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Discrimination in second hand consumer markets: Evidence from a field experiment*

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Abstract

This paper studies discrimination against immigrants in the consumer market in Spain. We send emails of fictitious buyers to a popular Spanish second hand market webpage similar to *ebay*. Sellers are approached randomly by buyers with Spanish native or foreign sounding names to signal their ethnic origin. We find that those buyers with a foreign sounding name are contacted around 7.8 percentage points less than those with a Spanish sounding name. We then turn to explore how the price of the advertised good influences the degree of discrimination against foreign sounding names. We find that differential treatment across names occurs with more intensity for cheaper goods.

Keywords: discrimination, second hand consumer market, field experiment.

JEL classification: J15, R23, C93.

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1. Introduction

Spain is one of the main "doors" for immigration entry into Europe, with a massive inflow of immigration, specially coming from South America and North Africa. During the last decade, the share of immigrant population in Spain has grown from 2.28% in 2000, to 12.08% in 2009 (data from INE, 2010). If this growth rate continues, in 2020 the immigrant population in Spain could represent more than 20% of the total population (INE, 2010).

This paper analyzes to what extent this new immigrant population faces discriminatory behaviour in consumer markets using a correspondence experimental design. We are not the first ones to analyze discrimination against minorities using an experimental design. A growing number of studies have focused in detecting discrimination in a variety of settings: US labour market (Bertrand and Mullainathan, 2004); US housing market (Carpusor and Loges, 2006; Riach and Rich, 2002); Swedish rental market (Ahmed and Hammarstedt, 2008); and US consumer markets (Yinger, 1998). In a similar setting to ours, Bosch et al. (2010) find a significant degree of differential treatment between Moroccan immigrants and Spanish natives in the Spanish rental market. On average, an individual with Moroccan sounding name is around 15 percentage points less likely to receive a reply than an individual with a Spanish sounding name.

Traces of discrimination are systematically found in this literature in various settings, such as labour market or the housing rental market. They all have common features: They usually involve transactions that require a continuous interaction between the two agents. Such is the case of renting a flat or obtaining a job. For similar reasons, the stakes of the transactions are high, normally involving large amounts of money. This implies that small differences in priors on the ability to perform a particular transaction may give rise to large gaps in differential treatment between natives and non-natives.

Instead, we focus on a setting where the interaction between the two agents is minimal, since the online second hand market does not require continuous interaction between the buyer and the seller. Furthermore, the possibility of default is also negligible, since the postal services will not deliver the goods unless payment has been

satisfied. Finally, we can explore how the amount of the transaction influences the degree of differential treatment. Hence, we argue that in our setting the differential treatment that we find is purged of many of the confounding factors that are present in other studies. Further, as Antecol and Cobb-Clark (2008) argue, "discrimination in restaurants or shopping is as important as discrimination in employment or housing. The latter two activities are more crucial to a person's life chances, but it also seems clear that discrimination in everyday transactions imposes significant psychological costs on its victims and is a clear violation of civil rights laws". There is thus ample justification both for wanting to know more about it.

We highlight three main results in our paper. First, we find that the differential treatment between buyers with a native sounding name and buyers with a foreign sounding name is around 7.8 percentage points. Although this is not entirely comparable to previous results, because they analyze different markets, we point out that the percentage of discrimination is substantially lower than the ones of Bosch et al. (2010) and Ahmed and Hammarstedt (2008) obtained for the Spanish rental market. This suggests that, as expected, lower interactions between agents and lower stakes that characterize the second hand market reduces the differential treatment between natives and ethnic minorities.

Second, we distinguish between three different foreign nationalities in our study. We include British sounding names, Moroccan sounding names, and Latin American sounding names. By nationality, our point estimate of discrimination is slightly lower for British sounding names (6.3 percentage points) compared to Moroccan (8.5 percentage points) or Latin American (8.7 percentage points) sounding names. This suggests that the differential treatment found in this study cannot be fully ascribed to priors on the income differences across nationalities.

Finally, we allow our estimates on the differential treatment between natives and foreign sounding names to vary across the amount of the transactions. In our sample we have a significant range of prices, from 1 Euro of a toy of to 50,000 Euros of a sports car. We do not find significant evidence that price alters the degree of discrimination in our experiment. If anything, higher prices reduce the amount of discrimination.

In our view, this new evidence of the Spanish second hand market signals that the traces of discrimination are present even for very cheap goods in situations were the possibility of default is low. The fact that British sounding names are also discriminated against Spanish sounding names and the lack of a systematic relationship between price and differential treatment suggest that statistical theories of discrimination are not responsible for the patterns found here.

