categorized, at its most basic level, as a discouraged worker-effect. We will come back to this when we explore heterogeneous effects below.

Looking at the intermediaries search effort trend, we see all but one of the pre-shock week coefficients close to zero and insignificant, thus supporting the identification condition. Similar to jobseekers, counselors appear to react in the third week after the shock, but in contrast to employers and jobseekers they massively increase their search effort on behalf of their minority jobseekers in the weeks following the shock. This positive effect tops out at over 0.5 standard deviations in the fifth week after the shock before it starts to trend back towards zero in the final weeks of the observation period.

Plotting the evolution of the raw data also helps us to better understand the weekly DDD estimates we have thus far examined. Figure A.3 plots bins of the de-trended weighted mean application rate to permanent contracts for each week in the study period. These weekly averages are overlaid with predictions from an OLS regression on these bins with a polynomial time trend of order three. For clarity, these graphs are the visual equivalent to examining the evolution of the DDD estimate in Figures 3 and 4: estimating a simple difference in differences specification on these de-trended averages will give you estimates for the exact same parameter as β_1 in equation 2. Again, we can provide strong evidence for the identifying condition: the evolution of de-trended group averages are quite similar before the shock date (as denoted by the vertical line) for the three matching channels. The impact on employer initiated potential matches (Figure A.3a) is perhaps the least visually dramatic in the raw data, but still quite apparent: the gap between de-trended employer-initiated matches made after the shock period is significantly larger than the average pre-shock gap. Concerning jobseeker search intensity in Figure A.3b there is a more significant drop in the minority jobseeker application rate during the holiday season starting in week 8 in the pre-shock period, but if we look at the proportional drop between week 7 and 8, the difference is perhaps not as dramatic (0.5 for majorities versus 0.6 for minorities). This may also be linked to the fact that potential matches simply tend towards zero during the holiday season thus we would expect a larger drop for the group with the higher level de-trended match rate. That being said, we must acknowledge that the pre-shock difference is not perfectly constant during the holiday weeks (8-9).

Nevertheless, we see a much larger differential change in the search intensity trends for minorities after the shock. Illustrating the weekly coefficients that we plotted earlier, Figure A.3b shows a substantial drop in search effort of minorities as compared to majorities in the 3rd week after the shock that continues for six weeks before the gap gets closer to its pre-shock average in the final week. The impact on counselor behavior is equally dramatic. We see a significant closing of the gap between the average number of potential matches made for minorities compared to majorities so that the difference in average match rates between the two groups becomes almost indistinguishable in the post-shock period.[20]

We now turn to Table 2 to examine absolute and relative effects on search effort for the different labor market actors. Though we will consistently focus the analysis on the search intensity for permanent contracts because it is the standard contract in France and the vast majority of salaried employment is on this type of contract, it is also instructive to break down the effects we have thus far seen by contract type.[21]

Panel A of Table 2 displays impacts on potential matches made for all contract types. In the first column, we see a drop in total matches of roughly 0.002 matches per minority jobseeker, per week as compared to majorities, significant at the 5% level. This represents a reduction of about 3% off the mean minority weekly potential match rate in the pre-shock period (displayed at the bottom of each panel). As can be seen in the following columns, this effect masks considerable heterogeneity across matching channels. Minority search intensity drops by 0.005 applications per minority jobseeker, equal to a 10.7% drop in the 10 weeks following the shock. In contrast, we

[20]To better understand these de-trended graphs, we refer you to Figure A.4 in which we show these trends side by side by year. The trends for 2014-2015, the year of the attack ($T = 1$), are in the first column of graphs and the placebo year, 2013-2014 ($T = 0$), in the second.

[21]Permanent contracts are the most prized contract in France because it provides very stable employment (difficult to absolve on the employer side) and also dictates access to credit markets. France does not have credit scoring to manage debtor risk. As we will see below, short-term contracts may also be a way that employers could manage risk when faced with a new market context. In addition, the usage of short-term contract flows are highly industry dependent and thus do not provide an accurate picture of stable hiring on aggregate. In 2014, 86.4% of salaried employees were contracted with permanent contracts. This compares with 9.7% in fixed-term contracts 2.4% in temp work and 1.6% apprenticeships. See https://www.insee.fr/fr/statistiques/fichier/version-html/1560271/ip1569.pdf for a snapshot of the French labor market in 2014.

see an augmentation in counselor matching intensity for minorities compared to majorities equal to .0029 more matches, or +13% off the pre-shock mean. For all potential matches initiated by employers we see a much smaller and insignificant point estimate.[22]

When examining by contract type, the largest absolute and relative impacts for employer, jobseeker and counselor search behavior are seen for permanent contracts. Contextualizing the standardized effects presented above, we see that minority jobseekers make 13% less ($\hat{\beta}_1 \approx -0.0033$) candidatures to jobs offering permanent contracts while counselors increase their effort for minorities by 16.5% ($\hat{\beta}_1 \approx 0.0016$) compared to majorities. These are the aggregate DDD estimates that we examined in the preceding figures. We also see in panels C-D that the direction of the effect for jobseekers and counselors is consistent over the different types of contracts. This is not the case for employer search behavior. Column 4 in panels B-D show that the null overall effect of the shock for employer initiated matches on minorities masks differential effects by contract type. Employers reduce their search for minority candidates for their standard (and best) contracts while increasing the number of minorities that they contact for their fixed-term and temp-work jobs. Finally, in panel E we see very small mean levels of potential matches made on seasonal contracts and estimated coefficients are close to zero for all channels.

Returning to panel A in Table 2 is also instructive in judging the utility of the DDD specification as opposed to a standard difference in differences approach. The estimates of β_3 on the ($minority * period$) term are highly significant. This formally tests the change in the difference between minority and majority search behavior in $t = 1$ during the placebo year (we previously examined this visually in Figure A.1). And it shows that regardless of the presence of a major discrimination shock, minorities and majorities would have had differential outcomes in $t = 1$ regardless. Thus if model 2 is the true population model, simple DD would have given an estimated effect of the shock on potential matches that would have been significantly biased.

In sum, the evidence thus far presented supports the idea that minority jobseekers dramatically reduced their search effort in response to the shock and that this was, at least in part, a rational response because their employment prospects have indeed deteriorated: employers significantly reduce their search for minority candidates for their permanent contract vacancies, indicating that minorities do now face more bias on the labor market. The novel finding is that intermediaries also react to the shock, potentially compensating the deterioration of the matching efficiency.

To better understand these results it is necessary to examine how the search effort of each labor market actor co-varies naturally with respect to movements in other the actors' search intensity. As an example, denote s_i the search intensity of jobseeker of type i and c_k^i counselor of type k's matching effort for jobseekers i. If we allow for $cov(s, c) \neq 0$ then the two search efforts are functions of one another $s(t, c)$ and $c(t, s)$ over time t, omitting scripts. We can immediately see how an understanding of the natural covariance between labor market actors is important. A change in

[22] We see that the mean of the dependent variables presented in these tables are 1/10th that of the averages presented in the descriptive statistics table. This is simply because observations are now at the weekly level compared to Table 1 where we aggregated data over the total pre-shock period. Thus it suffices to multiply the coefficient and standard error by 10 to obtain the average DDD estimate over the entire period.

intermediary search intensity over time can be expressed as,

$$\frac{dc}{dt} = \frac{\partial c}{\partial t} + \underbrace{\frac{\partial c}{\partial s}\frac{ds}{dt}}_{\substack{+\text{ if complement} \\ -\text{ if substitute}}} \tag{4}$$

Indeed, counselor effort may be a substitute for jobseeker effort in normal times. A jobseeker may be discouraged in job search and the counselor picks up the slack. Or they may be complementary. This might be the case if counselors are most active with jobseekers who are also actively searching, also a case of positive trading externalities among supply side actors. Empirically we can check this. Figure A.5 plots coefficients from a regression of weekly counselor search effort on lags and leads of jobseekers (left-hand side) and employers (right-hand side) in the pre-shock period. The specification includes week dummies and counselor fixed effects. Variables are divided by their standard deviations so that bars indicate the change in counselor effort from a one standard deviation change in the other actor's effort level. We see that counselor and jobseeker search covary positively in normal times and that the largest correlation with jobseeker effort is evident from w_{-2} to w_0, with a one standard deviation increase in jobseeker effort leading to a contemporaneous 0.08 standard deviation increase in counselor effort. This suggests that counselor effort is indeed a function of jobseeker behavior and that it is largely complementary to their matching effort in normal times. Looking at the right panel we also see positive contemporaneous correlation between intermediary effort and the amount of overtures that firms are making to jobseekers in the counselors' portfolio, but the correlation is much smaller suggesting that counselors are more receptive to the behavior of their jobseekers. Looking at the bottom graph, which tests how jobseeker effort covaries with firm search we again see overall positive correlation that is strongest within the same week. This fits with the idea that there are indeed positive trading externalities between supply and demand side actors in the labor market and, more simply, that search effort may indeed be endogenous to the probability of a successful match being formed.

This evidence makes the counselor response to the shock all the more striking because counselors appear to dramatically switch the sign of the "natural correlation." Additionally, the magnitude of their reaction is striking as it is much larger than the co-movements we see before the shock. This provides evidence that the effect we are seeing is not mechanical or a quota system of potential matches that needs to be fulfilled. It appears more that counselors may target hiring outcomes for their jobseekers and that they are having a behavioral response to the shock, something that we'll explore more in-depth in section 5 when we look at heterogeneous results.

4.3 Impacts on hires

We now turn to examining the shock's impact on minority hiring rates. Though they may be what we are really interested in, I consider them "second order outcomes" because we've seen in the previous section that the shock led to large changes in search behavior. Thus it is difficult to disentangle the effect of, say, a minority who does not get an interview because employers are now more biased, versus the effect of a change in the job-finding rate for majorities due to lowered minority search effort. Or, for example, that a minority jobseeker becomes more selective about where they apply, hoping to avoid discriminatory employers. After presenting the impacts we discuss their interpretation at length to better understand what we are exactly measuring with the baseline empirical specification.

Panel A of Table 3 presents results of the shock's impact on contract flows for minorities as compared to majorities for the three main contract types in the French labor market. The table shows impacts over all contract flows, column 1, and then breaks down the impact by contract type in columns 2-4. Perhaps surprisingly, we detect a positive impact on total flows. Minorities are predicted to sign about 1.3% more contracts than majorities, overall. We can immediately see that this impact is entirely driven by an effect on fixed-term contracts for minorities (column 3). We see no impact on permanent contract flows, nor for temp work contracts. If we turn immediately to Panel B we see that this effect is driven by an increase in very short-term contracts. We see positive DDD estimates significant at the 1% level for 1 day and 2-7 day contracts as well small increases in 8 day - 1 month contracts and 3-6 month fixed-term contracts. In contrast we see a negative estimate for fixed-term contracts longer than six months, significant at 10% (column 6).

It is insufficient to examine fixed term contract flows to measure potential impacts on employment. Over two thirds of these fixed terms flows are 1 day contracts in which the same individual may be hired repeatedly. Therefore I aggregate the total number of workdays created within fixed-term contracts signed by our jobseekers using the start and end dates of the contract available in the hiring declaration data.[23] This metric combined with the point estimates on permanent contract hires and temp work is thus an adequate measure of whether the shock hand an overall significant impact on employment creation for minorities as compared to majorities. Column 7 of panel B shows a very small and non significant point estimate on fixed term workday creation for minorities compared to majorities after the shock. The reason for this is evident: though we see small positive impacts on very short-term contracts this is counter-balanced by the negative point estimate on contracts lasting longer than six months.

The graphs in Figure 5 illustrate the dynamic impacts on aggregate by again estimating weekly DDD coefficients excluding a reference week in the $t = 0$ period for permanent contracts (Figure 5a) and workday creation in fixed-term contracts (Figure 5b). To gauge effect sizes the dependent variables are measured in standard deviations. We see that the detrending does not absorb all of the differential variation between majorities and minorities in the pre-shock period as one of the week coefficients is statistically significant, but the general trend does not provide strong evidence that there are differential hiring trends in permanent contracts neither pre- nor post-shock, reflecting the insignificant point estimate in column 2 of Table 3. When looking at total workdays created in fixed-term contracts in subfigure 5b) we see some variation in the pre-shock period, but nothing statistically significant. Looking at coefficients after the shock we see very weak evidence that fixed-term hiring trends are differentially changing between minorities and majorities until the

[23] Unfortunately, start and end dates are not available for temp hire declarations.

week of February 18th, 2015. For this week, and only this week, we see a large increase in workdays created in fixed-term contracts for minorities relative to majorities. This week is indeed driving the significant effects we see in Panel B of Table 3.

To understand the variation this coefficient is picking up we can examine the binned de-trended raw data on fixed-term contract flows visually in Figure 5c. We immediately see that weeks 11 and 18 of February show a sharp drop in fixed-term contract flows for both groups compared to the same weeks in the previous year. The coefficient is simply picking up that the week of the 18th of February sees a relatively larger drop in the de-trended data for majority jobseekers. Hence, it does not appear that the shock has caused minorities to flock to short-term or poor quality contracts; this could be the case if, for example, a jobseeker's demand for work is inelastic to discrimination and the only work they can get is in short-term jobs because employers now discriminate more (this would be consistent with the employer search effect on short-term contracts presenting in Table 3). But it may simply be that that this is just noise in the data that the de-trending does not account for.

These results are a good starting point to think about the employment dynamics that are at work, both within and across groups (for the remainder of this section the term "group" will refer to jobseekers and employers, rather than minority and majority jobseekers). For example, the fact that minorities reduce their search effort for permanent contracts due to a real or perceived drop in their job finding probability has an impact on the job-finding probability of majorities (and other minorities) due to congestion effects that pass through changes in the relative efficiency of the labor market matching function.

I now try to give a more formal interpretation of these hiring outcomes in light of the large changes in search effort we have documented thus far. Pissarides (2000) models search effort on the supply and demand side (advertising effort) as technologies that augment the efficiency of the labor market matching function and I adopt this framework to explore the adjustments we might expect from changes in the technology of the matching function. To simplify the analysis we abstract away from intermediation and focus on jobseekers and firms. The implications for intermediation will become clear below.

Assume there are two types of jobseekers who put forth search effort $s_i,\ i \in \{0, 1\}$ and two types of firms, $j \in \{0, 1\}$ which also put forth search (advertising) effort into attracting the two different types of workers meaning that we allow firms to change the matching efficiency for both types of jobseekers: a^i_j. Thus su and av are efficiency units of jobseekers and firms respectively with u the level of unemployment and v the vacancy rate at average levels of search and advertising intensity. These are arguments in the aggregate matching function: $m(su, av)$. It has constant returns to scale and is increasing and concave in both u and v. The job finding rate f_i of jobseeker type i can be defined as,

$$\frac{s_i m(su, a^i v)}{su} \equiv f_i(s_i, s, a^i \theta) \tag{5}$$

where $\theta = \frac{v}{u}$ is the tightness in the labor market and a^i is the average search effort made by firms to type i workers. From this expression it is clear that $\frac{\partial f_i}{\partial s_i} > 0$, $\frac{\partial f_i}{\partial s} < 0$ and $\frac{\partial f_i}{\partial a^i} > 0$. In words, this implies that the job finding rate for type i jobseekers is increasing in their own search effort, decreasing in the average search rate over all jobseeker types and increasing in the search effort by firms.

Similar to jobseekers there are negative intra-group externalities between firms such that firm j's vacancy filling rate decreases in other firms' advertising effort.[24] A change in the job finding rate of jobseekers of type i with respect to a change in b_i (bias) can be expressed as,

$$\frac{df_i}{db_i} = \underbrace{\frac{\partial f_i}{\partial s_i}\frac{ds_i}{db_i}}_{\substack{\text{Direct search}\\ \text{effect}}} + \underbrace{\frac{\partial f_i}{\partial s}\frac{ds}{db_i}}_{\substack{\text{Intragroup}\\ \text{externality}}} + \underbrace{\frac{\partial f_i}{\partial a^i}\frac{da^i}{db_i}}_{\substack{\text{Intergroup effect}\\ \text{direct+externality}}} \tag{6}$$

Assume that an exogenous shock increases b_1 and this has direct negative effects on jobseeker and firms of type 1. This means that s_1 and a_1^1 are directly and negatively affected. Given equation 6 we can examine how the two groups of job seekers are directly and indirectly impacted. We start with type 1 jobseekers' job finding rate:

$$\frac{df_1}{db_1} = \underbrace{\frac{\partial f_1}{\partial s_1}\frac{ds_1}{db_1}}_{\substack{A_1\\ -}} + \underbrace{\frac{\partial f_1}{\partial s}\frac{ds}{db_1}}_{\substack{B_1\\ +}} + \underbrace{\frac{\partial f_1}{\partial a^1}}_{+}\left(\underbrace{\frac{\partial a^1}{\partial a_1^1}\frac{da_1^1}{db_1}}_{\substack{C_1\\ -}} + \underbrace{\frac{\partial a^1}{\partial a_0^1}\frac{da_0^1}{db_1}}_{\substack{D_1\\ +}}\right) \tag{7}$$

The direct effects are captured in terms A_1 and C_1. They correspond to the impact of a shock to b_1 on the job finding probability from a drop in search intensity by and for jobseekers of type $i = 1$. The indirect effects are B_1 and D_1. They are positive and highlight the intra- and inter-group externalities at work. A drop in s_1 implies a drop in s. This means that a drop in one's own search effort is partially offset because other similar jobseekers have also reduced effort, thus the marginal efficiency of search improves (B_1). Likewise, the fact that $j = 1$ type firms reduce their search for jobseekers of type $i = 1$ improves the match probability of the advertising of $j = 0$ type firms that are directed at type $i = 1$ jobseekers (D_1). Type 0 jobseekers are only affected indirectly:

$$\frac{df_0}{db_1} = \underbrace{\frac{\partial f_0}{\partial s}\frac{ds}{db_1}}_{\substack{B_0\\ +}} + \underbrace{\frac{\partial f_0}{\partial a^0}}_{+}\left(\underbrace{\frac{\partial a^0}{\partial a_1^0}\frac{da_1^0}{db_1}}_{\substack{C_0\\ +}} + \underbrace{\frac{\partial a^0}{\partial a_0^0}\frac{da_0^0}{db_1}}_{\substack{D_0\\ -}}\right) \tag{8}$$

Impacts on $i = 0$ type jobseekers pass exclusively through the inter- and intra-group externalities. B_0 captures the fact that average search effort has gone down thus the return to i's search increases. C_0 captures the effect that $j = 1$ type firms make less effort to $i = 1$ jobseekers and this has a positive externality on the advertising they make to $i = 0$ types. In contrast, D_0 captures the effect

[24]This is evident when we define a vacancy-filling probability: $\frac{a_j m(su,av)}{av} \equiv q_j(a_j, a, s/\theta)$. We easily see that firms j fill their vacancies less quickly as overall average advertising efficiency a increases.

that firms that are not directly affected by bias benefit from the increased availability of type $i = 1$ workers and this negatively affects $i = 0$ chances to be hired in these firms.

An example of these direct and indirect effects are displayed graphically in Figure 6. Graphs show the Beveridge curves (BC) expressed as $u_i = \frac{\delta}{\delta + f_i(s_i, s, a^i \theta)}$ in the (u, v) plane where δ is an exogenous job destruction rate. The slopes of the job creation curves (JC) are given by $\theta_i = \frac{v}{u_i}$. For exposition, assume all jobseekers start with the same unemployment level given by the intersection of $BC_{0/1}$ and JC_θ. Figure 6a shows the shifts in the BC resulting from the direct drops in type $j = 1$ employer search for $i = 1$ jobseekers resulting from changes in b_1. In order to introduce a bit more sophistication, assume that only certain selected $i = s1$ jobseeekers drop their search effort from an increase in b_1. These direct effects are the terms C_1 and A_1 from equation 7. Holding the actual number of vacancies constant results in an unambiguous transitory increase in the unemployment rate for $i = 1$ type jobseekers that results directly from the loss in efficiency of the matching function.[25] This drop in f_1 causes a shift in the BC essentially creating three submarkets, all with the same number of vacancies but with different levels of unemployment. Figure 6b then shows how these direct effects translate into new partial effects when we take into account the intra- and inter-group congestion externalities. We see how the direct effects on type $i = 1$ jobseekers is attenuated by terms B_1 and D_1: In context, the changes in minority matching efficiency are partially offset by the intra-group decongestion in the market and the matching efficiency gain by $j = 0$ firms, i.e. employers not affected by the shock. Majority matching efficiency almost certainly improves as their relative search effort becomes more efficient as minorities drop out (B_0) and as affected employers reduce their search for minorities, thus improving the chances that majorities are hired in these firms (C_0). But these externalities are attenuated by the fact that non-discriminating employers' returns to advertisement for minorities increases (D_0). The actual magnitude of these within- and across-group externalities will be a complex function of the selection mechanism determining search effort of minorities, occupational segregation between the three different types of jobseekers and the relative proportion of unaffected employers. This last point, in a Beckerien sense, captures the idea of how well the market is able to segregate so that minorities can avoid biased employers. Of course, most of these partial effects may disappear in equilibrium if wages are allowed to adjust. For example, if minorities accept lower wages then employers are incentivised to create more jobs, thus tightness increases and unemployment drops as we travel along the BC. This explanation might rationalize the findings by Davila and Mora (2005) and Kaushal et al. (2007) who only find wage effects looking at much longer-term outcomes.

