Gender and Gender Identity in the Rental Housing Market: Evidence from a Correspondence Study

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We present novel evidence from the first correspondence study on discrimination based on a signal of gender identity in the rental housing market. Eight hundred fictitious letters were sent to the landlords of 800 available apartments in a preregistered trial. We find that a signal of having changed gender, indicated by a name change, does not significantly decrease the positive response or invitation to a showing rate compared to nongender changing applicants. However, changing from a female to a male name led to fewer positive responses, while changing from a male to a female name led to fewer direct invitations compared to a nongender changing female applicant. The intention-to-treat main null result is robust to instrumenting an indication of compliance with the transgender treatment.

JEL: J15, J16, J71, R21 **Keywords:** Correspondence test, Transgender people, Discrimination, Housing market, Internet

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I. Introduction

Survey evidence suggests that discrimination against transgender people is widespread in Western societies (European Union Agency for Fundamental Rights, 2014; James et al., 2016). More than half of the transgender individuals in Europe perceive that they have endured discrimination or some other form of harassment in the last year (European Union Agency for Fundamental Rights, 2014). In a USbased study, approximately one-fourth of the respondents reported discrimination in hiring and within the workplace, and one-fifth reported discrimination regarding housing in 2015 (James et al., 2016). The deep fear of discrimination and mistreatment has further been noticed in health care avoidance (Kcompt et al. 2020), and it could be one of the factors contributing to the high rates of suicide and hospitalization that affect the transgender minority population (Dhejne et al., 2011). Despite all this, studies that cover this topic mainly rely on self-reported experiences (e.g., Schilt and Wiswall, 2008; Grant et al., 2011; European Union Agency for Fundamental Rights, 2014; James et al., 2016); few experimental studies have been carried out. Key exceptions are the work of Granberg et al. (2020), which presents evidence from a correspondence study on the Swedish labor market, and Button et al. (2020), who investigate differences in access to mental health treatment using an audit study.

We present novel evidence from a preregistered online correspondence study¹ in which we investigate how a signal of transgender identity affects discrimination in the rental housing market in Sweden. Specifically, we look at applicants who signal

¹ https://www.socialscienceregistry.org/trials/7566

a recent name change to a name of the opposite gender (henceforth, we refer to these individuals as transgender applicants). Between April 17 and May 2, 2021, we sent applications to 800^2 listings on the Swedish site Blocket.se, which is known for its large market for rental apartments, among other things. To signal the gender identity of the applicant, we indicated a name change in the message to the landlord by providing the former name in parenthesis after the new name and in the email address used when responding to the ad.³ We randomly allocated different signals of gender identity to each application and then recorded whether the applicant received an invitation for further contact and in that case, if the positive response also contained a direct invitation to a showing.⁴ Our results showed no significant discrimination against transgender people compared to nontransgender (henceforth, cisgender⁵) people in the Swedish rental housing market, with estimates implying that transgender applicants received 0.022 fewer invitations to further contact and 0.016 fewer direct invitations to apartment showings. This can be compared to the negative difference between trans- and cisgender people in the labor market in Granberg et al. (2020) of 0.06, which we used in our power calculation.

 $^{^2}$ The sample size was determined using past estimates in the literature along with a power calculation; see Section II.D.

³ This specific method for investigating treatment was chosen based on the previous literature (Granberg et al., 2020) and an interview with a transgender individual. A drawback of the correspondence study design is that we cannot know how the landlords perceive our signal. A possible interpretation of our results is thus that the landlords reacted to the genderincongruent information in our applications. To investigate this further, we ran a survey which unfortunately had a very low response rate. The results are presented in the online appendix section B. Furthermore, we view this treatment signal as an individual who recently decided to adopt a name change to better reflect their gender identity. Observational studies have suggested that having gender-concordant IDs improve mental health among transgender individuals; see Scheim et al. (2020). Our experimental design does not however allow us to separately study people with a nonbinary gender identity, as we have no separate treatment status for these individuals.

 $[\]frac{4}{4}$ It is common in the correspondence literature to have multiple outcome variables, depending on how positive the response is. In the rental housing market literature, it is usually recorded if there is a positive response with an invitation to further contact, and if this positive response also contains a direct invitation to a showing (see for instance Ahmed and Hammarstedt (2008), Ahmed et al. (2008), Ahmed and Hammarstedt (2009) and Ahmed et al. (2010)). Responses coded as a direct invitation to a showing is also always coded as an invitation to further contact.

⁵ Cisgender is a common term used to describe a person whose gender identity corresponds to their gender assigned at birth.

The lack of a general effect, however, masked a tendency for landlords to treat transgender applicants somewhere between cis women and cis men. Specifically, cis women fared the best among the applicants, while cis men fared worst, with 0.108 fewer invitations to further contact and 0.09 fewer direct invitations to apartment showings compared to the cis women. The trans men, on the other hand, were treated similarly to cis men in terms of invitations for further contact, with 0.126 fewer such responses compared to cis women, but no significant effect on direct invitations. For trans women, there was instead an indication that they received fewer direct invitations to apartment showings; the estimate implies 0.074 fewer direct invitations compared to cis women, which is significant at the 10 percent level.

As in any correspondence study, our main estimates should be interpreted as intention-to-treat (ITT) effects, implying that we do not know if the landlords did notice and perceive the signal of transgender status correctly. If some landlords do not notice the signal and thus fail to follow the treatment protocol, our estimates should be biased toward zero, as we would classify nontreated individuals as treated.⁶ Moving beyond our preregistration plan, we documented any form of uncertainty in the treatment signal on behalf of the landlord in our communication. We then found that transgender applicants more often receive questions about their gender, while cis men less often received questions about whether they would live on their own, which we interpret as a disinterest in their relationship status, and they were addressed with the wrong name more often, which we interpret as a lower degree of respect for the applicant.

⁶ It is technically possible that our estimates are biased away from zero in case the landlord falsely interpret the signal as some other form of treatment, for instance as a fake application. However, we have a comparatively large rate of invitations to further contact compared to previous studies, which makes this less likely.

