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2015

Disability Discrimination in the Rental Housing Market – A Field Experiment on Blind Tenants, in Italy*

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Abstract. In this study, I show that with the appropriate experimental strategy, a correspondence test can be adapted to investigate disability discrimination in the rental housing market. I focus on discrimination against blind tenants assisted by guide dogs in Italy and obtain very robust results. The utilization of three fictitious household tenants (that is, a married couple, a married couple with a blind wife who owns a guide dog, and a married couple where the wife is normal sighted and owns a pet dog) allows me to investigate whether discrimination is due to the blindness or to the guide dog. I find that apartment owners discriminate blind tenants because of the presence of the guide dog alone. According to the Italian law, this is indirect discrimination, which in the US corresponds to the refusal to provide reasonable accommodation.

JEL Classification: C93; I12; J14; R21
Keywords: Disability; Discrimination; Housing Market; Field Experiment

* Opinions and views expressed in this paper are those of the author and do not reflect in any way those of STATEC.
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Acknowledgements: I would like to thank a number of scholars: my supervisors, Dan Olof Rooth and Magnus Carlsson, for guidance and many useful suggestions; Judith Rich, Simone Scarpa, Giangiacomo Bravo, Guido Ortona, Stijn Baert, Andreas Kotsadam, Cesare Riillo and Francesco Sarracino for providing helpful feedback; the participants of the seminars series of the Centre for Labour Market and Discrimination Studies, at the Linnaeus University and PhD fellows from the Department of Economics and Statistics at the Linnaeus University; the participants of the Annual Ph.D. Conference in Economic Demography, Lund University; the participants of the 8th RGS Doctoral Conference in Economics, Germany; Piermassimo Pavese for the provision of some administrative data; Stefania Ottone, Ferruccio Ponzano and Matteo Migheli for feedbacks on ethical aspects of field experiments.

A later version of the paper has been accepted for publication in Land Economics, Volume 93.

1 Introduction

Disabled people are protected against discrimination. Art. 21 of the Charter of Fundamental Rights of the European Union, 2000, states that any discrimination based on disability and other grounds shall be prohibited.[1] However, a number of economic studies provide evidence that they are discriminated against in the labor market (Jones, 2008, for a literature review). Unfortunately, economic scholars have neglected to study disabled people discrimination also in the housing market; however, disability discrimination also in this market deserves attention: a dwelling is a primary necessity that determines social inclusion, job opportunities, and enjoyment of public services.

This study proposes a new application of a standard field experiment to investigate disability discrimination in the rental housing market. The experiment focuses on people affected by blindness, who are more than 2.7 million in Europe alone (Pascolini & Mariotti, 2012).

What field experiment is suitable to analyze discrimination against blind tenants? In the US, where disabled people are protected from discrimination in the housing market through the Reasonable Accommodation Under the Fair Housing Act, 2004, the Urban Institute (Turner et al., 2005) suggests to use the "in-person audit test." Two actors, playing the role of tenants enquiring about housing units, are matched over all characteristics except for one (one actor is blind and the other is not) or two (one actor is blind and owns a guide dog; the other actor neither is blind nor owns a dog). The disabled actor visits a number of housing agencies to inquire about available housing units; if he owns an assistance dog, he also requests a waiver of restrictions on, and fees

[1] There also are EU directives to which individual member countries should align their legislations. Directive 2000/78/CE aims at combating employment discrimination also on grounds of disability. Directive Proposal COM, 2008, 426, aims at protecting disabled people even beyond the employment market; however, because this directive is still a proposal, single countries' legislations remain heterogeneous on this matter.

related to, the dog. The non-disabled actor who owns no dog visits the same housing agencies and inquires about housing units. There is evidence of discrimination against disabled tenants when the percentage of housing units made available for the disabled actor is statistically significantly lower than that made available for the non-disabled actor. There is evidence of discrimination also when the blind applicant receives a refusal to accommodate his guide dog in the housing agency premises (Turner et al., 2005);[2] for this illegal behavior, no statistical evidence is needed. This test can be carried out via phone in a similar manner (Heylen & Van den Broeck, 2015).

Also American fair housing organizations and other nonprofits investigate this topic using in-person audit tests.[3] These studies can be divided into two groups. In one group, disabled actors are matched to non-disabled counterparts (Fair Housing Center of Washington, 2015; Fair Housing Center of Central Indiana, 2013; Fair Housing Project of Champlain Valley Office of Economic Opportunity, 2003), as suggested by the Urban Institute. In the other group, disabled actors are unmatched (Murphy, 2007; Housing Opportunities Made Equal of Virginia, 2008). The purpose of the latter audits is to investigate whether housing agents satisfy the request of reasonable accommodation, or housing unit modification, from disabled tenants.

Results from a pilot experiment conducted by the Urban Institute suggest that blind tenants could suffer discrimination, with a lower percentage of housing units made available to them (Turner et al., 2005). Additionally, audit reports from other American institutes show that blind tenants frequently receive refusals to waive restrictions and/or fees on their guide dogs (Fair

[2] The Americans with Disabilities Act (ADA), 1990, states that privately owned businesses, such as housing agencies, restaurants, retail stores, and taxicabs, should allow people with disabilities to enter the business premises with their service animals.

[3] A list of these institutes is provided in Turner et al. (2005). Names of additional institutes might be found on the website of the US Department of Housing and Urban Development, at http://portal.hud.gov/hudportal/HUD?src=/program_offices/fair_housing_equal_opp/partners/FHIP. This web page provides a list of those institutes that are granted funding by the Fair Housing Initiatives Program, from 2008 up to date, and use them to investigate discrimination in the housing market, not only through field experiments and not only against disable people.

Housing Center of Washington, 2015; Fair Housing Center of Central Indiana, 2013; Housing Opportunities Made Equal, 2008, Murphy, 2007).

There are at least three concerns with in-person audit tests. First, in general, in-person audit tests may produce biased results if some actors' characteristics are not appropriately matched (Heckman, 1998; Heckman & Siegelman, 1993; Pager, 2007; Riach & Rich, 2002). These characteristics are observed by the landlords but not by the researchers, and within the social sciences, they are called *unobservable characteristics*. Pager (2007) and Riach and Rich (2002) suggest that such characteristics might be reflected in subtle differences in the way applicant tenants interact with housing brokers. Furthermore, within studies on blind tenants with a guide dog, not all dog's features might be accounted for by the researchers and thus represent additional unobservable characteristics.[4] Taken together, these unobservable characteristics might convey systematically different information on the matched applicants, drive the differential treatment and thus cause biased estimates of discrimination (Pager, 2007). Second, a serious general threat to the validity of in-person audit tests is the *experimenter effect* (Pager, 2007). Actors might be (sub)consciously motivated (not) to obtain evidence of discrimination and consequently adjust their behavior during their interactions with brokers (Pager, 2007). These two concerns apply to in-person audit tests implemented over the phone as well. Third, available evidence on disability discrimination in the rental housing market usually comes from audit reports of fair housing organizations and other nonprofits, which typically are not designed to perform statistical inference (Turner et al., 2005).

In alternative, researchers may use a written field experiment, also known as *correspondence test*. This method has established itself in recent years as the field experiment that conveys

[4] For instance, conditions of the dog's hair, the dog's smell and behavior during interactions with brokers.

the clearest estimates for discrimination in the rental housing market.[5] Since no actor is involved, the use of correspondence tests allows for complete control over the applications (Pager, 2007), thus reducing the bias caused by unobservable characteristics and by the experimenter effect. Moreover, written applications are delivered via the Internet, which provides three advantages: i) for the same budget, researchers can contact a larger amount of agents compared to those they could contact with in-person audit tests; ii) also apartment owners can be targeted, these are people that rent out their own apartments, without having a specialized background like housing brokers do; iii) the cost of the experiment is reduced since no actor is involved.

However, the implementation of correspondence tests presents an obstacle: the *disclosure problem* (Ahmed & Hammarstedt, 2008). The target characteristic (for instance, gender, ethnicity or blindness) has to be clearly disclosed in a way that does not seem unnatural (Pager, 2007). Correspondence tests in the labor market that analyze gender and ethnic discrimination in hiring use applicants' names as natural and clear disclosure devices (for the US, Bertrand & Mullainathan, 2004; for Sweden, Carlsson & Rooth, 2007). How could also blindness be naturally and clearly disclosed, since blind applicants' names cannot be used as disclosure devices and these applicants would unlikely reveal their blindness directly in written applications?

This is the first study to propose to solve the disclosure problem by mentioning the presence of a guide dog in the household of the disabled applicant. This disclosure device should work for two reasons: i) the guide dog is one of the internationally well recognized symbols of

[5] Because of the clean results, this methodology is used to investigate also other types of discrimination in the rental housing market, namely, discrimination based on ethnicity (for Italy, Baldini & Federici, 2011; for Sweden, Carlsson & Eriksson, 2013, and Ahmed, Andersson & Hammarstedt, 2010; for the US, Hanson & Hawley, 2011, and Carpusor & Loges, 2006; for Spain, Bosch, Carnero & Farré, 2010), gender (for Sweden, Ahmed & Hammarstedt, 2008), sexual orientation (for Sweden, Ahmed & Hammarstedt, 2009, and Ahmed, Andersson & Hammarstedt, 2008), age and employment status (for Sweden, Carlsson & Eriksson, 2013), single-parenting (for Canada, Lauster & Easterbrook, 2011), socioeconomic class (for Norway, Andersoon, Jakobsson & Kotsadam, 2012).

blind persons; ii) dogs are often seen as family members or friends by their owners,[6] so it should seem natural to mention their presence in written applications.