Our interpretation of the results is that the differential treatment observed in this experiment is due, to some extent, to the preference of the seller to limit interaction with foreign buyers.

The rest of the paper is structured as follows. Section 2 reviews the literature. Section 3 explains in detail our experimental design. Section 4 presents the main results of the paper. And section 5 concludes.

2. Evidence of discrimination in consumer markets

Economists have historically had a great deal to say about the causes and consequences of racial discrimination in employment. However, the impact of discrimination on other economic interactions as consumer markets remains relatively unstudied. Anecdotal evidence would suggest, however, that discriminatory treatment in everyday market transactions is a fact of life for many US consumers who find themselves being unable to obtain (or paying higher prices for) the goods and services they wish to purchase (Antecol and Cobb-Clark, 2008).

Yinger (1998) reviews a number of studies related to discrimination in consumer markets. Two main techniques to search for the existence of discrimination in various consumption markets have been used: regression analysis and audits. The regression methodology employs some consumption outcome, typically a price, as the dependent variable, and group membership indicators, along with relevant controls, as the explanatory variables. The test for discrimination is whether the coefficient for the relevant group membership variable is significant. The regression methodology, originally developed for labor markets, also has been applied to housing (Chambers, 1992; Wachter and Megbolugbe, 1992; Keil and Zabel, 1996) and car sales (Goldberg, 1996). As shown by Yinger (1998), the regression approach is subject to several potential biases; moreover it is indirect, as it attempts to isolate the impacts of

discrimination on prices without directly observing discriminatory behavior. In contrast, an audit can literally catch economic agents in the act of discriminating. It minimizes the differences in treatment caused by variables that can go unobserved by studies employing regression. Audit studies do not need to make assumptions about the form of the relationship between the independent and dependent variables. By an audit, one can match similar individuals, assign them characteristics that do not differ more than in race, ethnicity, or sex, that is relevant to their treatment in the market place.

At the same time, audit studies have a number of disadvantages. First, like other experimental methods, audit studies are often limited in their external validity. Consequently, an audit study does not provide evidence on discrimination in general, but rather informs us about discrimination within the specific context defined by the study's sampling frame. Because of this, audit studies are limited in distinguishing the broader circumstances—in particular the market conditions—in which discrimination in commercial transactions might occur (Yinger, 1998). Second, inferences from audit studies can be quite sensitive to alternative assumptions about the distribution of unobserved heterogeneity (Heckman, 1998). Finally, audit studies are not particularly useful in situations where the risk of discrimination per transaction is low, for example in shops or restaurants, or in measuring the effects of disparate impact discrimination (Siegleman, 1998).

The technique of conducting carefully controlled field experiments to measure discrimination in the market place is 35 years old (Riach and Rich, 2002). One usual way for audits is that in which people from two different groups are selected, trained, and assigned to two-group pairs such that teammates are equally qualified to buy a house or a car, etc. A sample of the agents whose behavior is being studied, landlords or car dealers, for example, is then drawn. Audit teammates successively visit each agent to inquire about an advertised housing unit or a type of vehicle. After the visit, each teammate independently records how he or she was treated. The problem with this process is that discriminatory treatment can bother auditors and so compromise their ability to fill out audit survey forms accurately.

Using emails for the audit can eliminate this problem. Bosch et al. (2010) find signals of discriminatory behavior in the housing rental market in Spain. Concretely,

they investigate discrimination against Moroccan immigrants by conducting an experiment consisting on emails. Fictitious applicants send email showing their interest in vacant rental apartments, and signing with names of Moroccan or Spanish origins. Moreover, the emails show different amounts of information on their ability to pay the rent (information related to socioeconomic status), in order to analyze if this information has any effect on the type of response. They find that applicants with a Moroccan name are 15 percentage points less likely to receive a response than those with a Spanish name. On the other hand, revealing positive information about the socioeconomic status of the Moroccan applicant increases the probability of being contacted by 9 percentage points, although this information does not completely eliminate discriminatory behavior.

Many studies have explored the magnitude and statistical significance of discrimination in housing in The United States. Most of them demonstrate that black and Hispanic home seekers encounter discrimination in many aspects of a housing transaction. They are told about fewer available units and must put forth considerably more effort to obtain information and to complete a transaction. These barriers are not absolute, but they impose significant costs on black and Hispanic home seekers relative to comparable whites in the form of higher search costs, poorer housing outcomes, or both (Yinger, 1998).

A few others studies have examined discrimination in car sales markets. All these studies yield similar results: audit studies show that women and blacks often face higher prices for new cars than do corresponding white men. Moreover, the differences were quite large (Yinger, 1998).

