Language Proficiency in Audit Experiments: Evidence from Rental Housing Markets*

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Abstract

Immigrants face two major barriers in accessing core markets: a foreign-name disadvantage and language-limitation penalties—yet little is known about the latter. We jointly estimate both components in the rental housing market, and assess the effectiveness of high-investment versus low-cost actions. In a large-scale field experiment in four Central European countries, we send fictitious apartment-viewing requests, randomly varying local-language proficiency, name origin, and a language-learning signal. We find a 50-percent penalty for foreign-name, non-local-language (proficient-English) inquiries relative to native-name, fluent local-language inquiries. Language-proficiency benefits are non-linear, with no measurable benefit to functional local-language versus English or signaling local-language learning, but substantial returns to fluency. The foreign-name disadvantage is modest in comparison, and can be overcome by foreign applicants following-up with non-responders. Overall, language limitations account for the bulk of the disadvantages foreigners face in rental housing access. Our results highlight the importance of attaining high local-language proficiency and persistence in reducing integration frictions for immigrants.

JEL codes: J15, R31, C93, Z13

Keywords: Linguistic integration, Language proficiency, Rental markets, Field experiment, Immigration

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1   Introduction

The share of residents with a migration background has risen across advanced economies, increasing the importance of understanding how immigrants gain access to core markets (Dorn and Zweimüller 2021). A large literature uses correspondence tests that compare responses to otherwise identical inquiries—such as requests to view a rental property—that differ only in whether the sender has a native- or foreign-sounding name. In these settings, however, correspondence tests typically assume fluent local-language communication by foreign-sounding applicants. This assumption is at odds with institutional realities: governments frequently promote local-language learning through subsidized courses and often require demonstrated proficiency for permanent residency or citizenship, reflecting the fact that many newcomers initially lack full fluency. While recent evidence shows that higher local-language proficiency improves immigrants' labor-market prospects (Carlsson, Eriksson, and Rooth 2026), much less is known about how limited proficiency shapes access in settings characterized by brief, informal interactions. Rental housing is a leading example. Landlords and realtors often screen prospective tenants through short written exchanges, making communication frictions potentially decisive at the first point of contact (Bundesgerichtshof 2026). This raises a central question: to what extent do correspondence tests that assume fluent communication understate the barriers immigrants face in accessing core markets such as rental housing?

Identifying the role of language proficiency in shaping immigrants' access to core markets is challenging in observational data. Measures of language skills are often noisy (e.g., self-reports), and even standardized test scores can be endogenous: immigrants who achieve higher proficiency may differ systematically in unobserved traits–such as social networks or familiarity with local traditions–that also affect search outcomes. These challenges complicate efforts to isolate the role of language itself and motivate the use of correspondence tests, which offer experimental control at the first stage of access. However, standard correspondence test designs typically abstract from realistic variation in language proficiency. To address this limitation, we conduct a multi-country correspondence field experiment that explicitly varies the implied level of local-language proficiency in written rental inquiries. We focus on rental housing markets in Central Europe, where local languages are rarely spoken fluently by new immigrants and where prior work documents disadvantages for foreign-sounding names in social settings (Gomez-Gonzalez, Nesseler, and Dietl 2021; Van Tubergen 2025). Our enhanced design allows us to assess how incorporating realistic language constraints alters measured access in correspondence tests, using rental housing as a case study. To our knowledge, this is the first correspondence study in the housing market context to experimentally vary applicants' local-language proficiency.

In the field experiment, we send over 3500 email inquiries to landlords and realtors advertising apartments in Austria, Czechia, Hungary, and Slovakia, requesting a viewing for a specific listing. We implement a randomized design that varies three features of the inquiry: (i) the applicant's

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language (English only, functional A2-level local language, or fluent local language); (ii) the applicant's name origin (foreign- versus native-sounding); and (iii) whether the applicant signals ongoing local-language learning (i.e., mentioning enrollment in a local language class). To mirror natural search behavior and capture the role of persistence at the first point of contact, we send a single treatment-consistent follow-up after three business days if the initial inquiry receives no reply.[1] Our primary outcomes are whether the inquiry receives a viewing invitation, measured both within three business days and after a single follow-up to non-responders.

To ensure realism and comparability across countries, we follow standard best practices in the correspondence-test literature. We begin with a neutral rental inquiry written in proficient English, drawing on established templates in the literature (Bosch, Carnero, and Farre 2010; Horr, Hunkler, and Kroneberg 2018; Murchie and Pang 2018). We then employ professional linguists and language teachers to produce country-specific versions at functional (A2) and fluent proficiency levels. Third, we validate our name signals (i.e., foreign vs. natives) using surveys that identify sets of names residents clearly perceive as native- versus foreign-sounding in each country. For the foreign names, we use common names from the top three foreign groups in each country that are unlikely to speak the local language.[2] We also contact each landlord or realtor at most once to maintain the study's internal validity and mitigate spillover effects; when a poster advertises multiple properties, we randomly select a single listing to contact.