Once the disclosure problem is overcome, I proceed with the implementation of the correspondence test. I prepare applications for three types of fictitious applicants: i) married tenants, which represent the control group;[7] ii) married tenants where the wife is blind and owns a guide dog, which represent the treatment group;[8] iii) married tenants where the wife is normal sighted and owns a pet dog, which is a second control group and represents an additional original methodological element of this study. Then, I send these applications in response to online housing advertisements in Italy.

The utilization of the second control group belongs to a strategy that aims at disentangling two aspects of disability discrimination through a sort of difference in difference. Based on a reasonable assumption to be discussed later in the paper, the second control group allows me to investigate whether blind tenants assisted by a guide dog are discriminated against specifically because of their disability or because of the presence of the assistance dog. Therefore, the utilization of two control groups potentially allows me to extend the results to blind tenants with no guide dog; also, it allows me to obtain results that could concern disabled tenants with other assistance dogs as well.

This study also specifically suggest to analyze two separate subsamples of advertisers, namely, housing brokers and apartment owners, because of their expected different knowledge of

[6] For Italy, this is confirmed by Eurispes (2013) and for the US by the American Veterinary Medical Association (2012).
[7] The usage of households rather than individuals is a standard strategy adopted also in studies on sexual orientation discrimination in the rental housing market (for example, for Sweden, Ahmed & Hammarstedt, 2009), where the tenants' sexual orientation is disclosed via a brief description of the household.
[8] I have not used married tenants where the husband is blind because of the greater risk for these households of being discriminated based on their expected lower income. This risk is reduced by using a blind wife since women's employment rates and wages are on average lower than men's in most OECD countries, Italy included. Source: OECD Employment and Labour Market Statistics.

the legislation and risk of exposure in case of discriminatory behaviors. Housing brokers are professional intermediary agents who advertise and rent out apartments that belong to someone else.[9] Apartment owners advertise and rent out their own apartments on their own. Housing brokers have a brokerage license and thus are more likely to know in detail the legislation. Moreover, they conduct more frequently rental housing transactions, which increase the risk of being taken to court in case of misconduct. All things considered, a legitimate expectation is that housing brokers discriminate against disabled tenants less often than apartment owners.

Comparisons of the frequencies at which different tenants' groups are invited to visit apartments provide a number of interesting results. Although the Italian legislation protects disabled people from discrimination (law no. 67, 2006),[10] advertisers discriminate: on average married tenants with a blind wife assisted by a guide dog are less likely to receive an invitation to visit the apartment, compared to married tenants. Moreover, the results from the two separate subsamples of advertisers provide statistically significant evidence that this discrimination is driven by apartment owners' behavior and also suggest that it is owed to the presence of the guide dog alone, not to the disability status. This is the first correspondence test to provide evidence of this special type of illegal disability discrimination. Finally, as a byproduct of the experimental strategy, there is statistically significant evidence of discriminatory (but legal in Italy) behavior against non-disabled tenants due to pet ownership.

Virtually in the same period when this experiment was conducted (that is, the first half of 2013), Heylen and Van den Broeck (2015) conducted an experiment in the Belgian rental housing

[9] They receive commissions for their service. For instance, after a housing broker has found a new tenant for a housing unit, the new tenant could be asked to pay the equivalent of one month rent to the broker.
[10] According to this law, disabled people cannot be discriminated against based on their impairment (that is, blindness) or, more indirectly, on factors related to it (that is, the presence of the guide dog). This law embodies the principles suggested in Directive Proposal COM, 2008, 426.

market, which also focuses on blind people and uses a correspondence test.[11,12] However, this study and mine were developed and conducted completely independently; moreover, besides the similar disability being considered and the utilization of the correspondence test, these two studies present five important differences. First, Heylen and Van den Broeck (2015) use a different strategy to solve the disclosure problem: fictitious disabled applicants (without assistance dog) ask landlords to describe the dwelling in detail and. Second, they use the matched application technique (that is, they send one enquiry per group of applicants to each advertisement), whereas I use the random assignment technique (that is, I send a single enquiry, to which the applicant's group is randomly assigned to each advertisement). Later in the paper, I argue why my approach is more suitable when scholars implement correspondence tests in the housing market and compare more than two tenants' groups. Third, I use two control groups instead of one and try to isolate different types of disability discrimination. Fourth, they compare individual tenants, whereas my experiment approaches disability discrimination from a household perspective. Fifth, they focus on male disable tenants, whereas I focus on female disabled tenants. Therefore, these two studies are no directly comparable.

The remainder of the paper proceeds as follows. Section 2 discusses experiment design, descriptive results and limitations of the study. Section 3 describes the model and the results. Section 4 concludes.

[11] A third study, from Verhaeghe, Van de Bracht and Van de Putte (2016), presents similar experimental characteristics to those proposed in these two studies, but their experiment started one year later, in 2014.
[12] In their study, Heylen and Van den Broeck (2015) use also an in-person audit test via phone; additionally, they study gender and ethnic discrimination as well as disadvantageous treatment based on financial means.

2 Experiment Design and Descriptive Results

2.1 Experiment Design

I conducted a correspondence test in the Italian rental housing market from April 12 to June 22, 2013, sending 1,000 fictitious written applications in response to advertisements on the Italian classified website *Subito.it*. The usage of the Internet to find a house is an increasingly popular solution among Italians: according to a survey performed by Nielsen, six million people used the Internet to find an apartment in 2013, in Italy.[13]

A random assignment procedure was used to assign to each advertisement only one application randomized over the applicant's group. This procedure is utilized in a number of published researches on discrimination in the housing market (Carlsson & Eriksson, 2014; Baldini & Federici, 2011; Ahmed, Andersson & Hammarstedt, 2010; Carpusor & Loges, 2006; Lauster & Easterbrook, 2011; Andersson, Jakobsson & Kotsadam, 2012). There are three reasons for using this technique in lieu of the matched applications technique. First, since only one fictitious application per advertisement is sent, the risk of being exposed is reduced. This risk is especially high for correspondence tests in the housing market because it is difficult to accurately keep track of advertisers' identity. As Ahmed and Hammarstedt (2008) explain, advertisers may post multiple housing vacancies under different names. Many could be the reasons for this behavior; in particular, apartment owners might not want to indirectly and publicly disclose their wealth on the Internet. Furthermore, housing brokers may post multiple vacancies under the name of their real estate agency. Therefore, advertisers could be accidentally contacted multiple times, could realize they

[13] See http://www.nielsen.com/it/it/insights/news/2014/cercare-casa-online-e-piu-semplice-e-veloce.html. This survey suggests also that Subito.it is one of the top Italian websites to search for apartments; see http://www.nielsen.com/it/it/insights/news/2014/cercare-casa-online-e-piu-semplice-e-veloce.html. This website is the same that Baldini and Federici (2011) use in their study on ethnic discrimination in the Italian rental housing market

are being deceived, and respond altering their behavior. Second, one application per housing unit minimizes advertisers' inconvenience. The importance of these first two advantages increases with the increase in the amount of tenants' groups being compared.[14] Third, the random assignment procedure is simpler to implement and cheaper than the matched applications technique. Even if this point is not grounded on a scientific basis, it is important in light of researchers' time and budget constraints.

The applications were randomized as follows. I created three tenants' identities (that is, Andrea Rossi, Francesco Russo, and Alessandro Ferrari) and an email account for each of them.[15] Then, I entered them into a spreadsheet on which the three identities were repeated approximately 330 times each; after that, I randomly ordered the identities by drawing without resampling using a normal distribution. Afterwards, I created three applicants' statuses (that is, married tenants, married tenants with blind tenant plus guide dog, and married tenants with pet dog), I prepared a list on which the applicants' statuses were repeated approximately 330 times each, and again I randomly ordered them by drawing without resampling using a normal distribution. Thereafter, I paired the list of applicants' statuses with that of applicants' identities. Finally, although correspondence tests are usually focused on housing units in large cities, in my experiment I try to preserve geographic representativeness. Therefore, for all of the fictitious applicants, I also randomly determined the general location of the apartment for which they had to apply (that is, the

[14] Consider a case similar to that in this experiment, where three groups of tenants are compared. If the experimenter used the random assignment procedure and randomly selected three housing units that by chance were managed by the same advertiser, this advertiser would receive three fictitious applications (that is, one fictitious application per vacant housing unit). If instead the experimenter used the matched technique, that same advertiser would receive one fictitious application per type of applicant per vacant housing unit (that is, nine fictitious applications in total). In the latter case the risk of exposure is much larger and the advertiser's inconvenience is greater.

