

Better residential than ethnic discrimination!
Reconciling audit and interview findings
*in the Parisian housing market**

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Abstract

This article investigates discrimination and the interplay of residential and ethnic stigma on the French housing market using two different methods, paired-testing audit study of real estate agencies and face-to-face interviews with real estate agents. The juxtaposition of their findings leads to a paradox: interviews reveal high levels of ethnic discrimination but little to none residential discrimination, while the audit study shows that living in deprived suburbs is associated with a lower probability of obtaining an appointment for a housing vacancy but ethnic origin (signaled by the candidate's name) has no significant discriminatory effect. We have three priors potentially consistent with this apparent paradox and re-evaluate their likelihood in light of these findings: (i) agents make use of any statistical information about insolvency, including residency; (ii) there are two distinct and independent taste discriminations, one about space and one about ethnicity; (iii) these two dimensions exist and complement each other.

Keywords: Audit, Discrimination, Neighborhood Effects, Housing

JEL codes: J71, R23

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Introduction

Ethnic minorities are often concentrated in underprivileged neighborhoods in Western societies (Johnston, Poulsen and Forrest, 2007; Musterd, 2005; Peach, Robinson and Smith, 1981). Emanating from discriminatory processes, ethnic segregation may also lead in turn to “residential traps” that affect minority populations’ socioeconomic achievements, from access to education to economic success and the building of social networks (Crane, 1991; Sampson, Morenoff and Gannon-Rowley, 2002).

While there is a wide range of literature on the cumulative aspect of residential and ethnic/racial inequalities in the United States (Denton and Massey, 1993; Wilson, 1978, 1987), the interaction between ethnic and residential stigma has seldom been analyzed in audit studies on housing discrimination; to our knowledge, the only exception is a study of the credit market by Ross and Yinger (2002).¹

Chronic urban riots in the French *banlieues* in 2005 once again raised the question of ethnic minorities in France (Waddington, Jobard and King, 2013), and yet urban studies carried out in France rarely consider ethnicity in general and ethnic discrimination in the housing market in particular. Some exceptions can be found in the form of recent works providing evidence of both residential and ethnic discrimination; these are reviewed below. By “residential discrimination” we mean discriminatory decisions precisely oriented against the ability of individuals to choose their residential area. This practice has been explicitly illegal in France since 14 December 2013: residential discrimination is the 20th criterion for assessing whether discrimination against individuals has taken place.²

France is an interesting case to study discrimination because the country ostensibly promotes a color-blind ideal of race relations (Sabbagh and Peer, 2008; Safi, 2008; Simon, 2008). For instance, the French Republican model forcefully rejects ethnicity, culture, and religion as a basis for political organization, claims-making, and even as the basis of categories for official statistics. Ethnicity *per se* is therefore not reported in any public-sector statistical survey in France, which makes it difficult for race-based affirmative action to be enforced and for inequality to be documented through representative data.

In light of the above, our article aims at analyzing the relationship between ethnic origin (specifically North African descent) and residential origin (residency in a deprived neighborhood), in a potentially discriminatory interaction in the housing market in France. Our goal is to explore how ethnic and residential stigma can be disentangled in the measurement of discrimination by analyzing practices and discourses relating to the overlap between these two criteria. To this end, we employ two methodological designs:

- An experimental paired-testing audit study, in the tradition of statistical analyses of discrimination, which aims to measure the interaction of ethnic and residential effects.
- A qualitative study, based on interviews with real-estate agents who were asked open-ended ques-

¹The link between racial and spatial stigma has been explored more frequently in qualitative research; see for instance Kirschenman and Neckerman (1991).

²It should be noted that residential location in deprived and segregated areas has also considerable legal socioeconomic consequences, well documented in the urban literature, undermining people’s life chances in terms of education, health, employment, etc.

tions about the selection process for housing applicants.³

Each method has its own advantages and limitations. While the audit study actually measures the effect which the two dimensions have on discrimination, it does not detail the mechanisms underlying ethnic and residential stigma. Conversely, face-to-face interviews provide discursive evidence on discrimination and describe its underlying micro-social processes, but do not assess the magnitude of discriminatory practices. The complementary use of both methods helps overcome the shortcomings of each. In fact, the juxtaposition of our audit and interview findings leads to a double paradox:

1. Although real estate agents believe ethnic discrimination to be widespread, it is not statistically significant in an audit study that controls for residential origin but it is when residential origin is not controlled for.
2. Although real-estate agents do not mention residential discrimination, the audit findings suggest that it is statistically significant.

This article analyzes this discrepancy highlighted in many studies of discrimination between discourses and practices, and attempts to provide interpretations of it. We first review the research background and present our two sets of findings, and then we attempt to decode the double paradox that emerges from them. We then review the various hypotheses consistent with these findings that may help us explain the paradox.

1 The relationship between residence-based and ethnic discrimination in housing: Background and hypotheses

1.1 Space and ethnicity in discrimination studies

There is now extensive sociological and economic literature showing, through survey results, that residential location has a significant effect on employment, education and crime, among other things (Brueckner and Zenou, 2003; Fernandez and Su, 2004; Sampson and Sharkey, 2008). Despite the well-documented role played by segregation and residential location in the production and perpetuation of inequality, the concept of “residential discrimination” has seldom been used and its effect is rarely measured in audit studies on discrimination. Conversely, ethnic and racial discrimination in American cities has been measured with audit studies for more than three decades (Committee on National Statistics, 2002, 2004; Fix and Struyk, 1993). These studies regularly document the unequal treatment disadvantaging ethnic and racial minorities at various stages in the housing search on both the rental and ownership markets (Turner et al., 2013). The 2012 study by the U.S. Department of Housing and Urban Development (HUD)

³The agents audited are not necessarily the same as those interviewed, and we will explain why this is not of consequence below.

showed in particular that, of all ethnic groups, Hispanics face the highest levels of discrimination on the rental market (followed by Blacks and Asians); for example, they become aware of roughly 12% fewer of the housing units available than white housing applicants when they contact real-estate agents to inquire about recently advertised properties.