Moving on to the results, four findings illustrate how relaxing standard correspondence-test assumptions alters measured access to rental housing. First, when correspondence tests are implemented under the conventional assumption of fluent local-language communication, we estimate a modest native–foreign name gap that is comparable to prior rental-market studies (e.g., Flage 2018; Auspurg, Hinz, and Schmid 2017; Ghekiere and Verhaeghe 2022). Among fluent local-language inquiries, native-sounding names receive about 5 percentage points more viewing invitations in the first three business days (i.e., the *initial* invitation rate). Second, and central to our contribution, we show that local-language proficiency is a first-order determinant of housing access. Moving from fluent local language to English substantially reduces invitation rates; the lowest success occurs when the inquiry is written in English and does not signal ongoing language learning. The resulting gap relative to fluent inquiries–about 15 percentage points–is substantially larger than the native–foreign name gap estimated under the typical fluent-language design. Third, we find no evidence that functional A2 proficiency improves outcomes relative to English, and signaling enrollment in language classes has at most small effects on access. Finally, persistence partly mitigates these barriers. Sending a single follow-up increases viewing invitations by roughly 4 to 9 percentage points, with larger gains for foreign-sounding names with fluent inquiries, thereby further reducing the native–foreign name gap.

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[1]Based on our pilot testing, responses are unlikely after three business days from the initial inquiry.

[2]For example, in Austria foreign names are chosen from Romania, Slovakia, and Turkey. Although there is a large population of Germans in Austria, we omit them as German as they speak the local language.

Having established that local-language fluency can substantially improve foreigners' housing access, we next ask whether foreign residents internalize these returns, since perceived benefits plausibly shape incentives to invest time and money in language acquisition. We carry out online and in-person forecast surveys in the four countries that elicits beliefs about the experiment's initial positive response rates. Participants are first shown a benchmark outcome for the control condition (a native-sounding name written in fluent local language) and are then asked to forecast the positive response rate when the inquiry is sent with a foreign-sounding name under three language conditions: English, functional A2 local language, and fluent local language. For simplicity and to focus on language acquisition, we elicit forecasts for the initial inquiry only, omitting both the follow-up and the language-learning signal. For all participants who are surveyed online, we provide bonus payments for forecasts that match our experimental findings.[3] We find substantial underestimation of the returns to fluency: roughly three-quarters of foreign respondents predict a smaller fluency premium (relative to English) than the one observed in the experiment, and on average, they underestimate the gain from becoming fluent by about 50%.[4]

Our paper makes two main contributions. First, we contribute to the correspondence-test literature by showing how standard design choices influence measured access to rental housing and, more broadly, to core markets. A large body of work uses correspondence tests that vary applicants' names while holding all other features fixed, and consistently documents lower response rates for minority- and immigrant-sounding names across a wide range of contexts, including labor markets (Bertrand & Mullainathan, 2004; Oreopoulos 2011; Booth et al., 2012; Kaas & Manger, 2012; Thijssen et al., 2021; Valfort, 2020), rental housing markets (Ewens, Tomlin, and Wang 2014; Bosch, Carnero, and Farré 2015; Acolin, Bostic, and Painter 2016; Öblom and Antfolk 2017; Auspurg, Schneck, and Hinz 2019; Gusciute, Mühlau, and Layte 2022), the sharing economy (Edelman, Luca, and Svirsky 2017; Ge et al. 2020), dating (Jakobsson and Lindholm 2014; Ranzini, Rosenbaum, and Tybur 2022), entrepreneurship (Gornall and Strebulaev 2024), and social participation (Gomez-Gonzalez, Nesseler, and Dietl 2021; Nesseler et al. 2023). In these designs, language proficiency is typically held constant by writing all inquiries in fluent local language—a convenient but potentially restrictive assumption. Replicating this benchmark, we find a modest native–foreign name gap of about 5 percentage points when inquiries are written in fluent local language. However, once language proficiency is allowed to vary, it emerges as a first-order determinant of access: fluent local-language inquiries substantially outperform English and functional (A2-level) inquiries for foreign-sounding names. In addition, our design incorporates an omitted behavioral margin–applicant persistence. We show that sending a simple follow-up to non-responders as a foreign-sounding name applicant increases viewing invitations and can substantially reduce the initial native–foreign name gap. Together, these findings imply that correspondence-test designs

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[3]Most of our participants are surveyed on Prolific, where it was logistically easy to provide incentives for correct forecasts. Such incentives were not provided for those surved in-person due to logistical constraints.

[4]We identify foreign respondents as those whose native language is not the local language.

that fix language at fluent proficiency and abstract from persistence may understate the barriers faced by many immigrants at first contact, and that allowing language proficiency to vary alongside names improves the external validity and interpretation of correspondence-test estimates.

Second, we contribute to the growing literature on the economic and social importance of immigrants' local-language proficiency (Carlsson, Eriksson, and Rooth 2026; Ek, Hammarstedt, and Skedinger 2024; Fossati, Liechti, and Auer 2020). The study most closely related to ours is Carlsson, Eriksson, and Rooth 2026, which examines how local-language proficiency shapes hiring outcomes in the Swedish labour market. Methodologically, Carlsson, Eriksson, and Rooth 2026 extends the standard correspondence-test framework by embedding second-language features in cover letters to generate multiple proficiency levels, and the authors find that higher proficiency substantially increases the likelihood of receiving an interview. We extend the design in three respects in the rental housing context, where first-stage screening operates largely through brief written exchanges rather than formal hiring procedures. First, we include a fluent local-language inquiry sent under a native-sounding name, providing a natural benchmark for interpreting returns to language proficiency alongside baseline name-based differentials in access to viewings. Second, we introduce an English-only condition, which allows us to test whether functional (A2-level) proficiency delivers benefits beyond English for foreign applicants.[5] Third, to mirror natural applicant behaviour and to study the effects of persistence, we send a treatment-consistent follow-up to non-responders after three business days and evaluate viewing invitations both before and after this follow-up. In terms of findings, we similarly document large gains from reaching fluency, but we do not detect returns to functional proficiency beyond English. This contrasts with Carlsson, Eriksson, and Rooth 2026, which reports a monotonic increase in callback rates at each step of improved Swedish proficiency across four cover letter language levels.

