



Living in the gender spectrum: Evidence from non-cisgender applications in the rental housing market

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ABSTRACT

We present novel evidence from the first correspondence study investigating the effect of individual non-cisgender signals in the housing market. In a preregistered trial, 800 fictitious letters were sent to rental apartment landlords in Sweden. Cisfemale applicants received fewer positive responses compared to ciswomen, while non-cisgender applicants had response rates that fell between those of ciswomen and cismen. The effects were strongest for apartments located outside of major cities. Non-cisgender applicants were also more often asked to clarify their gender. Additionally, cisfemale applicants were more likely to be addressed by the wrong name and were less frequently asked if they would bring any cohabitants.

1. Introduction

Survey evidence suggests that discrimination against non-cisgender people, i.e. individuals whose current gender does not align with the sex assigned at birth,¹ is widespread in Western societies (European Union Agency for Fundamental Rights, 2014; James et al., 2016). For instance, 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 US-based study from 2015, approximately one-fourth of the respondents reported discrimination in hiring and within the workplace, and one-fifth reported discrimination regarding housing (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 non-cisgender minority population (Dhejne et al., 2011). Despite this, studies that cover this topic mainly rely on self-reported experiences (e.g., Schilt and Wiswall, 2008; Grant

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¹ Here, we follow the definition in Carpenter et al. (2022).

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et al., 2011; European Union Agency for Fundamental Rights, 2014; James et al., 2016) or focus on documenting differences in outcomes (e. g. Carpenter et al., 2022; Badgett et al., 2021; Kolk et al., 2023); few experimental studies have been carried out.² In particular, scant attention has been devoted to the treatment of non-cisgender applicants in the rental housing market.³ Yet understanding the determinants of where individuals reside is of great importance, as where you live determine several of your key living conditions and future life outcomes (Chetty et al., 2016; Chetty and Hendren, 2018; Chyn, 2018; Bergman et al., 2024).

We present evidence from the first preregistered online correspondence study examining the impact of non-cisgender status in single households on the rental housing market. Specifically, we analyze how two non-cisgender signals—a woman-to-man signal and a man-to-woman signal—influence the likelihood of receiving a positive response in Sweden’s rental market, compared to signals associated with cisgender males and females. Conducting our study in Sweden—a country renowned for its progressive stance on gender equality—adds additional significance to our findings.

Over the period spanning April 17 to May 2, 2021, we sent applications and collected responses to 800 listings on the Swedish site Blocket.se, renowned for its expansive rental apartment market. The sample size was determined using past estimates in the literature along with a power calculation; see Section 2.D. The implementation of the non-cisgender signal involved the provision of two given names, with the first followed immediately by a second enclosed within parentheses. The second given name (in parenthesis) was also reiterated in the email address provided at the end of the response text. Cisgender applicants were characterized by both given names aligning with a singular gender identity, while non-cisgender applicants featured one male and one female name.⁴ This treatment design was based on interventions used in the previous literature and an interview with a non-cisgender individual who suggested its use (see Granberg et al. (2020)). Each application was then randomly assigned a gender signal.⁵ Landlords clearly noticed the non-cisgender signal, as they were significantly more likely to ask these applicants to clarify their gender.

Ex-ante, the differential treatment of non-cisgender individuals in comparison to both cismen and ciswomen is uncertain. This uncertainty is however arguably tied to the factors influencing how cismen and ciswomen are treated in the market. The market could for instance be gender segregated, wherein some landlords exclusively rent to women and others to men. The presumption, in this scenario, would be that non-cisgender individuals might face more adverse treatment, given a perceived deviation from traditional gender categorizations. On the other hand, previous research has demonstrated the existence of discrimination against male applicants within the rental housing market, evidenced by approximately 10 percentage points lower positive

response rates compared to their female counterparts (Ahmed and Hammarstedt, 2008; Bengtsson et al., 2012). Thus, there might be some specific male trait(s) that the landlords wish to avoid for taste or economic reasons. Non-cisgender individuals might then end up being treated somewhere in between cismen and ciswomen, if they are partially perceived as having this unwanted cismale characteristic(s). Finally, non-cisgender individuals typically have worse economic outcomes than their cisgender counterparts, which could lead to lower positive call backs than cismen and ciswomen due to statistical discrimination (Carpenter et al., 2022). Thus, our paper also holds the potential to shed light on the mechanisms underlying the disparity in call back rates between cismen and ciswomen.