One point of this exercise is to show that using employment outcomes as a measure for how much more discrimination minorities face on the market could be spurious. From this exercise we note that the difference in differences strategy employed in this paper would capture, $DDD =$

[25] Holding v constant helps with exposition, but if there are positive externalities between jobseekers and employers then firms should respond to the drop in search intensity by reducing the number of vacancies that they post. Nevertheless this drop should be relatively small and will not change the interpretation of the exercise. In addition, we should not be too worried about this if we believe that having majorities as a control group in the empirical specification accounts for changes in demand.

$\left[\omega f_1(\theta_1^{''}) + (1-\omega) f_{s1}(\theta_{s1}^{''})\right] - f_0(\theta_0^{''})$ where the weight ω is determined by the volume of selected minorities ($i = s1$). Hence a novel contribution of this paper is that we observe changes in real employer search behavior. This is a much better metric for determining if minorities actually do face more discrimination in the labor market. For example, if there was no direct employer effect (C_1) we could still see a negative employment effect on minorities even though the bias of employers has not changed.

The second point brings into focus the potential role for intermediaries. With no intermediation the previous exercise highlighted that we should probably see negative employment effects on minorities from an increase in b_1, even taking into account inter- and intra-group externalities. We saw large drops in the search intensity of employers for minorities and by minorities themselves. Yet at the beginning of this section we presented very small and mostly insignificant employment effects: null effects on permanent contract hires and on workday creation on aggregate. So it should now be clear that if we reformulate the matching function and job finding rate to include the presence of counselor intermediation for jobseeker i,

$$\frac{(s_i + c^i) m((s+c)u, a^i v)}{(s+c)u} \equiv f_i(s_i, s, c^i, c, a^i \theta) \tag{9}$$

it follows that the sign and magnitude of $\frac{dc^1}{db_1}$ will be very important in compensating or exacerbating the drop in matching efficiency. We have shown that this term appears to be positive and large in magnitude overall. In section 5 we will try to better understand this term and test whether employment effects are constant over micromarkets more or less exposed to PES intermediation *ex ante*.

4.4 Robustness

Placebo names

The first way we test the robustness of results is to substitute another name to connote minority status. We simply reproduce our main results using a name that should not be correlated with discriminatory tastes. Table 4 displays results for this test using names that are etymologically British in Panel B and etymologically Southern European in Panel C for our search behavior variables. Panel A reproduces our estimates for the impact on search behavior for permanent contracts from our main results in Panel B of Table 2 for comparison. We find strong supporting evidence for our hypothesis that the shock disproportionately affects minorities of Maghreb/Mashriq decent. When substituting British or Southern European names for minority status we find much smaller and inconsistently signed coefficients for the DDD estimate that are almost all insignificant. We do detect a positive and significant coefficient (at the 5% level) for counselor initiated matches, but the point estimates are between 3 and 5 times as small as our baseline definition of minority status, with the null hypothesis of the equality in coefficients easily rejected at the 1% level for both placebo names.[26]

Parametric controls

We now turn to adding fixed covariates, interacted with our time and group indicators, to our baseline specification. Though we control for group level differences that our fixed over time in model 2 it is informative to explore if our DDD estimates are significantly influenced when we add pertinent controls interacted with time and minority status. Indeed, we want to be able to exclude the possibility that the impacts we have thus far demonstrated are simply the result of characteristics of jobseekers that might be correlated with minority status *and* changes over time.

Table 5 presents results for estimates of our baseline specification while progressively adding variables from Table 1 interacted with the period t, year T and $t*T$ indicators as well their pairwise interactions with minority status, $m*t$, $m*T$ and $m*t*T$. As a reminder, these variables are average group proportions within agencies during the $t=0$ (pre-shock) period. They capture the nationality, the types of jobseeker, demographic characteristics and occupation.[27] We center these control variables at the group mean level in order to interpret our DDD parameter in the same way as in our baseline specification. In the first column we reproduce our results from panel B of Table 2, matches made to permanent contracts, for ease of comparison. In the second column we start to add controls, and for the sake of brevity, across the board as we continue to add these controls we see very small changes in the point estimates.[28]

This is quite heartening in our effort to interpret our results as causal. In effect, we are parametrically matching minority and majority agency populations on important characteristics that might be differentially correlated with changes in time periods t, T and m. For instance, the descriptive statistics show that minorities may be more tenuously attached to the labor market. Thus an adverse shock like the terrorist attacks may disproportionately affect them simply because demand changes. In addition we documented large levels of occupational segregation and the baseline results may be driven by sectors that are disproportionately affected by the shock.[29] The stability of our results suggests that our results are not being driven by underlying differences in group characteristics that interact with the shock and minority status. This analysis provides supporting evidence that the majority group is a suitable non parametric control group in the empirical specification.

Compositional changes

Though we exploit panel data at the agency level, jobseekers flow in and out of these agencies.

[26]Though beyond the scope of this paper, this could also indicate spillovers of counselor behavior onto other minority groups. If these other non-French names are common among other minority groups in France then these small counselor point estimates could be capturing the spillover of compensatory effects on other sensitive groups, even though these groups are not directly affected by the shock.

[27]Because the proportions add up to one for the categories of jobseeker type, nationality and profession, we exclude one variable from each category as our reference to avoid multicolinearity between regressors.

[28]The one exception is when we add regional fixed effects interacted with the time dummies. We cannot center these variables because they are binary, nor can we fully interact them with the minority dummy because β_1 would give the effect on the excluded reference region. The reason for the change is most likely due to the fact that they absorb a significant amount of variation related to the large differences in the presence of minorities across regions. Nevertheless, the inference on the point estimates remains the same.

[29]For instance the tourist industry might have been disproportionately affected.

Thus our sample is essentially repeated cross sections of registered jobseekers within the agency and we would therefore like to test if the effects we find on search effort are being driven by changes in the underlying composition of registered jobseekers. This is because the composition itself may be affected by the shock. For example, if the shock leads to an increase in low qualified minority jobseekers registering with the PES and counselors initiate more matches for this demographic naturally, i.e. in absence of the shock, then the increase in counselor initiated matches could be mechanically driven by a change in the composition of their jobseeker portfolio (that differs over years). Table A.3 in the appendix presents impacts on the composition using our DDD specification on the available characteristics of jobseekers. We see that out of a total of 29 regressions, six are significant at at least the 5% level. These significant effects are found for the average number of highly qualified jobseekers, young jobseekers, those with Maghreb nationality and jobseekers searching for jobs in construction, the trades, or in the theater and film industry. Thus it appears that the shock may also be correlated with compositional changes in the types of minority jobseekers registered with the PES as compared to majorities. But if we carefully read the table we see that the estimated impact of these compositional changes are relatively small compared to the impacts we see on job search intensity. For instance, on the three demographic characteristics for which we have significant effects, the coefficients are very small and when taken over the minority pre-period average they represent changes of between 0.2% - 0.3%. This is an order of magnitude smaller than the impacts seen on job search intensity. For the remaining significant estimates in Table A.3 the proportional changes are larger, but only because the baseline levels are so small.

Even if these compositional effects are relatively small, we would like to interpret our job search results holding them constant. Indeed, though these could be impacts in and of themselves, the premise of this paper is to interpret the impacts we find as originating from a change in the decision process of labor market actors. Unfortunatley, we cannont simply include them (along with their interactions with minority and time dummies) as controls in the regression because we have seen that they are correlated with the shock and therefore are endogenous, thus potentially making our DDD estimate inconsistent (see Frölich (2008) for a discussion on the use of endogenous controls). We thus adopt an instrumental variables approach. Since we have 10 weeks of pre-shock data we can take the average over these 10 weeks and use it to instrument the endogenous control variables. For example, the average number of highly qualified jobseekers in the $t = 0$ pre-shock period is highly correlated with its average in all weeks, but is orthogonal to the shock. Table 6 displays results of the shock on search intensities using our baseline specification while also including these potentially endogenous compositional controls. Each of the controls is centered and interacted with the period, year and minority indicators as well as their pairwise interactions. These endogenous controls are then instrumented with their analogous counterparts using the pre-shock means.

We see that our results are very consistent with our baseline specification with no substantive changes to the DDD estimates. It thus appears that the changes in labor market actor search effort is not likely being driven by the underlying, albeit small, changes in the pool of registered jobseekers. Of course there could be many other compositional changes that we do not observe in the data and are thus unable to control for, but given that our results are so stable when we include these endogenous controls we would like to infer that this is not a large source of potential bias in our impact estimates, nor in their interpretation.

Our final formal robustness check involves assesing the validity of our $T = 0$ placebo year compared to a previous year ($T = -1$) and we explore this in depth in the appendix. Lee (2016) suggests that differencing again by another anterior year (say, $T = -1$) to obtain a quadruple difference (QD). If the estimated QD parameter is similar to the DDD estimate, he argues that it provides credibility to the DDD identification strategy. Yet Bell, Blundell, and Van Reenen (1999) and Blundell and Dias (2009) argue that the previous year is most likely to be the best counterfactual because macro economic trends should be most comparable. Using the previous year is, of course, the stategy that we have adapted here.[30] And as we have seen, an advantage of our data structure compared to these studies is that we can directly examine the $t = 0$ detrended trends over the 10 pre-shock weeks with which we were able to present evidence to back up our identifying condition. Nevertheless we would like to develop a formal test to decide which placebo year is the most pertinent to use rather then simply arbitrarily choosing a year to use as the placebo or adding additional differences to equation 2 which may actually add bias to results if macro economic trends differ substantially by group as we go back in time. In section A.3 in the appendix, we develop a simple test that exploits the $t = 0$ data in $T = 0$ and $T = -1$ to explore their relative comparability with the actual shock year, $T = 1$. We find that the $T = 0$ year indeed appears to be the most comparable with our shock year. We then elaborate a very simple scalar weighting system to create a synthetic placebo year where outcomes are a weighted average of the two previous years given the comparability of outcomes in $t = 0$.[31] This allows us to include information from previous years in the estimation, but in a data driven, non-arbitrary fashion. We find that using this synthetic placebo year gives comparable results on outcomes for which we have data in the two previous years to the shock.

Finally, if the impacts that we are measuring are truly causal and due to a shock that increases discrimination against our minority population, we might expect effects to be correlated with an existing measure of discrimination or with the minority status of the counselor. This is what we find and we detail this analysis in the following section.

5 Heterogeneity in impacts

Warning: This section uses and discusses racially charged words that may be hurtful to some readers.

[30] This is also out of necessity as the potential matches data only started to be collected by the PES in 2013. We thus use the hiring data in this exercise.

[31] This is, of course, inspired by Abadie et al. (2010), but instead of the synthetic control being a function of weighted covariates that minimize the distance of the treated entity and other panel subjects, the weights here are constructed within entity over time using pre-treatment outcomes.

We now explore these results through a dimension of latent discrimination. Unfortunately we do not have individual level measures of discriminatory attitudes, but we try to proxy for this at the municipality level using the vote share for the extreme-right political party in France, the Front National (FN), in the 2012 presidential election. The Front National is France's major far-right political party and it has a long and robust relationship with islamophobia (start with Mayer and Perrineau (1996) for a history of the political movement). We therefore link our agency observations using this vote data at the commune, or municipality level of which there are over 36,000 in France. As described above, each municipality is attached to a local employment agency, thus we create agency level vote shares for the FN as a proxy for latent discrimination.

We begin by testing whether the Front National vote share in the first round of the 2012 French presidential election is a predictor of existing, or latent, levels of bias towards our minority group. We follow the work of Stephens-Davidowitz (2014) and use Google Trend data to examine whether people in areas with high vote shares for the FN search at a higher rate for terms that indicate the presence of discrimination against our minority population and whether this discrimination denotes racial animus. We thus intend to provide evidence to support the hypothesis that the FN vote share is positively correlated with discrimination that existed before the attack. We use the terms "islamophobie" and "bougnoule", the first term proxying for the existence of prejudice in a vague and larger sense and the second, the nature of the prejudice. Bougnoule is the most common racial slur used in France for people of Maghreb origin.[32,33] On average, this term is searched for once for every 11 searches for "find a job" and once for every 14 searches for "bake a cake" (faire un gateau) in the year preceding the shock.[34]

Though we will use very local measures of the FN vote share (municipality level that are attached to our employment agencies) in our impact analysis, Google Trend data are only available at the regional level, of which there are 22 in metropolitan France. Hence, we aggregate the vote shares to the regional level to study correlations. The trend score per region is calculated as the number of times a term was searched for over total searches within the region. These proportions are then normalized to the region with the highest proportion. Thus the regional scores have meaning when compared against one another.[35] We regress these scores on the regionally aggregated FN vote share as well as the proportion of minority jobseekers in the region,

$$score_r = \beta_0 + \beta_1 (FN\ Vote\%)_r + \beta_2 (Prop.\ Minority)_r + u_r \tag{10}$$

We present these results visually by predicting $\widehat{scores}_r$ in region r and plotting them on a graph over the scatter plot of the raw data in Figure 7 in which we also display the p-value for the FN

[32]It has the highest comparative Google search rate in the year preceding the shock compared to other "popular" racial slurs against this population, such as "bicot," "boucaque" and "meteque." Among these terms it also appears to be the most generally used across French regions as these other terms exhibit high regional correlation.

[33]Wiktionary notes the English language equivalent of "bougnoule" would be "Sand Nigger", "Camel Jockey" and "Camel Fucker". See https://fr.wiktionary.org/wiki/bougnoule.

[34]See section 4 for an interpretation of the relative Google search scores.

[35]This subsumes that there we not large, differential changes in total search volume by region.

vote share coefficient. We control for the proportion of minority jobseekers so as to proxy for the underlying proportion of the minority population in the region. As best we can, we want to interpret the score's correlation with the FN vote share holding the number of potential searches by minorities constant. In examining the graphs we see strong positive correlation with the search volume for these terms and the FN vote share in the year preceding the attacks. And even though we only have 22 regions, the p-values using robust standard errors for islamophie and the racial slur are 0.11 and 0.06, respectively. Because we hold the proportion of minorities constant in these regressions, it is not simply that minorities are searching more for these terms in high FN areas and that this is driving the search volume. It appears that, on average, relatively high extreme right regions appear to be more associated with both the presence of discrimination and that this discrimination may be, at-least in part, taste-based.

5.1 Theoretical motivation

Before we present the empirical results on heterogeneous effects with respect to this latent bias measure, we make use of some theory to motivate our analysis and generate some hypotheses that we can test. We focus on the behavior of jobseekers and counselors. The theory literature on the effect of discrimination on employer behavior is already expansive (start with Lang and Lehmann (2012) for a summary). In sum, we endogenize jobseeker and counselor search effort with respect to discrimination in the market.

Jobseekers

I start with a standard value function of unemployment with endogenous search effort (Pissarides, 2000). For simplicity, I assume that that minority jobseekers act independently of what counselors do. The utility of unemployment is formalized as follows.

$$rU_i = z - \phi(s_i) + f(s_i, s, a\theta)(E - U_i) \tag{11}$$

Where rU_i is the discounted value of unemployment for jobseeker i, z any benefits that accrue from that state, ϕ is a cost of search effort (s_i) function that is assumed to be positive and convex. As previously, f is the job-finding rate which is an increasing function of tightness θ and scaled by the search effort technology. Finally, the present discounted value of employment, E, is assumed to be the same for everyone and we'll assume that its value is larger than the unemployment state: $E > U_i$. Search effort s_i is correlated with the b_i so we can indirectly obtain optimal search through differentiating equation 11 with respect to b_i and setting it equal to 0. This gives,

$$\frac{d\phi(s_i)}{db_i} = \frac{df(s_i, s, a\theta)}{db_i}(E - U_i) \Longleftrightarrow \frac{\partial\phi(s_i)}{\partial s_i}\frac{ds_i}{db_i} = \frac{df(s_i, s, a\theta)}{db_i}(E - U_i) \tag{12}$$

giving

$$\frac{\partial\phi(s_i)}{\partial s_i} = \frac{df(s_i, s, a\theta)}{ds_i}(E - U_i) \tag{13}$$

Thus the first order condition tells us that jobseekers will set their search effort so that the marginal cost of search will be proportional to the marginal change in the job-finding rate. This clearly shows that if b_i increases then s_i will drop accordingly to reflect the change in the job finding rate. This reflects recent work by Skandalis and Philippe (2016) who find that information about lower perceived job finding probabilities reduces search effort, i.e. evidence of the discouraged jobseeker effect. We see in equation 12 that the shape of the search function with respect to bias will be very important. We have provided strong empirical evidence that $\frac{ds_i}{db_i} < 0$ but we do not know if it is increasing or decreasing: $\frac{d^2 s_i}{db_i^2} \gtrless 0$? Indeed, this will dictate whether the impact on minority search will be stronger in markets with relatively lower or higher levels of existing bias.

Counselors

Assume that counselor of type k search effort on behalf of jobseeker type i can be expressed simply as a function of the effort put forth by jobseekers in their portfolio and bias,

$$c_k^i(s_i, b_i) = g(s_i) + p_k(s_i) - \phi_k(b_i) \tag{14}$$

where $\frac{\partial g}{\partial s_i} > 0$ reflecting the natural complementarity in counselor and jobseeker effort we saw in Figure A.5a. The function $p_k(s_i)$ is the term of interest. We'll call it a perception function and it allows counselors to have a supplementary reaction to changes in jobseeker effort depending on their type k where $\frac{\partial p_k}{\partial s_i} \leq 0$. Finally, for simplicity, counselors pay a cost of bias $\frac{\partial \phi_k}{\partial b_i} > 0$ and that this cost is linear, $\frac{\partial^2 \phi}{\partial b_i^2} = 0$. A change in b_i gives

$$\frac{dc_k^i}{db_i} = \frac{\partial g}{\partial s_i}\frac{ds_i}{db_i} + \frac{\partial p_k}{\partial s_i}\frac{ds_i}{db_i} - \frac{\partial \phi_k}{\partial b_i} \tag{15}$$

And we see that an increase in b_i will only result in an increase in intermediary search intensity when

$$\frac{\partial p_k}{\partial s_i}\frac{ds_i}{db_i} > \frac{\partial \phi_k}{\partial b_i} + \frac{\partial g}{\partial s_i}\frac{ds_i}{db_i} \tag{16}$$

Thus the size of the compensatory effect is dictated by the magnitude of the slope of $\frac{\partial p_k}{\partial s_i}$ which may differ across types k and, again the shape of the jobseeker search function with respect to bias. Looking at the raw data can give us an idea about the shape of jobseeker search effort with respect to latent bias that will impact the way in which jobseekers and, in-turn, counselors react to an increase in b_i. Figure A.7 illustrates the correlation between jobseeker search effort and the extreme-right vote-share in the pre-shock period. For minorities we see that the slope is negative, steep and that the magnitude of the correlation indeed appears to be decreasing, i.e. flattening at higher levels of latent bias. Interestingly, we see very little correlation between the extreme-right vote share and average majority search effort suggesting that this correlation may capture more than simple differences in labor market conditions in high and low extreme-right vote municipalities. Though only correlation, this also provides evidence that $\frac{d^2 s_i}{db_i^2} < 0$, i.e. the effect of bias could indeed be diminishing on minority jobseeker and counselor search effort. We now turn to testing this hypothesis using the exogenous shock.

5.2 Empirical results

We start by estimating equation 2 separately for agencies that have a vote share below the median (Low FN) and for agencies above the median (High FN). Table 7 displays results. Below the point estimates we also display the p-value for a test in the equality of the DDD estimates between the two sub-samples.

We see in column 2 of Table 7 that jobseekers in both low and high FN areas reduce their search effort after the discrimination shock, but that the reduction is over three times as large in *low* FN areas with this difference being statistically significant. The heterogeneous impact on counselor behavior is even more striking. We see that the increase in potential matches made to minorities after the shock is completely centered on agencies in low extreme-right vote share areas. We see no increase in counselor matching effort for minorities in areas that exhibit relatively larger levels of existing discrimination. For employers we see a larger point estimate in high FN areas, but we cannot reject the null hypothesis of equality in impacts in the different areas.

These results provide evidence to support the hypothesis that there may be diminishing marginal effects of bias on the search intensity of jobseekers and counselors. To look closer at this we can plot the marginal effect of the shock over the entire support of the FN vote share. We do this by interacting the terms in our baseline equation 2 with the continuous measure of the agency-level FN vote share and its square. We then take the derivative of the $(m * t * T)$ term evaluated at increasing levels of the FN vote share. We refer to Figure 8 to examine these results. We see strong evidence supporting the fact that the effect on search effort for both jobseeker (Figure 8a) and counselors (Figure 8b) is strongly decreasing over the distribution of b_i. For jobseekers, the shock's impact steadily drops in magnitude until becoming insignificant around the 80th percentile of the vote share. The shock's impact on search intensity of counselors for their minority candidates shows even stronger decreasing effects. The effect dramatically decreases as we move towards the 50th percentile after which we see no effect. These results suggest that levels of existing bias play a major role in determining the magnitude of impacts that a shock to discrimination might entail. Put another way, if minorities already face high levels of bias then their perception of the returns to search effort are not as affected as minorities who face relatively low levels of initial bias. These results also point to a scenario where only counselors in areas with relatively lower levels of latent discrimination internalize the effects of discrimination on their minority jobseekers as this effect quickly diminishes over the support of b_i.[36]

[36] Assuming another functional form for the cost function of counselors $\phi(b_i)$ would not contradict these empirical results but would give another, perhaps more nuanced, interpretation of what we're seeing. For example we could assume it is convex. Then the arbitrage that the counselor faces would depend on the comparative changes with respect to b_i between the perception and cost functions. It could be that the steep decreasing marginal effect of the shock over the discrimination measure is the result of the change in the cost function quickly dominating the change

The maps presented in Figure 9 illustrate the relationship between the extreme-right vote share and the counselor compensatory effect. Figure 9a plots the municipality level extreme-right vote share from the first round of the 2012 presidential election for 36,519 municipalities in mainland France (separated by white lines). Figure 9b plots the DDD estimates from separate regressions for each of the 810 local agencies in our sample. This estimate is then merged to all the municipalities in each agency's purview. In both maps darker shading indicates higher levels. We see that, on average, the shading appears to have an inverse relationship between the two maps: areas exhibiting higher extreme-right vote levels present lower levels of the compensatory effect and vice versa. This indicates that the compensatory effect is not necessarily being driven by just a couple of regions or major metropolitan areas. Rather, it appears that the change in counselor behavior varied considerably depending on the underlying propensity to vote for the extreme-right throughout the country.