[15] These are the most frequent Italian names and surnames. Sources: demo.istat.it and italygen.com. The three email accounts took the form of name.surname###@gmail.com. Names were randomly matched with surnames. A complete randomization of the name and surname matching was not carried out because it would have implied handling nine email accounts, which would have increased the complexity of the experiment.

region of the apartment and whether the apartment had to be in a metropolitan city or not).[16] The amount of applicants per region reflects the proportion of the national population living in that region, and in each region the number of applicants per metropolitan city reflects the proportion of the regional population living in that city.[17]

I use household tenants rather than single tenants for two reasons. First, this strategy follows recent developments of the literature on economics of disability (Parodi & Sciulli, 2012). Second, although technologies exist that allow blind persons to use computers, some people might ignore their existence. The risk of being detected could increase if some landlords viewed with suspicion applications written by blind persons. Therefore, each application was written by the normal sighted husband, who revealed the composition of the household, which implied revealing whether the wife owned a dog and whether this dog was either a well behaved pet dog or a guide dog. The specification "well behaved" serves to reduce as much as possible any perceived difference between these two dog types, in terms of burden for the apartment.[18]

Thus, the standard application for a vacant housing unit can be translated as follows:

"Good morning/evening,

[16] Similar to the two previous steps, I first created the list of randomly ordered locations, and then I paired this list with the list of applicants.
[17] A "metropolitan city" is an administrative institution that is expected to be operative as of 2015. However, this administrative institution was already described in Law no. 142, 1990, and, before that, it was solicited in the Italian Constitution, art. 114. A metropolitan city includes a large core city and its smaller surrounding towns; the core city and towns are closely related in terms of economic activities, provision of public services, cultural aspects and territorial features. The metropolitan cities have large populations, ranging from hundreds of thousands to a few million inhabitants.
[18] The version of this study which is published in my doctorate thesis (Fumarco, 2015) does not discuss the specification "well behaved," as it was lost in translation.

My family is interested in the apartment for rent described in the advertisement you posted at the website Subito.it. I would like to move in with my wife [and her (guide/well behaved) dog].

If the apartment is still available, we would like to visit it."

The length of this message is similar to that of other studies that use a correspondence test to investigate discrimination in the housing market (Ahmed & Hammarstedt, 2008; Ahmed, Andresson & Hammarstedt, 2010, 2008; Hanson & Hawley, 2011; Baldini & Federici, 2011).

When the actual experiment began, I sent the fictitious applications following the list of applicants' identities with assigned apartment location. Applications were sent to the most recent advertisements to minimize the probability of contacting advertisers whose apartments had already been rented out. Advertisements for apartments smaller than 40 square meters and for those more expensive than 1,500€ per month were not taken into consideration. The size restriction was adopted because of the Italian law that legislates the maximum number of tenants per square meter;[19] this limit does not apply to pets. The rent restriction was adopted to avoid rent outliers.[20] Advertisements that explicitly discouraged dog owners from applying were not taken into consideration. Advertisements on shared housing units were mostly posted by university students who searched for other student housemates, so they were not contacted. The contacted advertisers were given 31 days to answer the query; if they replied after this deadline, the observation would have been excluded from the analyses.[21]

[19] Ministerial Decree, June 20, 1975, art. 5, and its modification in 1986.
[20] The estimated average monthly rent in Italy is 1,000€. Source: number.com.
[21] Only one advertiser answered after 31 days, and thus was excluded from the analyses.

Each invitation to view an apartment was promptly declined to minimize any inconvenience to the advertiser.[22]

2.2 Descriptive Statistics

2.2.1 Invitation Rate

The outcome of interest is the frequency at which applicants receive an invitation to visit apartments they applied to. The invitation rate for each group is reported in Table 1.[23]

****Table 1 about here****

Group A comprises married tenants; Group B is the treatment group and consists of married tenants where the wife is blind and is assisted by a guide dog; Group C is composed of married tenants with a pet dog.

This table suggests one important result. The comparison in the invitation rates for Groups A and B signals the presence of disability discrimination. This discrimination is due to the two characteristics that differ between Groups A and B, and landlords cannot discriminate disabled people based on neither of them: disability status (in this case, being blind) and specific condition related to the disability (in this case, owning a guide dog). Furthermore, comparing Groups A and C suggests the presence of discriminatory behaviors against pet dog owners, which is legal; the only difference between these two groups is the presence of a pet dog in Group C. Comparing invitation rates for Groups B and C suggests that tenants with a guide dog are treated

[22] When the matched application technique is used to compare more than two groups, also these messages may increase the risk of detection. Once the advertiser notice two very similar messages that decline the invitation to the same housing unit, she might read anew the correspondent application messages, notice whether there are similarities also between them, and pay additional attention to future applications.
[23] "Invitation rate" is equivalent to the traditional term "callback rate."

equivalently to tenants with a pet dog; note that the dog type is not the only difference between the two groups: in Group B the wife is also blind. Without an additional assumption, this comparison is not useful. Table 2 presents the independent group t-tests for the three comparisons.

****Table 2 about here****

The comparison of Groups B and C is meaningful contingently on what I call *equality assumption.* This assumption requires that, from the point of view of the advertisers, there was no difference between well behaved pet dogs and guide dogs in terms of burden for the apartment, and advertisers had no preferences for one over the other. Discussions with members of associations for blind people suggest that this is a credible assumption. Moreover, exactly as it is for pet dogs, guide dogs' behavior within housing units depends on the education they have received from their owners, and not from the education they have specifically received to become guide dogs. Finally, newspapers report frequent law violations on the free access of guide dogs to public places and means of transportation (this specific aspect of blind people discrimination is regulated by law no. 34, 1974); violators often justify their behaviors explaining that guide dogs are not different from pet dogs (for example, they bark and dirty the furniture).[24]

If the equality assumption really reflects reality, the wife's blindness would be the only difference between Groups B and C, and thus, this comparison would suggest that blindness per se is not the cause of discrimination, which is a positive result. Consequently, the 12 percentage

[24] In the US, violators of ADA who do not allow guide dogs in the business premises frequently report similar justifications.

points gap in the invitation rate between Groups A and B seems to be caused by the presence of the guide dog alone. This is a special case of illegal discriminatory treatment of disabled people.

The analysis of separate subsamples based on the advertiser type sheds additional light on the results. There are two different advertisers: housing brokers and apartment owners, who are expected to behave very differently. Housing brokers are professional intermediary agents who advertise and rent out housing units that belong to someone else; housing transactions and interactions with applicants represent part of their daily job. Apartment owners advertise and rent out their own housing unit on their own; housing transactions and interactions with applicants do not represent part of their daily job, rather they do it in their spare time (for instance, after the working day or in the weekend). In Italy, housing brokers must either pass an exam or spend a period of at least 12 months as practitioners in a housing agency in order to obtain the brokerage license (Federazione Italiana Mediatori Agenti d'Affari, 2006). Thus, housing brokers are more likely to have complete knowledge of the legislation compared to apartment owners. They also conduct more frequently housing transactions, which increases their risk of being taken to court in case of misconduct and thus might decrease discriminatory behaviors. Table 3 reports the main statistics for the three groups of tenants, divided by advertiser type. [25]

****Table 3 about here****

[25] This information can be obtained in two ways: i) indirectly, if the name of the advertiser is the name of a real estate agency; ii) directly, the advertiser self-identifies as an agent (in this case, the word "Azienda" that is, "Company", appears in parenthesis by the advertiser's name).

As expected, the statistics in Table 3 suggest that the two types of advertisers behave very differently: housing brokers do not discriminate against blind tenants with guide dogs, whereas apartment owners do discriminate.

There is an information fact worth mentioning in Table 3. Housing brokers invite potential applicants to a higher rate compared to apartment owners, independently from tenants' type. The main possible reason could be that the interaction with applicants is part of housing brokers' daily job.

Independent group t-tests for the three comparisons in the two subsamples of advertisers give statistical support to this interpretation; see Table 4.

****Table 4 about here****

This finding is policy relevant. To decrease discrimination against households with blind tenants and guide dog, policy makers could provide incentives to these household tenants to turn to housing agencies to research an apartment.

Statistics on the rejection rate provide with similar insights; see Table A.1 and A.2 in Appendix A. The rejection rate is the frequency at which applicants receive direct negative answers from advertisers.[26]

Table A.3 and A.4 in Appendix A provides the exact number of invitations, non-responses and rejections per group of tenants, and by advertiser type.

[26] Even if answers presented different reasons for directly rejecting applications, no distinction was made because of their limited amount.

2.2.2 Apartment and Advertisement Characteristics

I collected extensive information from each advertisement. I have taken note of the city name, monthly rent, apartment size and presence of furniture; I have documented the presence of photo(s) and telephone number; I recorded the date when each advertisement was posted, the date when each application was sent and the date when each response was received. The latter information allowed me to compute the time it took for each application to receive an answer. Additionally, information on the population density of the town where the housing unit is located was retrieved from the 2012 census conducted by the Italian national institute of statistics (that is, ISTAT). From the same census, information on county size and blind population ratio at the county level was retrieved. Advertisements did not usually contain information on whether the apartment was relatively isolated, within a condominium, duplex, or townhouse. They did not even contain information on whether the apartment owner lived in the same building where the vacant apartment was located.