Yinger's study is referred in a report of The Urban Institute (1998), together with some other studies on discrimination on housing, employment, business, and "everyday" commercial transactions, as car buying, TV repair, hailing a taxi, or being served in a restaurant. Siegleman (1998) refers a test about public accommodations, in which 45% of blacks believed they had been discriminated against at least once in the past 30 days: 30% while shopping, 21% while dining out.

Field experiments suggest that women face longer wait times in coffee shops (Myers et al., 2010), and that minority buying agents receive worse initial offers for sports memorabilia (List, 2004), while fast-food prices have been shown to rise with the size of the black population in the corresponding neighborhood (Graddy, 1997).

Riach and Rich (2002) emphasize the significance of carefully-controlled field experiments as a research technique for economists to analyze economic discrimination. In a survey about field experiments of discrimination in the market place, they make reference to significant levels of discrimination against nonwhites and women in labor, housing and product markets in other countries. Rates of employment discrimination against non-whites, in excess of 25% have been measured in Australia, Europe and North America. A small number of experiments have also investigated employment discrimination against the disabled in Britain and the Netherlands, and against older applicants in the United States.

Antecol and Cobb-Clark (2008) analyze the relationship between the characteristics of local markets and the propensity for consumers to report racial and ethnic discrimination in their everyday commercial transactions. They carry out the study by using the Armed Forces Equal Opportunity Survey, that asks directly about off-base discrimination in patronizing local businesses, as well as in acquiring non-governmental housing. Beyond the analysis of the housing market, they also consider discrimination in other kinds of routine commercial transactions like shopping, eating in restaurants, banking, etc. On the other hand, they analyze the extent to which consumer market discrimination is related to the ethnic and racial composition, economic vulnerability, housing market, and social context in the local community. They find one in eight soldiers reporting that they or their families have experienced racial discrimination in finding non-government housing or in patronizing businesses in their local communities.

3. Experimental design

This experiment is based on an email correspondence testing method. We send written applications to second hand items sale ads on the Internet. Information about second hand items for sale is obtained from one of the most popular buy and sale sites

in Spain, *Segundamano.es*. These items are classified by provinces, and then by categories and subcategories. We focus our study in six provinces (Madrid, Barcelona, Valencia, Alicante, Castellón and Murcia), and four main categories: (1) House and garden: Housing, garden and agriculture, housing objects for children, fashion, jewellery, beauty and health articles. (2) Electronics: Computers and games, audio, video, photography, phone and others. (3) Hobbies and sports: Sports, pets and accessories, music, films, books, hobbies. (4) Motor vehicles: cars and accessories, motorcycles and accessories, quads, caravans and trailers, ships and nautical, industrial and agriculture vehicles.

On *Segundamano.com* owners can advertise their products at no cost. Similarly, individuals interested in a particular item can email the owner free of charge. The only information required is the name, email address and a short message.

The normal transaction would consist on an exchange of a couple of emails agreeing on the terms of the transaction followed by the sending of the item via postal service payable upon receipt in a post office. For larger and more expensive items such as cars, the buyer and seller would probably meet to inspect the product and arrange the terms of the transaction.

Our experimental design is aimed at answering three main questions: (a) Are inquiries sent by immigrants treated differently than those of natives? (b) Are applications sent by immigrants treated differently depending on their origin (British/Moroccan/Latin American)? (c) Does the differential treatment across there differential treatment depending on the price of the item?

In order to answer these questions, we sent emails to 2242 private sellers expressing interest on the advertised article. In our mails we ask for further information about the particular product and provide a contact and email to the seller (the first mail is sent via the web *Segundamano.es*, so the buyer does not know the seller email address). The experiment was conducted for four months between December 2009 and March 2010. During this period, our candidates applied to all ads on *Segundamano.es* for the provinces and categories indicated.

For each contact we recorded the following information: date of the sent mail, the geographical location of the product, the heading of the ad, the category and subcategory of the type of product (according to *Segundamano.com*) and the price. All ads were tracked during the experiment to avoid being contacted more than once. During the span of the experiment we recorded whether or not the seller replied back to the buyer, the date of the answer (if so), the text of the answer, and the kind of answer (positive or negative, that is, whether or not the seller was determined to send more details about the article, or to proceed with the transaction). Deals were after politely declined by the buyers. Hence our measure of differential treatment across origins will be the difference between rate for natives and that of foreign buyers.

In order to signal the origin of the applicant we use a common approach. In each email we sing with either Spanish or a foreign-sounding name. We alternate male and female names to check whether differential treatment varies with gender as others have found in different settings (see Bosch et al., 2010, and Ahmed and Hammarstedt, 2008).