Impact of shock on search trends

We now turn to examining the shock's impact on Google search trends for these two terms and whether the effect size is disproportionately correlated with the FN vote. We do this to anchor our understanding about shifts in attitudes that might have been caused by the attacks and to better understand the previous heterogeneity analysis.

The graphs in Figure 10 present evidence on this. The top row of graphs display the search volume for the terms "islamophobie" and "bougnoule" in the weeks before and after the shock. As noted in the introduction, this top row of graphs do not display total search volume, only the volume relative to the highest point on the chart during the window of observation.

We see that the reference point for search volume is dictated by the shock. There is a massive increase in the relative search volume for both these terms following the shock that quickly dissipates for the racial slur and slightly less quickly for islamophobie. This provides strong evidence that the shock exogenously triggered interest, not only in the potential existence of discrimination towards minorities, but also increased discriminatory animus.

In the second row of Figures 10 we try to determine if the large increase in search rate for these terms is correlated with the FN vote share. The p-values using robust standard errors for islamophobie and the racial slur are 0.053 and 0.037, respectively, reflecting the regional results using data in the year before the shock.

We repeat this exercise using trend scores for terms that might be considered antithetical to the negative terms. The results are presented in Figure 11. We look at the total French search volume and relative search volume by region for "solidarite" and "fraternite" around the shock date. Again, we show the score's correlation with the FN vote share below. As with the negative terms, we see big spikes in the relative search volume for these terms starting at the week of the attacks. But in contrast, the FN vote share by region is negatively correlated with these search terms.[37] It appears that our proxy for existing discrimination not only proxies for discriminatory tastes, but also for

in perception function.

[37]For fraternite, the negative correlation appears to be largely driven by one region.

less interest in terms that connote preferences for social cohesion around the date of the shock.

Finally, we would like to test how the shock affected search trends over time in these regions. We do this using the following regression equation,

$$\begin{aligned} score_{rT} = \gamma_0 + \gamma_1 Shock_T + \gamma_2 (High\ FN)_r + \gamma_3 (High\ FN * Shock)_{rT} \\ + \gamma_4 (Prop.\ Minority)_r + \gamma_5 (Prop.\ Minority * Year)_{rT} + u_{rT} \end{aligned} \tag{17}$$

where the *score* is the Google trend search score for region r in year T. $High\ FN$ indicates if the region's FN vote share is above the median and shock is equal to one for regional scores measured during the shock year and zero otherwise. $Prop.\ Minority$ is defined as above and we center the interaction at the mean level in order to interpret γ_1 and γ_3 as effects at the mean minority population level. u is a normally distributed, mean-zero error term. Region scores for $Year = 0$ are taken from Groogle trends over the year preceding the attack while scores for $Year = 1$ are taken for the 10 weeks following the shock. Table 8 show results from this specification using OLS with clustered standard errors at the regional level. Each column is a separate regression with the regional score for the search term noted in the column title.[38] Examining columns 1 and 2, we see large increases in average search scores (compared to the reference region) associated with the shock. On average regional scores jump 6.9 and 13.3 points for islamophobie and bougnoule in low FN regions ($\hat{\gamma}_1$), respectively. And even with the very small sample size, this effect is significant at the 1% level for the ethnic slur. This reflects the large jump seen in the search volume for these words in the weeks following the shocks (Figure 10). Importantly, we do not detect a qualitatively large differential mean across time for high FN regions ($\hat{\gamma}_1 + \hat{\gamma}_3$) as compared to low FN regions, suggesting that the discrimination shock affected perception and animus throughout France, on average. Because we are able to difference the search scores over time, the correlation we see in Figure 10 appears to be driven by the underlying discrimination that existed before the shock. Though this data is highly aggregated, this suggests that the functional form of our simple job search functions for jobseekers and counselors create the large differential impacts we see across low and high FN areas, as opposed to larger changes in bias in low extreme-right vote areas.

Looking at columns 3 and 4 of Table 8 we see a similar story. We see large increases in the search for these positive terms in the shock period, but the growth in search rate for these terms is present over both low and high FN regions. Hence the correlation we see in Figure 11 is primarily a function of the underlying characteristics of low and high FN areas as we do not see significant differential changes across periods between low and high FN areas.

[38]As a recap, the dependent variable is a score going from 0-100. A score of 100 is automatically assigned to the region with the highest search rate for the term calculated over all searches within the region. All other regions are then given their score normalized to this highest score. Hence a score of 50 in another region means that the search rate for the term is half of what it is in the region with the highest search rate.

5.3 Compensatory effect

To further test the theory of compensatory effects by job counselors, we test two new hypotheses: (1) Do minority counselors react similarly to majority counselors in response to the shock in terms of the matches they make towards their minority jobseekers? (2) Do counselors who specialize in assisting the most marginalized jobseekers react similarly to "regular" job counselors? We can test these hypotheses because we know two key things about counselors: their minority status and their specialization. Counselors that specialize in assisting jobseekers the most at risk of long-term unemployment are called "Conseiller de parcours renforcé", and we will refer to them simply as "intensive counselors." They typically have a much smaller case work load (volume of jobseekers they are responsible for) and are expected to provide much more personalized assistance to their clients.[39]

In order to test these hypotheses we create a data set where observations are now at the counselor level over the 20 weeks of the observation window. For jobseekers that we can match to counselors, we aggregate the number of counselor initiated matches by counselor for each type of jobseeker and take the average. Hence, we create the analog of our main data set, but instead of observations at the agency-group level, we follow counselors. We then run DDD specifications where instead of $Minority$ indicating the minority status of the population in the agency, it indicates the minority status of the counselor, m^c.

$$y_{jtT} = \lambda_0 + \lambda_1(m^c * t * T)_{jtT} + \lambda_2(t * T)_{jtT} + \lambda_3(m^c * t)_{jt} + \lambda_4(m^c * T)_{jT} + \lambda_5 t_t + \lambda_6 m^c_j + \lambda_7 T_T + e_{jtT} \quad (18)$$

Our dependent variables will be the average match rate they make for majority jobseekers, minority jobseekers and, more importantly, the difference between the two.[40] This difference in average counselor initiated matches between minority and majority jobseekers is the analog of our main DDD estimate.[41]

We begin this anaylsis by examining Table 9 in which we present estimates for the coefficients of interest: λ_1 and λ_2. They give the effect of the shock for majority counselors (λ_2) and the differential effect for minority counselors compared to majorities (λ_1). Consistent with our main results, we see that the rise in the overall matching rate is driven by a positive differential increase for minority jobseekers. What is striking is that the point estimates in column 4 indicates that - though we see a compensatory effect for majority counselors significant at 10% - minority counselors increase their

[39] The guidelines for the number of jobseekers assigned to intensive counselors at any one time as opposed to regular counselors is as follows. 70 jobseekers maximum versus 100-350 for regular counselors. Regular counselors are defined in the PES jargon as either Conseiller de parcours "Guidé" or "Suivi."

[40] Regression equations are now weighted by $\sqrt{n^m_{jtT}}$ where n^m_{jtT} is the number of minority jobseekers in the counselor's portfolio. Unweighted regressions give very similar results.

[41] The analog is actually the combined effect of $\lambda_1 + \lambda_2$ and λ_2 weighted by the relative proportion of majority and minority counselors. Even if weighted by the proportion, we will not have the exact same point estimate because we are not able to link all jobseekers to their counselors, only a subset. Furthermore, we are constrained to looking at all potential counselor initiated matches for all contract types because in this data we cannot link the contract type to the match. But we will see that the point estimates are very comparable to Panel A in Table 2.

matching rate by about four and half times as much.[42] It is important to note that the effect seen on aggreate in the previous sections is not being driven by minority counselors as they make up only 9.7% of the counselor workforce. Yet, the difference is striking and appears to be strong evidence for a rejection of the hypothesis that majority and minority counselors behaved similarly towards their minority jobseekers following the shock. In light of our simple model where counselor effort inceases in the percpetion of discrimination that the jobseeker faces, these results tell the story that minority counselor effort is more sensitive to the shock that increases discrimination against their own type. This story would be consistent with both Dee (2005) and Behncke et al. (2010) who find that students and jobseekers perform better when paired with teachers and counselors that are similar to them. In addition, recent work on the effects of in-group bias has been shown to have consequences on the decision making of individuals when group identification is made more salient (Shayo and Zussman, 2011). Indeed, these results taken alone could be explained through a typical model of in-group bias. Yet it could also be that minority counselors better perceive a potentially worse labor market environment for their minority jobseekers, or that the cost that they pay for exerting this effort is simply lower. We now present evidence that in-group bias cannot completely explain the compensatory effect.

As mentioned above, we can distinguish between two types of counselors: intensive and normal. These intensive counselors regularly see their jobseekers face-to-face and specialize in getting particularly marginalized individuals back to work. We thus propose that counselors who engage in intensive follow-up of their jobseekers may be more or less reactive to a discrimination shock. This idea is related to Prendergast (2007) who explores the motivation of bureaucrats and the relative bias[43] they display either towards a principle or towards the client. He shows that certain types of bureaucracies such as employment agencies (principle) will tend to hire agents (counselors) that are the most altruistic towards clients (jobseekers). This is because client and principle preferences are aligned, i.e. both want the jobseeker to find work. Thus agents who care most about the client exert the most effort and will be hired into intensive roles accordingly. We take this insight to the data and test whether counselors who should provide more or less direct advocacy for their jobseekers are more or less affected by the shock. Table 10 displays results where our dependent variable is the difference in average match rates between minority and majority jobseekers in the counselor's portfolio as shown in column 4 of Table 9. The first column uses all counselor observations while columns 2 and 3 look at effects isolating the sub-sample of intensive support counselors and normal counselors, respectively. Panels further split the sample by our proxy for latent discrimination. Panel A presents very interesting results. Intensive counselors appear to change their behavior the most. We see strong effort effects by majority counselors. They increase their match rate for minority jobseekers substantially (compared to majority jobseekers). In addition we see that the effect size for minority intensive counselors is about double the size, even though the standard error is very large due to the much smaller sample size and the fact that minority counselors make up

[42] We simply take the ratio $(\hat{\lambda}_1 + \hat{\lambda}_2)/\hat{\lambda}_2$.

[43] In this case we talk about bias not as discrimination.

only 10% of the counselor sample. When looking at the decomposition by our latent discrimination variable we see that this effect is primarily driven by *majority* intensive counselors in low FN areas. This contrasts with majority normal counselor behavior for which we see no significant change in their match rate, regardless of the underlying latent discrimination in their area. For normal counselors, only minority counselors react to the shock, and this reaction is only apparent in low FN areas.[44]

In summary, we have provided evidence that minority counselors appear, on average, to provide a higher level of compensatory behavior, suggesting that the shock may either be more salient to them and/or they pay a lower cost for their effort towards minority jobseekers and this may be evidence of increased in-group bias. Perhaps, more intriguing, we have also uncovered strong positive effects for majority counselors, but these are isolated to individuals whose job is based on the close support and understanding of the difficulties that their jobseekers face in getting back to work. This result can not be explained by a typical model of in-group bias. And, as highlighted in the main results, these effects are only present in low discrimination areas reflecting the much larger drops in their minority jobseekers' search effort, but also potentially that counselors preferences may vary over the distribution of latent bias.[45]

5.4 An occupational dimension

I have shown that counselors increased their matching effort for their minority jobseekers and that we find no significant employment effects on aggregate. To examine whether this compensatory effect is indeed key to rationalizing the overall non employment effect, we now explore impacts within industries. The basic premise behind this section is that if we find variation in intermediation rates across sectors then employment effects may actually also be heterogeneous over the intermediation distribution.

Jobseekers can be categorized into 1 of over 700 different occupations when they register with the PES.[46] These occupation codes are a key parameter in the PES administrative system. The main role of these codes it to facilitate the matching of vacancies to jobseekers for all three actors we study, as each vacancy registered with the PES is also categorized with a code. The occupation codes are designed hierarchically with the letter prefix corresponding to 14 major industries.

We first test the hypothesis that the impacts on search intensity are constant across sectors. I again reformat the original data structure to add an agency-industry level dimension. This provides variation in outcomes across 11,340 "micromarkets" throughout the country. Figure 12 displays the DDD parameter estimated separately by the 14 industries for the three actors using equation

[44] Interestingly, we also note a large and negative point estimate for minority intensive counselors in high FN areas. Though statistically insignificant, this may speak to the types of minority counselors that are hired in these agencies, a theme related to the alignment of principle and client preferences, or, regardless of their own preferences, how the cost of their effort may be too high given the environment they work in.

[45] Compensatory effects may not be isolated to labor market intermediaries. Breda and Ly (2015) have recently shown that test correctors may internalize gender bias by giving more favorable evaluations to women for fields in which they are underrepresented and vice versa for men.

[46] Jobseekers can have secondary and tertiary occupation codes, but this is actually quite rare.

2. For employers (Figure 12a) and jobseekers (Figure 12b) the impacts by sector are broadly consistent with the average overall impact as denoted by the dashed grey horizontal line (main estimates taken from Panel B, Table 2). Only three jobseeker and two employer coefficients are statistically different (at 95% confidence) from the aggregate impact estimate. This suggests that the shock lowered matching efficiency through decreased employer and jobseeker effort similarly across industries in France.[47] In contrast, we see in figure 12c considerable heterogeneity in the compensatory effect depending on industry. The variation in magnitudes of the coefficients is striking. In addition we can formally reject the equality of the within-sector coefficient from the average effect for 6 industries: agriculture, commerce and sales, construction, manufacturing, trades and theater/film. These results show that counselors responded strategically or were constrained in the matches that they could make. The selection decision by the counselor is not something that we observe, but we can examine a potentially important constraint that the intermediaries face: Counselors may be more specialized in some sectors than others and thus will have more margin to act.

Figure A.8 shows the pre-shock ($t = 0$) mean level of counselor matches for the 14 different industries in descending order. We see large variation in the level of intermediation services which points to the fact that the PES counselors may simply have sectors that are more or less in their purview. For example, there may be large differences in the level of counselors' expertise to make matches across micromarkets. And, as a reminder, the three actors are all matching to the same pool of vacancies registered with- or aggregated by the PES. Hence if differences in the volume of vacancies by sector was a driving element of the heterogeneous effects we would expect to see the same patterns across all three matching channels.

We now test whether this variation maps to heterogeneous impacts on the job finding rate for minorities. Given the previous results, the hypothesis we test is whether employment effects are similar across micromarkets which had a high or low propensity for intermediation, pre-shock. In other words, does the compensatory effect bite where the PES has strong purview? Table 11 presents impacts on the job finding rate in permanent contracts in micromarkets with below (columns 1-2) and above (columns 3-4) median pre-shock intermediation levels using equation 2. In column 1 we see that the shock actually lowered the job finding rate for minorities compared to majorities by .0002 contracts per week in markets with relatively low PES intermediation levels. This represents about a 8.6% drop. In column two, we see this result is robust to controlling for the overall pre-existing job finding rate in the micromarket. In this specification the pre-shock job finding rate is centered and interacted with all the time and group dummies from our baseline equation. Controlling for labor market conditions is important because the underlying intermediation propensity may be strongly correlated with the tightness of the local labor market.

[47]This is a broad claim and the richness of the data would allow for a more detailed sectoral analysis. For instance these results could lend themselves to exploring whether jobseekers or employers make less effort for jobs that require interaction with customers (Holzer and Ihlanfeldt (1998); Combes et al. (2016)), as in the service sector (hotel, restoration and tourism) where we indeed see a significant drop in matches for minorities in both channels. This would provide a more nuanced analysis of the type of discrimination at play in the market.

In a Beckerian sense, an increase in labor market discrimination will not result in worse employment outcomes for minorities if employers can avoid minority jobseekers. This would indeed be the case in depressed micromarkets where employers have a large choice of candidates for their vacancies and this might also be correlated with counselor intermediation rates. Controlling for this in column 2, we see that the negative employment effect on minorities is slightly attenuated, but still significant. It appears that the shock may have significantly degraded the employment outcomes of minorities where PES intermediation services were less present. In looking at columns 3 and 4 we see why on average we detect no significant employment effects in permanent contracts: We see positive and marginally significant coefficients for high intermediation markets.[48]

For a bit more granularity we refer to Figure 13 which plots the employment effect non-parametrically by quintile of the indermediation distribution. We see a negative and significant coefficient in the bottom quintile and a positive coefficient in the top quintile with the middle quintiles negative and insignificant. If we look at the relationship of the DDD parametrically interacted with the pre-shock intermediation rate we see that the coefficient on the interaction term is positive with at t-stat of 2.37 supporting the interpretation of the effect seen by quintile.

6 Conclusion

This paper exploits, rich, high-frequency data to explore the job search intensity of three principal labor market actors – employers, jobseekers and their job counselors – during the weeks preceding and following the January 2015 terrorist attacks in France. I present evidence that this shock significantly reduced employer search for Muslim minority jobseekers to fill their standard job vacancies. Minority jobseekers themselves respond by drastically reducing their job search effort. Given the large decline in the matching efficiency due to the significantly lowered search intensity, it would follow that this translates into degraded employment outcomes for minorities, but this not the case. I find no significant employment impacts, on average. The reason for this may be linked to how labor market intermediaires reacted to the shock. I show that the lowered matching efficiency was compensated for by a comparable increase in counselor search effort for their minority jobseekers. Jobseeker and counselor search effort effects are centered in areas that exhibit *low* levels of existing prejudice against minorities, proxied for by the local vote shares for the extreme-right political party. Furthermore, this counselor "compensatory effect" is much larger for counselors who are themselves a minority and for a subset of majority counselors – those who specialize in getting the most marginalized jobseekers back to work. I find no compensatory effect by "normal" majority counselors. Overall, intermediation appears to matter in mitigating shocks that degrade labor market matching efficiency: In markets outside of the job counselors' purview, minority job finding rates fall significantly in the 10 weeks following the shock, a drop of 8.8% within the lower half of the pre-shock intermediation distribution.

[48]The effects do not perfectly average to the overall effect because of the weighting. There are more jobseekers on average per agency in the micromarkets with above mediation intermediation levels.

This paper also makes the case that employer search effort, rather than hiring outcomes, is a much more accurate metric for testing whether the bias that minorities face in the labor market has changed. This is because bias can also affect minority jobseeker search effort. Hence there can be significant decongestion externalities from these changes and minority employment can shift regardless of actual changes in employer preferences.

To better understand the nature of the shock and the heterogeneous effects across the distribution of the extreme-right vote share, I exploit regional Google search trend data before and around the shock date. I show this vote share is highly correlated with search terms that denote the presence of bias against Muslim minorities before the shock. I find that search for these terms greatly increased after the shock, but find no differential impacts in these Google search rates across regions with high or low vote shares. This result suggests that the marginal effects of bias on job search outcomes must be strongly decreasing in order to explain the heterogeneous effects.

Finally, the existence of similar compensatory effects may not be isolated to intermediation from professional caseworkers as many jobs are found through informal channels and personal networks. And future work is needed to better understand how intermediation might buffer against adverse shocks in other markets and contexts.

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7 Tables

Table 1: Descriptive statistics

	(1) All	(2) Majority	(3) Minority
Jobseeker type			
Unemp. looking for full-time work in permanent contract	0.708	0.694	0.772
Unemp. looking for part-time work in permanent contract	0.103	0.104	0.100
Unemp. looking for work in fixed-term, temp or seasonal contract	0.077	0.082	0.055
Unemp. but not immediately available for work	0.039	0.041	0.032
Emp. looking for other work	0.072	0.079	0.042
Nationality			
French	0.914	0.978	0.621
Maghreb	0.055	0.000	0.303
Western Europe	0.010	0.008	0.023
Sub-Saharan Africa	0.017	0.012	0.041
Other	0.004	0.003	0.012
Demographics			
Male	0.518	0.503	0.584
$<$ 35 years	0.416	0.412	0.433
College degree	0.240	0.252	0.188
High qualification	0.596	0.623	0.475
Lives in Sensitive Urban Zone	0.084	0.051	0.234
Profession searched for			
Agriculture	0.041	0.044	0.026
Arts	0.006	0.006	0.004
Banking, insurance and real estate	0.013	0.013	0.010
Commercial and Sales	0.143	0.149	0.117
Communications, marketing and media	0.020	0.023	0.007
Construction	0.093	0.081	0.148
Hotel, restorants and tourism	0.081	0.081	0.081
Manufacturing industry	0.082	0.083	0.080
Trades	0.041	0.042	0.038
Health	0.036	0.038	0.028
Personal services	0.202	0.195	0.231
Theater and film	0.024	0.028	0.006
IT, secretarial, accounting and RH	0.122	0.129	0.090
Transport	0.096	0.088	0.133
Potential matches by initiator			
Jobseeker	0.303	0.264	0.479
Counselor	0.186	0.179	0.218
Employer	0.037	0.037	0.040
Hiring flows			
Permanent	0.031	0.031	0.030
Fixed-term	0.317	0.341	0.209
Temp work	0.389	0.368	0.488
Number of jobseekers by ALE			
Ave. Num. of Jobseekers	5428	4456	972
-in low FN areas	5454	4333	1122
-in high FN areas	5401	4579	823
Agencies	810	810	810

Note: Statistics are the agency average over the 10 week pre-shock period. Column 1 shows the overall average proportion per agency while columns 2 and 3 show the relative proportion within majority and minority populations, respectively.