I have also collected advertisers' names. This information allowed me to distinguish between apartment owners and housing brokers, and to retrieve advertisers' gender.[27,28] The information on advertisers' names also provide further ground to support the decision to use the random assignment over the matched application technique for this experiment. I have detected around 100 cases in which names and emails of respondents obviously differ from those displayed in the original advertisements.[29] Non-transparent behaviors from advertisers make it hard

[27] In principle, this information could help distinguishing also between advertisers with Italian and foreign background. However, only a few observations have foreign sounding names.

[28] In the version of this study which is published in my doctorate thesis (Fumarco, 2015), there were only a few observations on housing brokers' gender. Since then, I have thoroughly checked anew the content of each email to gain this information also for this type of advertisers.

[29] Some apartment owners have posted advertisements in name of friends or relatives, and acknowledge it in their answers. Few advertisers have signed multiple advertisements with different names, but answers were received from the same emails. Some advertisements report the same housing agency name, but housing brokers' name in the email

to correctly keep track of them. Furthermore, about half of contacted housing brokers posted advertisements under the name of their real estate agency and answered applications without signing the email. Thus, frequently it is not possible to keep track of individual brokers either. In these cases, advertisers might have been accidentally contacted multiple times and the matched application technique would have increased the chances of detection.

Table 5 reports the main descriptive statistics for these variables, for the whole sample and by advertiser type.

****Table 5 about here****

One interesting insight can be obtained from this table. Both apartment owners and housing brokers answer on average very quickly to apartment queries (1 day), sometimes even on the same day (0 days). Quick responses as well as high response rates of this experiment must be due to the specific moments of the day I sent the applications, namely, early morning and late afternoon. These are arguably the two main moments of the day to check the email account, so my messages must have been often on top of receivers' mailbox.

Comparisons of the response time between the three groups of household tenants, in the two subsamples of advertisers, suggest that there is no differential treatment in terms of apartment owners' and housing brokers' response time.[30]

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differs. Moreover, a couple of answers from apartment owners suggest the existence of fraudulent schemes. Initial invitations were similar in the content to standard ones. However, after the polite rejection to these invitations, additional (seemingly automatic) follow up messages were received, which contained a similar storyline (that is, "I am a foreigner, live abroad, and have bought this apartment for my daughter who studied in an Italian university, but now moved to another country for work") that concluded inviting to provide private sensitive information and a deposit before to visit the housing unit.

[30] Results on independent group t-tests can be provided upon request.

2.3 Experiment Limitations

Before I turn to the econometric analyses, I discuss three possible limitations to my experimental design. First, the disability status is signaled through the presence of a guide dog; this signal may be judged ambivalently.[31] On one hand, less experienced advertisers, such as apartment owners, might fail to understand the cue. However, it is worth noting that some answers from apartment owners openly acknowledge blind tenants' condition.[32] On the other hand, someone might argue that this signal is too strong: besides the guide dog being considered as a friend, why should blind tenants disclose its presence? The reason is that blind people may wish to screen landlords, to find out who intend to discriminate based on dog ownership prior the visiting. Had they to disclose the presence of the guide dog only at the visiting, blind tenants might end up wasting time and energy for a pointless visiting. Moreover, failure to disclose the presence of the guide dog prior signing the rental contract could cause future frictions with neighbors and the landlord. For similar reasons, also normal sighted tenants might desire to screen landlords.

Second, the size of the population of blind people assisted by guide dogs could be small in Italy, where there is no official statistics. For the UK, estimates suggest the presence of 4,500 guide dog owners (Chur-Hansen et al., 2014). For the US, estimates suggest the presence of about 9,000 guide dog owners (Eames et al., 2001). Had the size of this population to be equivalently small in Italy, one could argue that the additional time spent by Italian guide dog owners in sending a few more applications to find an apartment would entail a low social cost. However, this argument is not a valid reason for neglecting the study of this population of disabled people, from

[31] The strength of the signal is a common concern in correspondence tests. Also seminal papers, such as those of Bertrand and Mullainathan (2004) and Ahmed and Hammarstedt (2008), discuss this issue.
[32] Some of them mention that the apartment is not on the ground floor, or that there are a few steps in front of the building door, and ask whether this is a problem for the wife. In other answers, they explicitly talk about the wife's blindness.

the stand point of society's fairness and equity principles, which are also reflected into the Charter of Fundamental Rights of the European Union. Moreover, based on the equality assumption, my experimental design allows the distinction between discrimination due to blindness from discrimination due to the guide dog alone. Therefore, the results from this experiment could interest also two wider populations: households with a blind tenant without guide dog[33] and households with a disabled tenant who owns an assistance dog (for instance, deaf people assisted by signal dogs; mobility impaired people helped by dog fetchers; people with psychiatric disabilities, such as Asperger syndrome, posttraumatic stress disorder, and autisms, assisted by therapy dogs; people with health problems, such as diabetes and epilepsy, assisted by alert dogs).[34] The extension of the results to blind tenants with no guide dog is straightforward, if the equality assumption reflects reality. Differently, these results might concern also other disabled tenants assisted by different specialized dogs only if the equality assumption were stretched further to include all types of assistance dogs: landlords do not differentiate between common pet dogs and guide dogs, and all other types of assistance dogs.

Third, the equality assumption is based on anecdotal evidence and is not testable, what if it did not hold? The main finding of this experiment would still remain: blind tenants with guide dogs are discriminated against by apartment owners, and this behavior is illegal: they should be treated the same as normal sighted tenants with no dog. However, there would be problems in terms of results interpretation and their policy implications. If the equality assumption did not

[33] According to the 2012 ISTAT census, at the moment of the experiment there were 129,000 blind people in Italy; see http://www.salute.gov.it. In my dataset, the sum of the amounts of blind people per county is about 3,500 units smaller. In fact, my randomization process is meant to preserve geographic representativeness at regional and metropolitan level, not at county level as well. Therefore, 5 counties out of 110 Italian counties ended up being excluded from the experiment: Gorizia (in Friuli-Venezia-Giulia), Benevento (in Campania), Ogliastra (in Sardinia), Medio Campidano (in Sardinia), Belluno (in Veneto).

[34] There is no official estimate about the size of these populations either.

hold, the statistics displayed in Tables 1 and 3 could be produced if advertisers treated different dogs differently and these preferences were counterbalanced by preferences in opposite directions for disabled and non-disabled dog owners. Otherwise, invitation rates of Groups B and C could not be equal. In this case, it would not be possible to distinguish between discrimination based on the disability status from that based on the presence of the guide dog.

There could be at least two types of discriminatory behaviors based on the disability status: taste based (Becker, 1957) and statistical discrimination (Phelps, 1972; Arrow, 1973; Aigner & Cain, 1977). Advertisers could prefer to avoid contacts with blind people, even if this implied that they would forgo business opportunities; in this case, discrimination based on the disability status would be taste based. Advertisers could discriminate households with a blind tenant because they are perceived as being on average less financially stable, for example; in this case, discrimination based on the disability status per se would be statistical.

The next section investigates through a series of econometric analyses whether the main results of this experiment are concealing discrimination based on disability status.

3 Model and Results

3.1 Main Results

The main purpose of the econometric analyses in this section is to verify whether the randomization process worked as intended. The data are analyzed with a linear probability model. The dependent variable, $Invitation_i$, is a dummy variable that equals 1 if the applicant has received an invitation to visit the housing unit and equals 0 if the applicant has not received an invitation or has received a negative answer. This outcome variable is regressed on two variables of interest: the dummy $Blind_i$, where i is a household with a blind wife who owns a guide dog, and Dog_i,

where *i* is a household with a pet dog.[35] A vector of control variables, $\boldsymbol{X}$, includes apartment and advertisement characteristics: apartment square meters and monthly rent; a dummy for the apartment being in a metropolitan city; a dummy for the apartment being furnished; and dummies that indicates the presence of apartment pictures as well as phone number in the advertisement. Apartment square meters and monthly rent are centered; furthermore, monthly rent is rescaled, specifically, divided by 10. The model also includes $\boldsymbol{F}$, a vector of fixed-effects,[36] as well as a stochastic individual term, ε_i. The linear probability model looks as follows:

$$Invitation_i = \beta_0 + \beta_1 Blind_i + \beta_2 Dog_i + \boldsymbol{\beta X} + \boldsymbol{\beta F} + \varepsilon_i \qquad (1)$$

This model is first estimated only with the independent variables of interest, then the vector of control variables is added, and finally also the vector of fixed-effects is included. Because the descriptive results suggest that housing brokers and apartment owners are very different in terms of discriminatory behaviors, the model is estimated on the two separate subsamples of advertisers. The main estimates are reported in Table 6.

****Table 6 about here****

[35] Household tenants without a pet dog and where the wife is not disabled (Group A) is the reference group.