Comparisons of the HUD's findings over time show that although "blatant" discrimination has declined, with minority applicants now less likely to have a door slammed in their faces, overall unequal treatment remains high because of more subtle forms of discrimination. For example, minority applicants are more likely to be told that they must talk to a lender before being shown an advertised home for sale, whereas a white tester is more likely to meet with the agents without being asked about prequalification. This changing framing of discriminatory practices challenges the methods which pair-testing studies have traditionally used to measure discrimination and requires more attention to several details in the nature and quality of the interactions between the testers and the audited agencies or landlords.

Finally, in the majority of studies, evidence on ethnic and racial discrimination is interpreted as being related to conscious motivations and taste-based mechanisms.⁴ This is corroborated by some findings on ethnic and racial neighborhood preferences highlighting whites' unwillingness to live in neighborhoods with a high proportion of ethnic minorities, particularly African Americans (Charles, 2009). Finally, in an audit similar to ours, Ahmed and Hammarstedt (2008) find that male applicants on the rental market with a Swedish name are much more likely to be called back than those with Arab names; evidence for female applicants is less categorical.

1.2 Review of discrimination and immigration studies in the French context

Studies such as those cited above have rarely been conducted in France, and research on ethnic segregation and discrimination in the area of housing has emerged only recently there. Most scholars have analyzed ethnic segregation as being directly linked to "color-blind" market mechanisms of social stratification. Recent studies challenge this, however, showing that ethnic segregation is more prevalent than socioeconomic segregation and that it decreases very slowly (Préteceille, 2009; Rathelot and Safi, 2014; Safi, 2009; Verdugo, 2011). French government statistics on housing also document considerable inequality between natives and immigrants. Such inequality occurs with regard to not only housing access and tenure, but also housing quality, in terms of factors such as amenities and apartment size (Barou, 2002; Breem, 2009). The immigrants' housing situation is particularly disadvantaged for non-Europeans and is resistant to standard socioeconomic controls, suggesting underlying ethnic discrimination. In a recent comprehensive survey on immigration and discrimination, first- and second-generation immigrants were shown to report twice as much discrimination in housing access as natives of non-immigrant background (Pan Ké Shon and Scodellaro, 2011; Safi and Simon, 2014). Controlling for socioeconomic variables suggests that ethnic or racial discrimination may, at least partly, be at play. In the same survey (TeO), the

⁴Taste-based discrimination still seems to be a major mechanism explaining persisting unequal treatment toward African Americans in the US labor market (Bertrand and Mullainathan, 2004).

majority of respondents reported that this discrimination was based on skin color, ethnic origin, nationality or religion. Some ethno-racial discrimination has also been reported by qualitative studies on public housing in France (Pala, 2005; Tissot, 2005). However, at present, studies assessing for ethno-racial discrimination on the private housing market are extremely rare. Some limited evidence may be found in an audit study carried out by the *Haute Autorité de Lutte contre les Discriminations et pour l'Égalité*, according to which Africans are four times less likely to be selected to rent an apartment compared with their paired French white candidates (HALDE, 2006).

There is, however, well-established evidence of discrimination in the French labor market (Duguet et al., 2010; Duguet, L'Horty and Petit, 2011). One of the most comprehensive audit studies, conducted jointly by the ministry of labor and the International Labour Organization, shows that, four times out of five, employers prefer mainstream candidates to strictly identical candidates of African immigrant background (Cediey and Foroni, 2008). Research on ethnic discrimination in the labor market also provides evidence of “residential” discrimination. For instance, job applicants have lower interview rates when their curriculum vita provides an address indicating a poor suburb. The Cediey’s and Foroni’s audit findings also indicate that living in a poor suburb *per se* undermines employability. Their explanatory hypotheses draw on unobserved characteristics that employers may infer from the candidate’s residential location (dependability, work ethic), or other observable contextual characteristics that may affect productivity (transportation connectivity to inner cities, crime, etc). Wasmer and Zenou (2002) provide a model of urban equilibrium unemployment where access to employment is negatively linked to distance between residential location and jobs due to information loss, employer’s discrimination or connectivity of public transportation networks. Gobillon, Rupert and Wasmer (2014) point out the important role played by commuter distances and housing frictions in the employment pathways of minority workers, relative to the majority, in France. Statistical studies have also documented a significant effect of residential location on job-finding (Duguet, L'Horty and Sari, 2009; Gobillon, Magnac and Selod, 2011), and have related these findings to the spatial mismatch literature. This body of research raises the question of the existence of discrimination against residential origin in France and the extent to which it may interact with ethnic discrimination.

France’s emerging racial question is linked to the post-colonial nature of a considerable proportion of its immigration. While Europeans constitute the majority of first- and second-generation immigrants in France, the most recent waves increasingly come from former French colonies (Algeria, Morocco, Tunisia and sub-Saharan Africans, mainly for Francophone West Africa) (INSEE, 2012). There is growing empirical evidence to suggest that North Africans (also referred to as Arabs or Maghrebi) and sub-Saharan Africans are the most disadvantaged groups in regard to socio-economic position and labor market outcomes, residential segregation, law and justice, health, etc. (Meurs, Pailhé and Simon, 2006; Safi, 2013; Silberman, Alba and Fournier, 2007). The overwhelming majority of paired-testing audits conducted in France during the last decade hence use North or sub-Saharan Africans as the potentially discriminated group. More recently, some studies have focused on religious discrimination against Muslim origin (Adida, Laitin and Valfort, 2014). Although France’s first anti-discrimination law dates back

to 1972, state-level anti-discrimination action has traditionally been weak and fragmented. In 2004, the French government created a centralized anti-discrimination agency (Haute Autorité de Lutte contre les Discriminations et pour l'Égalité) in a context of growing concerns related to discrimination in the political and scientific debates. The HALDE was however dismantled in 2011 and specific action towards racial and ethnic discrimination has been increasingly re-framed into a more general discourse promoting diversity (Bereni and Jaunait, 2009; Doytcheva, 2010).