Our results first reassuringly replicate the gender difference found in prior research, finding that cismale applicants are consistently around 10 percentage points less likely than their cisfemale counterparts to receive positive responses.⁶ This replication underscores the robustness of our results and mitigates concerns such as the COVID pandemic or our research design having had any unforeseen consequences on our estimates. Second, our results indicate that non-cisgender applicants receive a positive response rate between that of cismen and ciswomen, with a point estimate around 0.06–0.07 lower than ciswomen.⁷ It is important to emphasize that cisgender men encompass both straight and gay men, and previous research has revealed that gay male couples are less likely to receive positive responses in the rental housing market compared to their heterosexual counterparts. No significant differences exist between lesbian and heterosexual couples, however (Ahmed et al., 2008; Ahmed and Hammarstedt, 2009; Schwegman, 2019; Gouveia et al., 2020). Thus, all cisgender men, irrespective of sexual orientation, appear to face some form of discriminatory treatment. In combination with our results, these findings suggest the presence of some general male characteristic(s) that landlords either statistically discriminate against or have a distaste for.⁸ To further investigate the underlying mechanisms, we analyze heterogeneous effects by apartment location. Residents of large cities generally hold more progressive attitudes toward gender stereotypes and sexual minorities, leading us to expect smaller effects for apartments located in major cities (Stockholm, Gothenburg, and Malmö). Consistent with this reasoning, we observe that the coefficients are more negative and statistically significant for apartments located outside these cities, particularly for cismale applicants.⁹

During data collection, we observed that some landlords addressed applicants with a given name different from those provided. These alternative names did however typically align with the applicants’ gender and ethnicity. Additionally, some landlords sought information regarding whether the applicant intended to live alone or with a cohabitant. Although not initially incorporated into our pre-analysis plan, we recorded these behaviors as two supplementary outcomes, to further help us understand the workings of the market. We found that the cismale applicants were 5 percentage points significantly less likely to be asked if they would be co-habiting than ciswomen, but no significant difference between the non-cisgender applicants and ciswomen. This suggests that if you constitute a heterosexual couple, you are better

² A key exception is the work by Granberg et al. (2020), which provides evidence from a correspondence study in the Swedish labor market. Additionally, a recent working paper presents the results of a correspondence study on the rental housing market in which applicants are randomly assigned different preferred nouns (Tomlin, 2023). Two non-peer-reviewed reports, based on smaller samples and employing in-person actors, examine the effects on transgender applicants in the US housing market (Levy et al., 2017; Langowski et al., 2017). Finally, an audit study by Fumarco et al. (2024) investigates disparities in access to mental health treatment.

³ An important exception is Abbate et al. (2024), whose study examines discrimination against couples with one transgender member in four South American countries.

⁴ Table 1 outlines the different treatment signals, while B.2 in the online appendix provide the application letters.

⁵ To avoid raising suspicion, we decided against sending matched applications to each landlord, as that approach would require sending four separate but very similar responses to each ad. Instead, we used random assignment of gender identity.

⁶ Like many previous correspondence studies conducted in the rental housing market (see for instance Ahmed and Hammarstedt, 2008, 2009; Ahmed et al., 2008, 2010), we recorded whether the applicant received a positive response and, in that case, if the positive response also contained a direct invitation to a showing of the property. The results do not vary in any significant way by these two outcomes; see the discussion of the results and Table 4 in section 3 for details.

⁷ This is equivalent to having a 0.03–0.04 higher positive response rate than men.

⁸ There are indications that discrimination in the housing market is taste based (Ahmed et al., 2010).

⁹ These results align with Bengtsson et al. (2012), which documented a significant discriminatory effect only in Stockholm’s city center, and not in its surrounding suburbs or satellite cities.

off letting the woman handle the application procedure, since the landlord is less likely to even bother asking if you are part of a couple otherwise. Furthermore, our results reveal that cis-men are 4.9 percentage points more likely to be addressed by the wrong name, again with no significant difference between non-cisgender applicants and ciswomen. Our interpretation of this is that the cis-men are sometimes treated with less respect in the application process.¹⁰

Our paper thus contributes to two strands of the economic literature. First, we contribute to the large and diverse literature on correspondence studies (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 et al., 2013](#); [Ahmed and Lång, 2017, 2019](#); [Carlsson and Eriksson 2019](#) and [Granberg et al., 2020](#)), where we in particular add to studies investigating gender and sexual orientation in the rental housing market ([Ahmed and Hammarstedt, 2008, 2009](#); [Ahmed et al., 2008, 2010](#); [Lauster, Easterbrook, 2011](#); [Bengtsson et al. 2012](#); [Friedman et al., 2013](#); [Carlsson and Eriksson, 2014](#); [Mazziotta et al., 2015](#); [Koehler et al., 2018](#); [Murchie and Pang, 2018](#); [Gouveia et al., 2020](#); [Hellyer, 2021](#)). Secondly, our research adds to the rapidly growing literature centered on non-cisgender and transgender individuals (see for instance [Geijtenbeek and Plug \(2018\)](#), [Badgett et al. \(2021\)](#), [Shannon \(2022\)](#), [Carpenter et al. \(2022\)](#), [Folch \(2022\)](#), [Kolk et al. \(2023\)](#)). Few correspondence studies utilize different transgender or non-cisgender signals, however. [Abbate et al. \(2024\)](#) is the first peer-reviewed study to examine the impact of a transgender signal in the housing market. Their findings indicate discrimination against couples with a transgender member in four South American countries. [Granberg et al. \(2020\)](#) offers insights from a correspondence study in the Swedish labor market, indicating transgender status through a name change. They find that transgender applicants are disfavored in male and female dominated occupations compared to the dominant gender. [Fumarco et al. \(2024\)](#) investigate differences in access to mental health treatment using an audit study, finding discrimination against transgender or non-binary African Americans and Hispanics. Finally, [Tomlin \(2023\)](#), in a recent working paper, explores the impact of stating a preferred pronoun when applying for rental apartments in the U.S. Their findings reveal a negative effect associated with using a preferred pronoun, but no additional adverse impact of indicating transgender status.