Although our empirical setting is the rental housing market, the design considerations we highlight are likely to extend to other contexts commonly studied using correspondence and audit methods. Many economically relevant interactions—such as job applications, access to financial services, and interactions with public agencies—are governed by standardized exchanges in which decisions are made quickly on the basis of limited information. In these settings, correspondence tests similarly fix language at fluent local proficiency when varying applicant characteristics, even though limited language proficiency is common among newcomers and may affect how written requests are interpreted. From this perspective, rental housing serves as a particularly salient and measurable gatekeeper setting, but our findings suggest that correspondence-test designs that assume fluent local-language may understate first-stage access in a broader class of markets.

The remainder of the paper is organized as follows. The next section describes the institutional context. Section 3 details the field experiment. Section 4 describes the listings data and the

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[5]It is reasonable to think that many economic migrants in Central European countries will speak English as English proficiency is generally high in Europe.

forecasting survey. Section 5 outlines the empirical framework, and section 6 presents the main results. Mechanisms underlying the findings, together with policy implications is covered in section 7. Finally, Section 8 concludes.

2   Institutional Background

Our study is set in four neighboring countries in Central Europe: Austria, Czechia, Hungary, and Slovakia. The countries share many economic similarities because of their proximity to each other and membership in the European Union. At the same time, they differ in language distance, migration exposure, and housing market structures.

Geography and regional integration.   The four countries each share borders with at least two of the others (Austria borders both Czechia and Slovakia; Slovakia borders both Czechia and Hungary; Hungary borders Slovakia; Czechia borders Austria and Slovakia). All four are EU member states, and all participate in the Schengen area, facilitating cross-border mobility within the region. The monetary environment differs across countries: Austria and Slovakia use the euro, whereas Czechia and Hungary retain national currencies (the Czech koruna and Hungarian forint). In our analysis, we convert all rental prices to the Euro.

Language environment.   The countries differ sharply in linguistic environment, which is central to our study design. Czech and Slovak are closely related West Slavic languages, while German and Hungarian are much more linguistically distant. This linguistic heterogeneity implies meaningful cross-country variation in the difficulty of local-language acquisition and in the extent to which English can serve as a fallback language in initial housing inquiries. English proficiency is generally high in the major cities of these countries, as some of them are popular tourist destinations.

Permanent residence and citizenship language requirements.   Reflecting the policy emphasis on integration, these countries commonly condition long-term residence and naturalization for third-country nationals on meeting integration requirements, including local-language proficiency. For example, in Austria, German at the B1 level (Integration Agreement Module 2) is required to apply for long-term residence; in Czechia, applicants must pass a Czech-language exam at A2 for permanent residence and at B1 for citizenship; and in Slovakia, applicants for long-term residence must demonstrate Slovak proficiency at A2.

Migration exposure and population diversity.   Migration exposure differs substantially across the four settings. Eurostat reports that, in 2023, non-nationals accounted for about 9% of the EU

population (citizens of another country than their country of residence).[6] Austria is among the EU countries with the highest shares of non-nationals (about 19%), whereas Slovakia (around 1%) and Hungary (around 3%) are among the lowest. Czechia's share of non-nationals is close to the EU average (around 8%). These differences are relevant for interpreting cross-country heterogeneity because both screening practices and beliefs about foreigners may plausibly depend on local exposure to migration.

In our study, we focus on the top three foreign groups in each country that are unlikely to be native speakers of the host-country language (summarized in Table B.1). Concretely, the foreign-name profiles correspond to (i) Romania, Slovakia, and Turkey in Austria; (ii) Poland, Ukraine, and Vietnam in Czechia; (iii) China, Slovakia, and Ukraine in Hungary; and (iv) Germany, Hungary, and Ukraine in Slovakia. Overall, our study covers nine origin groups, allowing us to assess heterogeneity in housing access by nationality.

Austro-Hungarian Empire. Historically, all four countries in our sample were part of the Austro-Hungarian Empire, which dissolved after 1918. As a result, some cross-border migration flows within the region today involve populations that were previously part of a single political entity. For example, while a Slovak living in Hungary is classified as a foreigner under contemporary national borders, such distinctions did not exist within the empire. This unique historical context allows us to examine heterogeneity in the returns to language proficiency across foreign groups that differ not only in linguistic distance but also in whether their origins lie within the former Austro-Hungarian Empire.

Housing ownership. Housing systems in these countries differ markedly in the prevalence of renting versus owning. Eurostat data show that renting is relatively common in Austria: in 2024, 46% of Austria's population lived in rented housing, aligned with the large share of non-nationals.[7] In contrast, home ownership is much higher in Czechia (about 75%) and exceeds 90% in Slovakia (about 93%) and Hungary (about 92%). In these high-homeownership settings, the rental sector is comparatively smaller, which likely implies that a substantial share of rental units are supplied by locally owned properties and managed by local landlords or agents.