Table 2: Impact on potential matches by contract type

	(1) All potential matches	(2) Jobseeker	(3) Counselor	(4) Employer
Panel A: All contracts				
(Minority*Period*Shock)	-0.00216**	-0.00512***	0.00288***	0.00008
	(0.00085)	(0.00076)	(0.00036)	(0.00007)
(Period*Shock)	0.00723***	-0.00190***	0.00812***	0.00101***
	(0.00031)	(0.00022)	(0.00019)	(0.00003)
(Minority*Period)	0.00463***	0.00744***	-0.00260***	-0.00022***
	(0.00063)	(0.00056)	(0.00032)	(0.00004)
(Minority*Shock)	0.00758***	0.01122***	-0.00362***	-0.00002
	(0.00073)	(0.00069)	(0.00039)	(0.00006)
Period	0.00829***	0.01090***	-0.00324***	0.00063***
	(0.00028)	(0.00020)	(0.00017)	(0.00002)
Minority	0.01814***	0.01027***	0.00752***	0.00035***
	(0.00087)	(0.00056)	(0.00045)	(0.00004)
Shock	-0.00075**	0.00908***	-0.01172***	0.00189***
	(0.00031)	(0.00019)	(0.00022)	(0.00004)
Constant	0.04870***	0.01733***	0.02959***	0.00177***
	(0.00049)	(0.00028)	(0.00029)	(0.00002)
Mean Dep. Var. Minority	0.07367	0.04790	0.02177	0.00400
N	64800	64800	64800	64800
Panel B: Permanent contracts				
(Minority*Period*Shock)	-0.00182***	-0.00327***	0.00162***	-0.00017***
	(0.00050)	(0.00047)	(0.00022)	(0.00004)
Mean Dep. Var. Minority	0.03648	0.02482	0.00977	0.00189
N	64800	64800	64800	64800
Panel C: Fixed-term				
(Minority*Period*Shock)	-0.00063**	-0.00149***	0.00080***	0.00005**
	(0.00032)	(0.00026)	(0.00015)	(0.00002)
Mean Dep. Var. Minority	0.02464	0.01530	0.00865	0.00069
N	64800	64800	64800	64800
Panel D: Temp				
(Minority*Period*Shock)	0.00031	-0.00035*	0.00046***	0.00020***
	(0.00022)	(0.00019)	(0.00008)	(0.00004)
Mean Dep. Var. Minority	0.01242	0.00771	0.00330	0.00141
N	64800	64800	64800	64800
Panel E: Seasonal				
(Minority*Period*Shock)	-0.00002	-0.00002	-0.00001	0.00000
	(0.00002)	(0.00001)	(0.00001)	(0.00000)
Mean Dep. Var. Minority	0.00013	0.00008	0.00004	0.00000
N	64800	64800	64800	64800

Note: This table presents impacts using weighted least squares on equation 2. The dependent variables are agency averages of potential matches (the number of potential matches divided by the number of registered jobseekers by group) separated by channel as denoted in the column titles. Regression equations are weighted by $\sqrt{n_{imtT}}$ where n_{imtT} is the number of jobseekers contributing to the observation's average. The mean of the dependent variable for minorities is the weekly mean of the dependent variable during the 10 weeks preceding the shock in the year of the shock ($t = 0$, $T = 1$). Panel A presents results of the estimation for all coefficients in equation 2 while panels B-E display only the estimate for β_1. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 3: Impacts on hiring

Panel A: All contract flows

	(1) All Contracts	(2) Permanent	(3) Fixed	(4) Temp	(5)	(6)	(7)
(Minority*Period*Shock)	0.00095**	0.00001	0.00086***	0.00009			
	(0.00037)	(0.00005)	(0.00020)	(0.00030)			
Mean Dep. Var. Minority	0.07261	0.00295	0.02091	0.04875			
N	64800	64800	64800	64800			

Panel B: Fixed-contract flows and total workday creation

	1 day	$\leq$ 7 days	$\leq$ 1 month	$\leq$ 3 months	$\leq$ 6 months	$>$ 6 months	Workdays
(Minority*Period*Shock)	0.0005***	0.0003***	0.0001*	0.0000	0.0001**	-0.0001*	0.0551
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(4.9144)
Mean Dep. Var. Minority	0.0075	0.0051	0.0044	0.0018	0.0011	0.0011	12.6587
N	64800	64800	64800	64800	64800	64800	64800

Note: This table presents impacts using weighted least squares on equation 2 where the dependent variables are agency averages of contract flows or workdays (the number of contracts or workdays divided by the number of registered jobseekers by group). The type of contract is denoted in the column titles. Panel A shows results by contract type while Panel B breaks down results by fixed-term contract duration. Regression equations are weighted by $\sqrt{n_{imtT}}$ where n_{imtT} is the number of jobseekers contributing to the observation's average. Workdays are calculated using the start and end dates in the hiring declaration data. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: Name placebo tests for potential matches by channel

	(1) All potential matches	(2) Jobseeker	(3) Counselor	(4) Employer
Panel A: Minority=Maghreb/Mashiq names				
(Minority*Period*Shock)	-0.00182***	-0.00327***	0.00162***	-0.00017***
	(0.00050)	(0.00047)	(0.00022)	(0.00004)
N	64800	64800	64800	64800
Panel B: Minority=British names				
(Minority*Period*Shock)	-0.00017	-0.00044	0.00030**	-0.00003
	(0.00043)	(0.00036)	(0.00015)	(0.00005)
N	64800	64800	64800	64800
p-value Equality of Coefs.	0.055	0.000	0.000	0.068
Panel C: Minority=Southern European names				
(Minority*Period*Shock)	0.00040	-0.00021	0.00058***	0.00003
	(0.00051)	(0.00045)	(0.00018)	(0.00006)
N	64800	64800	64800	64800
p-value Equality of Coefs.	0.014	0.000	0.001	0.025

Note: The dependent variable is the mean potential match rate by channel as denoted by the column titles. Panel A replicates results from Panel B of Table 2 using our baseline specification, Panels B and C define minority status using British and Southern European first names, respectively. p-values come from a test in the equality of coefficients of the *DDD* parameter between the baseline definition of minority status, Maghreb/Mashriq, and British and S. European. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: Robustness to controls

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All potential matches						
(Minority*Period*Shock)	-0.00182***	-0.00163***	-0.00166***	-0.00153***	-0.00151***	-0.00212***
	(0.00050)	(0.00051)	(0.00050)	(0.00051)	(0.00050)	(0.00056)
N	64800	64800	64800	64800	64800	64800
Panel B: Jobseeker						
(Minority*Period*Shock)	-0.00327***	-0.00298***	-0.00305***	-0.00306***	-0.00301***	-0.00266***
	(0.00047)	(0.00046)	(0.00045)	(0.00044)	(0.00044)	(0.00046)
N	64800	64800	64800	64800	64800	64800
Panel C: Counselor						
(Minority*Period*Shock)	0.00162***	0.00153***	0.00156***	0.00172***	0.00165***	0.00081***
	(0.00022)	(0.00019)	(0.00019)	(0.00020)	(0.00020)	(0.00024)
N	64800	64800	64800	64800	64800	64800
Panel D: Employer						
(Minority*Period*Shock)	-0.00017***	-0.00017***	-0.00017***	-0.00019***	-0.00016***	-0.00027***
	(0.00004)	(0.00004)	(0.00005)	(0.00005)	(0.00005)	(0.00005)
N	64800	64800	64800	64800	64800	64800
Nationality	No	Yes	Yes	Yes	Yes	Yes
Type of jobseeker	No	No	Yes	Yes	Yes	Yes
Demographics	No	No	No	Yes	Yes	Yes
Sector	No	No	No	No	Yes	Yes
Regional FE's	No	No	No	No	No	Yes

Note: This table replicates results from Panel B of Table 2 (impact on potential matches to permanent contracts) using our baseline specification while progressively adding covariates. Each column shows the DDD estimates while adding the weekly mean of covariates in the pre-shock period from Table 1, interacted with the period t, year T and $t*T$ indicators as well their pairwise interactions with minority status, $m*t$, $m*T$ and $m*t*T$. Interactions are centered at the mean-group level. Standard errors in parentheses are clustered at the agency level. * $p<.1$, ** $p<.05$, *** $p<.01$

Table 6: Impacts on search intensity controlling for compositional changes

	(1) All potential matches	(2) Jobseeker	(3) Counselor	(4) Employer
(Minority*Period*Shock)	-0.00193***	-0.00337***	0.00161***	-0.00018***
	(0.00051)	(0.00048)	(0.00022)	(0.00004)
Mean Dep. Var. Minority	0.036	0.025	0.010	0.002
N	64800	64800	64800	64800

Note: This table replicates results from Panel B of Table 2 (impact on potential matches to permanent contracts) including endogenous controls for potential compositional changes in the average number of highly qualified jobseekers registered at the agency, young jobseekers, those with Maghreb nationality and registered jobseekers searching for jobs in construction, the trades, or in the theater and film industry. Each of the controls is centered at the group-mean level and interacted with the period, year and minority indicators as well as their pairwise interactions. The endogenous controls are instrumented with their analogous counterparts using the pre-shock means. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: Heterogeneity in impacts on search intensity across discrimination proxy

	(1) All potential matches	(2) Jobseeker	(3) Counselor	(4) Employer
Low FN				
(Minority*Period*Shock)	-0.00230***	-0.00460***	0.00244***	-0.00013**
	(0.00076)	(0.00069)	(0.00033)	(0.00006)
N	32400	32400	32400	32400
High FN				
(Minority*Period*Shock)	-0.00141**	-0.00146**	0.00028	-0.00024***
	(0.00058)	(0.00056)	(0.00019)	(0.00006)
N	32400	32400	32400	32400
p-value Equality of Coefs.	0.353	0.000	0.000	0.230

Note: This table replicates results from Panel B of Table 2 (impact on potential matches to permanent contracts) using our baseline specification, equation 2 separately for high and low FN vote share agencies (above or below the median). Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Change in relative regional score due to schock

	(1) Islamophobie	(2) Bougnoule	(3) Fraternite	(4) Solidarite
Shock	6.94	13.28***	11.06**	5.72*
	(4.77)	(2.72)	(4.56)	(3.31)
High FN	7.13	20.83***	-4.79	-3.75
	(7.59)	(7.31)	(6.37)	(3.62)
(High FN)*(Shock)	3.67	2.71	2.07	-2.26
	(9.88)	(5.41)	(7.04)	(3.97)
Prop. Minority	2.24***	0.69**	0.54**	1.13***
	(0.39)	(0.33)	(0.24)	(0.20)
(Prop. Minority)*(Shock)	-0.33	0.06	-0.83***	-0.29
	(0.55)	(0.26)	(0.25)	(0.23)
Constant	17.96**	14.87**	54.63***	55.90***
	(7.86)	(6.28)	(6.05)	(3.31)
N	44	44	44	44

Note: This table presents impacts using ordinary least squares on equation 17. The dependent variables are the relative regional Google trend search score for the terms in the column titles. Standard errors in parentheses are clustered at the regional level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 9: Counselor matches by counselor minority status

	(1) All counselor matches	(2) For Minority Jobseekers	(3) For Majority Jobseekers	(4) Difference: Minority - Majority
(Minority Counselor*Period*Shock)	0.00264***	0.00398***	0.00178*	0.00221**
	(0.00089)	(0.00110)	(0.00094)	(0.00093)
(Period*Shock)	0.01050***	0.01085***	0.01028***	0.00057*
	(0.00042)	(0.00049)	(0.00043)	(0.00031)
N	635856	635856	635856	635856

Note: This table presents impacts using weighted least squares estimates on equation 18 for parameters λ_1 and λ_2. The dependent variables are the average number of matches made by counselors for the type of jobseeker as denoted by the column titles. Regression equations are weighted by $\sqrt{n^m_{jtT}}$ where n^m_{jtT} is the number of minority jobseekers in the counselor's portfolio. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 10: Impacts for intensive verus normal counselors

	(1)	(2)	(3)
	All Counselors	Intensive counselors	Normal counselors
Dependent variable: Difference: (Minority - Majority) matches			
All			
(Minority Counselor*Period*Shock)	0.00221**	0.00303	0.00229***
	(0.00093)	(0.00499)	(0.00084)
(Period*Shock)	0.00057*	0.00363**	0.00017
	(0.00031)	(0.00143)	(0.00030)
N	635856	113410	522446
Low FN			
(Minority Counselor*Period*Shock)	0.00315***	0.00636	0.00304***
	(0.00119)	(0.00631)	(0.00109)
(Period*Shock)	0.00069	0.00560***	0.00006
	(0.00045)	(0.00213)	(0.00044)
N	330971	58014	272957
High FN			
(Minority Counselor*Period*Shock)	-0.00001	-0.00583	0.00070
	(0.00143)	(0.00818)	(0.00122)
(Period*Shock)	0.00042	0.00132	0.00030
	(0.00042)	(0.00186)	(0.00040)
N	304885	55396	249489

Note: This table presents impacts using weighted least squares estimates on equation 18 for parameters λ_1 and λ_2. The dependent variable is the difference between matches made to their minority jobseekers versus their majority jobseekers. Regression equations are weighted by $\sqrt{n^m_{jtT}}$ where n^m_{jtT} is the number of minority jobseekers in the counselor's portfolio. The first panel uses all counselor observations while the following two panels split the sample agencies that have below and above the median FN vote shares. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

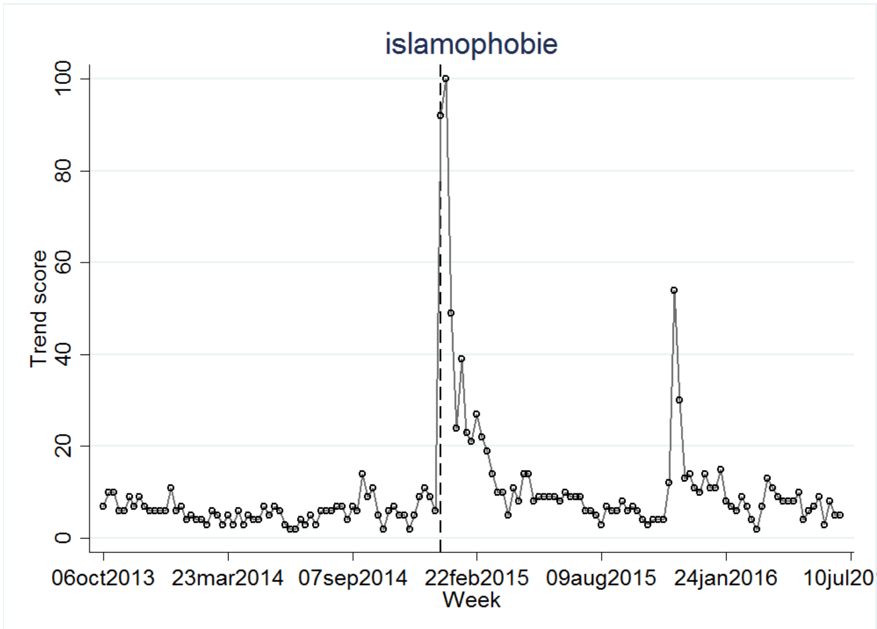
Table 11: Employment impacts by high and low intermediation levels

	Below median intermediation		Above median intermediation	
	(1)	(2)	(3)	(4)
(Minority*Period*Shock)	-0.00021***	-0.00016**	0.00008	0.00014*
	(0.00007)	(0.00007)	(0.00007)	(0.00007)
Mean Dep. Var. Minority	0.00243	0.00243	0.00354	0.00354
N	490622	490622	394215	394215
Labor market conditions	No	Yes	No	Yes

Note: This table presents impacts using weighted least squares on equation 2. The dependent variable is the job finding rate in permanent contracts. Columns 1 and 2 run the regression for micromarkets in the bottom half of the pre-shock intermediation distribution. Columns 3 and 4 repeat the exercise for the upper half. The labor market conditions control is the centered group level job finding rate over the pre-shock period within the micromarket interacted with the terms in equation 2. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

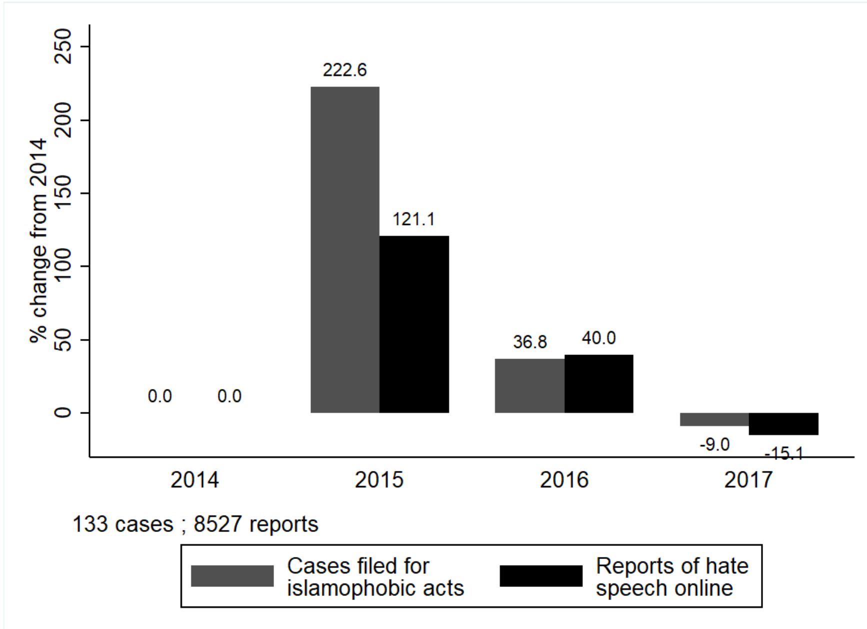
Figures

Figure 1: Terrorist attack effect on Google searches for “islamophobie”

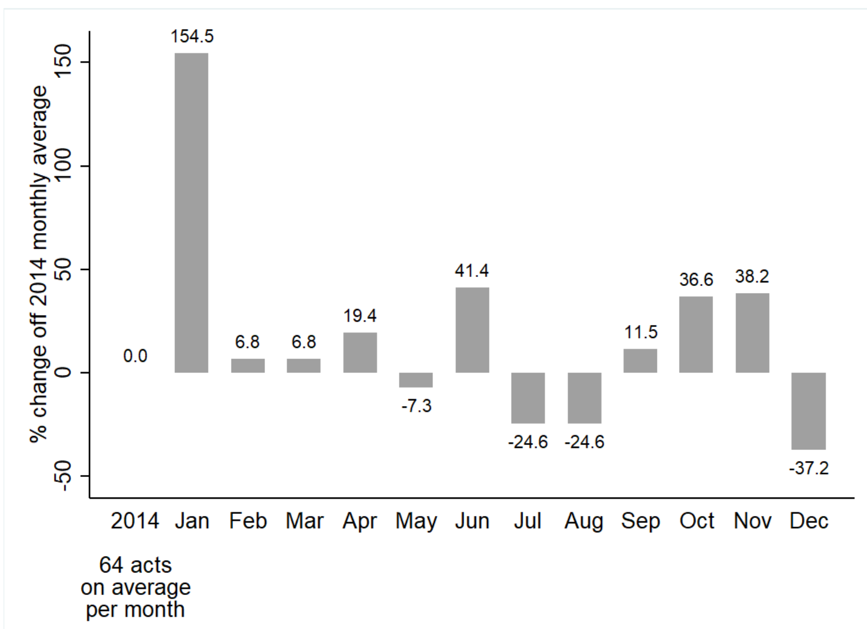


Note: Data are weekly series for the search interest in “islamophobie” in France. The vertical dashed line indicates the date of the January terrorist attacks. Search interest is calculated as (number of searches for term) / (total Google searches) in France during this time period. The search interest score is then normalized to the date with the highest search interest. See the introduction for more details on the interpretation of the search score.