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 next provided in [Section 3](#), while [Section 4](#) concludes the paper.

2. Design of the correspondence study

2.1. Background and outline of the application process

To investigate discrimination of non-cisgender applicants in the rental housing market, we used www.blocket.se, one of Sweden's largest online advertisement sites. On this site, private individuals and firms can place advertisements to sell, purchase, and rent various objects, with

¹⁰ The fact that landlords occasionally used an incorrect name could raise concerns about our treatment design by suggesting that some may not have fully recognized or noticed the signal. However, because they typically substituted with a name that correctly reflected the applicant's gender and ethnicity, it is unlikely that the signal was misunderstood. Moreover, if the misnaming were solely due to our design, similar rates of incorrect name usage would be expected across all treatments. Instead, we observe a notably higher incidence among cismale applicants, supporting our interpretation that landlords allocated less time and effort to respond to this group. Further, results in online appendix Table A4 confirm that our results are robust to controlling for instances of incorrect name usage.

rental apartments being one of the commonly advertised items. While there is a commission associated with placing an ad, responding to an ad is cost-free. Account creation merely necessitates an email address, a password, and a phone number to start responding to ads. Importantly, the phone numbers remain concealed from other users, with all initial interactions occurring through on-site messaging. Adding an individual profile to the account with additional information such as previous work experiences, interests, education, and pets is voluntary. We chose to not create any such profiles, instead providing simple applications along with the treatment signals.¹¹ 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 indicated that discrimination in the rental housing market in Sweden is primarily taste-based ([Ahmed et al., 2010](#)).

The preregistered correspondence test was carried out between April 17 and May 2, 2021, during which 800 applications were dispatched to landlords advertising rental vacancies on Blocket.se and subsequent responses were collected. To avert arousing suspicion among landlords, we elected to send only one application to each landlord, avoiding matched applications. A matched application procedure would have required sending four separate, nearly identical emails to the same landlord; therefore, we opted for a non-matched approach. 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 period. However, landlords who required or requested contact through phone calls, postal letters, or meeting up in person were not included in the study. We recorded the time and date for each application sent, as well as information regarding the vacancy, such as location, rental cost, rental size, number of rooms, and landlord characteristics.¹²

A one-week response timeframe was established, beyond which observations were classified as negative responses. It was very uncommon that we got any call back past this first week; most responses were received within the first two days.¹³ In line with prior correspondence studies examining the rental market (refer, for example, to [Ahmed and Hammarstedt, 2008, 2009](#); [Ahmed et al., 2008, 2010](#)), we recorded whether the response was positive—that is, whether it included an invitation for further discussion. For each positive response, we further determined if the applicant was directly invited to inspect the apartment. This process allowed us to establish two primary outcomes: a general invitation for further contact and, within that group, a subset where the invitation extended to an immediate property viewing. Information regarding the time and date of the response was also recorded. Each positive response, irrespective of the subsequent invitation, was politely declined within a brief timeframe. This practice was implemented to ensure that our interactions with landlords remained ethically sound.¹⁴

¹¹ At approximately 60%, our positive response rate is high compared to previous studies (see for instance [Ahmed et al. \(2008\)](#), [Ahmed and Hammarstedt \(2008\)](#) or [Ahmed and Hammarstedt \(2009\)](#)), suggesting that our application profiles are not commonly seen as fake. 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.

¹² 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.

¹³ Only a handful of responses were recorded after 7 days, not enough to affect our estimates in any meaningful way.

¹⁴ The data collection was performed by Sofia Fritzson as part of her master thesis work at Linnaeus University.

We also collected some additional outcomes that were not in the pre-analysis plan. Specifically, landlords frequently sought clarifications regarding the applicants' gender, inquired about their cohabitation plans, and, on multiple occasions, used incorrect given names. Recognizing the relevance of these unanticipated yet significant aspects, we made a deliberate decision to record and incorporate this additional data into our analysis.

2.2. The manipulation of the gender signal

In devising fictitious applicants, we aimed to convey non-cisgender status authentically and clearly. Following insightful discussions with a non-cisgender individual, we adopted a treatment design akin to that employed by Granberg et al. (2020). They utilized a name change to indicate a gender transition, employing the format "H Larsson (prev. L Larsson)" for the applicant's names. H and L represented differently gendered first names in the instance of transgender status, and names of the same gender for cisgender applicants. Building upon this, and incorporating feedback from our interviewee, we modified the presentation of names to "H (L) Larsson".¹⁵ To clearly distinguish between the former and new names, the applicant's previous name was used in the email address provided at the end of the letter. In contrast, the new name was prominently featured within the main text of the application. It is crucial however to underscore that, in contrast to Granberg et al. (2020), our conceptualization of the treatment effect revolves around a non-cisgender signal, i.e. an individual whose current gender does not align with the sex assigned at birth. Consequently, our focus extends beyond solely transgender applicants to encompass non-cisgender individuals more broadly.