[36] This vector includes: dummies for each applicant's identity (Andrea Rossi and Francesco Russo; Alessandro Ferrari is the baseline identity), dummies for each Italian region (Lombardy is the baseline region) and a dummy for the application being sent after a new condominium national regulation came into force. This vector includes: dummies for each applicant's identity (that is, Andrea Rossi and Francesco Russo; Alessandro Ferrari is the baseline identity), and dummies for each Italian region (with Lombardy being the baseline region). One additional dummy being used indicates whether the application was sent after a new national condominium regulation came into force (law no. 220, December 11, 2012, which came into force on June 18, 2013); according to this law, new condominium regulations can no longer include pet restrictions. A natural experiment within this field experiment, to analyze the effect of this law on disability discrimination is not possible. This law applies neither to other types of apartments nor to condominiums that enforced pet restrictions before this new law; moreover, advertisements did not presents information on the housing unit being in a condominium or not, and only 43 observations in total for Groups B and C were sent from June 18, 2013.

Table 6 confirms the insights provided by the descriptive results. These estimates show no evidence of disability discrimination by housing brokers; in fact, $\beta_1\hat{}$ is close to zero and never statistically significant. However, households where the wife is not blind and owns a pet dog experience discriminatory treatment; $\beta_2\hat{}$ is highly statistically significant and negative. The difference between $\beta_1\hat{}$ and $\beta_2\hat{}$ is statistically significant. The model augmented with control variables and fixed-effects gives equivalent estimates. The combination of these results suggests that housing brokers treat household tenants where the wife is blind and owns a guide dog similarly to the reference group (that is, household tenants with no dog). Differently, Table 6 provides evidence of disability discrimination by apartment owners. Given that $\beta_1\hat{}$ is negative and highly statistically significant, blind tenants are discriminated against; their invitation rate to visit an apartment is 21-24 percentage points lower than that of household tenants with no dog. Moreover, household tenants with a pet dog have an invitation rate that is 19-23 percentage points lower than that of the reference group; $\beta_2\hat{}$ is highly statistically significant. The difference between the estimates for $\beta_1\hat{}$ and $\beta_2\hat{}$ is never statistically significant. These results are robust to different specifications.[37]

Equivalent results are obtained also when using the rejection rate as outcome variable. The model specification for this analysis is similar to (1), the only difference is the outcome vari-

[37] Estimates of the marginal effects from a probit model are equivalent to those in Table 6, and may be provided upon request. Two alternative linear models have been implemented. The first model does not distinguish between different dogs. A new variable for the presence of any type of dog is introduced, $Canem_i$. This variable equals 1 regardless of the nature of the wife's dog; and it is interacted the variable $Blind_i$. The model looks as follows: $Invitation_i = \beta_0 + \beta_1 Canem_i + \beta_2\ Canem_i{*}Blind_i + \boldsymbol{\beta X} + \boldsymbol{\beta F} + \varepsilon_i$. Therefore, this model treats guide and pet dogs as being the exactly same and $Blind_i$ now represents the group of households who have a (guide) dog and also a blind person. The results are equivalent to those in Table 6. In a second alternative linear model, which also provides equivalent results, I pool observations on housing brokers and apartment owners and introduce the variable $Company_i$, which equals 1 if the advertiser is a housing broker. This variable is introduced alone and interacted with both $Blind_i$ and Dog_i. The model looks as follows: $Invitation_i = \beta_0 + \beta_1 Blind_i + \beta_2 Dog_i + \beta_3 Company_i + \beta_4 Company_i{*}Blind_i + \beta_5 Company_i{*}Dog_i + \boldsymbol{\beta X} + \boldsymbol{\beta F} + \varepsilon_i$.

able which is a dummy variable that equals 1 it the applicant has received a direct negative answer. The results are reported in Table A.5 in Appendix A.[38]

These findings on disability discrimination are in line with those from the two other correspondence tests on this topic (Heylen & Van den Broeck, 2015; Verhaeghe, Van der Bracht & Van de Putte, 2015).

Analyses with advertisers' response time as outcome variable, confirm that both types of advertisers treat the three groups of tenant households in the same way in terms of response time.[39]

Given the specific set of results in this study, if the equality assumption reflects reality, direction and size of the estimates suggest that discrimination against households with blind tenants assisted by guide dogs occurs because of the presence of their guide dogs alone. The evidence on this particular type of disability discrimination is in line with that provided through in-person audit tests by American fair housing organizations and other nonprofits on refusal to provide reasonable accommodation to guide dogs. These results raise concerns about the possible existence of a similar discrimination against disabled tenants assisted by other specialized dogs.

On the bright side, statistical and taste based discrimination related to tenants' disability status (in this case, blindness) seem to be ruled out.

[38] Equivalent results to those obtained with model (1) are obtained also with two different definitions of the outcome variable. In a first alternative definition of the outcome variable, $Invitation_i$, is a dummy variable that equals 1 if the applicant has received an invitation to visit the housing unit and equals 0 if the applicant has not received an invitation. Therefore, the observations on applications that received negative answers are dropped from the analysis. In a second alternative definition of the outcome variable, $Invitation_i$, is a dummy variable that equals 1 if the applicant has received an invitation to visit the housing unit and equals 0 if the applicant has received a negative answer. Therefore, the observations on applications that did not receive an answer are dropped from the analysis. These results can be provided upon request.

[39] These results can be provided upon request.

3.2 Additional Analyses

The next two sections reports robustness and heterogeneity analyses.

3.2.1 Robustness Checks

To understand whether the interpretation of the results were correct, the first best would be to test the equality assumption. If this test rejected the assumption, there would be reasons to believe that the results from this experiment were owed to the concomitant presence of differential treatment based on tenants' disability and different preferences for different dogs in opposite directions. In fact, only in this situation Groups B and C could have the same invitation rates.

It is not possible to test the equality assumption. However, it is possible to investigate whether the results are influenced by the presence of discrimination based on disability status; this is the purpose of the next two analyses.

In presence of statistical discrimination based on ability to pay, one could expect the invitation rate of households with a blind wife who owns a guide dog to vary with different levels of monthly rent. This variable is already present in model (1); in these additional analyses, it is also interacted with both $Blind_i$ and Dog_i.

The results are reported in Table B.1 in Appendix B. The discrimination level does not statistically significantly vary with rent, so statistical discrimination based on ability to pay could be ruled out. However, there could be statistical discrimination with respect to other characteristics either of the applicants or of the apartments not included in the regression.

The lack of statistical discrimination based on ability to pay seems to be reasonable in light of the experimental design. The experiment approaches disability discrimination from the perspective of a traditional family in a country were women's employment rate and average wage

are lower than those of men. Therefore, whether the wife is disabled or not might not make a difference in terms of ability to pay the rent, from the landlords' point of view.

In presence of taste based discrimination, one could expect the invitation rate of households with a blind wife and a guide dog to vary with the intensity of contacts between blind and normal sighted people, based on the intergroup contact theory (Allport, 1954; Pettigrew, 1998). Most studies suggest the existence of a negative relationship: the larger the amount of contacts the lower the prejudices (Pettigrew & Tropp, 2006).[40]

Given the available information in the dataset, a proxy for the intensity of contacts could be represented by the relative amount of blind people per 1,000 inhabitants of the county where the apartment is located. For this analysis, this variable is demeaned, and then added alone as well as interacted with $Blind_i$. Additionally, this model includes a control for county size, which is also demeaned and rescaled, specifically, being divided by 100.

The results are reported in Table B.2 in Appendix B. The discrimination level does not statistically significantly vary with this proxy for intensity of contacts; alternative model specifications confirm this result.[41].

This result should be considered carefully. The proxy for intensity of contacts reflects the amount of possible contacts, similarly to other studies on the effect of intergroup contact on discrimination (Finseraas et al., 2015, for a recent analysis; Pettigrew & Tropp, 2006 for a literature

[40] In alternative, since blind people are not randomly distributed across counties, one could think of the amount of blind people in a county as being a proxy for the attitudes toward them in that county. Under this light, this analysis would be following the methodology first suggested by Carlsson and Rooth (2011), who investigate the presence of taste based discrimination in hiring by exploiting geographic variation in ethnic attitudes.

[41] As an alternative specification, in lieu of (county blind population / [county population / 1,000]), I insert county blind population alone and interacted with $Blind_i$, and control also for (county population / 1,000) and (county size / 100); the result is equivalent. In a second alternative specification, I introduce the ratio (county blind population / [county size / 100]) alone as well as interacted with $Blind_i$ and control for (county population / 1,000); also in this case, the result is equivalent).

review), however other aspects of intergroup contact are neglected (Carrell, Hoekstra & West, 2015; Finseraas & Kotsadam, 2015; Pettigrew, 1998).

These findings suggest the absence of taste based discrimination and statistical discrimination based on ability to pay. Thus, they seem to rule out discrimination based on disability status, which support the interpretation of disability discrimination being due only to the guide dog.

3.2.2 Heterogeneity Analyses

The level of discrimination might vary based on additional characteristics that can confound the initial results. In particular, it might vary based on: vicinity of the housing unit to dog friendly amenities, presence of furniture, and advertisers' gender.

The vicinity to dog friendly amenities, namely, public or private gardens, might influence the level of discrimination. Some advertisers could discriminate household tenants who own either guide or pet dogs out of concern for the dog's wellbeing. Nonetheless, it is worth mentioning that even if this concern were genuine, differential treatment based on the presence of the guide dog would still represent illegal disability discrimination.