To test the robustness of our main ITT estimate for transgender individuals, we then define the group of complying landlords as those that did not need any more clarification on the gender, did not ask if the applicant would live on their own or did not use the wrong name in the communication. We then instrumented compliance by treatment assignment, which only marginally increased our point estimates to -0.025 for invitations to further contact and -0.019 for direct invitations to showings. This is based on a compliance rate of 0.88⁷, and to increase our estimates to the ex-ante expected -0.06, the compliance rate would have to be at most 0.37.⁸

Finally, our estimated discrimination effect for cis men was similar in size to that reported in previous studies (Ahmed and Hammarstedt, 2008; Bengtsson et al., 2012), suggesting that neither the COVID-19 pandemic nor the fact that we used a name change influenced these results.⁹

Ex-ante, the expected effect is not clear in our setting. There are indications that discrimination in the housing market is taste based (Ahmed et al., 2010), and previous studies investigating discrimination against homosexual couples tend to find a negative effect for gay but not lesbian couples compared to heterosexual couples (Ahmed et al., 2008; Ahmed and Hammarstedt, 2009; Gouveia et al., 2020; Schwegman, 2019). As gay men are viewed as less masculine than straight

⁷ Based on the simple Wald-estimator we get -0.022/0.88 = -0.025 and -0.016/0.88 = -0.019, the latter including rounding errors.

⁸ This maximum compliance rate is obtained by taking -0.22/-0.06 = 0.37. To collect more evidence on how landlords perceived our treatment signals we also ran a survey in several Facebook-groups for rental apartments in Sweden. Unfortunately, the response rate was very low. Nevertheless, the results are discussed in the online appendix section B.

⁹ It has been suggested to us that some transgender individuals might opt out of the regular rental housing market due to fears of harassment. This can especially holds true in our setting, given that transgender people in Sweden report above EU-average levels of experienced violence in the last 12 months (European Union Agency for Fundamental Rights, 2014). We do however still view our results as relevant for this group, as discrimination might be one other force driving them out of the ordinary rental housing market.

(Mitchell and Ellis, 2011), this difference in discrimination could reflect a distaste for gender transgressing individuals. This would also be in line with recent evidence that gay men tend to be disfavored in the labor market¹⁰ (Aksoy et al., 2019; Badgett et al., 2021).¹¹

There are also reasons for landlords to statistically discriminate against transgender individuals in the rental housing market, as transgender individuals tend to be younger, are less likely to be college educated and have worse labor market outcomes (Badgett et al., 2021). On the other hand, interactions between landlords and their tenants are typically infrequent, which, if it were taste-based discrimination, would lower our expectations of finding a significant effect. Finally, cis men typically fare worse than cis women in the rental housing market (Ahmed and Hammarstedt, 2008; Bengtsson et al., 2012). If transgender applicants are seen as less desirable than cis women but still better applicants than cis men, transgender applicants could end up being treated somewhere between these two identities.

Estimates of how large share of the population that defines themselves as transgender vary depending on which survey, country and age group that is used. Badgett et al. (2021) report an estimate of 0.5% for 35 US states, while the same estimate is 0.7% in the Gallup poll for 2022 in the US. Interestingly, for the younger gen-Z generation, the same number is 2.1% (Jones, 2022). In Sweden, an Ipsos survey estimated 3% to define themselves as transgender (Ipsos, 2021). Sweden is usually ranked as one of the most progressive countries in the world in terms of gender equality in the Global Gender Gap Report 2020 (World Economic Forum,

¹⁰ However, Aksoy et al. (2018) demonstrate, among other things, that there is no penalty for gay men in London, and that the estimated differences between homosexuals and heterosexuals are smaller for noncouples.

¹¹ Previous correspondence studies in the housing and labor markets have also tended to find larger discriminatory effects against gay men in the housing rather than the labor market, which would mean that the estimate in Granberg et al. (2020) should be seen as a low ex-ante estimate.

2020), and transgender people in Sweden typically have self-reported experiences of discrimination below the EU average (European Union Agency for Fundamental Rights, 2014). We therefore expect that a similar study performed in another country would find slightly larger effects.

Although the method of correspondence testing has become a common way to study discrimination against minority populations in the housing and labor markets (see, for example, Bertrand and Mullainathan, 2004; Bertrand and Duflo, 2017; Baert, 2018; Quillian et al., 2017; Kline et al., 2021; and for Sweden in particular, see Carlsson and Rooth, 2007; Ahmed and Hammarstedt, 2008, 2009; Ahmed et al., 2008, 2010, 2013; Ahmed and Lång, 2017, 2019; Carlsson and Eriksson 2019; Bengtsson et al. 2012 and Granberg et al., 2020), until now, only the study by Granberg et al. (2020) has used this method to study discrimination against transgender people in these markets.¹² Focusing on discrimination in the labor market, the authors found that discrimination was highest in occupations largely dominated by one particular gender, with no significant effect among firms with a relatively balanced gender composition.¹³

Attempts to study discrimination against transgender people have also been made using nonexperimental methods. Geijtenbeek and Plug (2018) used registry data to study the earnings of transgender workers in the Netherlands, finding a wage penalty of approximately 11% for male-to-female transgender workers when they

 $^{^{12}}$ However, as mentioned, Button et al. (2020) also use this method to study discrimination in access to mental health care.

¹³ Another experimental approach was conducted by Langowski et al. (2017), where discrimination against transgender people was investigated in the housing market in Boston through an in-person audit study. The results showed that 61% of transgender testers experienced discrimination, and they were 9% more likely than cisgender testers to receive a higher rental price. Similarly, Levy et al. (2017) performed an in-person audit study in Washington, DC and found that transgender applicants that disclosed their transgender identity where 11% less likely to be told that there where units available.

registered as females; only a slight effect was found when female-to-male transgender workers registered as males. The problem with using registry data, however, is that other omitted variables may have impacted wages (Neumark, 2018). Furthermore, according to Zeluf et al. (2016), only 34% of the transgender population in Sweden had legally changed their gender, leaving a large proportion of the group out of these studies.¹⁴ A few papers instead utilize survey data to study differences between transgender and cisgender individuals in labor market outcomes, typically finding that transgender people have worse outcomes than cismen (Shannon, 2022; Carpenter et al., 2022).¹⁵

In the next section of the paper, we outline the details of our correspondence study, the exact formulation of the treatment signal and application letters, and our power calculation from the preregistration plan. The results are provided in Section III, while Section IV concludes the paper.

II. Design of the correspondence study

A. Background and outline of the application process

To investigate discrimination against transgender people in the rental housing market, we used one of the largest online advertisement sites in Sweden: www.blocket.se. On this particular site, private individuals and firms can place advertisements to sell, purchase, and rent various objects, with rental apartments being one of the commonly advertised items. There is a commission paid for

¹⁴ We also add to the related literature that study the effect of minority names and labor market outcomes; see for instance Fryer and Levitt (2004) or Arai and Skogman Thoursie (2009).