For the non-cisgender applicants, we both had a former male name being changed to a female name, as well as vice versa. In contrast, the cismale and cisfemale applicants both had a name change within their respective genders, changing from one male name to another for cismen, and from one female name to another for cismen. Importantly, it's noteworthy that in Sweden, name changes are relatively common across the population. Data from Granberg et al. (2020) indicates that between 2013 and 2018, 95,090 individuals in Sweden underwent name changes, out of a population of around 10 million. Furthermore, we obtained a high average positive callback rate of approximately 60 %, a figure in line with, or slightly surpassing, rates reported in previous studies in Sweden (Ahmed and Hammarstedt, 2008, 2009). This response rate suggests that landlords did not perceive our applications as inauthentic or insincere, thus supporting the legitimacy and viability of our approach in the context of the rental housing market.

To conduct our study, we needed two fictitious names for each applicant. Recognizing that names may convey more information than just gender, such as a person's socioeconomic status, we adopted a randomized selection process from a pool of five prevalent Swedish names for each gender.¹⁶ These names were drawn from the most popular baby names in Sweden in 2000. For females, we randomly drew from the names Julia, Emma, Wilma, Hanna, and Elin, while the male names were picked from Filip, Oscar, William, Viktor, and Simon. Each applicant's two names were then paired with a common last name in Sweden—Andersson, Johansson, Karlsson, Nilsson, and Eriksson—again randomly assigned to ensure diversity. Using the most common baby names from the year 2000 means that the applicants may be perceived as currently being aged 20 to 30 years. We aimed for this age group, as it is more common among young people to identify themselves as non-

¹⁵ During the interview, this specific signal was proposed as a means to indicate when an individual begins adopting an identity different from the one assigned at birth. Notably, the interviewee had previously used a similar approach to express their identity.

¹⁶ See for instance Fryer and Levitt (2004) for a discussion on how socioeconomic signals in names can influence results in correspondence studies.

cisgender or transgender (Jones, 2022; Ipsos, 2021). Many within this demographic have also recently entered the rental housing market for the first time, making them a particularly relevant group for our study. The selected names lend authenticity and real-world relevance, having been consistently popular since the late 1990s (Statistics Sweden, 2021), indicating a broad potential age range for our applicants. Furthermore, these names clearly signal that the applicants are ethnic Swedes. Thus, we will not be able to draw inference for other ethnic groups, unless we are willing to assume homogenous treatment effects across ethnicities. Beyond ethnicity, the chosen names are sufficiently common to be borne by individuals from diverse socioeconomic backgrounds, contributing to the generalizability of our findings.

The next step was to construct email addresses for each of the fictitious applicants. To maintain consistency, we opted for one of the widely used email providers: hotmail.com. A detailed breakdown of the names and corresponding email addresses for each applicant can be found in Table 1.

To initiate communication with each landlord, we randomly assigned one of four differently formulated letters to each application, coupled with a randomly drawn gender signal. Each letter consisted of a greeting, an introduction by the applicant of themselves, an expression of interest in the advertised rental vacancy and a final courtesy. At the bottom it was then signed by the applicant with both the two given names as well as the surname and the email address. For the precise formulation of the letters, post-translation to English, please refer to the experimental instructions in Tables B.1 and B.2 in the online appendix.¹⁷ Below is one example of a letter:

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

While the content of the application letter may seem brief, it is worth highlighting that such conciseness aligns with common practices in expressing interest in rental vacancies in Sweden, especially when communicating online. Therefore, the construction represents an approach that is appropriate for retaining the authenticity of the study.

2.3. Empirical specifications

We use simple linear regressions to analyze the data, where we compare the means between the treated non-cisgender applicants to the cisgender, using the following specification:

$$Y_i = \alpha + \beta non_cisgender_i + \epsilon_i \tag{1}$$

In the regression equation, Y_i represents one of our two binary variables, indicating either an invitation to further contact or a direct invitation to a showing. The variable $non_cisgender_i$ is a dummy variable

Table 1
 The gender signals in the study.