Because we want to compare the differential treatment across different foreign origins, we choose names to proxy a variety of ethnic origins. We employ British, Moroccan and Latin American sounding surnames. These constitute three of the four most common immigrant origins in Spain (the fourth being Eastern Europeans). Further they allow us to compare our results with others that have been previously found in the literature (see Bosch et al., 2010). In order to choose the names we use name frequency data collected by the Spanish National Statistics Institute (INE) for Spanish, British, and Moroccan and Latin American people registered in Spain. For all these origins we randomly assign names to surnames within the 10 most common registered (see table A.1 for sample of names created). Then we create an email address for each of these fictitious applicants. The email accounts were created from three different free providers: *Gmail*, *Hotmail* and *Yahoo*.

Several facts merit attention in this strategy. First, for Spanish, Moroccan and British applicants we send the Spanish version of the following email:

"Hello,

I am interested in buying this article. I would be very grateful if you contacted me. Thank you.

NAME"

Although Latin American names and surnames are relatively similar to Spanish ones, they do not overlap and can be somehow relatively easy to identify. In any case, for Latin American applicants we send the same text, but using language constructions more typical of the Spanish spoken in Latin American countries and easily identifiable for native Spanish.

4. Results

Table A.2 presents the tabulations of our experiment. We show the response rates in our experiment for the different origin, male and females. Further, we divide our sample in four price quartiles to study discrimination patterns for different price ranges.

On average, the response rates are relatively high and very similar to those recorded in similar experiments, between 60 and 80%. Several results can be noted from table A.2. First, from this tabulation, it is already clear that native sounding names receive on average higher response rates. Spanish sounding names get an email back 72% of the times, while non-Spanish sounding names get a reply only 64% of the times. Second, we observe certain variation across nationality. Latin American and Moroccan sounding names show slightly lower response rates 63% than its Anglo-Saxon counterparts, 66%. Finally, it seems that there is not a clear discrimination pattern across different price ranges.

In order to present the results of our experiment more systematically we run a series of regressions. We estimate a model where the dependent variable, R_i, is an indicator that takes value 1 if buyer *i* has received a reply from the seller, and 0 otherwise. Our main explanatory variable is an indicator I_i, that takes value 1 if the buyer has signalled a foreign sounding name, and 0 otherwise. In order to estimate the effect of prices on discrimination we include the logarithm of the price of the item and

its interaction with I_i. We also include a set of dummy variables to capture the fixed effects of the category, D_c, and province, D_p.

$$R_i = a + \beta I_i + \chi P_i + \delta (P_i \times I_i) + D_p + D_c + u_i \qquad (1)$$

Tables A.3(a) and A.3(b) present the main results of our experiment. Column (1) shows our estimate of discrimination against non-Spanish. On average, emails signed with foreign sounding names are responded 7.8 percentage points less than those signed with typical Spanish names. Interestingly, our estimate is substantially lower than the 15 to 20 percentage points typically found in other studies using the rental market as a setting using the same experimental design. This suggests that in an environment with less interaction and a low probability of default natives tend to discriminate less.

In column (2) we introduce the logarithm of the price of the good and its interaction with the origin indicator. We do not observe any significant relationship between the price of the good and the level of discrimination. If anything, the point estimate of the interaction tends to suggest that differential treatment tends to be lower in transaction with higher prices. Note that there is a slightly drop in the number of observations from 2242 to 2148. This is due to the fact that price is missing for those observations

Column (3) further studies the relationship between price and discrimination. We divide the 2148 observations in four price quartiles, 1-60, 60-175, 175-800 and more than 800 Euros. Instead of the price variable we use a dummy variable for each quartile and the interaction with the origin dummy. Table A.3(b) shows the tests for the significance of discrimination by price quartiles. Interesting insights emerge here. Discrimination against foreign sounding names in the lower quartile is 10.55 percentage points, very similar to that of the second quartile. In both cases the estimates are significantly difference from 0. For those goods between 175 and 800 Euros the discrimination is slightly lower, 7 percentage points, although it is not significantly different from 0. In the upper quartile, for goods over 800 Euros, we do not observe any discriminatory behaviour.

Columns (4) to (10) of tables A.3(a) and A.3(b) present the same analysis for men and women separately. A couple of facts merit attention: our point estimate indicates that men tend to be slightly more discriminated than women, 9 percentage points vs 6 percentage points. This is consistent with other studies which have found that in this type of experiments men tend to be faced more discrimination than women (Bosch et al., 2010; Ahmed and Hammarstedt; 2008). The difference between men and women is particularly significant for the lower quartiles of the price distribution. In particular, for the lowest quartile, the foreign sounding male names receive 17 percentage points less responses, vs 5 percentage points of women, compared to their respective native counterparts. Interestingly, men are substantially more discriminated in cheaper products than in more expensive products, whereas we do not find any significant patterns for women.