Figure 2: Islamophobic acts



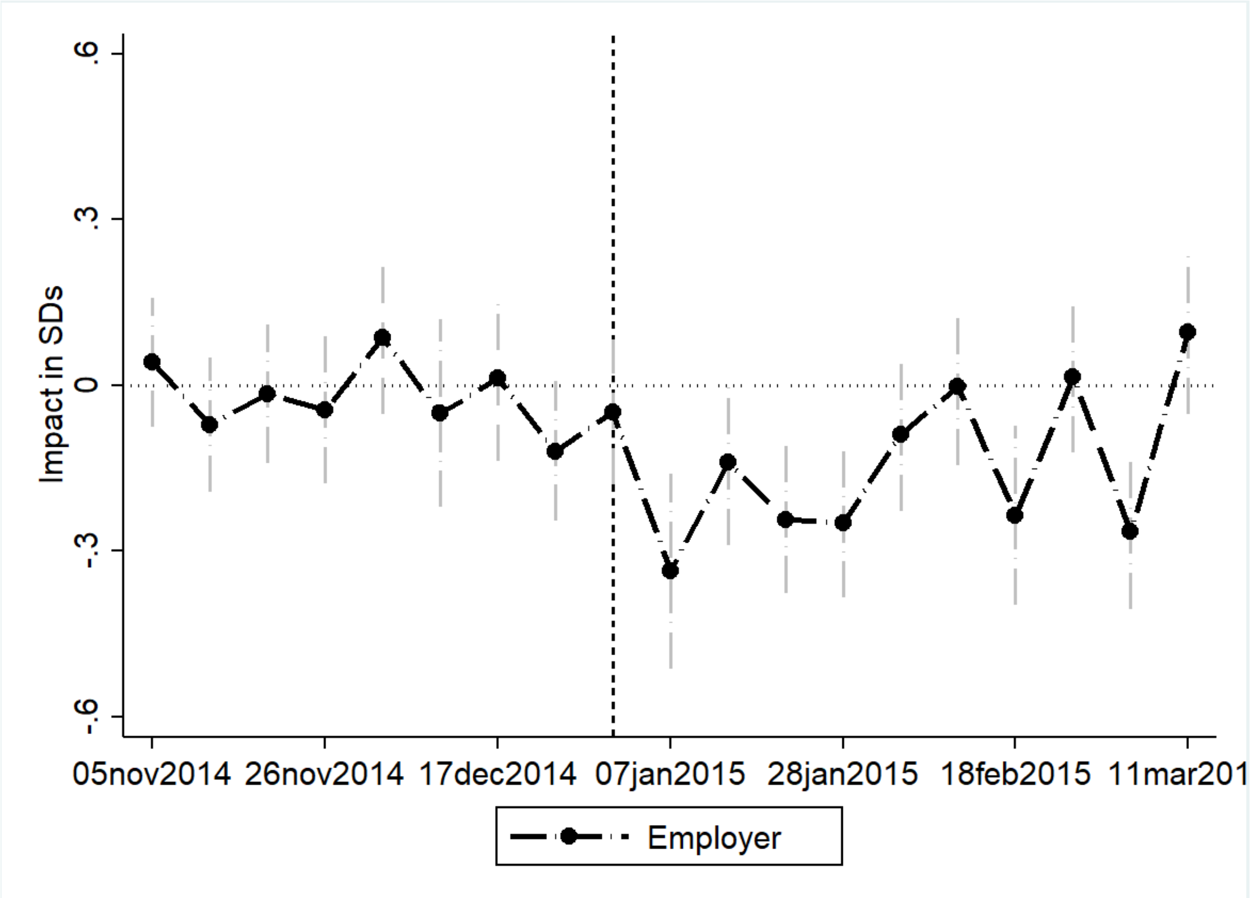
(a) Annual Official cases and reports



(b) NGO unoffical reports by month

Note: Figure 2a shows the percentage increases of cases opened for investigation for islamophobic acts compared to 2014 and the percentage change in reports of hate speech online collected by PHAROS, a department within the Ministry of the Interior that provides continual surveillance of the Internet. Figure 2b shows percentage changes off the 2014 monthly average by an NGO (CCIF) dedicated to investigating acts not necessarily registered with the police. Data for official acts are compiled using press releases by the Ministry of the Interior, *Bilan statistique 2015-17 des actes racistes, antisémites, antimusulmans et antichrétiens*, available at https://www.interieur.gouv.fr/Archives/Archives-ministre-de-l-interieur/Archives-Bruno-Le-Roux-decembre-2016-mars-2017/Interventions-du-ministre/Bilan-statistique-2016-des-actes-racistes-antisemites-antimusulmans-et-antichretiens/, and the 2017 report of the *Commission Nationale Consultative Des Droits de l'Homme*. The monthly acts are reproduced from the CCIF's 2015, 2016 and 2017 reports, available at http://www.islamophobie.net/.

Figure 3: Evolution of impact on employer search for minority jobseekers

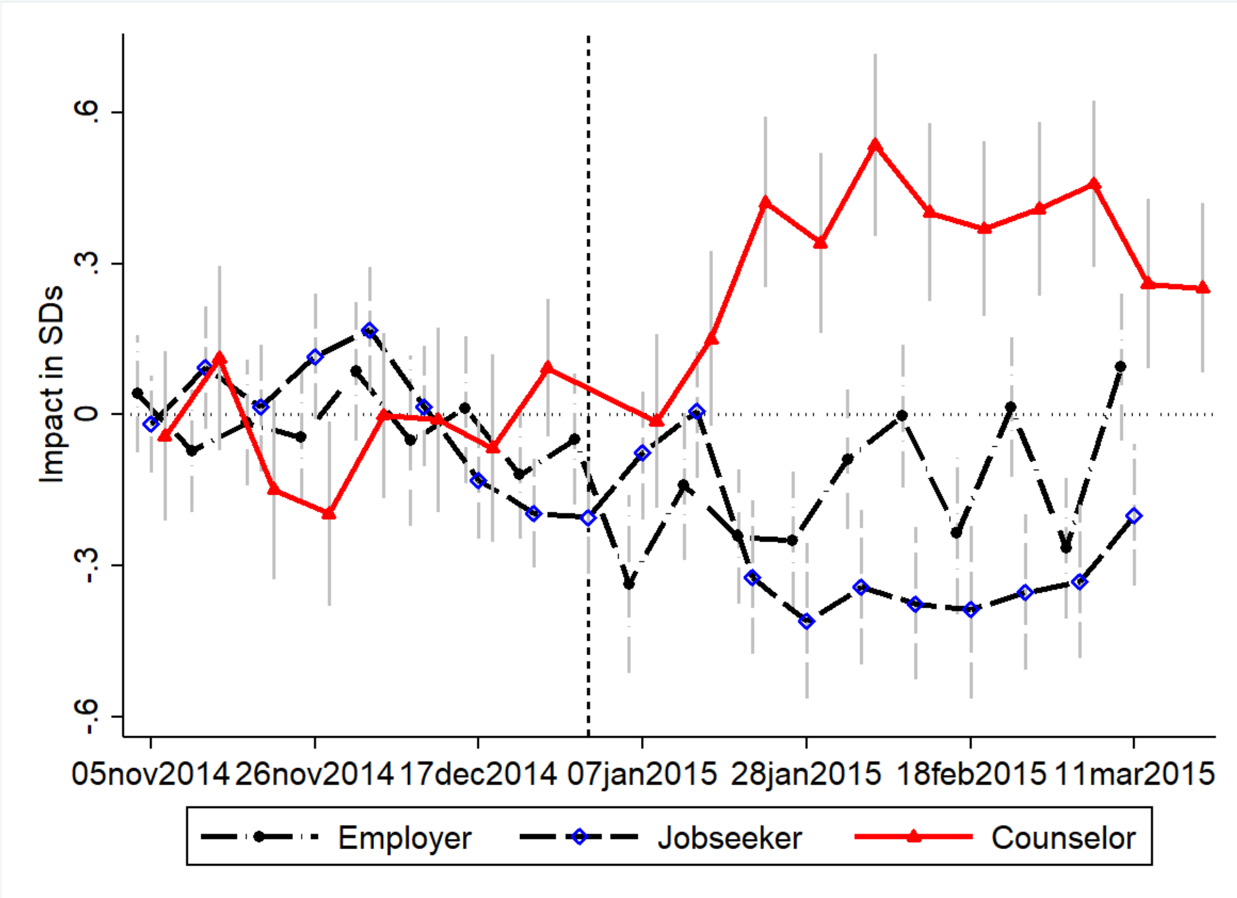


Note: This graph shows the evolution of employer search for minority jobseekers compared to majorities by plotting estimates of the weekly DDD parameters (β_w) from the following equation,

$$y_{iwT} = \delta_0 + \sum_{w=2}^{20} \beta_w (m * w * T)_{iwT} + \sum_{w=2}^{20} b_w (w * T)_{iwT} + \sum_{w=2}^{20} a_w w_{iw} + d(T * m)_{iT} + T_T + m_i + e_{iwT} \tag{19}$$

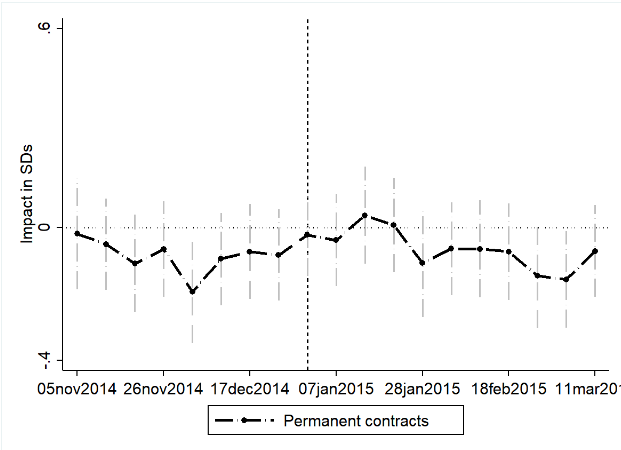
The black dashed vertical line indicates the final week before the shock. Coefficients are connected by lines with vertical grey lines denoting 95% confidence intervals. Standard errors are grouped at the agency level.

Figure 4: Evolution of impact on search effort for and by minorities by all three actors

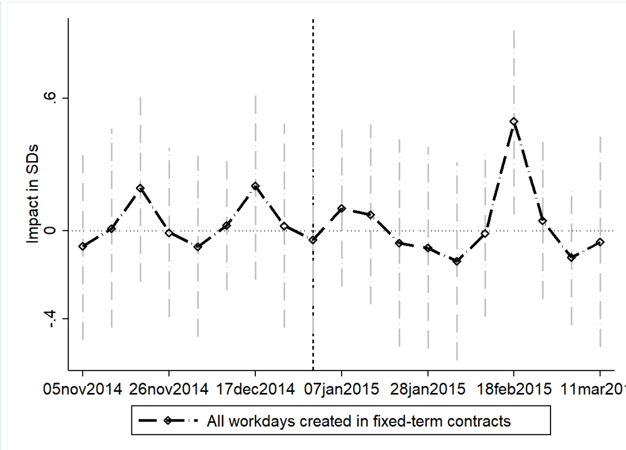


Note: This graph shows the evolution of search intensity for and by minorities as compared to majorities by plotting the weekly DDD parameter over the observation period in comparison to a reference week in the pre-shock period. See notes from Figure 3 for the specification. The black dashed vertical line indicates the final week before the shock. Coefficients are connected by lines with vertical grey lines denoting 95% confidence intervals. Standard errors are grouped at the agency level.

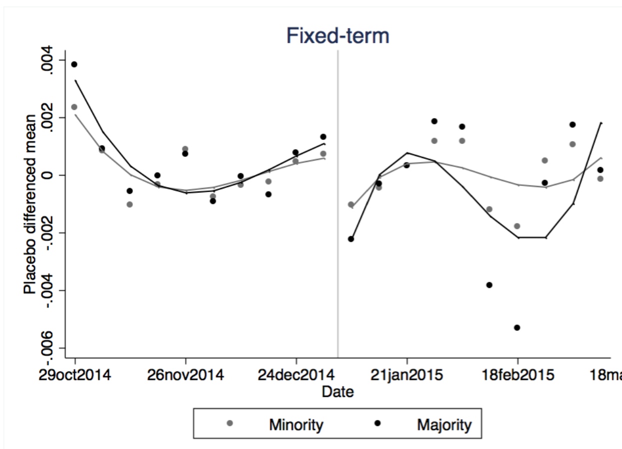
Figure 5: Evolution of hiring impacts



(a) Permanent contract flows



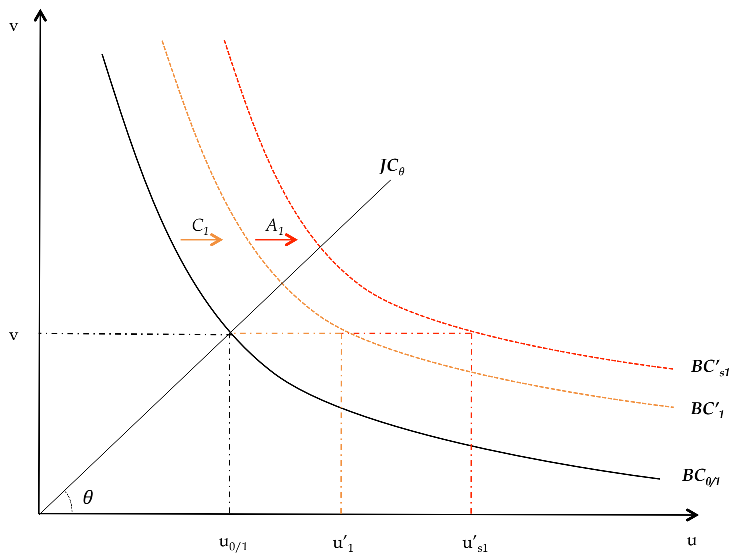
(b) Fixed-term workday creation



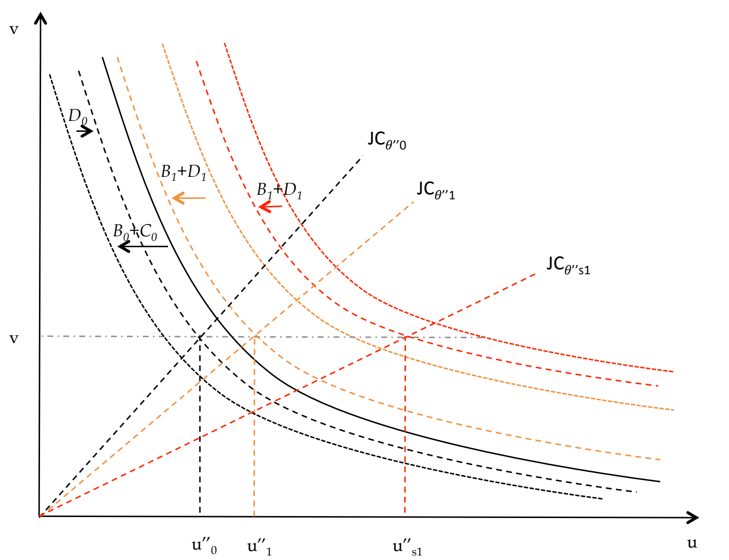
(c) De-trended fixed-term contract flows

Note: The top two graph show the evolution of permanent contract flows and total workday creation in fixed term contracts by minorities as compared to majorities by plotting the weekly DDD parameter over the observation period. See notes from figure 3 for the specification. The black dashed vertical line indicates the final week before the shock. Coefficients are connected by lines with vertical grey lines denoting 95% confidence intervals. Standard errors are grouped at the agency level. The bottom graph shows the raw de-trended data on fixed-term contract flows binned by week for the shock year $T = 1$ for minority and majority jobseekers. Observations are de-trended by differencing out the equivalent placebo year ($T = 0$) weekly mean. The weighted means are fitted using an OLS regression with a polynomial time trend of order 3. The vertical line indicates the shock discontinuity.

Figure 6: Direct and indirect hiring effects



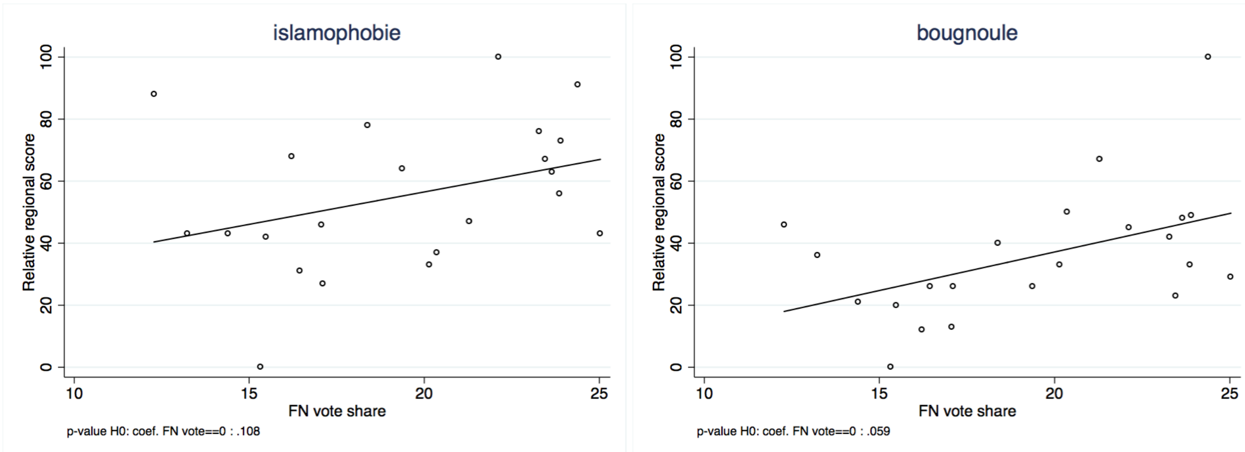
(a) Direct effects



(b) Indirect effects

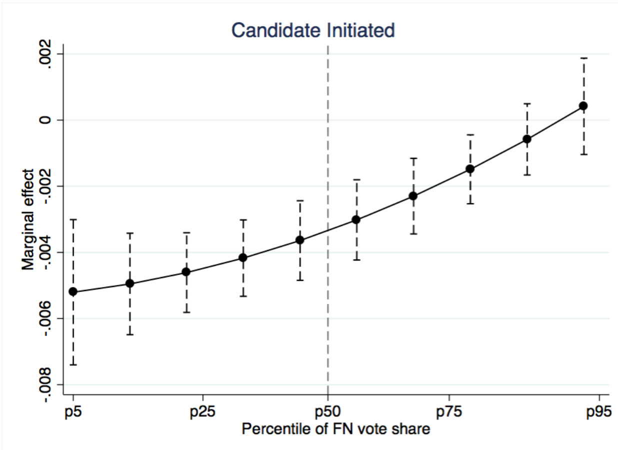
Note: Graphs show the Beveridge curves (BC) expressed as $u_i = \frac{\delta}{\delta + f_i(s_i, s, a^i\theta)}$ in the (u, v) plane where δ is an exogenous job destruction rate. The slopes of the job creation curves (JC) are given by $\theta_i = \frac{v}{u_i}$. The top graph shows shifts in the BC curve resulting from the direct drops in search intensity and the bottom graph gives the resulting partial effects after inter- and intra-group congestion externalities are taken into account. The effects denoted with arrows are derived in equations 7 and 8.

Figure 7: Correlation between extreme-right vote share and discrimination sentiment pre-shock

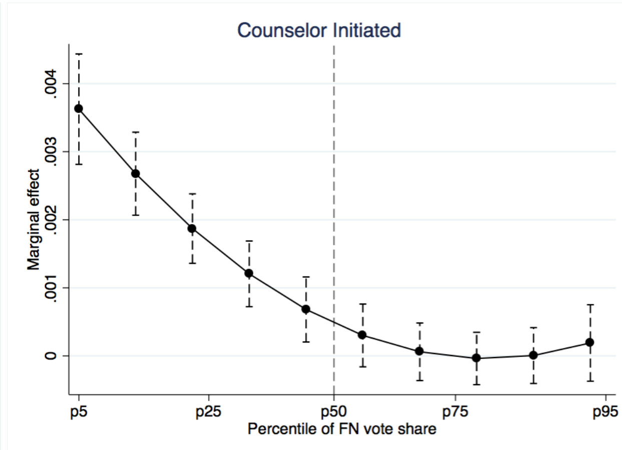


Note: Graphs represent the correlation between the relative regional search interest in the term with the FN vote share in the region in the year preceding the attack. Lines are fitted values from estimating equation 10 where we control for the proportion of minority jobseekers in the region.