Name	Gender signal	Email address
Emma (Elin) Karlsson	Ciswoman	elin.p.karlsson@hotmail.com
Filip (Viktor) Nilsson	Cisman	viktor.t.nilsson@hotmail.com
Hanna (Simon) Andersson	Man-to-woman	simon.r.andersson@hotmail.com
Oscar (Julia) Johansson	Woman-to-man	julia.v.johansson@hotmail.com

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

¹⁷ The original Swedish messages and the message used to decline the apartment, along with a screenshot of how to respond to an ad and some additional information can be found in the Online appendix.

that measures whether the sent application contains a non-cisgender signal. We also provide specifications where we control for the collected characteristics of the apartment and landlord. These encompass the apartment's area in square meters, the number of rooms, the rent in 1000 Swedish kronor, if the apartment is located in one of the major Swedish cities (Stockholm, Gothenburg, or Malmö), and whether the landlord is an individual or a company. Additionally, we recorded the landlord's gender based on their names in the advertisement.¹⁸ However, while we include the dummy denoting if the landlord is a company in our set of control variables, we do not include the dummy indicating the landlord's gender, since companies have no recorded gender. This decision ensures that we can utilize all our observations and control for whether the landlord is a company. Specifications that incorporate controls for the landlords' gender rather than company status are provided in Tables A.1-A.3 in the online appendix.

We use a similarly simple specification when we contrast non-cisgender applications to ciswomen only, by introducing a dummy for cismen:

$$Y_i = \alpha + \gamma_1 cisman_i + \gamma_2 non_cisgender_i + \varepsilon_i \quad (2)$$

where $cisman_i$ is a dummy for the applicant signaling being a cisman. Consequently, ciswomen serve as the reference group in this specification. Note that this specification was not part of the pre-analysis plan.

In our final specification, we examine how the effect varies across the full range of gender signals:

$$Y_i = \alpha + \pi_1 cisman_i + \pi_2 man_to_woman_i + \pi_3 woman_to_man_i + \varepsilon_i \quad (3)$$

where $man_to_woman_i$ is a dummy indicating being assigned a male name at birth but later on adopting a female name and finally $woman_to_man_i$ indicates an initial assignment of a female gender at birth with a subsequent switch to a male name. Again, the reference group consists of ciswomen.

Finally, we use robust standard errors in all regressions.

2.4. Power calculation and ethics approval

In our preregistration plan, we determined the necessary sample size through power calculations. For Group 1 (cisgender applicants), we anticipated an incidence of 0.45, while for Group 2 (non-cisgender applicants), we anticipated an incidence of 0.39. We based our expectations on previous correspondence testing studies in the housing market, which indicated a callback rate of approximately 40–50 % for control groups (see Ahmed and Hammarstedt, 2008, 2009; Carlsson and Eriksson 2014). The expected effect size of 6 percentage points was derived from the study by Granberg et al. (2020), where transgender individuals experienced a six-percentage point penalty in positive employer response rate compared to cisgender individuals in the labor market. Considering that Ahmed and Hammarstedt (2009) reported a roughly twice as large effect for homosexuals in the rental housing market compared to the labor market (see Ahmed et al., 2013), we presumed that this assumed effect size would fall in the lower range of the true effect. The standard deviation used was 0.3, based on the estimate reported by Bengtsson et al. (2012). With these parameters and aiming for a 5 % significance level, our calculations indicated a required sample size of 788 observations in total, evenly distributed between cisgender and non-cisgender applications. Rounding up, we sought a sample size of 800 applications.

It's noteworthy that this type of study is not subject to the Ethics Review Act, as determined by the Swedish Ethical Review Authority; confirmation of this exemption is provided in the supplementary material.

¹⁸ If the name is potentially gender neutral, it was recorded as missing, but no such names were observed in the data collection.

3. Results

Consistent with our preregistered analysis plan, we started our analysis by testing if the randomization was successful; results are presented in Table 2.¹⁹ In panel A, comparing non-cisgender signal applicants to those with a cisgender signal, no significant differences in apartment or landlord characteristics emerged. Moreover, the means of each group exhibited comparable magnitudes. Expanding our analysis to encompass all gender signals in panel B, we only observed a 10 % joint significance for the variable indicating whether the apartment was situated in a metropolitan area. However, all other apartment and landlord characteristics yielded nonsignificant results, with minimal disparities in mean sizes. Given the execution of twelve hypothesis tests, it is anticipated to encounter at least one significant coefficient at the 10 % level. Thus, we concluded that the randomization process was

Table 2
Balance test.

Panel A: Non-cisgender signal – coherent gender signal comparison					
	Coherent gender signal	Non-cisgender signal	p-value difference		
Apartment 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			
Panel B: Full gender signal comparison					
	Ciswoman	Cisman	Man-to-woman	Woman-to-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 1, 171 in column 2, 178 in column 3 and 169 in column.

¹⁹ Compared to our preregistered analysis plan, we have added comparisons between non-cisgender and cisgender applicants in Table 2, in addition to the comparisons by gender signal outlined in the preregistration plan.

successful.

Subsequently, we investigated whether landlords took notice of the non-cisgender signal, leveraging the information we collected on whether landlords posed gender-clarifying questions. Table 3 illustrates the difference in the probability of being asked to clarify the applicant's gender across various gender signals. In panel A, we compare the non-cisgender signals to the cisgender signals, while in panel B, we compare the non-cisgender signals and the cismale signal to ciswomen. Panel C provides a comprehensive analysis, comparing the full set of gender signals to ciswomen. Notably, non-cisgender signal applications were approximately 5 percentage points more likely to be asked to clarify their gender, and this difference was statistically significant. This trend persisted across all panels, even when controlling for landlord and apartment characteristics.²⁰

We next conducted an analysis of the average differences in callback rates among the various gender signals, and the results are presented in Table 4.²¹ The table structure mirrors that of Table 3, featuring panel A,

Table 3
Did landlords notice the signal?.