Two alternative margins of discrimination can be explored in this experiment. First, for the same number of contacts, immigrants may receive more negative replies than natives. Second, immigrants may act as a "costumer" of last resort and only contacted if native consumers do not respond. We run a regression with and indicator variable for positive/negative replies only for those emails that were responded. Further, we create a variable that indicates the number of days elapsed between the sending of the email and the reply. We do not find any trances of such margins (results available upon request).

Tables A.4(a) and A.4(b) replicate the results of tables A.3(a) and A.3(b) by nationalities. British, Latin American sounding and Moroccan sounding names all compared to their Spanish counterparts. Two facts are relevant from these tables. First, in correspondence to table A.2, discrimination seems to be slightly lower for Anglo-Saxon sounding names, 6 percentage points than for Latin American and Moroccan names, 8.5 percentage points. Second, regardless of the nationality we find the same price patterns suggesting more discrimination in cheaper goods.

Tables A.5(a) and A.5(b) analyze discrimination between Spanish and non Spanish by item category. The category "Electronics" presents the highest discrimination against immigrants: people with non Spanish sounding names receive an answer in a 14% less than people with Spanish names. This difference is 9 points in

"Vehicles", and nearly 8 in "Hobbies and sports". In "House and garden" the difference seems not to be significantly different from 0, however. However, table A.5(b) reveals that there are substantial differences across categories. While in "Vehicles" and "Electronics" discrimination appears more intensively on cheaper goods, in "House and Garden" there is positive discrimination in favour of immigrants for more expensive goods. This is entirely due to the behaviour of towards British immigrants.

Tables A.6(a) and A.6(b) show our results at the regional level[1]. Results for Madrid, Barcelona and Murcia are relatively similar with discrimination levels ranging between 8 and 14 percentage points. However, in the Comunitat Valenciana we do not find any traces of discrimination. There is not a clear explanation for this phenomenon. However, if discrimination is somehow related to the share of immigrants in a particular region, different composition in the immigration patters in the Comunitat Valenciana could be a possible explanation for these results (see table A.6(c))

5. Discussion and conclusions

Our experiment has unveiled three main facts in the Spanish second hand consumer market. First, there are clear trances of discrimination against foreign sounding names. Second, discrimination seems to be more acute at lower prices. Third, that differential treatment is slightly higher for Latin American and Moroccan immigrants, but not substantially more than for British names.

How do these results resonate in the discrimination literature? One common thread in this literature is how much of the discrimination is due to pure dislike of the ethnicity/nationality (taste-based discrimination) and how much is due to the fact that agents use the ethnicity/nationality to infer other characteristics that are important for the transaction (statistical discrimination)[2]. Although the results of our experiment do not provide a definite answer for this, they do provide some insights about the sources of discrimination.

[1] Percentages of immigrant population by region are shown in table A.6(c).
[2] See Aigner and Cain (1977)

One of main results of this experiment is that foreign sounding names are discriminated even in the most basic economic transactions. Even in products below 60 Euros, where there is very little possibility of default and the interaction between the buyer and the seller is minimal, we still find very significant traces of differential treatment against foreign sounding names. Further, although British sounding names report slightly higher response rates than either Moroccan or Latin American sounding names, they still face substantial differential treatment. It is difficult to argue that British sounding names will be discriminated, because the soundness of the name signals either the inability to pay, or a bad characteristic that inhibits the person to perform the transaction. Our reading of the results is that the differential treatment observed in this experiment is due to some extend to taste or dislike of the seller to minimally interact with foreign buyers.

In a comparable experiment, Bosch et al. (2010) show that in the Spanish Rental Market Moroccan sounding names are contacted 15 percentage points less than Spanish names in the rental market. Once positive information about the candidate is provided, discrimination falls to 10 percentage points. In our setting, where the economic characteristics of the buyers should not matter too much (at least for cheap transactions), we find for Moroccan a differential treatment of 8.5 percentage points. Again, this suggests that this residual differential treatment is hardly due to statistical discrimination.