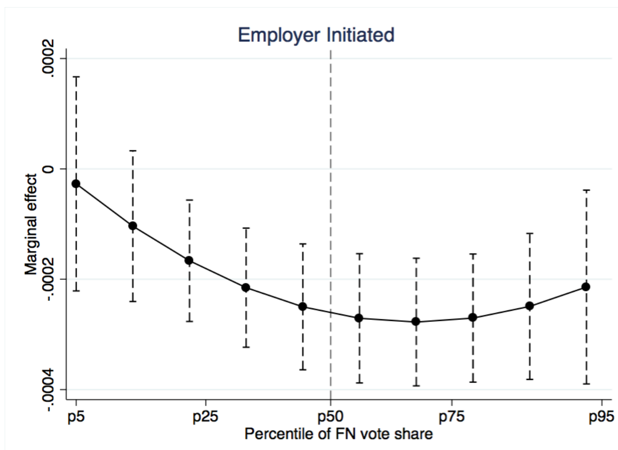
Figure 8: Marginal effects on potential matches over support of existing discrimination



(a) Jobseeker



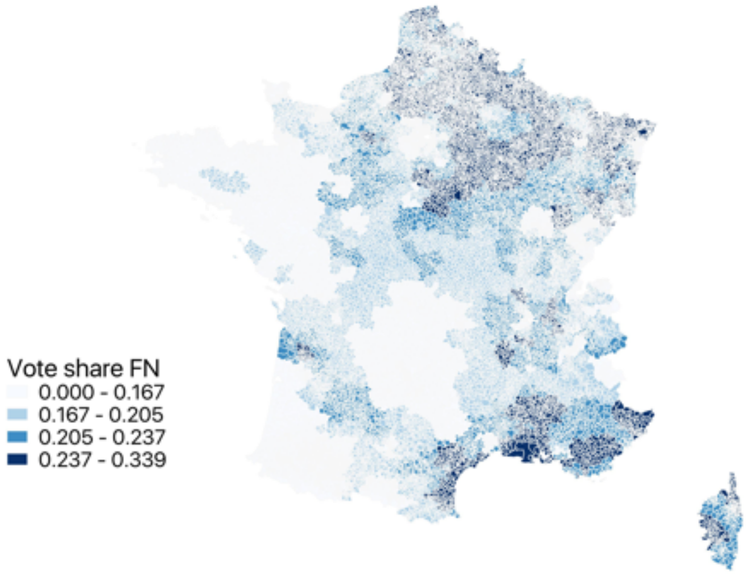
(b) Counselor



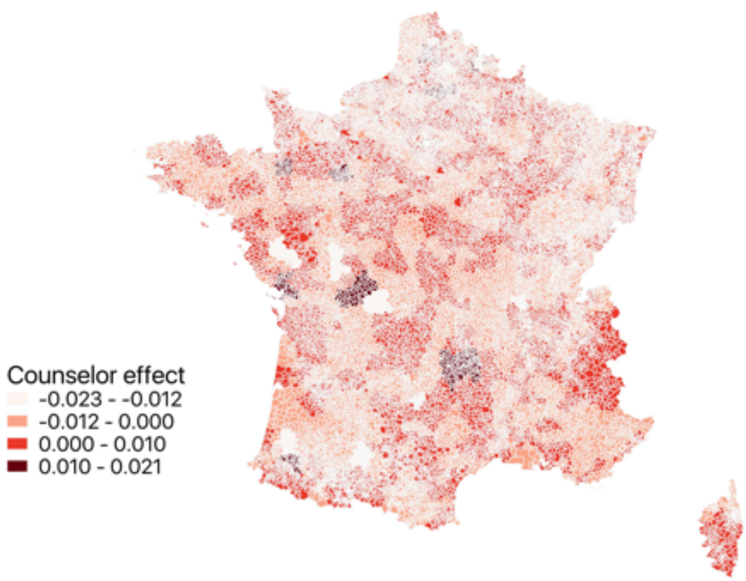
(c) Employer

Note: These figures plot the marginal effect of the DDD parameter on potential matches made by our three labor market actors over the support of the agency-level FN vote share. Results come from a WLS regression of equation 2 where each term on the right-hand side are interacted with the continuous measure of the vote share and its square. 95% confidence intervals are shown using dashed lines.

Figure 9: Geography of extreme-right vote share and compensatory effect



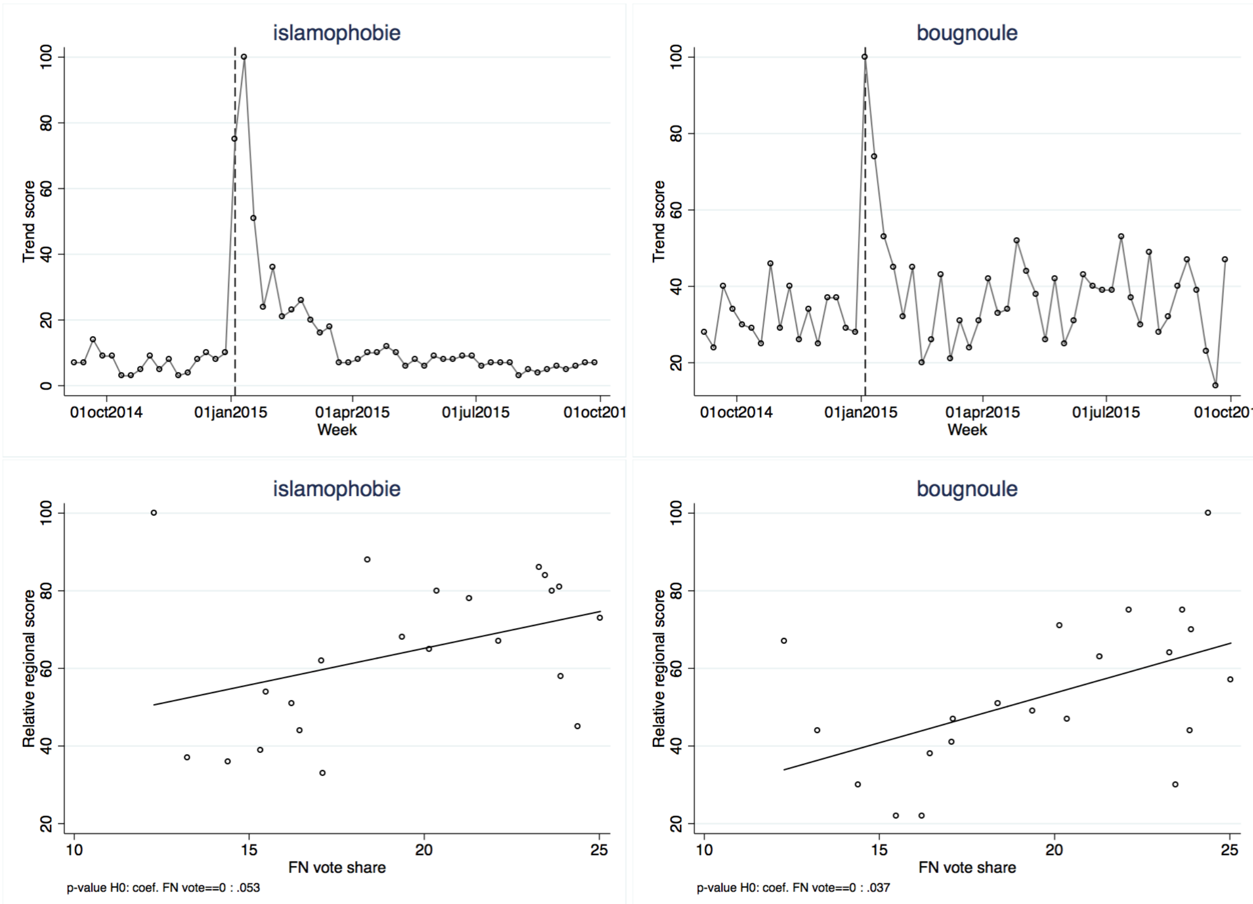
(a) Extreme-right vote share



(b) Counselor compensatory effect

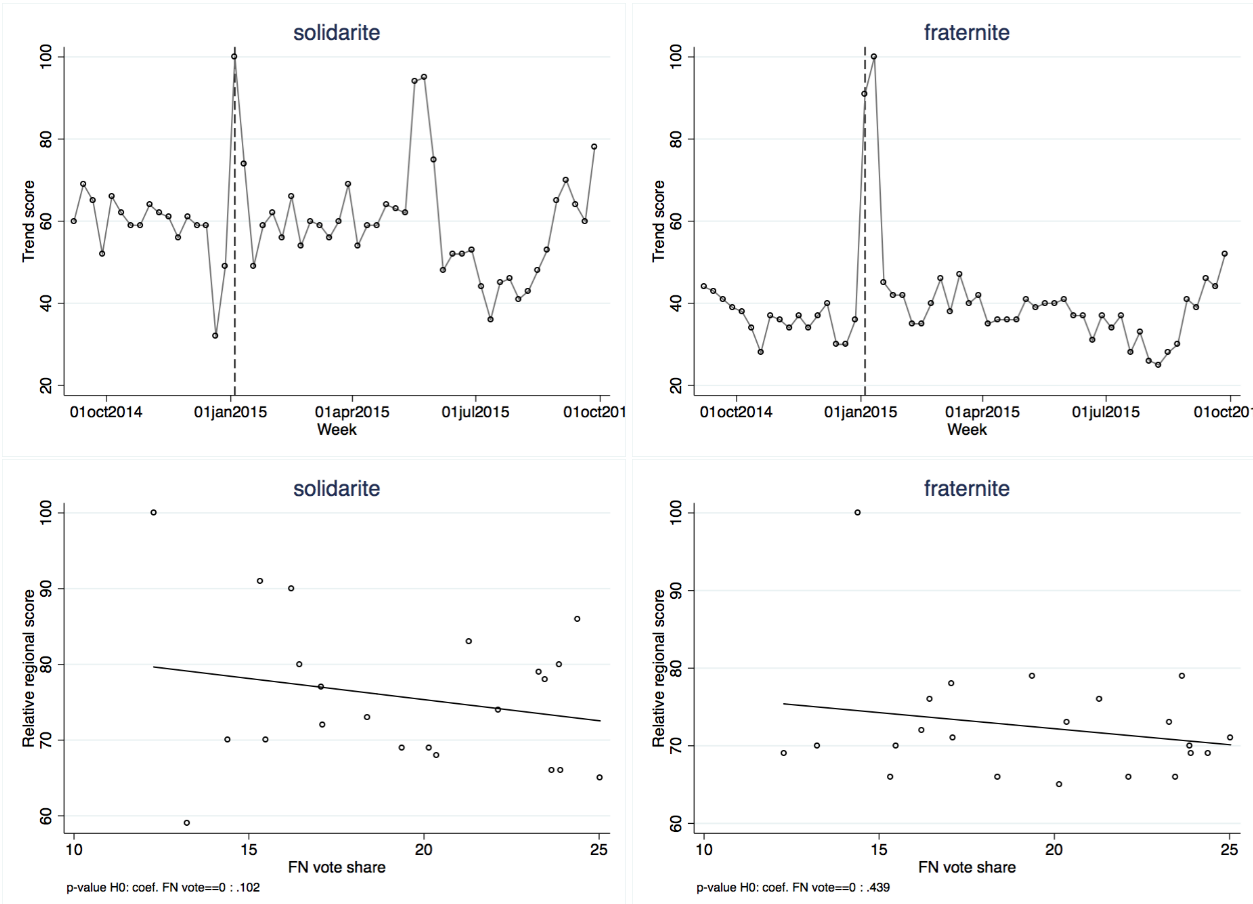
Note: These figures plot the municipality-level vote share for the extreme-right vote share (top) and the shock's impact on counselor potential matches for minority jobseekers (bottom). The DDD parameter is estimated separately for each local agency using OLS. These estimates are then mapped to the municipalities in the local agencies purview. Darker shading indicates higher values.

Figure 10: Shock impact on search terms and correlation with extreme-right vote



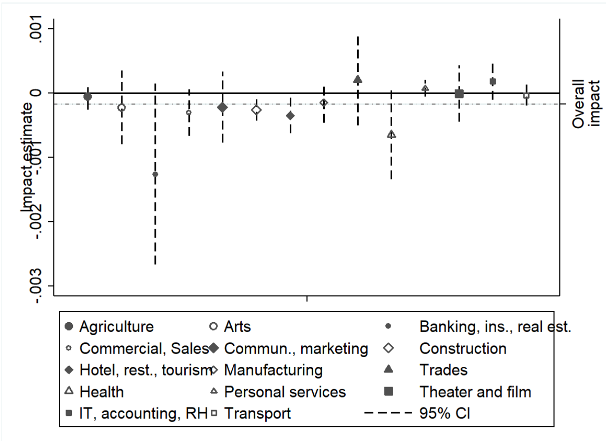
Note: Graphs in the top row are weekly series for the search interest for the term in the graph title in France. See the introduction for the interpretation of the search score. The vertical dashed line indicates the week of the shock. Graphs on the bottom row represent the correlation between the relative regional search interest in the term with the FN vote share during the same period.

Figure 11: Shock impact on positive search terms and correlation with FN vote

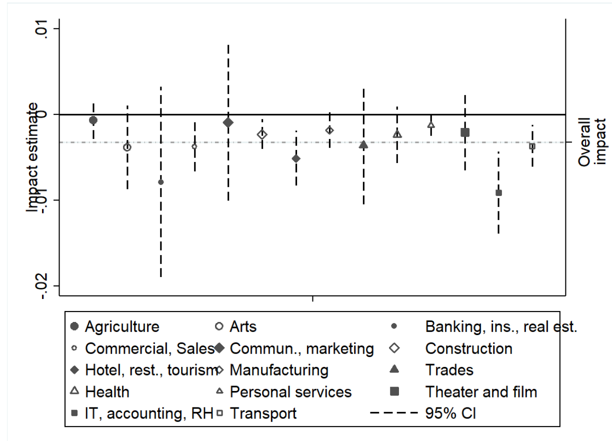


Note: Graphs in the top row are weekly series for the search interest for the term in the graph title in France. See the introduction for the interpretation of the search score. The vertical dashed line indicates the week of the shock. Graphs on the bottom row represent the correlation between the relative regional search interest in the term with the FN vote share during the same period.

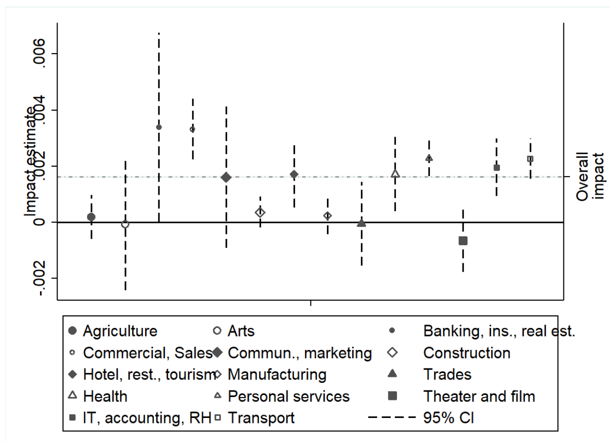
Figure 12: Effect magnitudes by sector



(a) Employer



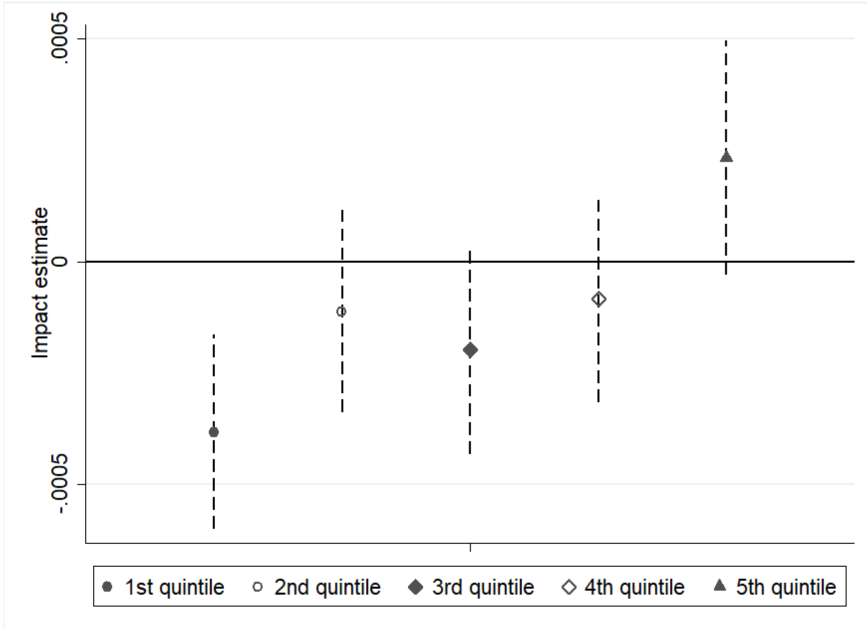
(b) Jobseeker



(c) Counselor

Note: These figures show estimates of the *DDD* coefficient estimated separately by industry for each potential matching channel. The grey dashed horizontal line marks the overall effect (point estimates from Panel B Table 2). 95% confidence intervals are denoted in vertical dashed lines.

Figure 13: Employment impact by quintile of intermediation rate



Note: This figure shows estimates of the *DDD* coefficient estimated separately by quintile of the pre-shock intermediation level in the micromarket. The intermediation rate is defined as the average counselor matching rate in the micro market in the $t = 0$ period. The micromarket is defined as the local agency x industry. 95% confidence intervals are denoted in vertical dashed lines.

A Appendix

A.1 Detrended difference in differences

In this section I demonstrate the usefulness of a detrended difference in differences (DDD) in this context and provide transparency about the assumptions needed for the identifying condition. Adopting the framework of Angrist and Pischke (2008), I assume that the effect of the discrimination shock, β_1, is additive to a group and time effect,

$$E(y_1|m,t) = E(y_0|m,t) + \beta_1 = \psi_m + \delta_t + \beta_1$$

where y_0 is the outcome in absence of the shock and ψ the specific group effect: $m = 1$ for minority and $m = 0$ majority. The time effect is δ for the pre- or post-shock periods, $t \in \{0, 1\}$.

Difference in differences will identify the effect of the shock on the minority group,

$$\begin{aligned} DD =& [E(y|m = 1, t = 1) - E(y|m = 1, t = 0)] \\ & -[E(y|m = 0, t = 1) - E(y|m = 0, t = 0)] \\ & = \beta_1, \end{aligned}$$

if the time trend ($\delta_{t=1} - \delta_{t=0}$) is assumed to be non group specific. But if it is period specific by group, γ_{mt}, we have,

$$DD = \beta_1 + (\gamma_{m=1,t=1} - \gamma_{m=1,t=0}) - (\gamma_{m=0,t=1} - \gamma_{m=0,t=0}).$$

The DD estimator will be biased whenever $(\gamma_{m=1,t=1} - \gamma_{m=1,t=0}) \neq (\gamma_{m=0,t=1} - \gamma_{m=0,t=0})$. This simply formalizes the standard common trends assumption that groups would need to evolve in the same way absence the shock.

As discussed in section 3 in the main text, Figure A.1 suggests that there may be a strong seasonal effect that impacts minorities differentially to majorities in the beginning of the year regardless of the presence of any discrimination shock. Conditioning parametrically using pre-shock outcomes and characteristics in order to improve the credibility of the parallel trends assumption is only a partial solution due to the problem of unobservables. And, perhaps even more importantly, we observe mostly parallel trends in the expectation function in the pre-shock, $t = 0$, period, thus it is not readily apparent how we might gauge the validity of controlling parametically for group differences that are correlated with the time effect.

With the additional season of data, we can relax this assumption non parametrically by doubling the difference in differences with the placebo year. Thus imposing the structure as

$$E(y_1|m,t,T) = E(y_0|m,t,T) + \beta_1 = \psi_m + \gamma_{mtT} + \beta_1$$

where $T = 1$ indicates that we are in the year where the shock takes place and $T = 0$ the preceding "placebo" year. The first DD gives

$$DD_{T=1} = \beta_1 + (\gamma_{m=1,t=1,T=1} - \gamma_{m=1,t=0,T=1}) - (\gamma_{m=0,t=1,T=1} - \gamma_{m=0,t=0,T=1})$$

and the second from the placebo year,

$$DD_{T=0} = (\gamma_{m=1,t=1,T=0} - \gamma_{m=1,t=0,T=0}) - (\gamma_{m=0,t=1,T=0} - \gamma_{m=0,t=0,T=0})$$

Thus,

$$DD_{T=1} - DD_{T=0} = \beta_1$$

if either time trends would have been equivalent *between groups* in each year - the standard DD assumption - or if

$$\begin{aligned}&(\gamma_{m=1,t=1,T=1} - \gamma_{m=1,t=1,T=0}) - (\gamma_{m=0,t=1,T=1} - \gamma_{m=0,t=1,T=0})\\ =&(\gamma_{m=1,t=0,T=1} - \gamma_{m=1,t=0,T=0}) - (\gamma_{m=0,t=0,T=1} - \gamma_{m=0,t=0,T=0}).\end{aligned}$$

And this expression we can be simply rewritten as a de-trended group specific time effect,

$$\gamma^{DT}_{m=1,t=1} - \gamma^{DT}_{m=0,t=1} = \gamma^{DT}_{m=1,t=0} - \gamma^{DT}_{m=0,t=0} \Longleftrightarrow \gamma^{DT}_{m=1,t=1} - \gamma^{DT}_{m=1,t=0} = \gamma^{DT}_{m=0,t=1} - \gamma^{DT}_{m=0,t=0} \quad (20)$$

where $\gamma^{DT}_{m,t} = \gamma_{m,t,T=1} - \gamma_{m,t,T=0}$. Hence, even if trends would have differed in $T = 1$ between groups in absence of any discrimination shock, we can still achieve identification of the shock's effect if the *detrended (DT)* evolution between groups would have been similar across periods in absence of the shock. Thus if the trend in de-trended time effects is constant between groups, the DDD parameter identifies the shock's effect. Equation 20 also shows, identical to a typical DD assumption, that we do not need the de-trended levels in the time effect to be equal between groups, only the difference must be constant moving from period $t = 0$ to $t = 1$.

Given that we observe multiple periods before the shock our main identifying test will involve examining the evolution of outcomes in period $t = 0$ to give credence to the assumption that the difference in de-tended trends would have stayed the same in absence of the shock. Outcomes are measured at the local employment agency over 20 weeks w - ten weeks before the shock and 10 weeks after - thus in examining the de-trended evolution of the two groups in the pre-shock period ($t = 0$), we will be able to provide evidence on whether or not the de-trended difference appears stable between the two groups in the weeks before the discrimination shock:

$$\gamma^{DT}_{m=1,w,t=0} - \gamma^{DT}_{m=1,w,t=0} = \gamma^{DT}_{m=0,w,t=0} - \gamma^{DT}_{m=0,w,t=0}, w \in (1, 10)$$

If "de-trended trends" are stable in the pre-shock period, this will support the assumption that they would have stayed the same in absence of the discrimination shock and we can therefore attribute a causal interpretation to any effect we see.