1.3 In search of “residential discrimination” in the housing market: Disentangling residential and ethnic effects

The majority of testing studies draw on the distinction between taste-based and statistical discriminations and are designed so as to empirically disentangle the two underlying processes. In the housing market in particular, statistical discrimination is tested for by adding information to the applicant's profile (see, for instance, Bosch, Carnero and Farre, 2010; Ewens, Tomlin and Wang, 2014). However, these studies frame only one criterion as being potentially discriminatory (namely ethnic/racial or immigrant background), while other added information is not regarded as possibly bringing another source of unequal treatment. Our study thus adds to the literature by reflecting on the relationship between two potentially discriminatory criteria that may affect housing outcomes. More specifically, we aim to test for three hypotheses:

- Hypothesis one (H1): Real estate agents are primarily concerned with insolvency and they use any potentially relevant information about the applicants as proxies to make inferences about their unobserved solvency. In other words, ethnic and residential origins may lead to statistical discrimination (Phelps, 1972), given the significant legal constraints in place on the French housing market. These constraints motivate landlords to select tenants on financial grounds, under the prior that the complexity of the rules on eviction and the financial cost for landlords are both associated with the risk of default on the rent.
- Hypothesis two (H2): ethnic discrimination and residential discrimination both exist and are combined through an additive effect on overall discrimination, with no particular interaction between them. Given this, real estate agents will engage in pure discrimination against a given type of neighborhood. This would be an instance of taste discrimination against a neighborhood (H2). Taste discrimination may be directly related to the prevalence of anti-minority prejudice (Allport, 1954), or, in Becker's words, it may stem from a tendency on the part of agents to sacrifice profit or other rational objectives in favor of more frequent interactions with co-ethnics (Becker, 1971).
- Hypothesis three (H3): the neighborhood does not matter *per se*: agents merely use the information about the neighborhood to infer an applicant's race or ethnicity. What appears as discrimination against a neighborhood is in fact discrimination against racial or ethnic minorities. Legal scholars

call this *indirect discrimination* (Hunter, 1992): the use of legally unprotected characteristics to achieve legally forbidden discrimination.

Our research design aims to examine these three different hypotheses, which are not necessarily mutually exclusive, by disentangling ethnic and residential stigma in the measurement of discrimination.

2 Research design: The complementary use of qualitative and quantitative frameworks

Direct accounts of discrimination in the social sciences usually fall into two categories (Blank, Dabady and Citro, 2004; Pager and Shepherd, 2008):

- Analyses of discriminatory attitudes, behaviors and discourses: studies that focus on the cognitive and social mechanisms underlying the construction of prejudice, which may lead to discriminatory acts (Fiske, 2000; Schuman et al., 1997).
- Measurements of discriminatory practices: studies that seek to uncover actual discrimination as a “caught-in-the-act case” through experimental situations in order to measure its magnitude, evolution and consequences (Fix and Struyk, 1993; Pager, 2007) following standard procedures of the HUD described in Online Appendix A1.

Only a few studies mobilize both qualitative and quantitative methodologies. These have led to contradictory results, showing that what people say or think with regard to discrimination is not coherent with what they actually do. This was the main finding of LaPiere (1934)’s classic study of discrimination in hotels (people discriminate less than they express prejudice). More recently, Pager and Quillian (2005) have conducted a similar study that shows the opposite discrepancy between attitudes and actions (people discriminate more than they exhibit prejudice). In both cases, the authors’ interpretations are inclined to trust the experimental measurement of discrimination acts, whereas people’s attitudes are often regarded as doubtful.⁵ The two methodologies of inquiring into discrimination are thus presented as concurrent rather than complementary.

In this article, we first use an audit testing strategy to explore the association between residential and ethnic effects, and then we conduct a qualitative survey to assess the prevalence of ethnic or/and residential stigma and how these stigma may affect real-estate agents’ decision with regard to tenants’ selection. It should be noted that, unlike other studies that combine testing methods with qualitative interviews while measuring discrimination, our qualitative and quantitative samples are not matched. We thus do not audit the people we interview. We believe that this is inconsequential to our argument because we constructed our sample of respondents for the qualitative study and of agencies for our audit study on the same principles. We selected richer and poorer neighborhoods in Paris and the surrounding region

⁵Economists call this a logic of revealed preferences, after Samuelson (1938).

where we audited agencies, and contacted interview respondents in richer and poorer neighborhoods in Paris and the surrounding region advertising the same type of goods as the audited agencies.

3 Measuring the intersection between ethnic and residential discrimination factors: An audit study on the Parisian rental market

3.1 The audit study⁶

Our audit study was carried out in March and April 2009. A team of eight individuals, aged 22 to 28, two women and six men (hereafter the “testers”), made a total of 500 phone calls to real-estate agencies for 250 different housing vacancies in the Paris region. We collected advertisements for housing units from the website <http://www.seloger.com>, the leading internet platform at the time of the study.

The test was designed to detect the effect of two potentially discriminatory criteria: the current place of residence (deprived neighborhood) of the fictitious applicant, and ethnic origin (North African background) as revealed by the applicant’s name. Testers were matched by pairs: each tester thus had a co-tester with whom he/she worked most of the time. They were instructed not to fake any particular accent during the phone conversations.⁷ Testers were assigned fictitious identities, which included a name, a place of residence, an occupation and an income level. These were designed to reflect a typical middle-class housing applicant, for instance: “Sébastien Fournier (French name) / Kader Boualem (North African name), lives in La Courneuve (deprived suburb) / Versailles (rich suburb), is 31 years old, works as an accountant and earns a monthly wage of 1,700 euros”. These fictitious identities also included a marital status (married with no children), an occupation and an income for the spouse.

As it was possible neither to test all combinations of geographic and ethnic origins on the same advertised dwelling nor to reveal all the relevant information within a phone conversation, we limited the number of scenarios and designed two different procedures:

- In the first procedure (173 audits), each tester revealed his/her fictitious place of residence at the beginning of the conversation and, as the conversation proceeded, he/she eventually gave his/her name.
- The second procedure (77 audits) was similar but reversed: the fictitious name was revealed first, with the tester then attempting to provide information on his/her current location, without forcing matters.

⁶A comprehensive description of our audit study can be found in Appendix A of the Online Appendix.

⁷This rule reflected a trade-off: introducing accent would have increased unobserved heterogeneity (arbitrariness and thus randomness) in the perception interpretations. Preventing accents likely biased downwards the intensity of discrimination. See Section A.4 of the Online Appendix for further discussions.

The phone conversations that resulted from these procedures usually developed as follows: in the very first sentence, the tester expressed his/her interest in the advertised dwelling. Here are examples of the introductory sentences testers would use:

- In procedure #1: “Hello, I’m calling about your ad for the apartment located in City X. I’m really interested in renting this apartment since I need to move from *city of the applicant* where I currently live to get closer to my job. Is it still available?”
- In procedure #2: “Hello, *name of the applicant* speaking. I’m calling about your ad no. X found on the internet. I’m really interested in the apartment in the ad because I need to move closer to my job. Is this still available?”