Table A.1 Sample of names used for the fictitious applicants

NAME	GENDER	ORIGIN
Antonio Rodríguez	M	Spanish
Carmen Gómez	W	Spanish
Mohamed Saidi	M	Moroccan
Samira El Moussaoui	W	Moroccan
Nelson Mendes	M	Latin
Nelly Valdez	W	Latin
John Evans	M	British
Susan Brown	W	British

Table A.2 Response rates by origin, gender, and price range

Origin	All	Men	Women	0<p≤60	60<p≤175	175<p≤800	p>800
Spanish	72.19	71.17	73.21	78.87	75.00	72.87	63.58
Non Spanish	64.49	62.26	66.71	68.47	65.42	65.07	59.56
British	66.07	63.57	68.57	70.90	67.41	64.58	61.90
Latin	63.81	62.14	65.48	69.66	63.57	66.40	56.29
Moroccan	63.57	61.07	66.07	64.57	65.35	64.43	60.51
TOTAL	66.41	64.50	68.33	71.17	67.74	66.91	60.62
N	2242	1121	1121	548	530	530	617

Table A.3(a) Discrimination and price

Dep. var: Answer	All			Men			Women		
Non Spanish	-0.0783***	-0.1345**	-0.1055**	-0.0907***	-0.2336***	-0.1749***	-0.0663**	-0.0127	-0.0468
	(0.0223)	(0.0634)	(0.0452)	(0.0319)	(0.0818)	(0.0665)	(0.0311)	(0.0991)	(0.0621)
log p		-0.0161			-0.0286*			-0.0010	
		(0.0111)			(0.0156)			(0.0161)	
(log) p x Non Spanish		0.0109			0.0279*			-0.0094	
		(0.0118)			(0.0165)			(0.0170)	
60<p≤175			-0.0188			-0.0090			-0.0311
			(0.0626)			(0.0984)			(0.0815)
175<p≤800			-0.0650			-0.1520			0.0098
			(0.0629)			(0.0945)			(0.0840)
p>800			-0.1197*			-0.2389**			-0.0097
			(0.0659)			(0.0978)			(0.0869)
60<p≤175 x Non Spanish			0.0004			-0.0057			0.0067
			(0.0702)			(0.1092)			(0.0919)
175<p≤800 x Non Spanish			0.0319			0.1063			-0.0360
			(0.0676)			(0.0931)			(0.0995)
p>800 x Non Spanish			0.0774			0.1841**			-0.0444
			(0.0623)			(0.0799)			(0.0959)
N	2242	2148	2148	1121	1069	1069	1121	1079	1079

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table A.3(b) Test by price range for All, Men and Women

	All	Men	Women
p≤60			
Coef.	**-0.1055**	**-0.1749**	**-0.0468**
chi2(1)	4.96	5.85	0.55
prob>chi2	0.0259	0.0156	0.4598
60<p≤175			
Coef.	**-0.1051**	**-0.1806**	**-0.0401**
chi2(1)	4.79	6.32	0.38
prob>chi2	0.0287	0.0119	0.5371
175<p≤800			
Coef.	**-0.0736**	**-0.0686**	**-0.0828**
chi2(1)	2.51	1.28	1.34
prob>chi2	0.1132	0.2571	0.2477
p>800			
Coef.	**-0.0281**	**0.0092**	**-0.0912**
chi2(1)	0.42	0.07	1.88
prob>chi2	0.5185	0.2571	0.1701

Table A.4(a) Discrimination by origin

Dep. var: Answer	Spanish – British			Spanish – Latin			Spanish - Moroccan		
Non Spanish	-0.0626**	-0.1151	-0.0839	-0.0851***	-0.1435*	-0.0949*	-0.0875***	-0.1525*	-0.1375**
	(0.0276)	(0.0854)	(0.0576)	(0.0279)	(0.0831)	(0.0572)	(0.0279)	(0.0836)	(0.0582)
log p		-0.0174			-0.0222*			-0.0195	
		(0.0116)			(0.0119)			(0.0120)	
(log) p - Non Spanish		0.0093			0.0106			0.0114	
		(0.0144)			(0.0141)			(0.0141)	
60<p≤175			-0.0316			-0.0207			-0.0110
			(0.0613)			(0.0616)			(0.0615)
175<p≤800			-0.0653			-0.0686			-0.0618
			(0.0612)			(0.0622)			(0.0621)
p>800			-0.1286*			-0.1680**			-0.1283*
			(0.0689)			(0.0708)			(0.0703)
60<p≤175 - Non Spanish			0.0007			-0.0297			0.0215
			(0.0820)			(0.0841)			(0.0822)
175<p≤800 - Non Spanish			0.0053			0.0252			0.0538
			(0.0804)			(0.0802)			(0.0770)
p>800 - Non Spanish			0.0718			0.0378			0.1055
			(0.0726)			(0.0761)			(0.0696)
N	1075	1073	1073	1122	1080	1080	1121	1075	1075