A.2 Weighted least squares regression

Because our dependent variables are averages taken over the number of minority or majority jobseekers within the agency, it is natural to weight observations to recapture the true underlying population parameters and take into account that some means have larger variances than others. This last point is an issue of heteroskedasticity as explored in Solon, Haider, and Wooldridge (2015) who show that weighting for efficiency gains actually depends heavily on the data structure. They point out that estimating $\sqrt{n_i}y_i = \sqrt{n_i}X_i'\beta + \sqrt{n_i}u_i$ can lead to large gains in precision when there is large variation in the underlying group sizes on which the dependent variable is calculated, but will actually inflate standard errors if this is not the case. In section 3 of their paper they provide a test that entails obtaining the residuals from the baseline specification using OLS and then regressing these residuals on $1/n_i$ and then use the t-stat on the estimated coefficient on $1/n_i$ as a test for the presence of significant heteroskedasticity and thus the utility of using WLS. Performing this test using all potential matches as the dependent variable gives a |t-stat| = 3.94 suggesting that WLS may provide considerable benefit in terms of precision given our data structure. This is most certainly due to the fact that we have very few minority jobseekers in a significant proportion of agencies.

A.3 A synthetic placebo year

In this section we compare the comparability of two potential placebo years with the year of the shock. We then develop a synthetic placebo as a weighted average of the two years.[49] Unfortunately, PES data collection on potential matches started in 2013, thus we cannot look at another placebo year for these outcomes, but we can look at contract flows for the 2012-2013 period because the PES began collecting this data in March 2012.

Here, we develop a simple test that entails comparing $t = 0$ (i.e. pre-shock outcomes) differences between mean outcomes for $T = 0$ (previous year) and $T = -1$ (2 years prior) and outcomes in $T = 1$. In comparing the average weekly agency level difference in the pre-shock period in the current year with previous years, we can provide evidence about which placebo year is the most appropriate. The choice of appropriate year to use could simply be given by year that exhibits the smallest absolute $t = 0$ differences with the year of the shock:

$$\min\{(\bar{D}_0 = |\bar{y}_{t=0,T=1} - \bar{y}_{t=0,T=0}|), (\bar{D}_{-1} = |\bar{y}_{t=0,T=1} - \bar{y}_{t=0,T=-1}|)\}$$

Isolating the $t = 0$, period we can rearrange the data so that we have observations for D for each population in the local employment agency for each placebo year. Results from a WLS estimation of the following regression equation will show differences in the comparability of the two placebo

[49]This technique could of course be generalized to multiple placebo years.

years,

$$D_{i,m,t=0,T} = \delta_0 + \delta_1 Year_{i,m,t=0,T} + e_{i,m,t=0,T} \tag{21}$$

In this equation $Year = 1$ for the previous year to shock ($T = 0$) and $Year = 0$ for two years before the shock ($T = -1$). Hence, $\delta_0 + \delta_1$ gives the average comparability in outcomes of our baseline placebo year with the year of interest and the constant, δ_0, the comparability of the placebo year taken two years before the shock. Table A.4 presents results from the estimation of equation 21 with contract flows as our dependent variables. For each type of contract, we separate the results by minority status and display the $t = 0$, $T = 1$ mean of the variable to gauge the size of the differentials. Ideally, we would like the estimate of $\delta_0 + \delta_1$ to be close to zero, meaning that on average there is very little difference in hiring rates between our baseline placebo year and the current year in the $t = 0$ period.

Looking at the results in panels A and B we see that the $T = 0$ placebo year appears to be much more comparable than the $T = -1$ year in terms of hiring rates of both minorities and majorities. Indeed we see that $|\hat{\delta}_0 + \hat{\delta}_1|$ is smaller across all contract types and groups than $|\hat{\delta}_0|$. In terms of the proportional difference off the mean of the original variable we see that for permanent contracts for minorities, the $T = 0$ difference is 2.8% while the $T = -1$ difference is 8.7%, almost 3 times as big. Looking at panel B we see the same story: $T = 0$ appears to be a better counterfactual than $T = -1$ in terms of average hiring rates for majorities as well.

We can visualize these average differences with the year of the shock in Figure A.6. Here we plot the binned de-trended data for both groups for the 10 weeks in the $t = 0$ time period. $T = 0$ data are solid dots and lines and the $T = -1$ data are dashed. We see that $T = 0$ data are much closer to zero, on average and, importantly, the differential between the two groups appears to be much more constant (this is especially true for permanent contracts).[50]

Our estimates of δ_0 and δ_1 in equation 21 allow us to gauge the comparability of years $T = 0$ and $T = -1$ with the year of interest, $T = 1$ in terms of average hiring rates in the pre-shock $t = 0$ period. Using these estimates at the panel entity level, I devise a simple weighting scheme to create a synthetic placebo year to use in the DDD estimation of the shock's effect. Within agency i for population m, combined total absolute deviation over the two years is given by $|\delta_{im0}| + |\delta_{im1} + \delta_{im0}|$. Thus, the proportion of total deviation gives weights for each placebo year within agency i for population m.

$$w_{imT_0} = 1 - \frac{|\delta_{im1} + \delta_{im0}|}{|\delta_{im0}| + |\delta_{im1} + \delta_{im0}|} \tag{22}$$

and

$$w_{imT_{-1}} = 1 - \frac{|\delta_{im0}|}{|\delta_{im0}| + |\delta_{im1} + \delta_{im0}|} = 1 - w_{imT_0} \tag{23}$$

Hence the weights are panel entity-level scalars representing the proportion of each year's deviation

[50] It should be clear that using the average distance between outcomes in the year of interest and the placebo years is arbitrary. For instance, one could develop weights based on the variance of the difference between minority and majority outcomes, i.e. the quality of the parallel trends pre-shock.

over the total deviation observed over all previous years. The "synthetic" placebo year is then generated as the weighted sum of the dependent variable over the two years for each population m in agency i:

$$y_{imtT_s} = w_{imT_0} * y_{imtT_0} + (1 - w_{imT_0}) * y_{imtT_{-1}} \tag{24}$$

Hence in its general form, we are able to "blend" as many placebo years as we have data for using the weights generated from the $t = 0$ data. Yet, given we have shown that our $T = 0$ year is clearly more comparable than the $T = -1$ year, we should probably consider results from the DDD estimation using this synthetic data as a conservative estimate. In essence, we are allowing ourselves to include data of potentially lower "counterfacual quality" in the synthetic placebo, albeit with lower importance.[51] Therefore, we believe it is useful to present results using the synthetic placebo as an "informed robustness check" to our main results. Table A.5 compares results from our baseline specification on contract flows with results obtained from using the synthetic placebo year. We find that results are similar using the two methods. While the coefficients are indeed more conservative using the synthetic control we cannot reject the null hypothesis in the equality of the DDD parameter for any contract type.

In summary, this simple method introduces a way to be selective about placebo years used in a DDD framework and provides a way to include data from multiple potential placebo years in a non arbitrary way in order to test the robustness of results. The test and method developed here are very simple and could be a possible avenue for more serious econometric work in the future.

[51] Indeed, we have no rule-of-thumb about at which point one should or should not use a placebo year. Do the differentials shown above have to be below 5%, 10%, etc.?

Appendix Tables

Table A.1: Potential matches for all contract types (unweighted)

	(1) All	(2) Jobseeker	(3) Counselor	(4) Employer
Panel A: All contracts				
(Minority*Period*Shock)	-0.00303***	-0.00311***	0.00095***	-0.00001
	(0.00098)	(0.00083)	(0.00034)	(0.00011)
(Period*Shock)	-0.00082**	-0.00203***	0.00857***	0.00104***
	(0.00037)	(0.00027)	(0.00021)	(0.00004)
(Minority*Period)	0.00383***	0.00429***	-0.00094***	-0.00014**
	(0.00066)	(0.00056)	(0.00026)	(0.00005)
(Minority*Shock)	0.00568***	0.00759***	-0.00195***	-0.00009
	(0.00089)	(0.00076)	(0.00031)	(0.00010)
Period	0.01838***	0.01131***	-0.00367***	0.00064***
	(0.00033)	(0.00024)	(0.00018)	(0.00002)
Minority	0.01150***	0.00583***	0.00494***	0.00028***
	(0.00073)	(0.00054)	(0.00030)	(0.00006)
Shock	0.00531***	0.00939***	-0.01228***	0.00199***
	(0.00036)	(0.00025)	(0.00024)	(0.00004)
Constant	0.05240***	0.01802***	0.03040***	0.00185***
	(0.00056)	(0.00032)	(0.00031)	(0.00003)
Mean Dep. Var. Minority	0.07488	0.04082	0.02111	0.00402
N	64800	64800	64800	64800
Panel B: Permanent contracts				
(Minority*Period*Shock)	-0.00164***	-0.00156***	0.00006	-0.00021***
	(0.00058)	(0.00050)	(0.00017)	(0.00007)
Mean Dep. Var. Minority	0.03421	0.02014	0.00853	0.00184
N	64800	64800	64800	64800
Panel C: Fixed-term				
(Minority*Period*Shock)	-0.00097**	-0.00102***	0.00060***	-0.00002
	(0.00043)	(0.00034)	(0.00021)	(0.00005)
Mean Dep. Var. Minority	0.02738	0.01371	0.00914	0.00069
N	64800	64800	64800	64800
Panel D: Temp				
(Minority*Period*Shock)	-0.00037	-0.00050**	0.00030***	0.00022***
	(0.00027)	(0.00023)	(0.00011)	(0.00006)
Mean Dep. Var. Minority	0.01310	0.00687	0.00337	0.00149
N	64800	64800	64800	64800
Panel E: Seasonal				
(Minority*Period*Shock)	-0.00006	-0.00003	-0.00001	0.00000
	(0.00004)	(0.00002)	(0.00002)	(0.00000)
Mean Dep. Var. Minority	0.00020	0.00010	0.00007	0.00000
N	64800	64800	64800	64800

Note: This table replicates results in Table 2 using OLS. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.2: Potential matches for all contract types (Count data)

	(1) All	(2) Jobseeker	(3) Counselor	(4) Employer
Panel A: All contracts				
(Minority*Period*Shock)	-0.05387***	-0.10367***	0.02543**	0.07547***
	(0.01015)	(0.01593)	(0.01120)	(0.01982)
(Minority*Period)	0.00950	0.03281***	-0.04400***	-0.11515***
	(0.00711)	(0.01182)	(0.00900)	(0.01553)
(Minority*Shock)	0.10203***	0.15496***	-0.00406	-0.06923***
	(0.00997)	(0.01598)	(0.01165)	(0.01884)
(Period*Shock)	-0.03898***	-0.19827***	0.35393***	0.06103***
	(0.00524)	(0.00737)	(0.00749)	(0.00948)
Shock	0.14573***	0.46954***	-0.45630***	0.77397***
	(0.00608)	(0.00761)	(0.00879)	(0.00998)
Period	0.30925***	0.49366***	-0.11018***	0.31099***
	(0.00398)	(0.00543)	(0.00612)	(0.00759)
Minority	-1.23479***	-1.08228***	-1.32086***	-1.36558***
	(0.03634)	(0.03866)	(0.03621)	(0.03660)
Constant	5.37198***	4.29869***	4.83339***	2.01899***
	(0.01504)	(0.01990)	(0.01520)	(0.01713)
Mean Dep. Var. Minority	80.2	46.6	21.2	3.9
N	64800	64800	64800	64800
Panel B: Permanent contracts				
(Minority*Period*Shock)	-0.06031***	-0.12199***	0.04951***	0.03963
	(0.01240)	(0.01869)	(0.01453)	(0.02733)
Mean Dep. Var. Minority	39.5	24.1	9.5	1.8
N	64800	64800	64800	64800
Panel C: Fixed-term				
(Minority*Period*Shock)	-0.05375***	-0.09837***	0.01980	0.04062
	(0.01103)	(0.01789)	(0.01249)	(0.04332)
Mean Dep. Var. Minority	27.3	14.9	8.4	0.7
N	64800	64800	64800	64800
Panel D: Temp				
(Minority*Period*Shock)	-0.01920	-0.04137	-0.01150	0.10406***
	(0.01645)	(0.02555)	(0.02062)	(0.03164)
Mean Dep. Var. Minority	13.3	7.5	3.2	1.4
N	64800	64800	64800	64800
Panel E: Seasonal				
(Minority*Period*Shock)	-0.09028	-0.17887	0.00630	0.59955
	(0.10558)	(0.18452)	(0.13925)	(0.51951)
Mean Dep. Var. Minority	0.1	0.1	0.0	0.0
N	64800	64800	64800	64800

Note: This table replicates results in Table 2 using Poisson regression. The dependent variables are count data as opposed to averages. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.3: Changes in jobseeker composition within agencies

	(1) Minority mean for $t = 0$, $T = 1$	(2) $\widehat{DDD}$
Unemp. looking for full-time work in permanent contract	0.77217	-0.00007
		(0.00041)
Unemp. looking for part-time work in permanent contract	0.09969	-0.00011
		(0.00023)
Unemp. looking for work in fixed-term, temp or seasonal contract	0.05479	-0.00003
		(0.00018)
Unemp. but not immediately available for work	0.03162	0.00037
		(0.00023)
Emp. looking for other work	0.04174	-0.00016
		(0.00023)
High qualification	0.47459	0.00095***
		(0.00037)
Lives in Sensitive Urban Zone	0.23354	0.00044
		(0.00040)
Male	0.58398	0.00032
		(0.00034)
$<$ 35 years	0.43258	-0.00110***
		(0.00035)
College degree	0.18838	0.00012
		(0.00029)
French	0.62076	0.00069**
		(0.00034)
Maghreb	0.30309	-0.00088***
		(0.00031)
Western Europe	0.02276	-0.00011
		(0.00011)
Sub-Saharan Africa	0.04137	0.00020
		(0.00013)
Other	0.01203	0.00010
		(0.00008)
Agriculture	0.02556	-0.00021*
		(0.00012)
Arts	0.00397	-0.00008*
		(0.00004)
Banking, insurance and real estate	0.00997	0.00011
		(0.00007)
Commercial and Sales	0.11748	0.00033
		(0.00026)
Communications, marketing and media	0.00743	-0.00000
		(0.00007)
Construction	0.14761	-0.00089***
		(0.00026)
Hotel, restorants and tourism	0.08134	-0.00013
		(0.00020)
Manufacturing industry	0.08018	0.00014
		(0.00019)
Trades	0.03805	0.00028**
		(0.00014)
Health	0.02764	0.00005
		(0.00013)
Personal services	0.23138	-0.00028
		(0.00030)
Theater and film	0.00631	0.00023***
		(0.00007)
IT, secretarial, accounting and RH	0.08963	0.00024
		(0.00020)
Transport	0.13278	0.00028
		(0.00023)
N=64800		

Note: Each row displays results from a separate regression using our DDD specification. The dependent variables are the average proportion for each compositional variable as denoted in the first column. We also display the pre-shock mean to gauge effect sizes. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.4: Validity test for placebo year

	(1) All contracts	(2) Permanent contract	(3) Fixed-term	(4) Interim
Panel A: Minority				
Year	-0.00162***	0.00017***	-0.00060***	-0.00118***
	(0.00037)	(0.00004)	(0.00018)	(0.00032)
Constant	0.00348***	-0.00026***	0.00085***	0.00289***
	(0.00044)	(0.00004)	(0.00020)	(0.00039)
Mean original var.	0.07261	0.00295	0.02091	0.04875
Proportional difference T=0	0.02564	-0.02825	0.01162	0.03492
Proportional difference T=-1	0.04791	-0.08653	0.04051	0.05923
N	16200	16200	16200	16200
Panel B: Majority				
Year	-0.00118***	0.00018***	-0.00055***	-0.00081***
	(0.00021)	(0.00003)	(0.00015)	(0.00016)
Constant	0.00272***	-0.00029***	0.00106***	0.00195***
	(0.00025)	(0.00003)	(0.00018)	(0.00018)
Mean original var.	0.07394	0.00312	0.03406	0.03675
Proportional difference T=0	0.02075	-0.03694	0.01488	0.03109
Proportional difference T=-1	0.03676	-0.09308	0.03099	0.05315
N	16200	16200	16200	16200

Note: This table presents results from estimating equation 21 with results separated by group status. The mean of the original variable is the weekly mean in the $t = 0$, $T = 1$ period. The proportional difference is calculated as $\frac{\hat{\delta}_1+\hat{\delta}_0}{Mean\ of\ orig.\ var}$ for $T = 0$ and $\frac{\hat{\delta}_0}{Mean\ of\ orig.\ var}$ for $T = -1$. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

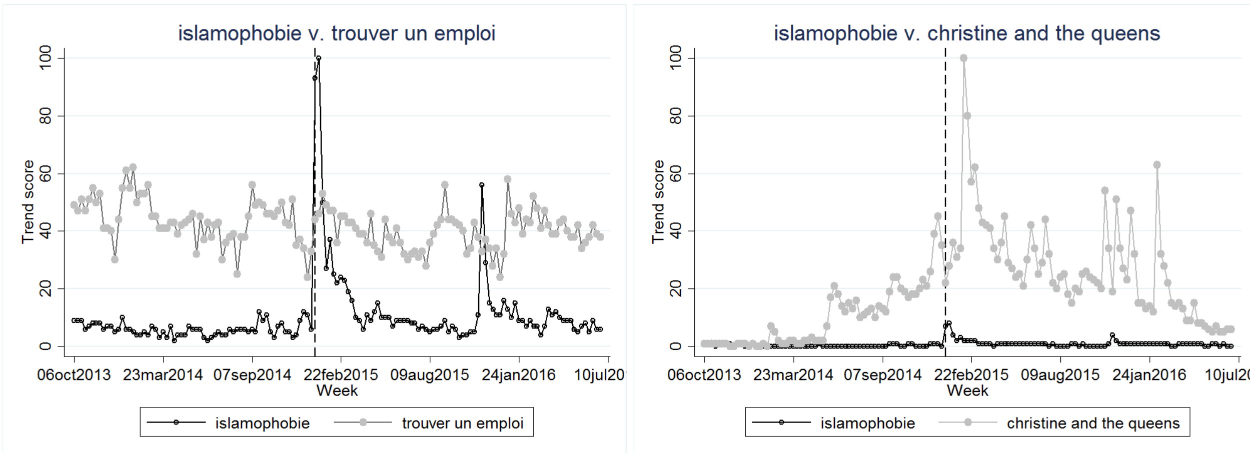
Table A.5: Comparaison with synthetic placebo year

	(1) All contracts	(2) Permanent contract	(3) Fixed-term	(4) Interim
Panel A: $T = 0$ placebo				
(Minority*Period*Shock)	0.00095**	0.00001	0.00086***	0.00009
	(0.00037)	(0.00005)	(0.00020)	(0.00030)
N	64800	64800	64800	64800
Panel B: Synthetic placebo				
(Minority*Period*Shock)	0.00055*	0.00005	0.00051***	-0.00001
	(0.00033)	(0.00004)	(0.00018)	(0.00027)
N	64800	64800	64800	64800
p-value Equality of Coefs.	0.413	0.528	0.181	0.813

Note: Panel A duplicates results from Panel A in Table 3 while panel B reproduces the results using a synthetic placebo year i.e. a weighted blend of outcomes from multiple years preceding the shock. See section A.3 in the appendix for details. Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

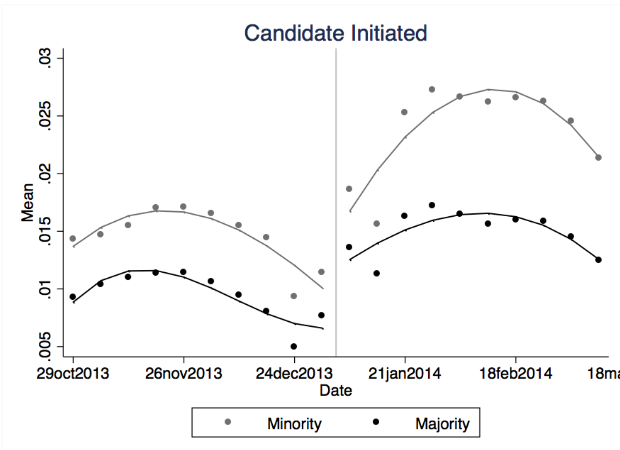
Appendix Figures

Figure A.2: Google searches for "islamophobie" compared to other search terms

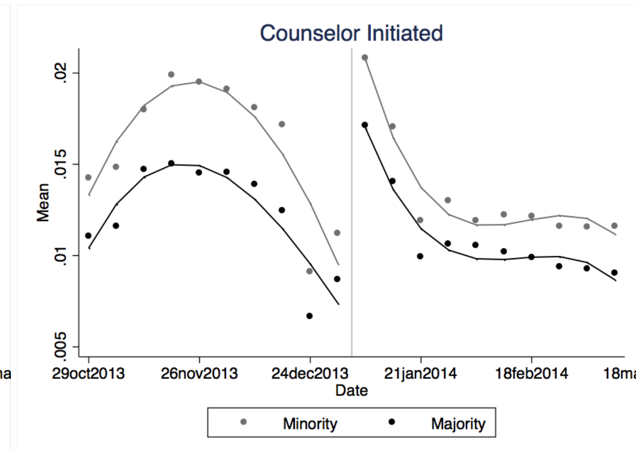


Note: Graphs are weekly series for the search interest for the terms in the graph title in France. See the introduction for the interpretation of the search score. The vertical dashed line indicates the week of the January 2015 terrorist attacks.