¹⁵ We also contribute to the rapidly expanding literature on questions related to LGBTQ and economics. See for example Badgett (2020), Aksoy et al. (2020), Aksoy et al. (2022) or Folch (2022).

placing an ad on the site, but replying to an ad incurs no cost. The only requirement for creating an account is signing up using an email address and a password. A phone number is then added in a verification step, which is a new requirement for responding to the site's ads. However, the phone numbers are hidden from other users, and all contact is made through messages on the site. Furthermore, adding an individual profile to the account with additional information such as previous work experiences, interests, education, and pets is strictly voluntary. We chose to not create any profiles, instead providing simple applications in addition to the treatment signals.¹⁶

The preregistered correspondence test was carried out between April 17 and May 2, 2021. During this period, a total of 800 applications were sent in chronological order to landlords advertising rental vacancies on Blocket.se. To avoid raising suspicion among the landlords, we chose to send only one application to each landlord rather than sending matched applications. The rental vacancies were chosen without regard to size, cost or any other characteristic. The aim was to respond to the full set of ads on the site during this time period. However, each landlord was controlled to only be contacted once, and landlords who required/requested contact through phone calls, postal letter, or meeting up in person were not included in the study.¹⁷ We recorded the time and date for each

¹⁶ As we have not uploaded any adds ourselves, we do not know how common it is for users to submit applications without a public profile. However, our positive response rate is high, at approximately 60%, compared to previous studies (see for instance Ahmed et al. (2008), Ahmed and Hammarstedt (2008) or Ahmed and Hammarstedt (2009)). One of us also have applied for apartments without a public profile on Blocket, which went well, and we had no indications in our survey, although based on a small sample, that our applications would be fake. See section B in the online appendix for a discussion of the results from the survey. Furthermore, evidence in Ahmed et al. (2010) suggests that including more information about the applicant in the application increases the overall call back rate, but does not change the estimated discrimination effect.

¹⁷ We did not record how common it was that landlords required answers through other means, but it is not a common practice. Similarly, we did not monitor the email addresses we provided for responses there, and we do not know how common it is to include email addresses in the written message as we have no experience of posting adds ourselves. However,

application sent, as well as information regarding the vacancy, such as location, rental cost, rental size, number of rooms, and landlord characteristics.¹⁸

The time frame for attaining a response was limited to one week; beyond this limit, the observations were categorized as negative responses. Our experience during the data collection was that it was highly unlikely to get any call back past the first week, with most responses coming within the first two days.¹⁹ The attained responses were then divided into two categories based on whether they were positive, such that further contact and/or an invitation to a showing was suggested, or negative, in which the application was rejected for any potential reason. Information regarding the time and date of the response was also recorded. Finally, the number of positive responses that only included an invitation to a showing, without asking for further information, was noted. Each positive response that asked for further information or invited the applicant to show the vacancy was politely declined within a short period of time.²⁰

B. The manipulation of gender identity

To create fictitious applicants, we needed a way to signal gender identity. After interviewing a transgender individual, we decided to use a treatment design similar to the one used by Granberg et al. (2020), where a name change is used to signal a gender transition. However, we slightly altered the specific treatment. Specifically,

it is typically assumed that the first contact is through the messaging system on the site, as it is the easiest, and often the only, way to get in contact with the landlord. See the experimental instructions for an example screenshot of how an add and the response dialog box looks.

¹⁸ We did not, however, observe the landlord's ownership form of the apartment. In other words, we did not know if the landlord rented the apartment or was the legal owner.

¹⁹ Only a handful of responses was recorded after 7 days, not enough to affect our estimates in any meaningful way.

²⁰ The data collection was performed by Sofia Fritzson as part of her master thesis work at Linnaeus University. As mentioned in footnote 5, recording if a positive response also contains a direct invitation to a showing is standard in most of the literature, and was also done by Fritzson by simply noting if the landlord did invite the applicant to see an apartment.

Granberg et al. (2020) used the form "H Larsson (prev. L Larsson)" when writing the applicant's name. We instead chose to write "H. (L.) Larsson" and then indicated which was the former name through the use of an email address. The slight alteration in the treatment design was suggested in the interview with the transgender person as a more authentic way of incorporating a recent name change when applying for rental vacancies in the housing market. We should emphasize that the name change did not have to be made legal in our treatment design, as it might just as well indicate an individual who is testing to live with a new name or want to use a new name but not make a legal change for some reason.²¹ With the inclusion of the email address along with our slightly altered name formulation, we thus aimed to make the treatment signal clear but also authentic.²²

For the transgender applicants, the name change was from a male to a female first name for the trans women and from a female to a male first name for the trans men. The cisgender applicants, meanwhile, had a name change from one male name to another for the cis men and from one female name to another for the cis women. It is worth noting that it is not uncommon in Sweden to change one's name, regardless of whether it is associated with a gender change. Between 2013 and 2018, 95,090 individuals in Sweden changed their names (Granberg et al., 2020). Therefore, having the applicant indicate a name change in their application letter should not

²¹ Correlational evidence also suggests that having a gender-concordant name on documents is correlated with mental health among transgender individuals (Scheim et al., 2020).

²² We cannot know exactly how the landlords perceives the signal, and thus one possible interpretation of our results relates to how landlords react to gender incongruent information in applications. In Section III.B, we provide evidence on how often the landlords asked for more information on the gender identity of the applicant. We also ran an online survey, unfortunately with a very low response rate, from which the results are presented in online appendix section B. One concern is that the applicants could realistically have adopted a new email address at the same time as the name change, although changing one's email address is also arguably associated with some costs. Yet, we use the applicants preferred name in the text as well, which should alleviate any concerns about the interpretation of which is the former and the new name.

be viewed as an oddity for the landlord receiving the letter. The main concern would otherwise be that the indicated name changes would have affected the callback rate differently for the treatment and control groups, which could in the end potentially affect our estimated treatment effect. However, given that we obtained a high (approximately 60%) positive callback rate in general, which is in line with or slightly higher than those reported in previous studies in Sweden (Ahmed and Hammarstedt, 2008; Ahmed and Hammarstedt, 2009), it does not seem to be the case that the indicated name changes affected the general callback rate in any major way.²³

Thus, to carry out the study, we needed two fictious names for each applicant. However, since names might signal more than just gender, such as a person's socioeconomic status, the two names were randomly chosen out of five common Swedish names for each gender.²⁴ The names were drawn from the most common baby names in Sweden in 2000; the female names were Julia, Emma, Wilma, Hanna and Elin, while the male names were Filip, Oscar, William, Viktor and Simon. The two names per applicant were then matched with a common last name in Sweden, such as Andersson, Johansson, Karlsson, Nilsson, and Eriksson, with the last name being randomly assigned to each applicant. The decision to select the most used baby names from the year 2000 was made to attain common names of people who would now be approximately 20 to 30 years old. We aimed for this particular age group, as it is more common among young people to classify themselves as transgender (Gallup, 2022; Ipsos, 2021). Many of these youths will soon enter the rental housing market to obtain an apartment on their own for the first time, making

 $^{^{23}}$ As previously mentioned, we also ran an online survey to look further into this potential issue, and saw no evidence that either the treatment or the control signal was perceived as being fake. However, this is based on a very small sample. See online appendix, section B.