In this dataset, there are two proxies for the availability of the housing unit to dog friendly amenities: the apartment being in a metropolitan city and the population density of the city where the apartment is located. The idea is that in densely populated cities, there are less green spaces. The dummy for the apartment being in a metropolitan city is already present in model (1); in these additional analyses it is also interacted with both $Blind_i$ and Dog_i. In the alternative model specification, when the population density is used, this variable is introduced alone and interacted with both $Blind_i$ and Dog_i; in this case, the dummy for the apartment being in a metropolitan city is dropped and the population density is demeaned and rescaled, specifically, divided by 1,000.

The results are reported respectively in Table C.1 and C.2 in Appendix C. It appears that the vicinity of the apartment to dog friendly amenities does not affect the level of discrimination.

The level of discrimination could vary with the presence of furniture. Apartment owners might discriminate dog owners because they are concerned the dog could damage the furniture. Also in this case, even if the concern were genuine, differential treatment based on the presence of the guide dog would represent discrimination.

There is already a dummy for the apartment being furnished in model (1); in this analysis, this variable is also interacted with both $Blind_i$ and Dog_i.

The results are reported in Table C.3 in Appendix C. The presence of the furniture does not appear to influence the level of discrimination.

Finally, the level of discrimination could vary with advertisers' gender. Gender differences could be due to a number of factors, such as cultural features or subconscious reasons; however, the study of the reasons for these different behaviors is beyond the scope of this study.

In this analysis, a dummy variable for advertisers' gender is introduced alone, with the reference group being composed of male advertisers, and interacted with $Blind_i$ and Dog_i.

The results from this analysis should be considered carefully: they could be affected by self-selection. In fact, information on gender is available for most apartment owners, but for only about half of the housing brokers.

Table C.4 in Appendix C shows three interesting results. First, independently from their own type, female advertisers invite applicants more frequently to visit the housing unit compared to male advertisers. Second, irrespectively of their own type, female advertisers consistently discriminate households with a blind wife who owns a guide dog. Female advertisers seem to discriminate also households where the wife is normal sighted and owns a pet dog; however, this

result is not statistically significant. Differently, male advertisers' behavior changes dependently on their type, but irrespectively of tenants' disability status. Male housing brokers do not discriminate against dog owners, while male apartment owners do discriminate against them. Third, even though there is no statistical significant evidence of discrimination against households with a blind wife who owns a guide dog from male housing brokers, the results in this table suggest that the lack of evidence for disability discrimination from housing brokers, in Table 6, could be due to different behaviors between male and female housing brokers.

Results in Table C.4 should be taken with a grain of salt. Estimates from the subsample of housing brokers could be biased because of self-selection, since gender is retrievable only for about half of them. Therefore, the results for this subsample are not directly comparable with those in Table 6. Additionally, because of non-transparent behaviors from advertisers, it is not possible to be absolutely sure that advertisers' gender is what appears to be from their names.

4 Discussion and Conclusions

This study presents different original contributions. The most important is that it shows that with the appropriate experimental strategy, a correspondence test can be adapted to investigate also disability discrimination in the housing market.

The focus of this research is on blind tenants assisted by guide dogs in the Italian rental housing market. I find sound evidence that blind tenants are discriminated against by apartment owners; this result is in line with those from the few similar studies on this topic.

Additionally, given a reasonable assumption, I argue that this discrimination is due to the presence of the guide dog alone. According to both EU and Italian legislation, this behavior can be referred to as *indirect discrimination* because it is based on a disabled tenants' characteristic that is indirectly related to the disability. Indirect discrimination occurs when an apparently neu-

tral requirement that is assumed to apply to everyone (in this study, a pet restriction) has an unfair effect on disabled people. This is the first time that evidence on this type of discrimination is obtained through a correspondence test.

Within the American context, indirect discrimination could be interpreted as the refusal to provide reasonable accommodation to guide dogs. Under this light, the evidence on indirect discrimination found in this study is in line with that provided by in-person audit tests conducted by American fair housing organizations and other nonprofits.

On a positive side, these results seem to rule out *direct discrimination*, that is, discrimination due directly to tenants' disability (in this study, blindness). This finding is confirmed by robustness checks.

Heterogeneity analyses suggest that, in Italy, women consistently discriminate based on dog ownership. Whether women are housing brokers or apartment owners, and whether dog owners are disabled or not, do not seem to matter. This result should be considered carefully as it could be affected by self-selection.

Awareness and information campaigns could decrease discrimination against households with blind tenants assisted by guide dogs. These campaigns should be tailored to apartment owners and thus be diffused through standard media. Awareness campaigns could focus on the role of guide dogs, and convey the message that they do not only provide emotional support to their handlers, as all other dogs do, but also assist them in multiple activities throughout the day. For instance, guide dogs identify, and help to avoid, obstacles that their owners cannot identify alone; guide dogs help their handlers to board public transportation, and to proceed safely along roads as well as to cross them. Awareness campaigns could potentially favorably influence those apartment owners who know the law but choose to ignore it, because they are not fully aware of the

importance of guide dogs for their owners. Differently, information campaigns should be conducted to educate apartment owners about the legislation; some of them might in fact not be fully aware of what is considered discrimination.

Based on the results of this study, there could be a third way to reduce discrimination. Since there is no evidence of discrimination from housing brokers, households with blind tenant and guide dog should be advised to turn to housing agencies. Since this service is costly, it could be (partially) reimbursed or financed by public institutes.

This field experiment also documents discriminatory behaviors based on dog ownership in general. When it is directed to non-disabled people, this behavior is not illegal, in Italy. However, it entails negative effects on tenants' happiness and wellbeing: dogs and other pets alleviate depression, loneliness, and ease the discomfort of aging to their owners.

Future correspondence tests on disability discrimination in the housing market could proceed to investigate (at least) three aspects. First, the disabled people population is large and does not include only tenants affected by blindness. Therefore, to provide a complete picture of disabled tenants' situation in the rental housing market, future studies could target tenants affected by other disabilities, such as those affected by mental disabilities. Second, future research could improve on the experimental strategy proposed in this study to analyze indirect discrimination and adjust it to accommodate other types of indirect discrimination, such as that related to mobility disabilities. Third, studies to come should explore whether disability interacts with other causes of discrimination and hence worsens conditions of tenants who are already at disadvantage in the rental housing market.

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Tables

Table 1. Invitation rates for the groups of tenants.

Tenants' group	N	Mean	Std. Dev.
	(1)	(2)	(3)
A (Not blind, no dog)	334	0.784	0.412
B (Blind, guide dog)	332	0.666	0.472
C (Not blind, pet dog)	333	0.634	0.482

Note: Columns (1)-(2)-(3) report respectively: number of observations, means and standard deviations of groups' invitation rates.

Table 2. Independent group t-tests; pairwise comparison of the invitation rate.

Tenants' group	A (Not blind, no dog)	B (Blind, guide dog)	C (Not blind, pet dog)
A (Not blind, no dog)		-3.459 (0.000)	-4.341 (0.000)
B (Blind, guide dog)	-3.459 (0.000)		-0.865 (0.387)
C (Not blind, pet dog)	-4.341 (0.000)	-0.865 (0.387)	

Note: T statistic (p values in parenthesis).

Table 3. Invitation rates for the groups of tenants, by advertiser type.

Tenants' groups	Advertiser type					
	Housing brokers			Apartment owners		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
A (Not blind, no dog)	138	0.819	0.386	191	0.764	0.425
B (Blind, guide dog)	133	0.812	0.392	191	0.555	0.498
C (Not blind, pet dog)	148	0.709	0.455	182	0.577	0.495

Note: In the panel "Housing brokers", columns (1)-(2)-(3) report, respectively: number of observations and means and standard deviations of groups' invitation rates from housing brokers. In the panel "Apartment owners", columns (4)-(5)-(6) report the equivalent statistics for apartment owners.

Table 4. Independent group t-tests; pairwise comparison of the invitation rate, by

	Housing brokers			Apartment owners		
Tenants' group	A (Not blind, no dog)	B (Blind, guide dog)	C (Not blind, pet dog)	A (Not blind, no dog)	B (Blind, guide dog)	C (Not blind, pet dog)
A (Not blind, no dog)		-0.144 (0.886)	-2.182 (0.030)		-4.417 (0.000)	-3.926 (0.000)
B (Blind, guide dog)	-0.144 (0.886)		-2.012 (0.045)	-4.417 (0.000)		0.426 (0.670)
C (Not blind, pet dog)	-2.182 (0.030)	-2.012 (0.045)		-3.926 (0.000)	0.426 (0.670)	

Note: T statistic (p values in parenthesis).

Table 5. Descriptive statistics, for the whole sample and by advertiser type.