A typical conversation would then proceed as follows:

- If the apartment was still available, the applicant would then ask for an appointment to view it. The rental broker would potentially ask for more information about the applicant, and the tester attempted to provide his/her name (Procedure #1) or his/her place of residence (Procedure #2).
- If the apartment was no longer available, the rental broker would generally ask some additional details to determine whether another apartment might meet the applicant’s needs, or would terminate the conversation quickly.

The resulting phone conversations were thus short, focusing on the candidate’s profile and usually homogeneous across testers and procedures. In all cases, the testers could collect information about the rent, the surface area and the location of the apartment from the internet advertisement.

Following the phone call, the testers reported the outcomes of the conversation, which were coded as follows:

| Code | Outcome |
|------|---|
| 1 | Apartment is already rented, nothing else available |
| 2 | Caller is asked to send a written application with personal details |
| 3 | Real -estate agent will call back, but no return call |
| 4 | Apartment is already rented but something else is available |
| 5 | Real-estate agent plans a group visit |
| 6 | Real-estate agent plans an individual visit |

These coding schemes were the outcome of several days of trials on about 50 calls (which are not included in our sample of audits). Overall, they proved sufficiently comprehensive for testers to report the outcomes of the different phone conversations without being concerned that a relevant category was missing.

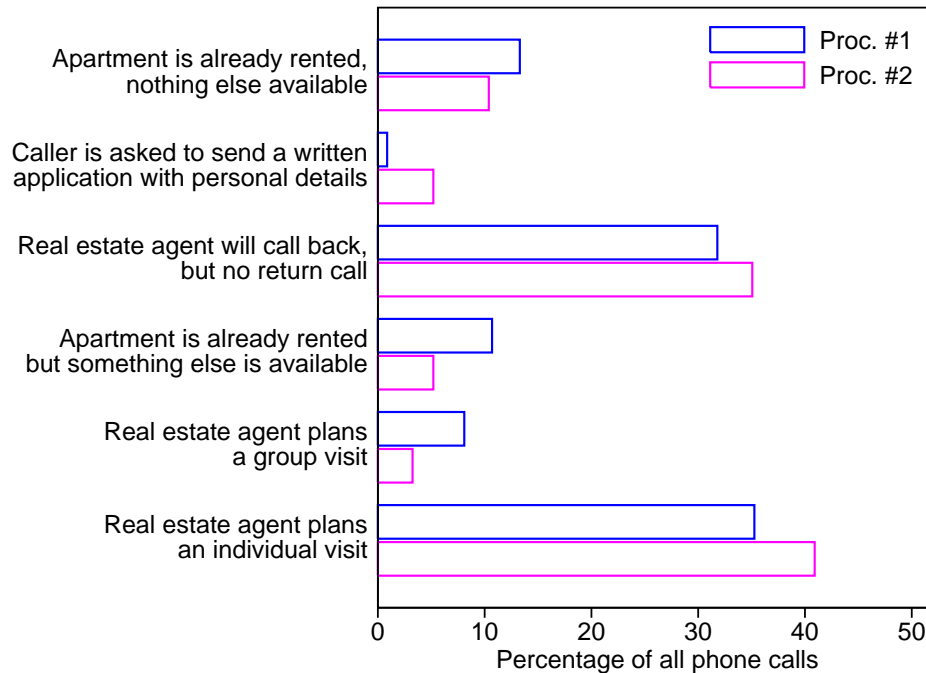


Figure 1. Outcomes of the phone conversations

NOTE: The figure represents in percentage the different possible outcomes of the phone conversations, under the first and the second procedure (residence revealed first and ethnicity revealed first, respectively). All applications are pooled together.

3.2 Outcomes of the phone conversations

Figure 1 compares the distribution of outcomes of the phone conversations obtained under the two procedures, initially ignoring the paired-testing dimension of the experiment, pooling all applicants. Those reveal similarities, suggesting that differences in the introductory sentence used by testers did not significantly alter the interaction with the rental agent. In other words, in both procedures, the fictitious applicants were able to arrange an individual viewing with the rental broker most of the time (35 to 40% of all phone calls), and they were also likely to be told that the rental agent would call them back (32 and 35% of all phone calls).

3.3 Detecting discrimination: Differences in outcomes across paired phone conversations

To simplify the presentation of the results, we group occurrences 1, 2 and 3 as “negative answers” and 4, 5 and 6 as “positive answers”.⁸ We use the term “minority candidate” to refer to an applicant from a

⁸As rightly noticed by a referee, Code 4 (*advertised unit was unavailable but “something else” was offered*) could be interpreted as either “some discrimination” or “steering”, and therefore aggregated with codes 1, 2 and 3 (“rather negative outcome”). Since there were few occurrence of this outcome (Figure 1), we adopt as a benchmark the aggregation of Code 4 with Codes 5 and 6 (rather favorable). In Appendix B of the Online Appendix, we repeat our estimations after aggregating Code 4 with Codes 1 to 3. We also re-run our estimations after excluding applications involving the occurrence of Code 4. None of these checks alter the findings presented in this section.

deprived suburb in the first procedure and to an applicant with a North African name in the second procedure. Note that the minority candidate in, for instance, the second procedure may have been assigned a fictitious residence in a privileged suburb: the term “minority” refers only to the information revealed first during the conversation. Accordingly, the paired-tester is labeled the “majority candidate”.

In Table 1, we cross-tabulate the outcomes of the two candidates obtained through each procedure (residence first, or ethnicity first). The full results are displayed in Tables B1 and B2 of Online Appendix B. In each panel, the first number is the number of cases and the second number is the frequency, i.e. the number of cases divided by the total number of cases studied. The on-diagonal coefficients report the number of cases where the minority and majority candidates were equally treated, at least when grouping the different outcomes. For instance, in the first procedure (Panel a.) this occurred in 26.6% + 34.7% = 61.3% of all cases. The off-diagonal coefficients report unequal treatment, in the remaining 38.7% of cases. The upper-right cell reports cases where the minority candidates was treated better than the majority candidate, which occurred in just 13.9% of cases. In contrast, the lower-left cell reports cases where the majority candidate was treated better than the minority candidate. This occurred in 24.9% of cases.