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table A.4(b) Test by origin

	Spanish-British	Spanish-Latin	Spanish-Moroccan
p≤60			
Coef.	**-0.0839**	**-0.0949**	**-0.1375**
chi2(1)	2.11	2.72	5.46
prob>chi2	0.1468	0.099	0.0194
60<p≤175			
Coef.	**-0.0832**	**-0.1246**	**-0.116**
chi2(1)	2.06	4.6	3.8
prob>chi2	0.1514	0.0319	0.0512
175<p≤800			
Coef.	**-0.0786**	**-0.0697**	**-0.0837**
chi2(1)	1.95	1.39	2.11
prob>chi2	0.1622	0.238	0.1462
p>800			
Coef.	**-0.0121**	**-0.0571**	**-0.032**
chi2(1)	0.02	1.09	0.21
prob>chi2	0.8769	0.3036	0.6474

Table A.5(a) Discrimination by category

Dep. Var.: Answer	House and garden			Electronics			Hobbies and sports			Vehicles		
Non Spanish	0.0139	-0.2545**	-0.0044	-0.1472***	0.0358	-0.1327	-0.0786*	-0.1465	-0.1478**	-0.0972**	-0.3031*	-0.2030*
	(0.0455)	(0.1241)	(0.1032)	(0.0436)	(0.2318)	(0.0884)	(0.0416)	(0.1332)	(0.0746)	(0.0468)	(0.1702)	(0.1041)
log p		-0.0676**			0.0302			-0.0288			-0.0228	
		(0.0311)			(0.0422)			(0.0260)			(0.0237)	
(log) p - Non Spanish		0.0656*			-0.0411			0.0169			0.0290	
		(0.0352)			(0.0479)			(0.0308)			(0.0267)	
$k_1 < p \leq k_2$ (2)			0.0865			-0.1634			-0.1123			-0.2245
			(0.1093)			(0.1230)			(0.1316)			(0.1408)
$k_2 < p \leq k_3$ (3)			-0.1344			0.0188			-0.1085			-0.1863
			(0.1240)			(0.1278)			(0.1258)			(0.1345)
$p > k_3$ (4)			-0.2582*			0.0046			-0.1583			-0.1447
			(0.1507)			(0.1248)			(0.1282)			(0.1397)
$k_1 < p \leq k_2$ - Non Spanish			-0.1439			0.0849			0.1251			0.0827
			(0.1523)			(0.1255)			(0.1132)			(0.1486)
$k_2 < p \leq k_3$ - Non Spanish			0.0469			-0.1413			0.0598			0.1334
			(0.1244)			(0.1535)			(0.1206)			(0.1361)
$p > k_3$ - Non Spanish			0.2130**			-0.0506			0.1381			0.1639
			(0.1001)			(0.1456)			(0.1038)			(0.1359)
N	560	546	546	561	547	547	560	499	499	561	556	556

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

(2) House and garden: $45 < p \leq 117.5$ Electronics: $55 < p \leq 120$ Hobbies and sports: $50 < p \leq 150$ Vehicles: $470 < p \leq 3972.5$

(3) House and garden: $117.5 < p \leq 300$ Electronics: $120 < p \leq 210$ Hobbies and sports: $150 < p \leq 400$ Vehicles: $3972.5 < p \leq 9000$

(4) House and garden: $p > 300$ Electronics: $p > 210$ Hobbies and sports: $p > 400$ Vehicles: $p > 9000$

Table A.5(b) Test by category

	House and garden	Electronics	Hobbits and sports	Vehicles
$p \leq k1$ (1)				
Coef.	**-0.0044**	**-0.1327**	**-0.1478**	**-0.203**
chi2(1)	0	2.02	3.17	3.25
prob>chi2	0.9657	0.1551	0.0751	0.0713
$k_1 < p \leq k_2$ (2)				
Coef.	**-0.1483**	**-0.0478**	**-0.0227**	**-0.1203**
chi2(1)	2.46	2.02	0.06	1.68
prob>chi2	0.1167	0.1551	0.8035	0.1951
$k_2 < p \leq k_3$ (3)				
Coef.	**0.0425**	**-0.274**	**-0.088**	**-0.0696**
chi2(1)	0.3	6.57	1.07	0.69
prob>chi2	0.5825	0.0104	0.3009	0.4056
$p > k_3$ (4)				
Coef.	**0.2086**	**-0.1833**	**-0.0097**	**-0.0391**
chi2(1)	5.23	3.37	0.01	0.19
prob>chi2	0.0222	0.0664	0.9354	0.6668

(1) House and garden: $p \leq 45$	Electronics: $p \leq 55$	Hobbits and sports: $p \leq 50$	Vehicles: $p \leq 470$
(2) House and garden: $45<p\leq117.5$	Electronics:$55<p\leq120$	Hobbies and sports: $50<p\leq150$	Vehicles: $470<p\leq3972.5$
(3) House and garden: $117.5<p\leq300$	Electronics:$120<p\leq210$	Hobbies and sports: $150<p\leq400$	Vehicles: $3972.5<p\leq9000$
(4) House and garden: $p>300$	Electronics: $p>210$	Hobbies and sports: $p>400$	Vehicles: $p>9000$