	(1) Asked to specify gender	(2) Asked to specify gender
Panel A – Compared to both cismen and ciswomen		
Non-cisgender signal	0.055*** (0.015)	0.054*** (0.014)
Constant	0.017*** (0.007)	-0.021 (0.023)
Panel B – Compared to ciswomen only (not preregistered)		
Non-cisgender signal	0.053*** (0.016)	0.052*** (0.016)
Cisman	-0.005 (0.013)	-0.005 (0.014)
Constant	0.020** (0.010)	-0.018 (0.026)
Panel C – Per sub-group		
Man-to-woman	0.055*** (0.021)	0.056*** (0.021)
Woman-to-man	0.051** (0.021)	0.048** (0.020)
Cisman	-0.005 (0.013)	-0.005 (0.014)
Constant	0.020** (0.010)	-0.018 (0.025)
Controls	No	Yes
N	800	800

Note: Panel A displays the estimated effect of the treatment, the non-cisgender signal dummy, on receiving a question about the applicant's specific gender. Panel B compare the non-cisgender signal to ciswomen only in turn. Finally, Panel C displays the estimated effect of all different gender signals. Note that the outcome, receiving a question about the applicant's specific gender, is not in the pre-analysis plan, since we did not expect to receive such questions. The control variables in column 2 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

²⁰ This also holds true when we control for landlord gender, rather than the landlord being a company. See Table A.1 in the online appendix.

²¹ Results where we control for landlord gender, rather than the landlord being a company, are presented in Table A.2 in the online appendix. The point estimates do not change much, but we lose some significance due to the smaller sample available when using landlord gender as control.

Table 4
Main results.

	(1) Further contact	(2) Inv to showing	(3) Further contact	(4) Inv to showing
Panel A – Compared to both cismen and ciswomen				
Non-cisgender signal	-0.024 (0.035)	-0.018 (0.026)	-0.022 (0.035)	-0.016 (0.026)
Constant	0.602*** (0.024)	0.172*** (0.019)	0.579*** (0.050)	0.175*** (0.036)
Panel B – Compared to ciswomen only (not preregistered)				
Non-cisgender signal	-0.075* (0.042)	-0.063* (0.034)	-0.076* (0.042)	-0.062* (0.034)
Cisan	-0.102** (0.049)	-0.088** (0.038)	-0.108** (0.049)	-0.090** (0.038)
Constant	0.653*** (0.034)	0.216*** (0.029)	0.641*** (0.056)	0.227*** (0.044)
Panel C –Per sub-group				
Man-to-woman	-0.031 (0.048)	-0.077** (0.038)	-0.028 (0.048)	-0.074* (0.038)
Woman-to-man	-0.120** (0.049)	-0.049 (0.040)	-0.126** (0.049)	-0.049 (0.040)
Cisman	-0.102** (0.049)	-0.088** (0.038)	-0.108** (0.049)	-0.090** (0.038)
Constant	0.653*** (0.034)	0.216*** (0.029)	0.638*** (0.056)	0.227*** (0.044)
Controls	No	No	Yes	Yes
N	800	800	800	800

Note: Panel A displays the estimated effect of the treatment, the non-cisgender signal 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. Panel B compare the non-cisgender signal to ciswomen only in turn. Finally, Panel C displays the estimated effect of all different gender signals. Note that Panel B is not in the pre-analysis plan. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

which compares the non-cisgender signals to the cisgender signals, panel B, which contrasts the non-cisgender signals and the cismale signal to ciswomen, and panel C, presenting estimates for the entire array of gender signals in comparison to ciswomen. In panel A, no statistically significant effects were observed for either outcome. The coefficients implied a 2.4 percentage point lower likelihood of receiving invitations for further contact and a 1.8 percentage point lower likelihood of receiving a direct invitation to a showing for non-cisgender applicants compared to cisgender applicants (see panel A, Columns 1 and 2). Furthermore, none of the estimated effects exhibited substantial changes when we introduced controls for apartment and landlord characteristics, as illustrated in columns 3 and 4.

In panel B we subsequently contrasted non-cisgender applicants with ciswomen by introducing a dummy variable for cismen.²² In this comparison, we found that non-cisgender applicants have a 7.5 percentage point lower likelihood of receiving invitations for further contact and a 6.3 percentage point lower likelihood of obtaining direct invitations to a showing compared to ciswomen. Both results are statistically significant at the 10 %, though not the 5 %, significance level. Notably, this effect size closely aligns with our anticipated effect of 6 percentage points. Additionally, we observed that cismale applicants exhibit a 10.2

²² Note that the results in Panel B are not part of the pre-analysis plan.

percentage point lower likelihood of receiving invitations for further contact and an 8.8 percentage point lower likelihood of obtaining direct invitations to a showing compared to ciswomen, both significant at the 5 % level. These estimated effects for cismale applicants mirror the magnitudes reported in prior studies (see Ahmed and Hammarsted (2008) and Bengtsson et al. (2012)), indicating that neither the COVID pandemic nor our signal design had unintended consequences for our estimates. Importantly, the estimates and significance levels remained consistent with the inclusion of control variables in columns 3 and 4.