Table 1. Cross-tabulation of the outcomes across paired phone calls

| a. Procedure #1 (residence first) | Outcome of the minority candidate | | Total |
|-----------------------------------|-----------------------------------|-------------------------|-------------|
| | Rather negative (1,2,3) | Rather positive (4,5,6) | |
| Outcome of the majority candidate | | | |
| Rather negative (1,2,3) | 46 (26.6%) | 24 (13.9%) | 70 (40.5%) |
| Rather positive (4,5,6) | 43 (24.9%) | 60 (34.7%) | 103 (59.5%) |
| Total | 89 (51.5%) | 84 (48.6%) | 173 |
| b. Procedure #2 (ethnicity first) | Outcome of the minority candidate | | Total |
| | Rather negative (1,2,3) | Rather positive (4,5,6) | |
| Outcome of the majority candidate | | | |
| Rather negative (1,2,3) | 30 (39.0%) | 11 (14.3%) | 41 (53.3%) |
| Rather positive (4,5,6) | 7 (9.1%) | 29 (37.7%) | 36 (46.8%) |
| Total | 37 (48.1%) | 40 (52.0%) | 77 |

In the second experiment, where ethnicity was revealed first (Panel b.), discrimination appears to be much less significant. In fact, the on-diagonal cases now constitute 76.7% of cases, i.e. those in which the minority and majority candidates are treated equally. The off-diagonal coefficients reporting unequal treatment reveal that the minority candidate was treated better than the majority candidate in 14.3% of cases, while the majority candidate was treated better in 9.1% of cases, that is, *in fewer cases*.

We can summarize the information in Table 1 by computing the discrimination rates displayed in Table 2.

Table 2. Discrimination rates under the two procedures

| | Procedure #1 (residence first) | Procedure #2 (ethnicity first) |
|-------------|--------------------------------|--------------------------------|
| N_M/N | 24.86% [18.42%, 31.30%] | 9.10% [2.67%, 15.51%] |
| N_m/N | 13.87% [8.72%, 19.02%] | 14.29% [6.47%, 22.10%] |
| z-statistic | 2.585 | -1.003 |
| p-value | 0.0097 | 0.3157 |

NOTE: N_M (resp. N_m) denotes the number of audits in which majority (resp. minority) candidate obtains a positive outcome while the minority candidate (resp. majority) does not. Numbers in brackets are 95% confidence intervals. The p -value is associated to the null hypothesis that the relative difference $N_M - N_m/N$ is significantly different from 0.

These discrimination rates confirm that a candidate from the deprived suburb was significantly less likely to have a positive response in the first procedure relative to his/her paired applicant, while there was no significant evidence of discrimination against the candidate with a North African name in the second procedure (some finding even suggesting positive treatment). The next subsection examines the robustness of these initial results.

3.4 Robust assessment of the presence of discrimination

We now test different specifications that use the panel structure of our dataset to analyze whether the phone conversations reveal discrimination against minority candidates. These specifications are variants of a linear probability model that accounts for the probability of obtaining a positive outcome. The generic model that we consider is:

$$y_{i,a} = z_a + \gamma d_{i,a} + \beta X_{i,a} + \varepsilon_{i,a} \quad (1)$$

where $y_{i,a}$ is an indicator taking the value of 1 if Applicant i obtains a favorable outcome in Audit a , z_a is a set of variables that characterize the paired audit, $d_{i,a}$ is a dummy with the value of 1 if the applicant is the minority candidate, and $X_{i,a}$ is a set of controls that characterizes Tester i in Audit a . In the baseline model, z_a includes some characteristics of the advertised dwelling, such as the surface, the rent, etc. We then drop these in favor of audit fixed effects. Tables 3a and 3b report the results of these different regressions.

The regressions in Tables 3a and 3b confirm the rough calculations in Table 2: the applicant from the deprived suburb in the first procedure (residence first) experiences more than a 10 percentage point drop in the probability of obtaining a favorable outcome. The coefficient is precisely estimated and robust to the set of controls included in the regression. Interestingly, there is no evidence of less favorable treatment of the minority candidate in the second procedure: having an Arab name has no significant impact on the probability of obtaining a viewing.

In the first procedure (residence first), subsequently revealing the name led to a significantly higher outcome, regardless of the ethnicity of the name. This may be a mere statistical correlation and not a causal link, as it occurred only when the candidate was able to arrange a meeting for the viewing, i.e.

Table 3a. Estimation results: Procedure #1 (residence first)

| | (I) | (II) | (III) | (IV) | (V) | (VI) | (VII) |
|---|----------------------------|----------------------------|----------------------------|----------------------------|----------------------------|----------------------------|----------------------------|
| Deprived suburb | -0.1440 (0.0638) | -0.1387 (0.0564) | -0.1387 (0.0564) | -0.1098 (0.0468) | -0.1227 (0.0480) | -0.1301 (0.0522) | -0.1373 (0.0520) |
| Minority name (North Africa) | 0.0567 (0.1110) | 0.0152 (0.1004) | 0.0152 (0.1004) | | | 0.1273 (0.0924) | 0.1046 (0.0939) |
| Name revealed | 0.2431 (0.0987) | 0.1788 (0.0833) | 0.1788 (0.0833) | | | 0.2415 (0.0997) | 0.2373 (0.1009) |
| Name revealed x Minority Name | -0.0932 (0.1276) | 0.0110 (0.1123) | 0.0110 (0.1123) | | | -0.1888 (0.1175) | -0.1747 (0.1161) |
| Controls: | | | | | | | |
| Individual call | Y | Y | Y | | Y | | Y |
| Vacancy characteristics | Y | Y | Y | | | | |
| City level dummies | Y | | Y | | | | |
| Fixed effects | | | | | | | |
| Audit level | | | | Y | Y | Y | Y |
| Pairs of testers | Y | | Y | | | | |
| Individual testers | | Y | Y | | | | |
| <i>N</i> | 327 | 327 | 327 | 346 | 346 | 345 | 345 |
| <i>R</i> ² | 0.7371 | 0.5972 | 0.5972 | 0.0311 | 0.0443 | 0.0668 | 0.0775 |
| <i>R</i> ² with fixed effect | | | | 0.8264 | 0.8288 | 0.8352 | 0.8370 |