²⁴ See for instance Fryer and Levitt (2004) for a discussion on how socioeconomic signals in names can influence results in correspondence studies.

them a particularly relevant group to focus on in our study. The names we selected therefore lend our study both authenticity and real-world relevance. Furthermore, these names have been among the most common baby names since the late 1990s (Statistics Sweden, 2021), signaling a wide possible age span of the applicants. All names also signal that these are ethnic Swedes, and thus, we only study discrimination for this particular ethnic group. Apart from that, these names are common enough that people from any socioeconomic group could carry them.

The next step was to construct email addresses for each of the testers. In this case, one of the most common email providers was used: hotmail.com. Table 1 outlines the names and email addresses of each applicant.

[Insert Table 1 Here]

Finally, each applicant received an individual account on Blocket.se, but without a public profile, and a separate prepaid phone card was used for each applicant in the final step of the registration.

To contact each landlord, four differently formulated letters were randomly assigned to each application along with a random allocation of gender identity. Each letter consisted of an opening phrase in which the landlord was greeted and the applicant made an introduction of himself or herself and expressed interest in the rental vacancy that was advertised. The message was then ended with a courtesy and signed by the applicant with both the current name and the former name, where the former name was presented in parenthesis and in the email address. The exact formulation of the letters, after our translation to English, can be found in appendix Table $A1^{25}$, but the construction of each message was similar to what is shown in the example below:

Hi!

My name is Hanna, and I'm interested in the rental vacancy. Feel free to contact me if it is still available!

Best regards, Hanna (Simon) Andersson simon.r.andersson@hotmail.com

The message in the application letter may appear to be short and not contain much content. However, it is worth noting that this is a common way to express interest in rental vacancies in Sweden, particularly in internet-based searches. Therefore, the construction represents an approach that is appropriate for retaining the authenticity of the study. We chose not to construct any online profiles for the applicants and instead opted for simple applications. In doing so, we also avoided the Heckman-Siegelman critique (Heckman and Siegelman, 1993), where the variance in the estimate depends on the characteristics provided in the application. Specifically, since we did not provide any specific details about the applicants other than the generic names, the variance cannot depend on any other characteristic provided by us. However, this also means that we are not able to distinguish between statistical or taste-based discrimination, although previous research has

²⁵ The original Swedish messages and the declining message, along with a screenshot of how to respond to an add and some additional information can be found in the Experimental instructions.

indicated that discrimination in the rental housing market in Sweden is primarily taste-based (Ahmed et al., 2010).

C. Empirical specifications and intention-to-treat analysis

We use simple linear regressions to analyze the data, where we simply compare the means between the treated trans-applications to the cis-applications using the following specification:

(1) $Y_i = \alpha + \beta trans_i + \varepsilon_i$

where Y_i is one of our two dummy variables measuring either an invitation to further contact or a direct invitation to a showing and *trans_i* is a dummy indicating if the sent application contains a signal that it is from a trans person or not. We also provide specifications where we control for the collected characteristics of the apartment and landlord. This includes the apartment area in square meters, the number of rooms in the apartment, the rent in 1000 Swedish kronor, whether the apartment is located in one of the three major Swedish cities (Stockholm, Gothenburg or Malmö) and whether the landlord is a company. We also collected if the landlord was a male or female based on their names in the ad.²⁶ However, as the landlords that are companies do not have any gender, we do not include this as a control variable in our main specifications, since that would exclude some observations and make it impossible to control for if the landlord is a company.²⁷

 $^{^{26}}$ If the name is potentially gender neutral, it was recorded as missing, but no such names where observed in the data collection.

 $^{^{27}}$ Regressions where we control for landlord gender rather than the company dummy are provided in Table A2 in the appendix.

We will also use a similarly simple specification to investigate how the effect differs depending on the gender identity of the applicant:

(2) $Y_i = \alpha + \pi_1 cisman_i + \pi_2 transman_i + \pi_3 transwoman_i + \varepsilon_i$

where $cisman_i$ is a dummy for the applicant signaling being a cisman, $transman_i$ is a dummy for a transman and finally $transwoman_i$ is a dummy for a transwoman, thus leaving ciswomen as the control group in this specification.

It is important to stress here that, as in almost all RCTs, we should interpret our main estimates as intention-to-treat (ITT) estimates, since we do not know if the landlords notice and correctly interpret our treatment signal. In case the landlord does not notice our signal, our estimates will be biased toward zero, making it less likely that we will see a significant effect if there is one. A standard solution to this compliance issue is to instrument compliance, measured as those that did follow the treatment protocol, with the initial treatment assignment. In our setting, the compliers are those who both noticed and correctly interpreted the signal, a subgroup that unfortunately is unobservable to us. Instead, and outside of our preanalysis plan, we collected information on some common questions or odd responses we obtained frequently from the landlords. Specifically, the landlords frequently asked for clarifications on the applicants' gender. This was typically to know the exact gender of the applicant. It is common in the Swedish rental market for some landlords to hire only female or male applicants, despite this being an illegal practice, which could be why some landlords asked to know more about the gender of the applicant. We then also noted that some landlords asked if the applicant would be living on their own, and some also used the wrong name in the response letter. Interestingly, it was often not the old name, but some completely different that we had not provided. However, if one of the old names was used, we also recorded this as the wrong name being used.

We then first document to what degree these responses differ per treatment group, using similar specifications to equations (1) and (2) above, but where the outcome variable has been replaced by one of the three collected dummies for odd responses. After this, we take all applicants who are treated with the transgender signal but did receive one of the abovementioned odd responses from the landlord and move these to the control group. The remaining treated group we label the compliers and then instrument compliance with the initial treatment assignment to test the robustness of our main ITT estimates.

Finally, we use robust standard errors in all regressions.