Online rental markets and contact modalities. Rental housing search in all four countries is mediated by large online listing markets in which both individual landlords and professional realtors advertise properties. Listings typically provide the rental price in local currency alongside standardized apartment characteristics (e.g., size in square meters, number of bedrooms, furnishing status) and a location descriptor (often including an address or a neighborhood-level zip code). In

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[6]Source: https://ec.europa.eu/eurostat/web/interactive-publications/migration-2024.
[7]Source: https://ec.europa.eu/eurostat/web/interactive-publications/housing-2025.

these markets, platforms differ in how prospective tenants initiate contact: some require inquiries through an internal messaging system, while others allow applicants to contact the poster directly via email. For example, email was the most common method of contacting posters for Czechia in our sample. In our study, the vast majority of listings in all four countries are represented by a professional Realtor. The Realtor's work for a reality and are employed by the landlord. The online ecosystem also spans audiences, with some platforms aimed primarily at international tenants and others serving both locals and internationals.

Our sampling strategy reflects how foreign applicants plausibly search for housing. In particular, we exclude platforms that are unlikely to be used by non-fluent newcomers because they are difficult to locate when searching in English and are therefore less salient to international search. We also focus on listings that permit direct email contact. This choice facilitates a standardized mode of outreach across countries and enables the measurement of email opens in addition to responses. Moreover, direct email avoids constraints imposed by some internal messaging systems, such as caps on the number of inquiries that can be sent per day, which could otherwise affect deployment and comparability across platforms and countries.

3 Experimental Setup

This section details the design of the field experiment. We describe the selection of names, the method of deployment, the email templates, the control group, and the five treatment variations.

Selection of native and foreign names. We began by identifying the three largest foreign groups in Austria, Czechia, Hungary, and Slovakia. We omit foreign groups that plausibly speak the local language (for example, we do not consider Germans living in Austria). For each foreign group, we created two typical male and two typical female names. We also created five native-sounding female and five native-sounding male names in each country. To validate that the names are perceived correctly as either native or foreign, we conducted an online Prolific survey of residents in each country. If respondents were unsure about whether a name belonged to a foreign group, we excluded that group and used the next-largest foreign group instead. For example, respondents in Slovakia often categorized Czech names as native, so we omit Czech names in Slovakia.[8] A complete list of names and the survey validation results are provided in the Appendix (Table A1).

Method of making rental viewing inquiries. Next, we created email accounts for all names. For each country, we used a different email ending (e.g., in Slovakia: @slovakiawave.com). The

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[8]A related concern is that Hungarian names may not be perceived as foreign in Slovak municipalities with large Hungarian-speaking populations (e.g., Dunajská Streda and Komárno). In our experimental sample, only one inquiry in Slovakia was sent from a Hungarian name to a listing in these municipalities, so this issue is unlikely to affect our results.

email address displayed the first name, last name, and the ending, for example: Anna.Molnarova@ slovakiawave.com.[9] We contacted listing posters with one of six email versions requesting a viewing for a specific rental property. In the control group, the applicant has a native-sounding name and writes fluently in the local language, used as a benchmark of comparison against the treatments.

Email variations for foreign-sounding names. In the five treatment groups, all applicants have a foreign-sounding name. We do not vary language proficiency for native-sounding names (e.g., native name with functional A2-level language) for three reasons. First, the prevalence of such inquiries is likely to be small. Second, such messages may signal low effort or a lack of interest rather than language proficiency. Third, our research question is focused on immigrants' language proficiency. The five treatment variations for foreign-sounding names are:

1. fluent local language,

2. A2-level local language with a signal of ongoing language learning,

3. A2-level local language without the learning signal,

4. Proficient English with a signal of ongoing language learning,

5. Proficient English without the learning signal.

For treatments 2 and 3, we use A2 level of understanding, as it is the minimal language level that allows someone to be able to follow simple conversations and read and write short texts. This level of language proficiency is reasonably achievable within a year of taking language classes on a part-time basis.

We pre-registered the hypothesis that positive response rates would increase monotonically across these treatments, reflecting higher levels of local-language proficiency and the potential role of signaling language learning in improving access. The conceptual framework motivating this hypothesis, along with the full set of pre-registered predictions, is presented in Appendix A.

Initial email templates. We first write a rental inquiry in proficient English with the language learning signal, building on standard templates in the literature. To improve readability, we present templates in a consistent format below:

> **Subject:** *Property for Rent: [Address as mentioned in the advertisement]*
>
> *Hello,*
>
> *I am interested in your advertisement about the rental property.*

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[9]All diacritical marks were excluded for the email account but visible in the text of the email. That means that someone would get an email from, e.g., Anna.Molnarova@slovakiawave.com but in the email the name was signed as Anna Molnárová.

Is it still available to rent? My schedule is flexible for viewing the property [, except Saturday morning because I am taking a Czech language class.]

Thanks,

[Foreign Name]

The language learning signal is indicated in brackets.[10] We provided the fluently written English email to either a professional linguist or language teacher within each country, together with detailed instructions to translate the email into (i) a functional A2-level version (mimicking a new language learner) and (ii) a proficient version (mimicking a native speaker).