NOTE: Estimates that are statistically significant at the 5% level are in boldface. Standard errors in parentheses. Standard errors are clustered at the audit level. Each column reports the coefficients γ and β estimated along with a specific set of controls for each procedure. Name revealed is a dummy indicating whether the tester revealed his/her name in the phone conversation. Characteristics at the level of the individual phone calls are: a dummy indicating whether the tester used the phone line with a number starting with 09 (indicating Internet box) and a dummy indicating whether he/she called first. Vacancy characteristics include the rent and the surface of the advertised dwelling. City level dummies are dummies at the level of the city when the dwelling is located outside of Paris or, otherwise, at the level of *arrondissements*.

when a positive outcome had been obtained. A name of North African origin is negative but not significant, or only very marginally, in Columns (VI) and (VII). In terms of the other potentially discriminatory criterion (North African name in the first procedure, deprived suburb in the second procedure), we find no significant impact when this was revealed to the rental agent. This may be due to the lower number of observations in the second procedure, which may affect the significance of the coefficients. To verify that this is not the case, we replicated the regression with a bootstrap procedure (500 times) randomly selecting 77 pairs of calls from the 177 initial pairs in the first procedure. The full results, reported in Table B3 of Online Appendix B, reveal that the conclusions above still hold: the negative effect of living in a deprived suburb remains true in all columns. The significance level is reduced: this is now 10% in four out of seven specifications, and marginally less significant in the other three specifications.⁹

⁹This result is not surprising in the first procedure given that Procedure #2 shows that having a North African name does not go against the minority candidate. Conversely, the fact that living in a deprived area has no significant impact in the second procedure may seem puzzling given the robust effect obtained from Procedure #1. This may be due to the small number of phone calls in the second procedure, during which the applicant from the deprived suburb managed to reveal this information to the rental agent (16 cases out of the 154 phone calls).

Table 3b. Estimation results: Procedure #2 (ethnicity first)

| | (I) | (II) | (III) | (IV) | (V) | (VI) | (VII) |
|---|---------------------|---------------------|---------------------|--------------------|--------------------|---------------------|---------------------|
| Minority name | 0.0299 (0.0785) | -0.1076 (0.1776) | -0.1118 (0.1984) | 0.0519 (0.0553) | 0.0484 (0.0604) | 0.0502 (0.0624) | 0.0419 (0.0728) |
| Deprived suburb | 0.0468 (0.0821) | 0.0481 (0.0768) | 0.0768 (0.0765) | | | 0.0261 (0.0706) | 0.0298 (0.0715) |
| Residence revealed | 0.1001 (0.2553) | 0.3255 (0.2494) | 0.2365 (0.2499) | | | 0.0686 (0.2213) | 0.1148 (0.2578) |
| Residence revealed x Deprived suburb | -0.2307 (0.2713) | -0.3588 (0.2887) | -0.4082 (0.3109) | | | -0.1434 (0.2361) | -0.1837 (0.2742) |
| Controls: | | | | | | | |
| Individual call | Y | Y | Y | | Y | | Y |
| Vacancy characteristics | Y | Y | Y | | | | |
| City level dummies | Y | | Y | | | | |
| Fixed effects | | | | | | | |
| Audit level | | | | Y | Y | Y | Y |
| Pairs of testers | Y | | Y | | | | |
| Individual testers | | Y | Y | | | | |
| <i>N</i> | 144 | 144 | 144 | 154 | 154 | 154 | 154 |
| <i>R</i> ² | 0.6295 | 0.5789 | 0.6358 | 0.0115 | 0.0194 | 0.0191 | 0.0305 |
| <i>R</i> ² with fixed effect | | | | 0.8771 | 0.8787 | 0.8780 | 0.8801 |

NOTE: Standard errors in parentheses. Standard errors are clustered at the audit level. Each column reports the coefficients γ and β estimated along with a specific set of controls for each procedure. Residence revealed is a dummy indicating whether the tester revealed his/her residential origin in the phone conversation. Characteristics at the level of the individual phone calls are: a dummy indicating whether the tester used the phone line with a number starting with 09 (indicating Internet box) and a dummy indicating whether he/she called first. Vacancy characteristics include the rent and the surface of the advertised dwelling. City level dummies are dummies at the level of the city when the dwelling is located outside of Paris or, otherwise, at the level of *arrondissements*.

4 “Whom do you discriminate against?” Asking real-estate agents

The qualitative part of this paper was designed to confirm, or at least verify, whether the surprising result of the audit testing strategy would be perceived by agents themselves, and whether they would be conscious of the apparent importance of residence in the outcome. In fact it tells a different story, however, which has the added interest of revealing the importance of employing different methodologies.

4.1 Interviewing real estate agents

This involved 29 face-to-face semi-directive interviews with real-estate agents in Paris and the surrounding region, conducted between June and October 2010. Each interview lasted at least one hour.¹⁰

These interviews first reveal that, while the overwhelming majority of agents tend to deny any form of discrimination in their own decisions, they have a lot to say about the functioning of the market in

¹⁰For more information about the interviews, please refer to Online Appendix C.

general and about other agencies' discriminatory actions in particular.

4.2 Ethnic and racial discrimination is everywhere... but not here!

Perhaps unexpectedly, interviewees felt comfortable talking about discrimination without our expressly mentioning the term. As soon as the section about the “ideal candidate” began, the word “discrimination” arose naturally in the discussion, and was systematically associated with ethnic or racial dimensions. The issue of ethnic or racial discrimination was spontaneously raised by 13 interviewees (out of 29) without any overt questioning by the interviewer. In all these cases, discrimination was directly associated to ethnic and racial criteria, with skin color (or African origin) being most commonly cited.

Table 4 shows the recurrence of words referring to ethnic/racial criteria, distinguishing whether these were used by the interviewees or the interviewers. Interviewer and respondent used the word “origin” with equal frequency, whereas words indicative of ethnicity were predominantly used by interviewees. On average, the words reported in Table 4 are used more than twice as frequently by the interviewee.