D. Power calculation and ethics approval

In our preregistration plan, we calculated the required sample size using power calculations. The anticipated incidence was chosen to be 0.45 for Group 1 (cisgender) and 0.39 for Group 2 (transgender). The usual callback rate in previous studies that performed correspondence testing in the housing market was approximately 40-50% for the control groups (see Ahmed & Hammarstedt, 2008, 2009; Carlsson & Eriksson 2014). The anticipated effect size of 6 percentage points comes from the study by Granberg et al. (2020), in which transgender individuals were seen to have a six-percentage point penalty in the positive employer response rate compared to cisgender individuals in the labor market. Given that Ahmed and Hammarstedt (2009) reported about twice as large an effect in the rental housing market for homosexuals compared to the effect in the labor market (see Ahmed et al. (2013)), we expected that this assumed effect size would be in the lower region of the true effect. The standard deviation used was 0.3, based on the estimate reported by Bengtsson et al. (2012). Given these values and settling on a statistical power of 0.8 for 5% significance, we found that a required sample size of 788 observations in total would be needed, evenly split between cisgender and

transgender applications. With regard to this lower limit, we sought a sample size of 800 applications.

This type of study is not covered by the Ethics Review Act according to the Swedish Ethical Review Authority; an email confirmation of this is provided in the supplementary material.

III. Results

A. Results within the preregistered plan

In line with our preregistered analysis plan, we began by testing for successful randomization; the results are shown in Table 2.²⁸ No significant differences were noted between transgender and cisgender applicants (panel A), and there were no large discrepancies in apartment characteristics between the two groups. When we investigated randomization at the gender identity level (panel B), however, there was a 10% joint significance for whether the apartment was located in a metropolitan area. All other apartment characteristics were nonsignificant, and there were only small differences in the mean sizes. Given that we performed twelve hypothesis tests, we should indeed expect to find at least one significant coefficient at the 10% level, and thus, we concluded that the randomization was successful.

[Insert Table 2 Here]

²⁸ Compared to our preregistered analysis plan, we have added comparisons between transgender and cisgender individuals in Table 2, in addition to the comparisons by gender identity outlined in the preregistration plan.

We then examined the average differences in callback rates between the cisgender and transgender groups, in line with our preregistration plan; the results are shown in Table 3.²⁹ For both outcomes, there was no significant effect, with a coefficient that would imply 2.4 percentage point fewer invitations for further contact and 1.8 percentage point fewer direct invitations to a showing for the transgender individuals compared to the cisgender individuals. Furthermore, none of the estimated effects changed to any meaningful degree when we controlled for apartment and landlord characteristics. Unfortunately, we cannot rule out sizeable effects due to the wide confidence intervals. For example, the 95% confidence interval in column 1 of Table 3 includes negative coefficients down to -0.0926, implying that the main estimate in the study by Granberg et al. (2020) of -0.06, on which we based our power calculation, is included in the confidence interval.

[Insert Table 3 Here]

Next, in line with our preregistration plan, we explored how the effect varied by gender identity (see Table 4). First, regarding both the invitations for further contact and the direct invitations to a showing of an apartment, we observed a significantly lower rate for the cis men compared to the cis women. More specifically, a cis male applicant could expect 10.2 percentage point fewer invitations for further contact and 8.8 percentage point fewer direct invitations to a showing. This held true even when we controlled for underlying apartment and landlord characteristics. Reassuringly, these estimates are very close to those reported by Ahmed and

²⁹ Results where we control for landlord gender, rather than the landlord being a company, are presented in Table A2 in the appendix. The point estimates do not change much, but we lose some significance due to the smaller sample available when using this measure.

Hammarstedt (2008) and Bengtsson et al. (2012), suggesting that the COVID-19 pandemic has not affected gender discrimination in the rental housing market in Sweden. When we proceeded to look at the results for the trans men and trans women, we see that the trans men had a 12 percentage point lower positive callback rate than the cis women. There was no significant difference for the trans women, however. When we looked at the data for the direct invitations to showings, we see some evidence of discrimination against trans women; a 7.7 percentage point lower invitation rate compared to the cis women, significant at the 5 or 10% level, depending on preferred specification. We see no significant negative effect for the trans men compared to the cis women for this outcome, however.

A possible interpretation of these findings is that the landlords seemed hesitant to offer a direct invitation to show the apartment to a trans woman, possibly to gather more information on applicant gender first. For the trans man, it might be that those landlords who made the effort to seriously consider the application deemed that the applicant had been born as a woman, while those who did not reply to the application viewed the applicant as a man. However, we have tested several hypotheses now, and thus, one needs to be careful to interpret these findings before they have been replicated.³⁰

[Insert Table 4 Here]

³⁰ Correcting the p-values for the 8 hypothesis we are testing, including the two for the main transgender effect, using the Romano-Wolf method (rwolf2 in Stata) only the effect for the trans mans negative positive response and the cis mans direct invitations to a showing survives at the 10 percent significance level however. Thus, one should not draw too strong conclusions from these results.

In the online appendix, Tables A.A.1-A.A.6, we investigate potential heterogeneous effects by landlord gender, landlord being a company and whether the apartment is in one of the three major cities (Stockholm, Gothenburg or Malmö) in Sweden. Overall, we found no significant differences between the different groups.³¹

B. Results outside the preregistered plan; sensitivity of the ITT estimates

During our data collection process, we noted a few recurring questions in the landlord responses that we had not considered in the preregistration plan. To be transparent about these unexpected responses and to investigate the robustness of our results, we decided to record these questions in our data. More specifically, we noticed that it was quite common for the landlord to ask for further details on the gender of the applicant, to ask if the applicant was going to live on their own, and to use the wrong name (however, usually not the former name) in communication with the applicant. Table 7 provides regressions outlining the differences between the various treatment groups for these three outcomes. Panel A shows that the transgender applicants were 5.5 percentage points more likely than the cisgender to be asked a question about their gender, but there was no difference with regard to being asked if they would live on their own. Furthermore, a significant negative effect was shown by the 2.7 percentage point lower probability of being called by the wrong name for the transgender applicants compared to the cisgender.

As shown in panel B, the trans men and the trans women were approximately equally likely to be asked about their gender (5.1 and 5.5 percentage points more than cis women, respectively), with no significant effect for the cis men. However,

³¹ If anything is to be said based on this analysis, it is that the coefficients are slightly larger outside of the major cities, and that there is an indication that male landlords might treat trans women similar to cis women in terms of positive responses, while female landlords treat trans men similar to cis women in terms of direct invitations.

the cis men were less likely to be asked if they would be living on their own, with point estimates of negative 0.05 compared to cis women. Regarding the probability of being addressed by the wrong name, the cis men were overrepresented by 4.9 percentage points compared to cis women, with no effects for either of the transgender applicants.