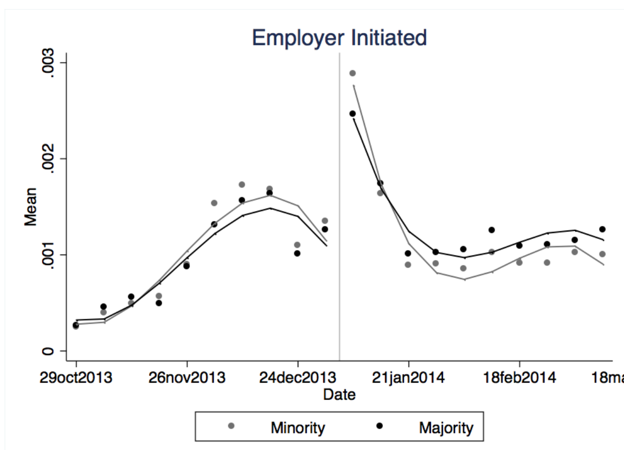
Figure A.1: Potential matches by channel in previous “placebo” year



(a) Candidate



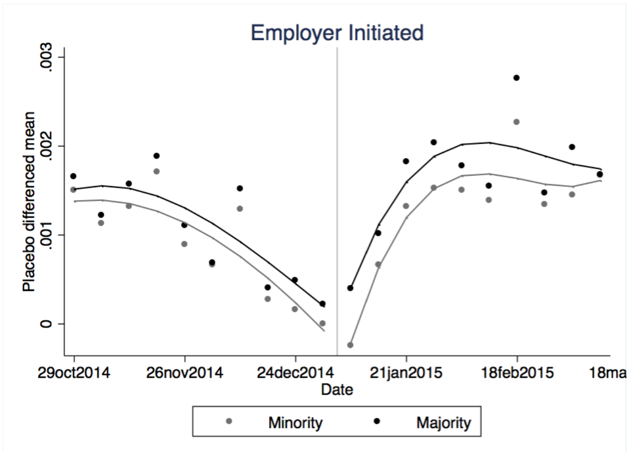
(b) Counselor



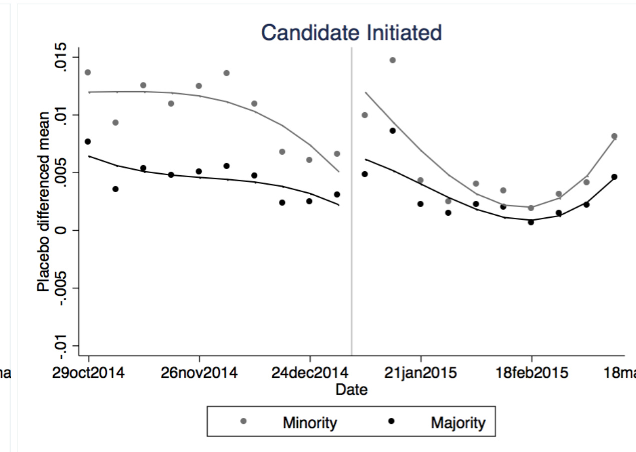
(c) Employer

Note: Outcomes are the average number of potential matches made by jobseekers (a), counselors (b) and employers (c) for the same dates, but in the year previous to the January attacks. Observations are binned averages at the weekly level for majority and minority populations. The points are fitted using an OLS regression with a polynomial time trend of order 3. The vertical line indicates the week of the discontinuity date of the attack for the following year.

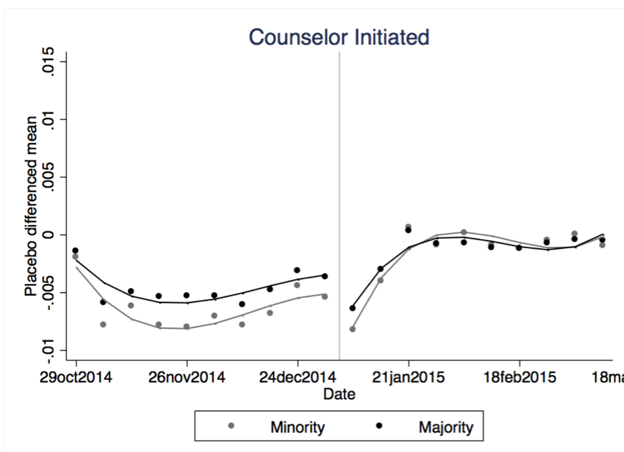
Figure A.3: De-trended potential matches by channel



(a) Employer



(b) Jobseeker

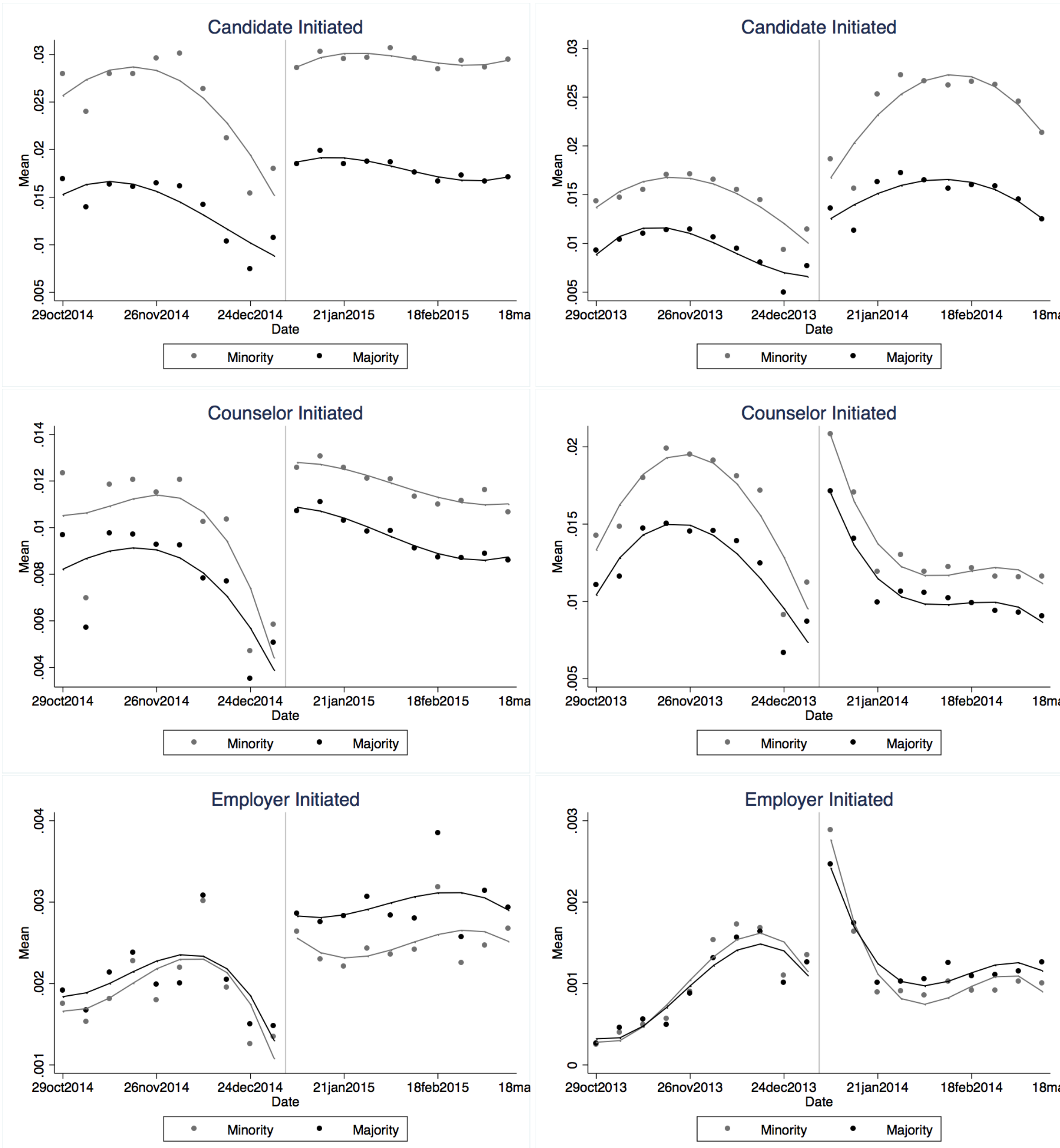


(c) Counselor

Note: Graphs show the de-trended evolution of average potential matching rates binned by week for the shock year $T = 1$ for minority and majority jobseekers. Observations are de-trended by differencing out the equivalent placebo year ($T = 0$) weekly mean. The weighted means are fitted using an OLS regression with a polynomial time trend of order 3. The vertical line indicates the week of the discontinuity date of the attack. Potential matches are shown for the three matching channels, employers (a), jobseekers (b) and counselors (c).

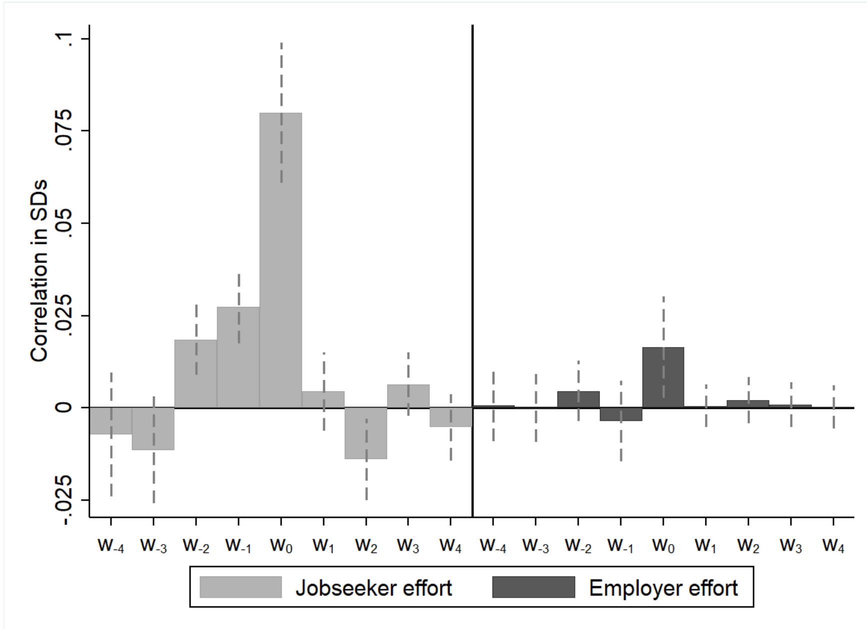
Figure A.4: Evolution of potential matches by channel, by year

(a) Shock year $T = 1$ (b) Placebo year $T = 0$

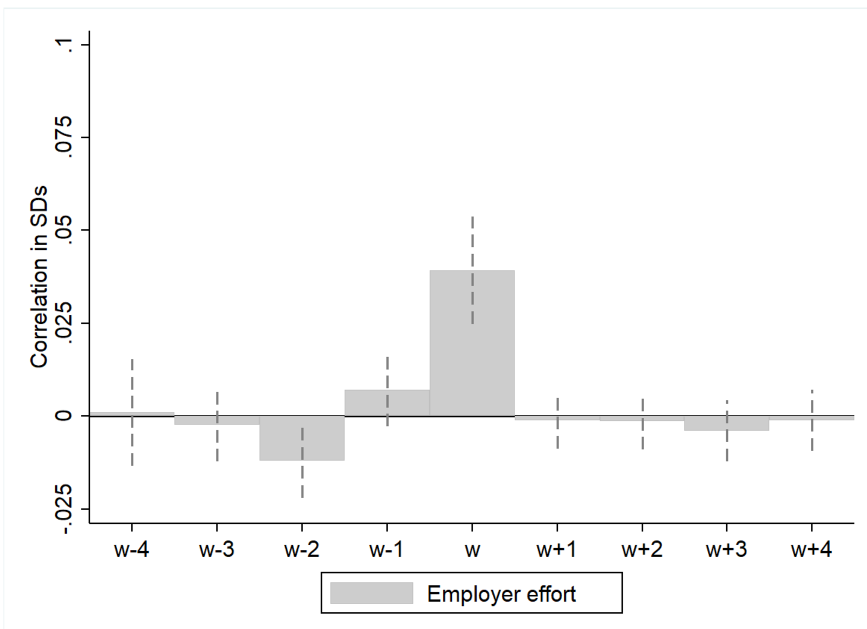


Note: Outcomes are the average number of potential matches made by jobseekers, counselors and employers. Graphs on the left display binned averages for the shock year $T = 1$ while graphs on the right display results for the placebo year $T = 0$. Observations are bins of the weighted average at the weekly level for majority and minority populations. The points are then fitted using an OLS regression with a polynomial time trend of order 3. The vertical line indicates the week of the discontinuity date regardless of the year.

Figure A.5: Pre-shock correlation in the search intensity between different actors



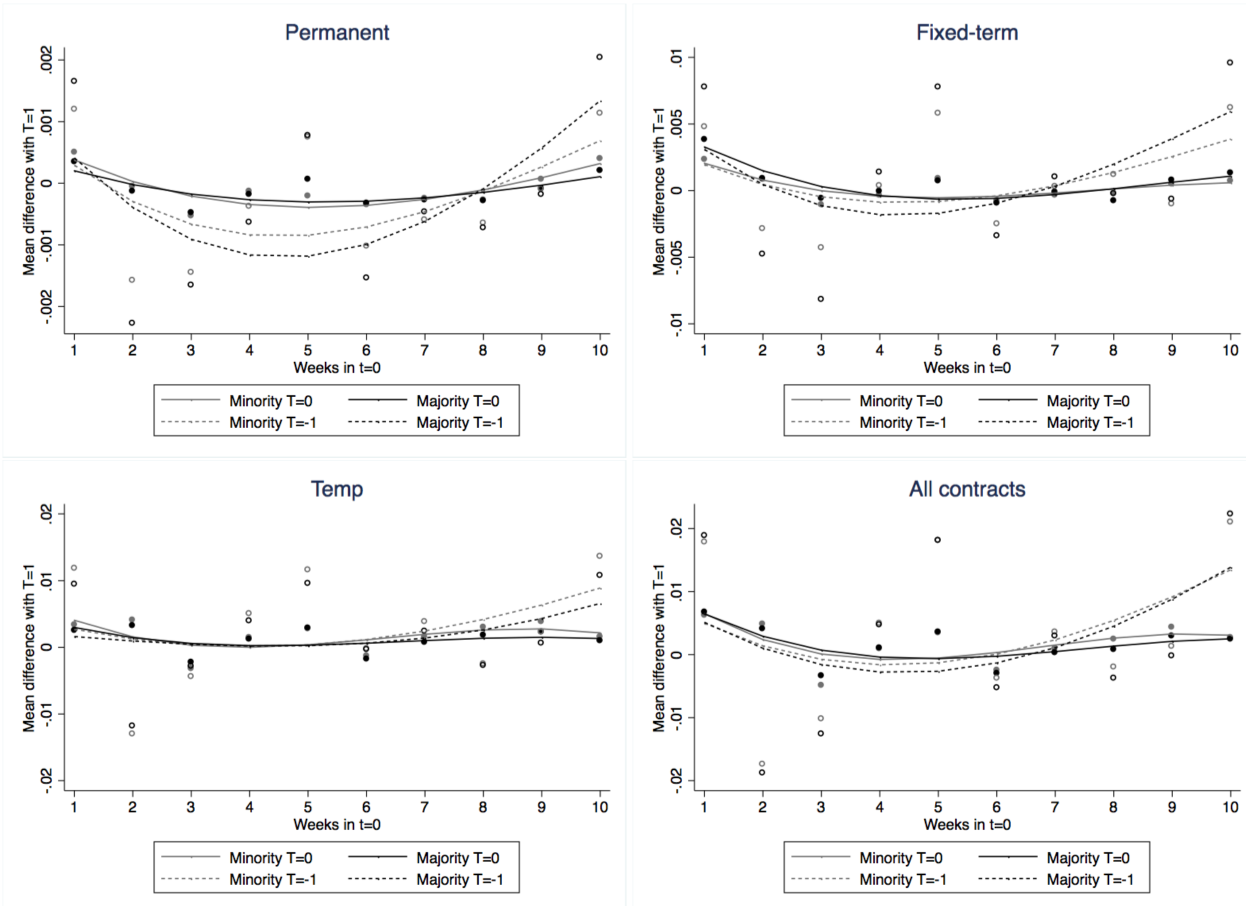
(a) Counselor effort correlation with other actors



(b) Jobseeker effort correlation with employer effort

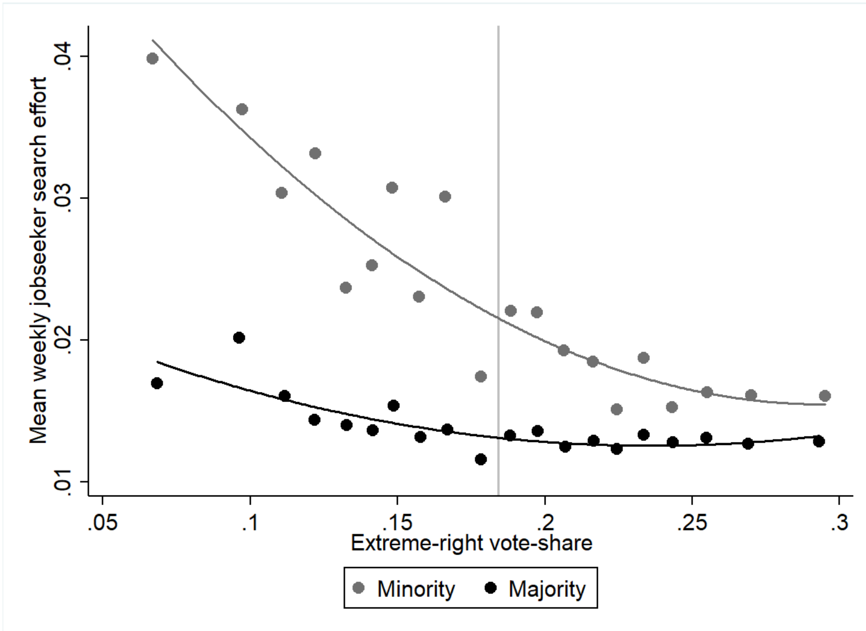
Note: The top graph plots the coefficients from a regression of counselor search intensity on lags and leads of jobseeker search (left side) and employer search (right side) for the weeks w in the pre-shock period ($t = 0, T = 1$). The regressions include week dummies and counselor fixed effects. The bottom graph repeats the exercise where the jobseeker search is regressed on employer search effort. 95% confidence intervals are denoted by vertical dashed lines.

Figure A.6: Placebo year and trend comparability with shock year



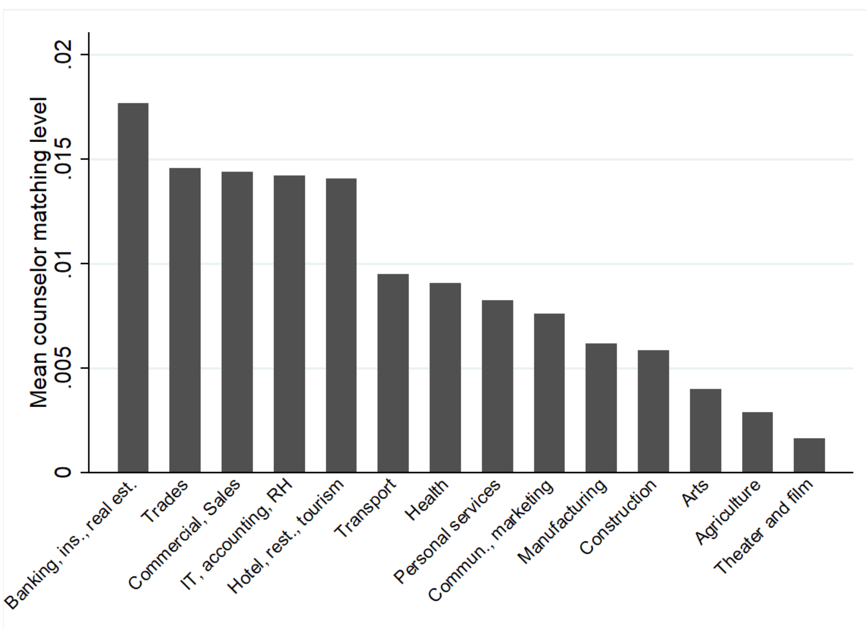
Note: Graphs show the binned de-trended data for the 10 weeks in the $t = 0$ time period. Data that are de-trended using $T = 0$ are solid dots and lines while $T = -1$ are dashed. The binned means are fitted using an OLS regression with a polynomial time trend of order 3.

Figure A.7: Correlation between jobseeker search effort and extreme-right vote share



Note: This graph estimates the conditional expectation function of jobseeker search effort over the distribution of the extreme-right vote share for minority and majority jobseekers in the pre-shock period ($t = 0, T = 1$). Bins are fit with an OLS regression with a second order polynomial. The vertical line denotes the median level vote-share of 18.4%

Figure A.8: Counselor intermedation rates pre-shock by sector



Note: Bars plot the average weekly counselor matching rate for permanent contracts by sector in the pre-shock period ($t = 0, T = 1$).