To support construction of the functional A2-level versions, we also employed an English language teacher and provided an English A2-level template as a benchmark. The email with an applicant speaking the local language on an A2 level and signaling ongoing language learning (in English) provided was:

Subject: *Property for Rent: [Address as mentioned in the advertisement]*

Hello,

I like your ad about the place for rent.

Is it still free? I can see it most times, except in Saturday morning becose my Czech language class.

Thanks,

[Foreign Name]

We allowed the language teachers to deviate slightly from these fluent and A2 benchmarks based on their professional judgment when a direct translation was unnatural in the local language.

Follow-up emails. If no response was received within three business days, we sent a short, treatment-consistent follow-up message. For example, the following is a follow-up in proficient English with the language learning signal (treatment 4): *"I am still looking for an apartment. Is it still available to rent? My schedule is flexible for viewing the property, except Saturday morning because I am taking a Czech language class."*

If a response was received, we sent a brief message thanking the recipient and indicating that we are no longer searching, to avoid imposing further time costs. In English: *"Thank you so much for your reply, I no longer need an apartment."*

For Austria, we have included the initial, follow-up, and thank you emails by treatment in Table B.4.

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[10]The brackets are not included in the original email when the language local learning signal is used.

Email construction and randomization. In the treatment groups, we randomly assign one of the foreign-sounding names to each inquiry (from the set of 12 foreign-sounding names). We use stratified randomization to assign listings to treatments. The strata include the day of the week and each major city, with all listings outside the major cities pooled into a separate stratum.

4   Data

We use two sources of data. First, we construct a dataset of rental listings in each country and use it to implement the correspondence field experiment. Second, we field online and in-person surveys of residents in each country to elicit forecasts of the experimental results.

4.1   Listings data

For each country, we gathered all active rental listings from the main online rental portals during the deployment period. For each listing, we record the advertised rent (converted to euros), size (m^2), number of bedrooms, and a set of common amenities (elevator, balcony, furnished). We also collect the location information reported in the listing (address or postal code), as well as the poster's name and contact email. When a listing is posted by an intermediary (e.g., a realtor), we additionally record the agency name.

We apply several sample restrictions to obtain a treatment-eligible set of listings that is relevant for typical immigrant search. First, we drop listings that fall outside a plausible range of apartment sizes. Second, because our mode of contact is email, we restrict attention to listings that include a valid email address. Third, to mitigate spillovers and reduce the likelihood that posters infer the study design, we ensure that each poster is contacted at most once. When the same landlord or realtor advertises multiple listings, we randomly select a single listing to contact. In addition, within each city we restrict contact to at most one realtor per agency, selecting a realtor at random when multiple are available. As a result, the final experimental sample consists of unique posters, and agencies are contacted at most once within a city. The data cleaning process results in nearly 3600 listings across the 4 countries.

For descriptive purposes, we geocode the locations of the treatment-eligible listings and illustrate their spatial distribution in Figure 1. The figure shows clusters around central cities such as Budapest or Vienna, but our sample also includes listings in rural areas.

Balance test. Table 1 presents the mean of the apartment characteristics for each experimental condition. In the control group, the average apartment's rent is around 800 euros with 2 bedrooms. The experiment is balanced across the baseline information, with the joint F-test failing to reject equality across the experimental conditions for each apartment characteristic.

4.2 Survey data

To complement the field experiment, we compare the experimental estimates to residents' beliefs about the returns to language proficiency (see appendix section D for the survey). We collect survey responses both online and in person in each country. For the online survey, we recruit respondents through Prolific and make forecasts incentive compatible: in addition to the participation payment, respondents receive a bonus payment based on the accuracy of each forecast.

Because we cannot directly target foreign residents, we survey all residents and classify respondents as natives or foreigners using self-reported background measures. Specifically, we ask whether the respondent (i) was born in the country, (ii) holds citizenship of the country, and (iii) speaks the local language as a native language. These measures are highly correlated, and in our main analysis we use native-language status to distinguish natives from non-natives.

After the background questions, respondents receive a short description of the experimental setting and are shown a benchmark outcome: the viewing-invitation rate for an inquiry sent with a native-sounding name in fluent local language. For simplicity, we elicit beliefs about the initial inquiry only and omit both the language-learning signal and the follow-up. To ensure comprehension, we include three attention checks about the information provided (e.g., the benchmark success rate). Around 20% of respondents fail at least one attention check. Because this group exhibits substantially higher variance in reported beliefs than those who pass all attention checks, we exclude them from the analysis.

Respondents are then asked to forecast the viewing-invitation rate for foreign-sounding names under three language conditions: (i) English only, (ii) functional A2-level local language, and (iii) fluent local language. Forecasts are elicited one at a time, and the benchmark is displayed alongside each question. To mitigate order effects, we randomize the order in which the three forecast questions are presented. Responses are recorded on a 0–100 slider, allowing respondents to express a wide range of beliefs.