Table 4. Recurrence of words referring to ethnic/racial criteria in the interviews

| | Respondent | Interviewer | Ratio |
|-----------------------|-------------------|--------------------|--------------|
| Turk(s) | 23 | 2 | 11.5 |
| Africa, African, etc. | 36 | 4 | 9.0 |
| Black(s) | 105 | 20 | 5.3 |
| Arab(s) | 26 | 6 | 4.3 |
| White(s) | 37 | 11 | 3.4 |
| Race, racial | 14 | 7 | 2.0 |
| Racist, racism | 45 | 23 | 2.0 |
| Foreigner(s) | 45 | 29 | 1.6 |
| Origin | 52 | 56 | 0.9 |
| Ethnic, ethnicity | 19 | 26 | 0.7 |
| Total | 402 | 184 | 2.2 |
| Total per interview | 13.9 | 6.3 | 2.2 |

Even though real-estate agents spoke extensively about ethnic and racial discrimination, they systematically denounced it as “bad” and “illegal” and denied the existence of such practices in their own agency.

Well I have people, blacks, who came to the agency and they told me, err... So I welcomed them and they told me, “We just got sent on our way by – well, I won’t tell their name, an agency not far from here” [he laughs] and err... they’ve been told, “No, no Sir, we have nothing to rent”. They had just arrived, and they were told, “We have nothing to rent”.

They most frequently blamed the landlords for pushing real-estate agents to discriminate against minorities, but said that whenever they had to deal with such landlords, they refused to comply. Besides the fact that all testimonies converge on the salience of discrimination against black and Arab minorities, some

real-estate agents provide the interviewer with objective figures quantifying such practices, saying that almost 10% of rental offers come with discriminatory recommendations from landlords (“I don’t want Arabs, blacks, etc.”).

- *Do you have recent experience of verbal complaint?*
- *Yeah, it happened maybe one year ago: someone went to complain to the person in charge of rentals.*
- *What do you say in those situations?*
- *Well, we tell them we have presented the application to the landlord, and the landlord decides. We can’t do anything about it. Because if we really did refuse that kind of persons, according to me we are all people, no matter the color, we are all human beings, therefore this behavior makes me mad. If the person is working, I don’t see where there is a problem. We can see the kind of people we’re dealing with. Now we can’t force landlords. But we won’t refuse to these people viewings.*
- *And if you had to assess the percentage of racist demands?*
- *Yes, you can say that. It’s not the majority, but not matter how small. . . I don’t know, 10%?*

Real-estate agents often imply that they understand the landlords’ aversion to certain profiles which experience has proven unreliable (e.g. resulting in unpaid rent, sublease, damage). This argument builds even more explicitly on statistical discrimination.

The landlord asks us to avoid certain categories of population. We don’t have the right to do that. We can’t practice racism like that. Otherwise, we would only get into big trouble. But, it is true that. . . Experience is such that we avoid certain categories of people because we realize that with those categories we always have problems.

Let’s say that experience suggests that. . . we will be more careful with certain categories than with others.

In short, when asked about the selection of applicants for housing rentals, real-estate agents find racial discrimination the obvious issue. However, they overtly condemn racial discrimination, describe it as a problem for landlords rather than the real-estate business itself. Some agents exhibit understanding attitudes toward forms of discrimination which they find rational, while others find the practice of discriminating on racial grounds economically irrational. But they all see racial discrimination as the issue at stake.

4.3 Looking for residence-based discrimination

In stark contrast, none of the respondents spontaneously mentioned residential stigma. The interview was planned so that the interviewer asked this question at the end of the conversation, after a lot of discussion

of discriminatory practices and the motivation behind them. The question was formulated thus: “There is much debate about ethnic and racial discrimination but do you think that other criteria might be at play, for example residential ones?”, and “If a candidate comes from an underprivileged neighborhood, the suburbs, etc. do you think this may disadvantage him/her on the renting market?”

This question seems disconcerting for most real-estate agents. An overwhelming majority could simply not see why residence might have any impact. Almost all respondents said that they paid no heed to where a candidate lives. Some of them asserted that they rarely check the candidate’s address when they are examining him/her, and claim that they would not know in what kind of neighborhood a given address is located. Only two respondents elaborated on this question further, mentioning association with linguistic stigma: according to them, the problem is not the area of residence itself, but the fact that people from these neighborhoods do “not express themselves well” or have a “particular accent”. Only one single respondent admitted that some locations (and he/she cited the most stigmatized French suburbs) can disadvantage candidates, although he/she then gave an example of labor market discrimination, not housing.

5 The paradox: A discussion

The comparison of our audit and interview findings leads to a double paradox:

1. Real-estate agents clearly deny the relevance of residence as a discriminatory factor in the housing market, while quantitative evidence tends to show that it has a significant disadvantaging effect in access to housing.
2. Ethnic origin does not have a significant impact when the area of residence is controlled for in the exploitation of the audit study data, while real-estate agents report that it has an indisputable discriminatory impact.

Thus, the widespread ethnic discrimination identified by real-estate agents is not statistically significant in the audit study that controls for residence, while residential discrimination, which is not deemed important by real-estate agents, is statistically significant in the same study.

The qualitative interviews lead to the elimination of one of the three hypotheses posited in Section 1.3, which suggested that there may exist a form of pure discrimination against a type of area of residence. The real-estate agents we interviewed never mentioned this, either directly or indirectly.¹¹ Furthermore, there is no reason to assume that this might be an effect of a social desirability bias in these responses, as real-estate agents are not shy about documenting racial discrimination, which is more controversial. We now discuss whether the remaining two hypotheses (hypotheses H1 and H3) are consistent with the double paradox revealed above.

¹¹One may suspect this result to be biased since real-estate agents may have denied the existence of residential discrimination in order to be categorical as to their not accepting any type of illegal bias. However, at the time of the interviews, only racial discrimination was both illegal and subject to testing by associations; this was not the case of residential discrimination.