We thus concluded that the landlords seemed to be asking for more information on gender for both the trans men and the trans women, indicating that the applicant's gender assigned at birth was seen as crucial information to the landlord. Meanwhile, the cis men were less frequently asked for information on their relationship status and were treated with less respect in general, as measured by the probability of being addressed with an incorrect first name.

[Insert Table 5 Here]

All of these responses were significantly linked to some of our treatment signals. Furthermore, as we noted in section II.C, in any correspondence study, we typically only observe the ITT estimate and thus do not know anything about compliance. We can, however, use the measures we collected as indications that the landlord did not at least fully perceive the treatment signal. Thus, we take the observations that did get the transgender treatment but also received questions from the landlords and coded these as noncompliers, along with the nontreated cisgender applications. The remaining treated observations were then coded as compliers. We can then use the standard approach of instrumenting the compliance rate by the initial treatment assignment. These results are provided in Table 6.

[Insert Table 6 Here]

The first column gives the first-stage relationship, which shows that, using our indication of compliance measure, we have a compliance rate of 0.88. The second and third columns then display the 2SLS estimated effects for the compliers. Both estimated coefficients increase only marginally, from -0.022 to -0.025 for an invitation to further contact and from -0.016 to -0.019 for direct invitations to showings. This is simply due to the high compliance rate at 0.88. We can use the standard formula for the Wald estimator to back out what the compliance rate can be at most to obtain our ex-ante expected effect of -0.06. Specifically, if we take our -0.22 estimate and divide by -0.06, we obtain a maximum compliance rate of 0.37 to obtain the same effect size as we expected in our preanalysis plan.³²

Finally, in Table A3 in the appendix, we pool the transgender treatment together once again but now drop all cis men from the control group. Thus, we compare both trans men and trans women jointly against cis women only. When we do this, we see point estimates of -0.075 for invitations to further contact and -0.063 for direct invitations to showings, which are significant at the 10% level. These coefficients are slightly lower than the estimated difference of -0.102 and -0.088 for the cis man, indicating that the transgender applicants are treated somewhere between the cis woman and the cis man.

IV. Conclusion

In this paper, we report the first results of a correspondence study investigating discrimination when an applicant signals a transgender identity in the rental housing

 $^{^{32}}$ As it is not as clear how we should handle compliance when we analyze the data based on the gender identities, we do not perform a similar exercise for these estimates. However, in Table A.A.6 in the online appendix we instead include the measures for signal uncertainty as control variables in both the baseline and the gender identity models. This gives slightly more negative coefficients, but does not alter our conclusions.

market. Our main results and the data collection process were both preregistered. We found that a signal of transgender identity had no significant general discrimination effect in the rental housing market in Sweden. Furthermore, this conclusion from the ITT estimates was robust to instrumenting an indication of compliance with the initial treatment assignment. For the true compliance estimate to be of similar magnitude to the -0.06 demonstrated in the labor market (Granberg et al., 2020), the compliance rate with the treatment would have to be 0.37 at most.

We detected an interesting pattern in differences between the various gender identities, however. Specifically, the cis men received fewer positive callbacks and direct invitations to a showing compared to cis women, which is in line with the previous literature. Furthermore, the treatment of both the trans men and the trans women seemed to be somewhere between the treatment of the cis women and the cis men: The trans men received fewer positive callbacks but not significantly fewer direct invitations to a showing than the cis women, while trans women did not receive significantly fewer positive callbacks but had significantly fewer direct invitations to a showing. Once we moved beyond our preregistration plan, we also observed that both the trans men and the trans women were asked more questions about their gender and that cis men received fewer questions regarding whether they would live on their own and were more often addressed with the wrong name.

A desire for authenticity guided us throughout our design process, where we aimed to signal gender identity clearly. A drawback of conducting a correspondence study is that we cannot know exactly how the landlords interpreted the signal. One possible interpretation of our results is thus that the landlords reacted to gender incongruent information in applications rather than to the applicant's transgender identity. Future research should thus investigate how different types of signals affect the callback rate in the rental housing market and try other signals of gender identity.

Although we did not observe any significant discrimination against transgender people compared to cisgender people in our study's setting, it is worth pointing out that there might still be other important forms of discrimination or factors that keep transgender people out of the rental housing market. For instance, a fear of violence or mistreatment might cause transgender individuals to seek housing through other channels, such as personal contacts. It might also be the case that a landlord's preference for applicants who are not transgender, and thus discrimination, only becomes apparent once the landlord and the prospective tenant meet, perhaps at a first showing. Given that in-person audit studies have found quite large effects (Levy et al., 2017; Langowski et al., 2017), future research should try to disentangle exactly where and how discrimination might occur. Other points to address are replication and deeper exploration of the differences in the observed treatment effects between the trans men and the trans women and the establishment of a more accurate estimate of the general transgender effect through the use of a larger sample size. In appendix section B, we provide updated power calculations based on our findings in this paper. These results suggest that a sample size of 13,090 observations would be needed to estimate a significant effect.

Data availability

Data and code for replicating the results in this paper can be obtained from Joakim Jansson's home page at http://sites.google.com/site/joakimjanssoneconomist.

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TABLES

Table 1: The fictitious applicants used in the study

Name	Gender identity	Email address
Emma (Elin) Karlsson	Cis woman	elin.p.karlsson@hotmail.com
Filip (Viktor) Nilsson	Cis man	viktor.t.nilsson@hotmail.com
Hanna (Simon) Andersson	Trans woman	simon.r.andersson@hotmail.com
Oscar (Julia) Johansson	Trans man	julia.v.johansson@hotmail.com

Note: The table displays the different fictious applicant names, gender identities and email addresses used in the applications to the ads.

	Panel A: Trans – Cis com	parison	
	Cis person	Trans person	p-value difference
Anartment area in sq meters	49 983	50 274	(0.885)
Number of rooms	2.183	1.950	(0.334)
Apartment rent in 1000 SEK	9.114	9.144	(0.922)
Metropolitan area	0.557	0.565	(0.817)
Female landlord	0.426	0.484	(0.129)
Company	0.154	0.128	(0.290)
Observations	402	398	

Table 2: Balance test

Panel B: Gender identity comparison

	Cis woman	Cis man	Trans woman	Trans man	Joint p-value
Apartment area in sq meters	50.568	49.409	47.687	52.914	(0.317)
Number of rooms	1.985	2.377	1.873	2.028	(0.291)
Apartment rent in 1000 SEK	9.576	8.661	8.989	9.301	(0.199)
Metropolitan area	0.618	0.498	0.592	0.538	(0.067)
Female landlord	0.432	0.421	0.483	0.485	(0.505)
Company	0.151	0.158	0.114	0.142	(0.584)
Observations	199	203	201	197	

Note: Each column except the last one displays the mean for each treatment group for the different control variables, which are different apartment and landlord characteristics. Apartment area in sq meters is the size of the apartment in square meters, number of rooms is the number of rooms in the apartment, apartment rent in 1000 SEK is the monthly rent for the apartment in 1000s of Swedish krona, metropolitan area is a dummy for if the apartment lies in one of the larger cities in Sweden (Stockholm, Göteborg or Malmö), female landlord is a dummy for if the landlord is a female or male (missing if the landlord is a company or has a gender neutral name) and company is a dummy for if the landlord is a company. The final column in panel A shows the p-value from a t-test for the difference between the two means. In panel B, the final column shows the p-value from an F-test for the joint probability that the different treatments have equal means. The number of observations for the variable female landlord is slightly lower, as landlords that are companies have no gender. Thus, in panel A column 1, the number of observations for female landlord is 340, and in column 2, it is 347. Similarly, in panel B, it is 169 in column 4.