Table A.6(a) Discrimination by region

Dep. Var.: Answer	Com. Valenciana			Madrid			Barcelona			Murcia		
Non Spanish	-0.0181	-0.0755	-0.0698	-0.0834**	-0.1511	-0.0556	-0.0887**	0.0019	-0.1181	-0.1248***	-0.2633**	-0.2486***
	(0.046)	(0.136)	(0.090)	(0.041)	(0.105)	(0.082)	(0.045)	(0.147)	(0.096)	(0.046)	(0.120)	(0.086)
log p		-0.0196			-0.0206			0.0262			-0.0393	
		(0.022)			(0.020)			(0.024)			(0.024)	
(log) p - Non Spanish		0.0071			0.0144			-0.0148			0.0315	
		(0.023)			(0.021)			(0.024)			(0.026)	
$k_1 < p \leq k_2$ (1)			0.0680			-0.0326			0.1383			0.1315
			(0.113)			(0.135)			(0.129)			(0.130)
$k_2 < p \leq k_3$ (2)			-0.0594			-0.1521			0.0573			0.2223*
			(0.129)			(0.143)			(0.135)			(0.116)
$p > k_3$ (3)			-0.1186			0.0652			-0.0406			0.2323**
			(0.125)			(0.112)			(0.143)			(0.113)
$k_1 < p \leq k_2$ - Non Spanish			-0.0574			-0.0153			-0.1504			-0.1614
			(0.140)			(0.115)			(0.137)			(0.134)
$k_2 < p \leq k_3$ - Non Spanish			0.1277			0.0028			-0.0891			-0.2334
			(0.126)			(0.112)			(0.129)			(0.143)
$p > k_3$ - Non Spanish			0.0596			-0.0927			0.0631			-0.3068**
			(0.122)			(0.117)			(0.133)			(0.148)
N	577	573	573	576	558	558	577	545	545	512	472	472

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

(2) Comunitat Valenciana.: 69<p≤190 Barcelona: 50<p≤150 Madrid: 60<p≤175 Murcia: 82.5<p≤200

(3) Comunitat Valenciana: 190<p≤950 Barcelona: 150<p≤500 Madrid: 60<p≤175 Murcia: 200<p≤750

(4) Comunitat Valenciana: p>950 Barcelona: p>500 Madrid: p>950 Murcia: p>750

Table A.6(b) Test by region

	Com. Val.	Barcelona	Madrid	Murcia
$p \leq k1$ (1)				
Coef.	**-0.0698**	**-0.0556**	**-0.1181**	**-0.2486**
chi2(1)	0.57	0.44	1.38	
prob>chi2	0.4511	0.5082	0.2407	6.31
				0.012
$k_1 < p \leq k_2$ (2)				
Coef.	**-0.1272**	**-0.0709**	**-0.2685**	**-0.41**
chi2(1)	1.77	0.85	0.05	1.96
prob>chi2	0.1837	0.3552	0.8159	0.1616
$k_2 < p \leq k_3$ (3)				
Coef.	**0.0579**	**-0.0528**	**-0.2072**	**-0.482**
chi2(1)	0.36	4.5	0.46	0.05
prob>chi2	0.5464	0.034	0.4985	0.8236
$p > k_3$ (4)				
Coef.	**-0.0102**	**-0.1483**	**-0.055**	**-0.5554**
chi2(1)	0.01	0.02	3.07	0.02
prob>chi2	0.9055	0.8995	0.0796	0.9008

(1) Comunitat Valenciana: $p \leq 69$	Barcelona: $p \leq 50$	Madrid: $p \leq 50$	Murcia: $p \leq 470$
(2) Comunitat Valenciana.: $69<p\leq190$	Barcelona: $50<p\leq150$	Madrid: $60<p\leq175$	Murcia: $82.5<p\leq200$
(3) Comunitat Valenciana: $190<p\leq950$	Barcelona: $150<p\leq500$	Madrid: $60<p\leq175$	Murcia: $200<p\leq750$
(4) Comunitat Valenciana: $p>950$	Barcelona: $p>500$	Madrid: $p>950$	Murcia: $p>750$

Table A.6(c) Percentages of immigrant population by region

	United Kingdom	Moroccan	Latin	Total
Cataluña	0.21	2.15	5.39	7.75
Comunitat Valenciana	1.95	1.10	3.94	7.00
Comunidad de Madrid	0.13	1.13	7.54	8.80
Región de Murcia	0.99	3.12	5.31	9.42

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