Discussion of H1: “Statistical discrimination through proxying applicants’ solvency”

The first hypothesis suggested that real-estate agents are primarily concerned with insolvency and that any correlates with insolvency (including residential or ethnic origins) may lead to biases in tenant selection through statistical discrimination mechanisms (*i.e.* the combination of information about ethnicity and residence interacts in signalling low socioeconomic background, which translates into a marker of greater likelihood of insolvency, leading real-estate agents’ decision to go against these applicants). This hypothesis is in line with former studies highlighting that the French housing market legal framework may be overly protective of tenants,¹² which leads to increasing concerns on the part of landlords about a potential default on rent payment.¹³

This hypothesis may appear to be consistent with the double paradox and it is also in line with a prevailing political and scientific belief that, unlike in the U.S.A., ethnicity and race are less prominent inequality factors in France (Wacquant, 2008). In other words, the nonsignificant effect of ethnicity when residential origin is controlled for can be read as a demonstration of the supremacy of class over race in generating social inequality in France, thus reducing the disadvantage experienced by ethnic minorities to socio-economic and “color-blind” factors. Within this framework, the double paradox in our findings is no more a paradox, but could be interpreted as evidence that *residential location is used as a proxy for insolvency or “bad tenant” (and not for “race”) and that real-estate agents perceive white applicants from “bad” neighborhoods just as negatively as minority applicants from “bad” neighborhoods.*

While this interpretation is appealing, it should be noted that it is not directly established by our findings but rather stems from a speculative explanation building on prior studies on the legal environment surrounding the French housing market. It may appear, nonetheless, to stand in contradiction with growing empirical evidence on ethnic and racial inequality in France documented in diverse social spheres (labor and housing markets, schooling and education, spatial segregation, health, access to law and public services, etc.). In fact, the contradiction is not so straightforward because hypothesis H1 could also be read in line with the sociological concept of systemic or indirect discrimination (Bonilla-Silva, 2013): the legal structure of the housing market in France could lead to systematic ethnic bias, even though its driving mechanisms may not be directly oriented toward ethnic minorities. All in all, if residential origin is a powerful proxy for insolvency, it indirectly undermines ethnic minorities’ chances on the rental housing market simply because ethnic minorities are highly concentrated in disadvantaged neighborhoods.

¹²We have observed, in this context and in other studies (Bonnet et al., 2011), that landlords are very cautious in their selection of tenants, probably because the French legal environment makes evictions a long and difficult process, with various steps to undergo when attempting to prosecute tenants for failure to pay rent, a process which can involve delays of several months before a court decision is handed down and the scarce execution of eviction decisions. We incidentally noticed that some landlords discriminate against lawyers, who may easily delay procedures.

¹³According to the European Community Household Panel (1994-2002), 10.6% of tenants self-reported in the survey that they had experienced a default on the rent in the last year. Some of these defaults were only temporary, but others were recurrent cases of default.

Discussion of H3: “*Neighborhood as a proxy for ethnicity*”

The other hypothesis is that the residential stigma is a proxy for an ethnic stigma. This is all the more likely given that the degree of ethnic concentration is high in the areas chosen for our audit study. In this case, ethnicity is the signal that affects real-estate agents’ decisions when selecting from applicants, and information about place of residence is used as a proxy for ethnic origin. Moreover, even if residential origin is translated into ethnic origin in real-estate agents’ minds, it may be more conducive to discriminatory decisions than overt information about ethnic origin (such as, for instance, an African name) because of a less pronounced desirability bias involved in residential discrimination. This may explain the fact that, in our data, the negative impact of ethnicity becomes less powerful when the applicant presents his/her residence first.

This assumption challenges the “cumulative perception” of stigmas according to which one disadvantaging factor would simply add a discriminatory effect to the other. Instead of this additive effect (residence would disadvantage everyone, but ethnic minority candidates are the worst off), our findings suggest that one variable (residence) may be a substitute for the other (ethnicity), thus making its effect redundant, especially when residence is the first of the two pieces of information revealed in a potentially discriminatory interaction. It is also important to note that this redundancy appears to take shape through a proxying process. Some elements in the interviews support the fact that residence may be used as a proxy for ethnicity, and in fact the very few interviewees who elaborated on the question about residential discrimination mentioned “cultural arguments” directly linked to ethnic origin: fluency and accent in French.

Overall, hypothesis H3 helps us to understand the results related to the effect of geographic origin in the regressions performed on the phone conversations from Procedure #2: because ethnic origin has already been revealed at the beginning of the conversation, the proxying process of ethnicity through residence is not activated. However, this still does not solve the puzzle of the lack of significance of the ethnic origin variable in the procedure where ethnicity is revealed first to the real-estate agent and residence later. Possible explanations may draw on the weakness of names in proxying ethnicity in France.¹⁴ Residential origin may therefore be a more effective proxy of ethnicity. Using in-person testers can be a useful strategy to eliminate this possible proxying process and better disentangle the effects of ethnic and residential origins while measuring discrimination.

Our approach inevitably presents certain limitations. A housing search involves several steps, and this audit study analyzes only the first stage in the process. Whilst this would be problematic were we measuring the overall level of discrimination, we believe that it is adequate for our less ambitious purpose, namely that of detecting the existence of discriminatory practices. These are readily visible in the first

¹⁴Names have been frequently used in paired-testing audit in France to signal ethnic origin and have been proven to have significant discriminatory effects. However, some recent quantitative analyses suggest that experiences of discrimination in France are also very common among “visible minorities” (Beauchemin et al., 2010) and may thus indicate that testing studies should try to measure discrimination through personal interactions. Personal interactions nevertheless increase methodological vulnerability of testing studies because of the well-known problem of individual heterogeneity.

contact between housing applicants and real-estate agents.

6 Conclusion

This paper contributes to the urban literature on residential segregation by providing empirical support for the argument that living in a given neighborhood may hinder households' residential mobility. The article thus contributes to the scholarship on place-based exclusion, which has already been documented in relation to redlining (Aalbers, 2007) and steering (Galster and Godfrey, 2005), by adding the rental market dimension. From a methodological perspective, it reveals a discrepancy between the results obtained using two different methods (audit and face-to-face interviews). This may be due to the unconscious nature of some discriminating acts, which face-to-face interviews should address in one way or another. It may also challenge the relevance of measuring interaction between discriminatory factors. The underlying framework of such studies is dominated by the cumulative or additive disadvantage paradigm. However, when two stigmas are so interconnected in social representations, substitution mechanisms can take place when one stigma proxies another. Further research should therefore explore the implications of such proxying processes on the measurement of discrimination.

From a substantive point of view, our dual research protocol has enabled us to discard one of our three hypotheses: the idea of pure residential discrimination does not make sense. Two non-exclusive hypotheses remain: a hypothesis of statistical discrimination whereby real-estate agents make inferences about insolvency through information about residence, and a hypothesis that there is purely ethnic discrimination where residence is a proxy for race/ethnicity. Further research combining qualitative insights and systematic data may help us resolve this.

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