4.3 Timeline of data collection

Field experiment. We implemented the correspondence field experiment over October–December 2024. Within each country, we conducted the initial email and the follow-up to non-responders within a short deployment window (approximately two weeks), so that follow-ups were typically sent while listings remained active. Because implementing the experiment simultaneously in all four countries would have required a larger research team, we rolled out the deployment sequentially, one country at a time. This sequencing does not affect identification, as treatment is randomized within country and our estimates rely on within-country contrasts across experimental conditions rather than on cross-country comparisons. The order of implementation was driven by operational feasibility in constructing the sampling frame and obtaining the posters' contact email at scale,

which varied across countries. The order of the data collection and email deployments was as follows: Czechia, Slovakia, Hungary, and Austria.

Residents' forecasts of experimental results. After completing the field experiment and examining the main treatment effects, we designed and fielded the forecast survey. The online survey was deployed on Prolific in October 2025. In some countries—especially Slovakia—the pool of available respondents was limited, and the share of respondents with an immigrant background was small. To increase coverage and ensure sufficient representation of non-native residents, we complemented the online data collection with in-person surveys administered in university classrooms in November–December 2025, focusing on large courses with substantial international student enrollment.

5   Empirical Framework

We will estimate the intent-to-treat effects of acquiring local language proficiency and participating in a local language learning class using the following specification:

$$y_{ir} = \alpha_r + \sum_{j=1}^{5} \beta_j TextVersion_{ir}^j + X_{ir}'\Theta + \epsilon_{ir}, \tag{1}$$

where y_{ir} is an indicator for whether the listing representative (e.g., realtor) i in region r provides a positive response. We also consider whether the email is opened and any response as separate outcomes. The variable $TextVersion_{ir}^j$ is an indicator for receiving message variation $j \in \{1, 2, 3, 4, 5\}$ (version $j = 0$ is omitted).[11] Since the randomization is carried out within each day of the week that the email is sent, we include the day-region strata fixed effects (α_r) in all specifications.[12] The vector X_{ir} includes characteristics rental property. For example, we control for the rent, size of property, number of rooms, and whether it is furnished.[13] Additionally, we control the message sender's gender and foreign group. As our sample varies across countries, we weight the observations so that each country gets equal weight.

The main coefficients of interest are the $\beta_j s$, reflecting the differential response rate of receiving text version j from a foreign-sounding name relative to a message written by a native speaker with a native-sounding name. As the randomization is at the listing-level, we will use robust standard errors. Our main analysis will involve pooling all the countries together, however, we will also estimate the model separately for each country.

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[11]Omitted text version is the control group, native sounding name and proficient local language.

[12]We use the most populated cities as the regions and pool the smaller cities together into a different strata.

[13]In the few exceptional cases when a control variable is missing, we will employ the missing indicator approach, using either the mean or mode for imputation.

6 Results

6.1 Main effects

Figure 2 (and Tables 2) summarize the main experimental effects for our primary outcome, the unconditional positive response rate (i.e., receiving a viewing invitation). We begin with inquiries sent under a foreign-sounding name. When the message is written in English, the initial positive response rate (within the first three business days) is about 14%. Adding a signal of ongoing language learning raises the response rate slightly to about 17%. Moving from English to functional A2-level local language yields response rates that are very similar to English, and the learning signal does not affect the success rate. By contrast, fluent local-language inquiries substantially increase access: the initial positive response rate rises to about 27%, nearly double the English baseline. Finally, holding language fixed at fluent local language, switching the name from foreign- to native-sounding yields a response-rate difference of roughly 5 percentage points, in the direction documented in the correspondence-test literature. We interpret this native–foreign gap cautiously: the experiment is primarily powered to detect larger language-proficiency contrasts, and the implied name gap is not precisely estimated in our setting.

We next consider the ultimate positive response rate after incorporating the follow-up (sent to non-responders after three business days). The follow-up shifts response rates upward across treatments, monotonically increasing for foreign-name inquiries with language acquisition, and a somewhat smaller increase for native-name inquiries. For English, the follow-up has the smallest increase by around 4 percentage points; however, a fluent inquiry by a foreign-name sender has the largest positive response rate increase by about 9 percentage points. Importantly, after the follow-up, the difference between native- and foreign-sounding names conditional on fluent local language decreases. A fluent foreigner who is willing to follow up receives a modestly higher response rate than a native who only makes a single inquiry.[14] The results suggest that persistence can help mitigate the initial disadvantage faced by immigrants at the point of contact.

Table 3 reports formal hypothesis tests for the main contrasts discussed above. Without applying observation weights, we estimate a statistically significant native–foreign name gap of approximately 5 percentage points in the initial response window ($p < 0.05$). However, this gap is no longer statistically detectable once observations are weighted inversely by country sample size, nor does it persist after incorporating the follow-up. The language-learning signal yields small positive effects, but statistical significance is limited: when country weights are applied, we detect a 6-percentage-point increase in the ultimate response rate for English inquiries ($p < 0.05$), with no corresponding effects in other specifications. By contrast, the returns to local-language fluency are large and precisely estimated. Regardless of whether country weights are applied, fluency generates

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[14]Although we do not randomize the follow-up by treatment condition, our pilot testing showed that the likelihood of receiving a response after 3 business days is very small.

statistically significant gains relative to English for both the initial and ultimate response rates ($p < 0.01$).

Appendix Section B discusses the robustness of the main results. All of the main effects are robust to alternative types of responses (e.g., non-rejections coded as "not no") and conditioning on whether the email was opened.

6.2 Heterogeneous effects

Because the language-learning signal has at most small average effects, we pool the signal and no-signal conditions within each language category to increase precision in the heterogeneity analysis. We also focus on the ultimate positive response rate after the follow-up.