Finally, in panel C, we delved into the variations in the effect across all different types of gender signals. Notably, the woman-to-man switching applicants exhibited a 12-percentage point lower likelihood of receiving invitations to further contact than ciswomen. Conversely, there was no statistically significant difference for the man-to-woman switching applicants for this outcome. When we used direct invitations to showings as our outcome variable, the opposite significance pattern emerged. The man-to-woman switching applicant displayed a 7.7 percentage point lower likelihood of receiving a direct invitation to a showing compared to ciswomen, achieving significance at the 5 % or 10 % level, depending on the preferred specification. However, for the woman-to-man switching applicant, no significant negative effect was observed compared to ciswomen for this outcome. Note that the estimate for the cismale applicant is by necessity the same as in panel B, since we still contrast cismen to ciswomen.

A plausible interpretation of these findings is that landlords exhibited hesitancy in extending a direct invitation to show the apartment to the man-to-woman applicant, possibly reflecting a desire to gather more information about the applicant's gender before committing to a showing. In the case of the woman-to-man applicant receiving fewer invitations to further contact, it could be inferred that landlords who actively considered the application seriously perceived the applicant as having been born a woman. Those who did not respond on the other hand might have categorized the applicant as a cisman. It is crucial to emphasize that this is just one potential interpretation, as the exact reasoning of the landlords remains unknown.

We further explore the robustness of our main estimates in Tables A1-A5 in the online appendix, where we add controls for landlord gender, if the landlord used the wrong name in the communication and date fixed effects. Overall, the results are robust to these additional controls.

While our experimental setting does not allow us to distinguish clearly between taste-based and statistical discrimination, heterogeneity analysis offers some insight into the underlying mechanisms. Specifically, we expect that the effect is stronger in rural areas than in major cities (Stockholm, Gothenburg, and Malmö), as city inhabitants tend to hold more progressive attitudes toward gender stereotypes and sexual minorities. We see indications in line with this pattern in Table 5: coefficients are generally more negative and statistically significant for apartments outside the major cities (columns 1 and 2) compared with those based on data from the major cities (columns 3 and 4). This trend is particularly pronounced for cismale applicants, which aligns with the results in Bengtsson et al. (2012). These findings remain robust when additional control variables are included (see online appendix Table A6). However, it is important to note that the relatively small sample sizes limit the ability to draw definitive conclusions.²³

In Table 6, we finally turn our attention to the effect on the additional outcomes that were not initially part of the pre-analysis plan—the likelihood of being addressed by the wrong name and the likelihood of

²³ As a further heterogeneity analysis, we explore potential heterogeneous effects by landlord gender in the online appendix (Tables A.7-A.8). If any conclusion is to be drawn from these tables, it is that the coefficients for the non-cisgender applicants are very small among female landlords in terms of direct invitations to a showing. However, female landlords are also less likely overall to give a direct invitation to a showing.

Table 5
Heterogenous effect by metropolitan area - without controls.

	(1)	(2)	(3)	(4)
	Further contact	Inv to showing	Further contact	Inv to showing
Panel A – Compared to both men and women				
Non-cisgender signal	−0.010	−0.006	−0.034	−0.027
	(0.052)	(0.041)	(0.047)	(0.033)
Constant	0.640***	0.185***	0.571***	0.161***
	(0.036)	(0.029)	(0.033)	(0.025)
Panel B – Compared to women only (not pre-registered)				
Non-cisgender signal	−0.120*	−0.071	−0.056	−0.062
	(0.062)	(0.058)	(0.056)	(0.042)
Cisman	−0.191***	−0.113*	−0.049	−0.076
	(0.070)	(0.060)	(0.067)	(0.048)
Constant	0.750***	0.250***	0.593***	0.195***
	(0.050)	(0.050)	(0.044)	(0.036)
Panel C –Per sub-group				
Man-to-woman	−0.091	−0.091	0.003	−0.069
	(0.073)	(0.064)	(0.063)	(0.047)
Woman-to-man	−0.146**	−0.052	−0.122*	−0.054
	(0.072)	(0.065)	(0.066)	(0.049)
Cisman	−0.191***	−0.113*	−0.049	−0.076
	(0.070)	(0.061)	(0.067)	(0.048)
Constant	0.750***	0.250***	0.593***	0.195***
	(0.050)	(0.050)	(0.044)	(0.036)
Major city	No	No	Yes	Yes
Controls	No	No	No	No
N	351	351	449	449

Note: Panel A displays the estimated effect of the treatment, the mixed gender signal 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. Panel B compare the mixed gender signal to women only in turn. Finally, Panel C displays the estimated effect of all different gender signals. Note that Panel B is not in the pre-analysis plan. Columns 1 and 2 look at the effect for male landlords only, while columns 3 and 4 look at female landlords. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

being asked if the applicant would be living alone. The structure of the table mirrors that of Tables 3 and 4. Overall, the table indicates that the cismale applicant is 5 percentage points less likely to receive a question about living arrangements and 4.9 percentage points more likely to be addressed by the wrong name compared to the cisfemale applicant. Both estimates are statistically significant, reaching at least the 5 % level and remain robust to the inclusion of control variables. There is no significant difference between the non-cisgender applicants and the cisfemale applicant, however.