Variable	Sample	Obs.	Mean	Std. Dev.	Min.	Max.
Continuous variables						
Rent per month	Whole sample	956	542.340	196.727	200	1,500
	Housing brokers	411	552.698	215.903	250	1,500
	Apartment owners	531	532.646	179.293	200	1,300
Size	Whole sample	999	76.336	27.999	40	250
(housing unit m2)	Housing brokers	419	77.270	27.497	40	200
	Apartment owners	564	75.117	28.203	40	250
Town pop. density	Whole sample	999	1.557	1.897	0.023	12.224
(town pop. /	Housing brokers	419	1.527	1.760	0.024	12.224
[city km2 * 1,000])	Apartment owners	564	1.584	1.993	0.023	12.109
Answer delay	Whole sample	741	1.061	1.709	0	22
(days to answer)	Housing brokers	340	1.065	1.433	0	15
	Apartment owners	390	1.067	1.937	0	22
County blind pop. ratio	Whole sample	976	2.165	0.851	1.117	7.674
(county blind pop. /	Housing brokers	411	2.166	0.862	1.117	7.674
[county pop. / 1,000])	Apartment owners	549	2.160	0.847	1.117	7.674
County size	Whole sample	997	30.629	17.579	2.125	73.984
(county km2 / 100)	Housing brokers	418	29.935	17.043	2.125	73.984
	Apartment owners	563	31.033	17.945	2.125	73.984
Dummy variables						
Metro	Whole sample	999	0.156	0.363		
	Housing brokers	419	0.134	0.341		
	Apartment owners	564	0.168	0.374		
Phone	Whole sample	990	0.896	0.305		
	Housing brokers	417	0.978	0.145		
	Apartment owners	557	0.871	0.371		
Furniture	Whole sample	999	0.581	0.494		
	Housing brokers	419	0.506	0.501		
	Apartment owners	564	0.640	0.480		
Photo	Whole sample	999	0.684	0.465		
	Housing brokers	419	0.766	0.424		
	Apartment owners	564	0.631	0.483		
Female	Whole sample	792	0.400	0.490		
	Housing brokers	230	0.456	0.499		
	Apartment owners	553	0.376	0.485		
Company	Whole sample	983	0.426	0.495		

Note: The sample is composed of 1,000 observations, 1 is excluded because was received after the 31 day dead-

line; for each variable the amount might be lower than 999 because of missing values. For each variable, the difference between the amount of observations in the whole sample and the sum of observations for housing brokers and apartment owners is due to missing values of the variables and 16 missing values on advertiser's type. Size and rent per month are reported as they appear originally; the other continuous variables are rescaled, following the econometric analyses in the next section, but are not demeaned.

Table 6. Linear probability model main estimates by advertiser type.

	Housing brokers			Apartment owners		
	(1.A)	(1.B)	(1.C)	(2.A)	(2.B)	(2.C)
Constant	0.819***	0.699***	0.711***	0.764***	0.852***	0.824***
(Not blind, no dog)	(0.032)	(0.161)	(0.192)	(0.034)	(0.059)	(0.077)
β_1^	-0.007	-0.009	-0.010	-0.209***	-0.227***	-0.239***
(Blind, guide dog)	(0.045)	(0.047)	(0.050)	(0.065)	(0.063)	(0.066)
β_2^	-0.109**	-0.119***	-0.112**	-0.187***	-0.208***	-0.232***
(Not blind, pet dog)	(0.041)	(0.041)	(0.044)	(0.040)	(0.042)	(0.041)
Control variables	N	Y	Y	N	Y	Y
Fixed-effects	N	N	Y	N	N	Y
P value (β_1^ - β_2^)	0.029	0.026	0.043	0.737	0.785	0.917
R squared	0.015	0.029	0.072	0.038	0.060	0.122
N	419	409	409	564	525	525

Note: Missing apartment characteristics, including the type of agent, cause the total sample to be smaller than 999 observations. Group A, which includes married tenants, is the reference group. The reference region is Lombardy, in columns (1.C) and (2.C). Robust standard errors corrected for day of inquiry are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix A

Table A.1 Negative answer rates for the groups of applicants.

Tenants' group	N	Mean	Std. Dev.
	(1)	(2)	(3)
A (Not blind, no dog)	334	0.021	0.143
B (Blind, guide dog)	332	0.057	0.233
C (Not blind, pet dog)	333	0.075	0.264

Note: Columns (1)-(2)-(3) report, respectively: number of observations and means and standard deviations of groups' negative answer rates.

Table A.2 Negative answer rates for the groups of applicants, by advertiser type.

Tenants' groups	Advertiser type					
	Housing brokers			Apartment owners		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
A (Not blind, no dog)	138	0.014	0.120	191	0.026	0.160
B (Blind, guide dog)	133	0.022	0.149	191	0.084	0.278
C (Not blind, pet dog)	148	0.074	0.263	182	0.077	0.267

Note: In the panel "Housing brokers", columns (1)-(2)-(3) report, respectively: number of observations and means and standard deviations of groups' negative answer rates from housing brokers per group of applicants. In the panel "Apartment owners", columns (4)-(5)-(6) report the equivalent statistics for apartment owners per group of applicants. For each group of tenants, the total number of observations in the two panels is lower than that reported in Table 1 because of missing information on the advertiser type.

Table A.3 Counts of invitations, non-responses and negative answers for the groups of applicants.

Tenants' group	Invitations	Non-responses	Negative answers
	(1)	(2)	(3)
A (Not blind, no dog)	262	65	7
B (Blind, guide dog)	221	92	19
C (Not blind, pet dog)	211	97	25

Note: Columns (1)-(2)-(3) report, respectively: number of invitations, non-responses, and negative answers per group of applicants.

Table A.4 Counts of invitations, non-responses and negative answers for the groups of applicants, by advertiser type.

Tenants' groups	Advertiser type					
	Housing brokers			Apartment owners		
	Invitations	Non-responses	Negative answers	Invitations	Non-responses	Negative answers
	(1)	(2)	(3)	(4)	(5)	(6)
A (Not blind, no dog)	113	23	2	146	40	5
B (Blind, guide dog)	108	22	3	106	69	16
C (Not blind, pet dog)	105	32	11	105	63	14

Note: In the panel "Housing brokers", columns (1)-(2)-(3) report, respectively: number of invitations, non-responses and negatives answers from housing brokers per group of applicants. In the panel "Apartment owners", columns (4)-(5)-(6) report the equivalent statistics from apartment owners per group of applicants. For each group of tenants, the total number of observations in the two panels is lower than that reported in Table 1 because of missing information on the advertiser type.

Table A.5 Linear probability model estimates, rejection rate as outcome variable.

	Housing brokers			Apartment owners		
	(1.A)	(1.B)	(1.C)	(2.A)	(2.B)	(2.C)
Constant	0.014	0.077	0.025	0.026**	-0.044*	-0.037
(Not blind, no dog)	(0.0102)	(0.105)	(0.110)	(0.012)	(0.024)	(0.033)
β_1^	0.008	0.002	0.002	0.058**	0.061**	0.058**
(Blind, guide dog)	(0.014)	(0.012)	(0.014)	(0.024)	(0.024)	(0.024)
β_2^	0.056***	0.060**	0.065***	0.051*	0.053*	0.043*
(Not blind, pet dog)	(0.022)	(0.023)	(0.024)	(0.025)	(0.028)	(0.024)
Control variables	N	Y	Y	N	Y	Y
Fixed-effects	N	N	Y	N	N	Y
R squared	0.020	0.030	0.111	0.011	0.045	0.121
N	419	409	409	564	525	525

Note: Missing apartment characteristics, including the type of agent, cause the total sample to be smaller than 999 observations. Group A, which comprises married tenants, is the reference group. The reference region is Lombardy, in columns (1.C) and (2.C). Robust standard errors corrected for day of inquiry are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B

Table B.1 Linear probability model estimates, interactions with rent.

	Housing brokers			Apartment owners		
	(1.A)	(1.B)	(1.C)	(2.A)	(2.B)	(2.C)
Constant	0.828***	0.690***	0.704***	0.781***	0.853***	0.823***
(Not blind, no dog)	(0.032)	(0.157)	(0.185)	(0.034)	(0.059)	(0.077)
β_1^	-0.016	-0.010	-0.010	-0.229***	-0.226***	-0.238***
(Blind, guide dog)	(0.046)	(0.048)	(0.051)	(0.065)	(0.064)	(0.067)
β_2^	-0.114***	-0.118***	-0.110**	-0.210***	-0.207***	-0.231***
(Not blind, pet dog)	(0.042)	(0.041)	(0.044)	(0.043)	(0.042)	(0.042)
β_3^	-0.000	-0.000	-0.000	0.000	0.000	0.000
(Rent)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
β_5^	0.000	0.000	-0.000	0.000	-0.000	-0.000
(Blind, guide dog)×(Rent)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
β_6^	-0.000	-0.000	-0.000	0.000	0.000	0.000
(Not blind, pet dog)×(Rent)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Control variables	N	Y	Y	N	Y	Y
Fixed-effects	N	N	Y	N	N	Y
R squared	0.021	0.031	0.073	0.048	0.061	0.123
N	411	409	409	531	525	525

Note: The variable Rent is centered and divided by 10. Missing apartment characteristics, including the type of agent, cause the total sample to be smaller than 999 observations. Group A, which comprises married tenants, is the reference group. Robust standard errors corrected for day of inquiry are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.2 Linear probability model estimates, interaction with county blind people per 1,000 inhabitants.