By country. Because our experiment spans four countries, we can assess whether the returns to local-language proficiency vary across institutional and market contexts. Figure 3a (and Table 4) reports treatment effects by country. The returns to fluency are largest in Slovakia, where fluent inquiries receive about 28 percentage points more viewing invitations than English inquiries. The effects are also sizable in Austria and the Czech Republic, at roughly 17 and 13 percentage points, respectively. In contrast, the estimated return to fluency in Hungary is small, around 3 percentage points, and is not statistically significant. Overall, with the exception of Hungary, becoming fluent in the local language substantially increases foreigners' access to rental housing.

By major cities. As English proficiency will vary across areas, we investigate heterogeneity by major cities versus other areas. Figure 3b reports the results. Outside major cities, A2 inquiries perform better than English, whereas in major cities the native–foreign name gap is not statistically detectable. This city pattern is consistent with the idea that screening frictions and the salience of language may differ across market segments. In major cities, some of which receive international tourists, English proficiency will be higher than in other regions. While many immigrants initially locate in large cities, housing costs may also push some search to smaller markets, making these differences relevant for understanding how language proficiency shapes access across locations.

Other heterogeneity. Appendix Section C reports additional heterogeneity analyses by listing rent, sender gender, sender origin, and the origin of the poster. Two patterns are worth highlighting. First, English-language inquiries perform particularly poorly for below-average rent listings, while overall positive response rates are higher for above-average rent listings. This suggests that access may be relatively easier for new immigrants in more expensive segments of the market. Second, we find heterogeneity by historical origin: foreign groups that were not historically part of the Austro-Hungarian Empire receive approximately 10 percentage points fewer viewing invitations

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than native-sounding names even when inquiries are written in fluent local language, whereas no detectable name-based gap is observed for foreign groups originating from within the former empire.

6.3 Perceived benefits of local-language learning

We next compare experimental estimates to beliefs elicited in the forecast survey. In addition to understanding the beliefs of foreigners which may affect their motivations to learn the local language, we also aim to survey the native residents. If native residents systematically underestimate the private benefits of immigrants' language acquisition, they may be less supportive of public investments in language training or may favor policies that emphasize basic proficiency rather than sustained pathways to fluency. In turn, such belief gaps could contribute to underprovision of integration programs precisely in settings where our experimental results suggest that the largest returns occur at the fluency margin.

Starting by pooling foreign and native residents, Figure 4a and Figure 4b show that beliefs are systematically below experimental benchmarks: roughly three out of four respondents underestimate the returns to becoming fluent in the local language (relative to writing in English). Figure 5a summarizes average perceived versus actual gains. On average, respondents believe that moving from English to fluency increases the positive response rate (for initial inquiries) by about 5.3 percentage points, roughly half of the experimental estimate of about 11 percentage points. This underestimation holds in all countries except Hungary. In Hungary, respondents expect meaningful returns to fluency even though the initial experimental estimates show little difference; however, when outcomes are measured after the follow-up, the experimental return becomes closer to the perceived return (noting that the follow-up was not described in the survey instrument).

Finally, Figure 5b compares beliefs between non-native respondents (i.e., foreigners) and native-language speakers. We find that both foreigners and natives report broadly similar average beliefs about the returns to local-language fluency, though belief levels vary across countries. With the exception of Austria, foreigners tend to report somewhat higher perceived benefits of learning the local language than natives. One interpretation is that foreigners in our sample have updated beliefs based on their personal experiences and realize the importance of local language fluency. For example, they may have experienced "near misses" in the housing search process—responses that are close to positive but hinge explicitly on communication and contracting in the local language—as observed in many instances in our email responses. Whereas natives will rarely account for difficulties in renting due to language constraints, and therefore may evaluate the returns to fluency from a different reference point.

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7 Discussion of Mechanisms and Policy Implications

This section provides suggestive evidence on underlying screening mechanisms, discusses the welfare and policy implications of our findings, and considers implications for the design and interpretation of correspondence tests.

Mechanisms. We use AI-assisted qualitative text analysis of email responses to provide suggestive evidence on mechanisms that may help explain the differences in positive response rates between fluent and non-fluent inquiries. Our primary sample consists of replies that are conditionally positive (coded as "yes, but ...") in which the respondent indicates that the apartment is available but requests additional information before scheduling a viewing (for example, asking about employment status). The analysis sample includes approximately 350 such replies. Using AI, we classify whether these follow-up inquiries pertain to language or communication, residency status or nationality, intended rental duration, employment status, or other topics.

Table B.7 reports the share of all replies (both positive and negative) that include each type of inquiry by treatment. As a basic sanity check, we find that inquiries written in fluent local language never elicit follow-up questions related to language or communication, whereas A2-level and English inquiries do so more frequently. More generally, non-fluent inquiries (A2 or English) are associated with a higher incidence of follow-up questions across several categories, as well as a greater number of follow-up inquiries overall, relative to fluent local-language messages sent under foreign-sounding names. For fluent inquiries, foreign-sounding names receive slightly more follow-up questions on average, though the differences are small. While these patterns are descriptive, they are consistent with the interpretation that non-fluent inquiries may trigger additional screening along multiple dimensions beyond language itself.