Table 3: General effect				
	(1)	(2)	(3)	(4)
	Further contact	Inv to showing	Further contact	Inv to showing
Transgender	-0.024	-0.018	-0.022	-0.016
	(0.035)	(0.026)	(0.035)	(0.026)
Apartment area in sq meters			-0.001	-0.000
			(0.001)	(0.001)
Number of rooms			0.003**	-0.003****
			(0.001)	(0.001)
Company			0.036	0.098**
			(0.052)	(0.045)
Metropolitan area			-0.118***	-0.029
-			(0.043)	(0.031)
Apartment rent in 1000 SEK			0.012**	0.002
L			(0.006)	(0.004)
Constant	0.602***	0.172***	0.579***	0.175***
-	(0.024)	(0.019)	(0.050)	(0.036)
N	800	800	800	800

Note: The table displays the estimated effect of the treatment, the transgender dummy, on receiving an invitation to further contact in columns 1 and 3 and being directly invited to a showing in columns 2 and 4. The control variables in columns 3 and 4 are apartment area in sq meters, which is the size of the apartment in square meters, number of rooms, which is the number of rooms in the apartment, apartment rent in 1000 SEK, which is the monthly rent for the apartment in 1000s of Swedish krona, metropolitan area, which is a dummy for if the apartment lies in one of the larger cities in Sweden (Stockholm, Göteborg or Malmö) and company, which is a dummy for if the landlord is a company. The gender of the landlord is not included as a control due to missing values for landlords that are companies making it impossible to include both in the regressions. Robust standard errors in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 4:	Effect	by	identity
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	(1)	(2)	(3)	(4)
	Further contact	Inv to showing	Further contact	Inv to showing
Cis man	-0.102**	-0.088**	-0.108**	-0.090**
	(0.049)	(0.038)	(0.049)	(0.038)
Trans woman	-0.031	-0.077**	-0.028	-0.074^{*}
	(0.048)	(0.038)	(0.048)	(0.038)
Trans man	-0.120**	-0.049	-0.126**	-0.049
	(0.049)	(0.040)	(0.049)	(0.040)
Apartment area in sq meters			-0.001	-0.000
			(0.001)	(0.001)
Number of rooms			0.003***	-0.003***
			(0.001)	(0.001)
Company			0.035	0.096^{**}
			(0.051)	(0.045)
Metropolitan area			-0.125***	-0.033
			(0.043)	(0.031)
Apartment rent in 1000 SEK			0.012**	0.002
			(0.006)	(0.004)
_				++++
Constant	0.653***	0.216***	0.638***	0.227***
	(0.034)	(0.029)	(0.056)	(0.044)
Ν	800	800	800	800

Note: The table displays the estimated effect of the different treatments, the gender identity dummies, on receiving an invitation to further contact in columns 1 and 3 and being directly invited to a showing in columns 2 and 4. The control variables in columns 3 and 4 are apartment area in sq meters, which is the size of the apartment in square meters, number of rooms, which is the number of rooms in the apartment, apartment rent in 1000 SEK, which is the monthly rent for the apartment in 1000s of Swedish krona, metropolitan area, which is a dummy for if the apartment lies in one of the larger cities in Sweden (Stockholm, Göteborg or Malmö) and company, which is a dummy for if the landlord is a company. The gender of the landlord is not included as a control due to missing values for landlords that are companies making it impossible to include both in the regressions. Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 5: Uncerta	ainty of signal		
	(1)	(2)	(3)
	Wondering about	Asking if living	Using wrong
	gender	on own	name
	Pan	iel A	
Transgender	0.055^{***}	0.005	-0.027**
-	(0.015)	(0.012)	(0.012)
Constant	0.017***	0.025***	0.045***
	(0.007)	(0.008)	(0.010)
	Par	iel B	
Cis man	-0.005	-0.050***	0.049**
	(0.013)	(0.016)	(0.020)
Trans woman	0.055***	-0.010	-0.015
	(0.021)	(0.021)	(0.011)
Trans man	0.051^{**}	-0.030	0.010
	(0.021)	(0.019)	(0.016)
Constant	0.020^{**}	0.050***	0.020**
	(0.010)	(0.016)	(0.010)
N	800	800	800

Note: The table shows the coefficients from simple regressions between the different treatment dummies, transgender in panel A and the different gender identities in panel B, and dummies for if the landlord asked for more information on the gender of the applicant (column 1), if the applicant would be living on their own (column 2) or if the landlord used the wrong name (column 3). Robust standard errors in parentheses. * p < 0.10, *** p < 0.05, **** p < 0.01

	(1)	(2)	(3)
	Compliers	Further contact	Inv to showing
Transgender	0.883^{***}		
	(0.016)		
Apartment area in sq meters	-0.000	-0.001	-0.000
	(0.000)	(0.001)	(0.001)
Number of rooms	-0.001	0.003**	-0.003***
	(0.001)	(0.001)	(0.001)
Company	0.033	0.036	0.099^{**}
	(0.022)	(0.052)	(0.045)
Metropolitan area	0.013	-0.118***	-0.029
	(0.019)	(0.043)	(0.031)
		**	
Apartment rent in 1000 SEK	-0.003	0.012**	0.002
	(0.002)	(0.006)	(0.004)
~			
Compliers		-0.025	-0.019
		(0.039)	(0.029)
	0.042*	0 =00***	0 1 ***
Constant	0.043	0.580	0.176
	(0.024)	(0.050)	(0.037)
H-statistic	3016.769		
Estimation type	First stage	IV	IV
N	800	800	800