A potential concern is that landlords' use of the wrong name could stem from our experimental design, potentially indicating that the signal was not accurately perceived. However, if this were the case, we would expect an even distribution of incorrect name usage across all treatments. Instead, we find that cismale applicants are significantly more likely to be addressed with an incorrect name, suggesting that landlords devoted less attention to these applications. Moreover, our main results remain robust when we control for the usage of incorrect names (see online appendix Table A4).

4. Conclusion

In this paper, we presented the findings from the first correspondence study exploring the impact of single household non-cisgender signals in the Swedish rental housing market. Our results revealed that applicants signaling non-cisgender status experienced a treatment effect between that observed for cismen and that observed for ciswomen. Specifically, non-cisgender applicants were 7.5 percentage points less likely to receive an invitation to further contact compared to ciswomen, while cismen faced a slightly more substantial 10.2 percentage point lower likelihood to receive such an invitation. Moreover, cismen

Table 6
Additional outcomes (not preregistered).

	(1)	(2)	(3)	(4)
	Asking if living on own	Use wrong name	Asking if living on own	Use wrong name
Panel A – Compared to both cismen and ciswomen				
Non-cisgender signal	0.005	−0.027**	0.005	−0.027**
	(0.012)	(0.012)	(0.012)	(0.012)
Constant	0.025***	0.045***	−0.017	0.015
	(0.008)	(0.010)	(0.015)	(0.020)
Panel B – Compared to ciswomen only (not preregistered)				
Non-cisgender signal	−0.020	−0.003	−0.019	−0.000
	(0.018)	(0.012)	(0.017)	(0.012)
Cisman	−0.050***	0.049**	−0.047***	0.054**
	(0.016)	(0.020)	(0.015)	(0.021)
Constant	0.050***	0.020**	0.010	−0.016
	(0.016)	(0.010)	(0.017)	(0.021)
Panel C –Per sub-group				
Man-to-woman	−0.010	−0.015	−0.008	−0.013
	(0.021)	(0.011)	(0.020)	(0.011)
Woman-to-man	−0.030	0.010	−0.029	0.012
	(0.019)	(0.016)	(0.018)	(0.016)
Cisman	−0.050***	0.049**	−0.047***	0.054**
	(0.016)	(0.020)	(0.015)	(0.021)
Constant	0.050***	0.020**	0.010	−0.015
	(0.016)	(0.010)	(0.017)	(0.021)
Controls	No	No	Yes	Yes
N	800	800	800	800

Note: Panel A displays the estimated effect of the treatment, the non-cisgender signal dummy, on receiving a question if the applicant will be living by themselves in columns 1 and 3 and being addressed by the wrong name in columns 2 and 4. Panel B compare the non-cisgender signal to ciswomen only in turn. Finally, Panel C displays the estimated effect of all different gender signals. Note that Panel B is not in the pre-analysis plan, neither is any of the outcomes. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

exhibited a reduced likelihood of being asked about living arrangements and were more frequently addressed by the wrong name. We argue that these results challenge the notion of a strongly gender-segregated housing market. A segregated market would suggest that non-cisgender applicants struggle to align with the preferences of both cismale- and cisfemale-preferring landlords. Furthermore, the observed treatment of non-cisgender applicants, falling between the treatment of cismale and cisfemale applicants, suggest that they are seen neither as fully cismale, nor fully cisfemale. Rather that they embody a blend of cismale and cisfemale attributes, and given the prevailing bias against cismale characteristics in the rental housing market, they are treated somewhere in between cismen and ciswomen.

This study has focused on a specific aspect of the rental apartment matching process. It is essential to acknowledge that there could be additional forms of discrimination or influential factors impacting the inclusion of non-cisgender individuals in the housing market. Concerns about potential violence or mistreatment might drive non-cisgender individuals to explore alternative housing channels, such as relying on personal contacts. Additionally, a landlord's preference for non-cisgender applicants could manifest more prominently during in-

person interactions between the landlord and prospective tenants. Future research should address these potential factors and extend the investigation to encompass a broader understanding of discrimination in the housing market. Other points to address in future research are deeper explorations of the differences in the observed treatment effects between cismen and ciswomen, for instance across different settings and in different countries.

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Sofia Fritzson: Writing – original draft, Software, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Joakim Jansson:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Project administration, Methodology, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary materials

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Data availability

[Replication Data for: Living in the Gender Spectrum \(Original data\)](#) (Dataverse)

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