	Housing brokers			Apartment owners		
	(1.A)	(1.B)	(1.C)	(2.A)	(2.B)	(2.C)
Constant	0.774***	0.623***	0.583***	0.895***	0.969***	0.852***
(Not blind, no dog)	(0.0645)	(0.162)	(0.199)	(0.0697)	(0.0838)	(0.114)
β_1^	0.0537	0.0642	0.0349	-0.331**	-0.344**	-0.343**
(Blind, guide dog)	(0.105)	(0.113)	(0.113)	(0.145)	(0.144)	(0.142)
β_2^	-0.0897**	-0.0996**	-0.0982**	-0.173***	-0.200***	-0.222***
(Not blind, pet dog)	(0.0419)	(0.0410)	(0.0453)	(0.0400)	(0.0417)	(0.0418)
β_3^	0.0165	0.0246	0.0620*	-0.0623**	-0.0630**	-0.0323
(County blind rate)	(0.0228)	(0.0226)	(0.0369)	(0.0257)	(0.0253)	(0.0523)
β_5^	-0.0211	-0.0278	-0.0142	0.0556	0.0533	0.0484
(Blind, guide dog)× (County blind rate)	(0.0403)	(0.0417)	(0.0406)	(0.0550)	(0.0550)	(0.0539)
Control variables	N	Y	Y	N	Y	Y
Fixed-effects	N	N	Y	N	N	Y
R squared	0.021	0.034	0.073	0.049	0.071	0.124
N	411	401	401	549	512	512

Note: The variable "County blind population" is given by the ratio "county blind population / (county population / 1000)." Missing apartment characteristics, including the type of agent, cause the total sample to be smaller than 999 observations. Group A, which comprises married tenants, is the reference group. Robust standard errors corrected for day of inquiry are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C

Table C.1 Linear probability model estimates, interactions with metropolitan city.

	Housing brokers			Apartment owners		
	(1.A)	(1.B)	(1.C)	(2.A)	(2.B)	(2.C)
Constant	0.825***	0.700***	0.713***	0.795***	0.867***	0.837***
(Not blind, no dog)	(0.033)	(0.161)	(0.195)	(0.034)	(0.060)	(0.075)
β_1^	-0.018	-0.028	-0.035	-0.239***	-0.254***	-0.263***
(Blind, guide dog)	(0.050)	(0.051)	(0.055)	(0.067)	(0.069)	(0.071)
β_2^	-0.107**	-0.118**	-0.113**	-0.194***	-0.221***	-0.241***
(Not blind, pet dog)	(0.049)	(0.048)	(0.050)	(0.038)	(0.042)	(0.040)
β_3^	-0.075	-0.130	-0.161	-0.195**	-0.201**	-0.176
(Metro)	(0.135)	(0.134)	(0.139)	(0.090)	(0.096)	(0.107)
β_4^	0.101	0.135	0.174	0.187	0.162	0.147
(Blind, guide dog)×(Metro)	(0.147)	(0.148)	(0.150)	(0.138)	(0.143)	(0.146)
β_5^	0.007	0.008	0.033	0.064	0.077	0.059
(Not blind, pet dog)×(Metro)	(0.186)	(0.179)	(0.183)	(0.139)	(0.146)	(0.149)
Control variables	N	Y	Y	N	Y	Y
Fixed-effects	N	N	Y	N	N	Y
R squared	0.017	0.032	0.075	0.049	0.063	0.125
N	419	409	409	564	525	525

Note: Missing apartment characteristics, including the type of agent, cause the total sample to be smaller than 999 observations. Group A, which comprises married tenants, is the reference group. Robust standard errors corrected for day of inquiry are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C.2 Linear probability model estimates, interactions with population density.

	Housing brokers			Apartment owners		
	(1.A)	(1.B)	(1.C)	(2.A)	(2.B)	(2.C)
Constant	0.815***	0.667***	0.691***	0.761***	0.834***	0.839***
(Not blind, no dog)	(0.032)	(0.161)	(0.193)	(0.034)	(0.057)	(0.077)
β_1^	-0.003	-0.011	-0.0073	-0.207***	-0.223***	-0.236***
(Blind, guide dog)	(0.046)	(0.047)	(0.051)	(0.065)	(0.064)	(0.066)
β_2^	-0.106**	-0.120***	-0.111**	-0.184***	-0.210***	-0.231***
(Not blind, pet dog)	(0.041)	(0.040)	(0.044)	(0.040)	(0.042)	(0.040)
β_3^	-0.034	-0.040*	-0.051**	-0.025	-0.031	-0.058**
(Density)	(0.025)	(0.023)	(0.025)	(0.020)	(0.020)	(0.022)
β_4^	0.045	0.047	0.053*	0.030	0.033	0.038
(Blind, guide dog)×(Density)	(0.028)	(0.029)	(0.030)	(0.026)	(0.026)	(0.027)
β_5^	0.043	0.048	0.057*	0.013	0.023	0.028
(Not blind, pet dog)×(Density)	(0.031)	(0.031)	(0.033)	(0.028)	(0.028)	(0.028)
Control variables	N	Y	Y	N	Y	Y
Fixed-effects	N	N	Y	N	N	Y
R squared	0.022	0.035	0.081	0.042	0.057	0.130
N	419	409	409	564	525	525

Note: Missing apartment characteristics, including the type of agent, cause the total sample to be smaller than 999 observations. Group A, which comprises married tenants, is the reference group. Robust standard errors corrected for day of inquiry are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C.3 Linear probability model estimates, interactions with furniture.

	Housing brokers			Apartment owners		
	(1.A)	(1.B)	(1.C)	(2.A)	(2.B)	(2.C)
Constant	0.792***	0.694***	0.712***	0.720***	0.841***	0.818***
(Not blind, no dog)	(0.043)	(0.165)	(0.197)	(0.066)	(0.077)	(0.010)
β_1^	0.025	0.028	0.037	-0.191	-0.224*	-0.248*
(Blind, guide dog)	(0.054)	(0.057)	(0.060)	(0.120)	(0.124)	(0.128)
β_2^	-0.120*	-0.114	-0.114	-0.151	-0.174*	-0.207*
(Not blind, pet dog)	(0.064)	(0.070)	(0.071)	(0.103)	(0.104)	(0.104)
β_3^	0.057	0.051	0.070	0.073	0.053	0.042
(Furniture)	(0.055)	(0.051)	(0.062)	(0.076)	(0.083)	(0.088)
β_4^	-0.067	-0.075	-0.094	-0.031	-0.005	0.013
(Blind, guide dog)×(Furniture)	(0.082)	(0.075)	(0.076)	(0.128)	(0.138)	(0.139)
β_5^	0.009	-0.013	-0.000	-0.061	-0.051	-0.038
(Not blind, pet dog)× (Furniture)	(0.100)	(0.101)	(0.102)	(0.127)	(0.132)	(0.139)
Control variables	N	Y	Y	N	Y	Y
Fixed-effects	N	N	Y	N	N	Y
R squared	0.019	0.031	0.074	0.041	0.061	0.123
N	419	409	409	564	525	525

Note: Missing apartment characteristics, including the type of agent, cause the total sample to be smaller than 999 observations. Group A, which comprises married tenants, is the reference group. Robust standard errors corrected for day of inquiry are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C.4 Linear probability model estimates, interactions with advertiser's gender.

	Housing brokers			Apartment owners		
	(1.A)	(1.B)	(1.C)	(2.A)	(2.B)	(2.C)
Constant	0.875***	0.644***	0.708***	0.714***	0.797***	0.781***
(Not blind, no dog)	(0.053)	(0.192)	(0.240)	(0.047)	(0.072)	(0.083)
β_1^	0.101*	0.115*	0.107	-0.164**	-0.182**	-0.188**
(Blind, guide dog)	(0.060)	(0.063)	(0.079)	(0.075)	(0.073)	(0.073)
β_2^	-0.011	0.004	0.016	-0.120**	-0.148**	-0.170***
(Not blind, pet dog)	(0.066)	(0.069)	(0.075)	(0.058)	(0.060)	(0.061)
β_3^	0.125**	0.136**	0.155**	0.141**	0.140**	0.129*
(Female)	(0.053)	(0.056)	(0.065)	(0.057)	(0.059)	(0.068)
β_5^	-0.165**	-0.163**	-0.170**	-0.153*	-0.152*	-0.163**
(Blind, guide dog)×(Female)	(0.073)	(0.071)	(0.087)	(0.083)	(0.082)	(0.080)
β_5^	-0.082	-0.096	-0.109	-0.166*	-0.144	-0.144
(Not blind, pet dog)×(Female)	(0.090)	(0.093)	(0.095)	(0.089)	(0.091)	(0.097)
Control variables	N	Y	Y	N	Y	Y
Fixed-effects	N	N	Y	N	N	Y
R squared	0.039	0.069	0.115	0.047	0.068	0.127
N	230	226	226	553	514	514

Note: Missing apartment characteristics, including the type of agent, cause the total sample to be smaller than 999 observations. Group A, which comprises married tenants, is the reference group. The reference macro region is Southern Italy. Robust standard errors corrected for day of inquiry are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.