Table 6: Compliance instrumented by transgender treatment

Note: The table instruments compliance with transgender treatment, where compliance is measured as being in the transgender treatment group and not asking any clarifying questions about gender, not asking if the applicant will be living on their own or not using the wrong name in addressing the applicant. If these conditions are not met, the observation is classified as a noncomplier and thus in the control group. The first column provides the first-stage relationship between the transgender treatment group and being a complier, conditioning on the control variables. The second and third columns instrument compliance with the transgender treatment and look at the relationship with an invitation to further contact and an invitation to show an apartment using 2SLS estimators. The control variables are apartment area in sq meters, which is the size of the apartment in square meters, number of rooms, which is the number of rooms in the apartment, apartment rent in 1000 SEK, which is the monthly rent for the apartment in 1000s of Swedish krona, metropolitan area, which is a dummy for if the apartment lies in one of the larger cities in Sweden (Stockholm, Göteborg or Malmö) and company, which is a dummy for if the landlord is a company. The gender of the landlord is not included as a control due to missing values for landlords that are companies making it impossible to include both in the regressions. Robust standard errors in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

APPENDIX

A. Additional tables

Table A1: The application letters (1-4) in English (our translation)

1) *Hey! My name is... and I'm interested in the apartment you are renting out. Feel free to contact me if it is still available!*

Best wishes, ... (...) ..., v.nilsson@hotmail.com

2) *Hey! My name is... and I'm interested in the apartment you rent. Feel free to get in touch with me if rental is still relevant!*

Best, ... (...) ..., v.nilsson@hotmail.com

3) *Hey! My name is... and I think your apartment looks interesting! Feel free to contact me if it is still available for rent!*

Best,

... (...) ..., v.nilsson@hotmail.com

4) *Hey! My name is... and I have an interest in the apartment you rent out. Feel free to get in touch with me if it is still available!*

Best wishes, ... (...) ..., v.nilsson@hotmail.com

	8 8	8		
	(1)	(2)	(3)	(4)
	Further contact	Inv to showing	Further contact	Inv to showing
Transgender	-0.037	-0.000		
	(0.038)	(0.027)		
Apartment area in sq meters	-0.002	0.000	-0.002	0.000
	(0.002)	(0.001)	(0.002)	(0.001)
Number of rooms	0.037	-0.021	0.037	-0.022
	(0.044)	(0.033)	(0.043)	(0.032)
				0.070*
Female landlord	0.010	-0.050	0.010	-0.050
	(0.038)	(0.027)	(0.038)	(0.027)
	0 11 4**	0.022	0.100***	0.025
Metropolitan area	-0.114	-0.033	-0.120	-0.035
	(0.046)	(0.032)	(0.046)	(0.032)
Apartment rent in 1000 SEK	0.012**	0.003	0.012*	0.003
- F	(0.006)	(0.005)	(0.006)	(0.005)
	(0000)	(0.000)	(00000)	(00000)
Cis man			-0.091*	-0.104***
			(0.053)	(0.039)
				. ,
Trans woman			-0.041	-0.065
			(0.052)	(0.040)
Trans man			-0.127**	-0.040
			(0.053)	(0.042)
Constant	0.566***	0.199***	0.617***	0.257***
	(0.058)	(0.042)	(0.064)	(0.049)
Ν	687	687	687	687

Table A2: Effects	when	controlling	for	landlord	gender
		• • • • • • • • • • • • • • •			

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Note: The table displays the estimated effect of the different treatments on receiving an invitation to further contact, in columns 1 and 3, and being directly invited to a showing, in columns 2 and 4. Columns 1 and 2 look at the effect for transgender against cisgender, while columns 3 and 4 use the cisman, transman and transwoman treatment dummies. The control variables are apartment area in sq meters, which is the size of the apartment in square meters, number of rooms, which is the number of rooms in the apartment, apartment rent in 1000 SEK, which is the monthly rent for the apartment in 1000s of Swedish krona, metropolitan area, which is a dummy for if the apartment lies in one of the larger cities in Sweden (Stockholm, Göteborg or Malmö) and company, which is a dummy for if the landlord is a company. Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

	(1)	(2)	(3)	(4)
	Further contact	Inv to showing	Further contact	Inv to showing
Transgender	-0.075*	-0.063*	-0.077^{*}	-0.064*
	(0.042)	(0.034)	(0.042)	(0.034)
Apartment area in sq meters			-0.000	0.000
			(0.002)	(0.001)
Number of rooms			0.013	-0.016
			(0.046)	(0.035)
Company			0.059	0.052
			(0.059)	(0.053)
Metropolitan area			-0.125**	-0.046
			(0.049)	(0.037)
Apartment rent in 1000 SEK			0.007	0.002
			(0.007)	(0.005)
	o ****	0 01 -***	0 < 10***	0.00***
Constant	0.653	0.216	0.649	0.230
	(0.034)	(0.029)	(0.063)	(0.052)
Ν	597	597	597	597

Table A3: General effect without cis men in the control group

Note: The table displays the estimated effect of the treatment, the transgender dummy, on receiving an invitation to further contact in columns 1 and 3 and being directly invited to a showing in columns 2 and 4 while excluding cis male applicants from the control group. The control variables in columns 3 and 4 are apartment area in sq meters, which is the size of the apartment in square meters, number of rooms, which is the number of rooms in the apartment, apartment rent in 1000 SEK, which is the monthly rent for the apartment in 1000s of Swedish krona, metropolitan area, which is a dummy for if the apartment lies in one of the larger cities in Sweden (Stockholm, Göteborg or Malmö) and company, which is a dummy for if the landlord is a company. The gender of the landlord is not included as a control due to missing values for landlords that are companies making it impossible to include both in the regressions. Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

B. Updated power calculation

We updated the power calculations for how large the sample needed to be to identify a 5% significance effect with 0.8 power for the general transgender discrimination.

In our data, we recorded invitations for further contact for 60.2% of the cisgender applicants and 57.8% of the transgender applicants, with a standard deviation of 0.49. Using these measures as inputs for our power calculation, we computed that at least 13,090 observations would be needed to detect a significant effect.

Although we observed no change in the rate of discrimination compared to previous studies conducted before the COVID-19 pandemic, it is possible that the pandemic increased the standard deviation in our sample period. In our preregistration plan, we based our power calculation on the standard deviation of 0.3 reported by Bengtsson et al. (2012). However, as noted above, we recorded a standard deviation of 0.49 in our data. If this increase is due to the pandemic's effect on the rental housing market, we might expect the standard deviation to decrease again once the pandemic is over. If we thus base our power calculation on our point estimates but with the lower standard deviation recorded in the previous literature, we would obtain a required sample size of 4,908 observations.