The higher incidence of follow-up inquiries for non-fluent messages is consistent with a screening process in which landlords and realtors attempt to manage expected transaction costs and uncertainty at early stages of contact. Non-fluent inquiries may be perceived as increasing potential communication frictions or ambiguity about longer-term plans, which could lead respondents to seek additional information before moving forward. From this perspective, language proficiency may influence first-stage access by shaping expectations about the ease of contracting and ongoing coordination during the tenancy, rather than reflecting taste-based preferences over applicant origin.

Welfare and policy Implications. The results have important implications for immigrant welfare and for integration policy. When limited local-language proficiency reduces the probability of obtaining a viewing invitation, it can distort housing choices along several margins. One channel is through prices: we find that English inquiries perform relatively better for above-average rent

listings than for below-average rent listings (Figure 11). The result suggests that non-fluent new-comers may face a steeper access penalty in the more affordable segment and may therefore either pay a price premium for responsiveness or rely more heavily on informal channels. A second channel is through location and segregation: if non-fluent applicants are screened out of the mainstream market, they may depend more on co-ethnic networks or referrals, potentially limiting access to preferred and diverse neighborhoods. A third channel is through search costs: lower response rates can mechanically lengthen time-to-match, increasing expenditures on temporary accommodation (e.g., an Airbnb apartment) and raising the opportunity cost of delayed negotiation. These welfare consequences are further amplified by our survey results: foreign residents underestimate the returns to reaching fluency by roughly 50%, implying that perceived incentives to invest in language learning may be too low.

Our findings point to three practical policy levers that could reduce the access penalty associated with limited local-language proficiency at the first stage of housing search. First, lowering communication frictions at initial contact may improve access: professional language support (e.g., assistance in composing the initial inquiry in the local language and, when relevant, help with phone calls or key interactions such as viewings and contract discussions) can plausibly increase responsiveness by reducing perceived transaction costs for landlords and realtors. At the same time, our qualitative evidence suggests that language assistance alone may not fully eliminate concerns about longer-run communication during the tenancy or uncertainty about the intended duration of stay, which may continue to shape selection at later stages. Second, search behavior matters: because many responses are "yes, but ..." and non-response is common, following up after a reasonable waiting period is a low-cost margin that may improve effective contact rates for applicants at all language levels. Third, language-training policies may be more effective when paired with clearer information about the returns to fluency. Given that foreign residents in our survey substantially underestimate these returns, providing credible, quantitative guidance on how fluency affects access to core markets could strengthen incentives to invest in language acquisition and improve targeting of integration resources.

Designing correspondence tests.   Finally, and central to our contribution, the results also have implications for the design and interpretation of correspondence tests or audit studies. Our findings show that omitting realistic variation in local-language proficiency and plausible applicant behavior, such as persistence, can meaningfully affect measured access at first contact. Two implications follow. First, the name-based gaps typically studied in correspondence tests may be overstated when follow-up behavior is omitted, since a simple follow-up by applicants with minority-sounding names may substantially reduce these differences. Second, and more importantly, the barriers faced by immigrants with limited local-language proficiency—such as those relying primarily on English in the European context—are substantially larger than what is implied by correspondence tests that

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assume fluent communication. These language-related access penalties are sufficiently large that they cannot be closed by persistence alone, highlighting the importance of incorporating realistic language constraints into correspondence-test designs.

8   Conclusion

Correspondence tests are a central tool for measuring immigrants' access to core markets, typically by comparing responses to otherwise identical inquiries that differ only in the sender's name. A key, often implicit, design assumption in this literature is that applicants communicate fluently in the local language. Our findings show that this assumption can be consequential in settings where access is determined through brief, informal screening. By extending the correspondence-test framework to incorporate realistic variation in language proficiency—and allowing for a simple, behaviorally plausible follow-up—we show that limited local-language proficiency is a first-order determinant of first-stage access in rental housing. The results further indicate that persistence can partially mitigate initial disadvantages, and that residents substantially underestimate the returns to achieving local-language fluency. Taken together, these findings underscore how standard correspondence-test designs may understate barriers faced by immigrants when language proficiency varies in practice.

In the rental housing context, three main findings summarize these patterns. First, local-language fluency is a central determinant of first-stage housing access. Relative to English inquiries from foreign-sounding names, fluent local-language inquiries nearly double the probability of receiving a viewing invitation. Second, we find no measurable benefit of functional A2-level proficiency beyond English, and signaling ongoing language learning has, at most, small effects on access. Third, persistence matters: sending a single follow-up to non-responders raises invitation rates and eliminates the detectable native–foreign name gap among fluent inquiries. Together, these results indicate that access frictions in rental housing are shaped not only by name-based differentials but also, more importantly, by whether applicants can communicate fluently at the first point of contact and whether they continue to engage after an initial non-response.

Our heterogeneity analysis shows that the returns to fluency are present across settings but vary in magnitude. Fluency yields especially large gains in Slovakia, sizable gains in Austria and Czechia, and smaller, statistically indistinguishable gains in Hungary. We also find that language-related screening frictions are particularly consequential in the more affordable segment of the market: the proficiency gradient is substantially sharper for below-average rent listings, implying that English-only newcomers may be screened out of a larger share of low-rent options. In contrast, overall response rates are higher for above-average rent listings, suggesting that access may be relatively easier in higher-rent segments even when language assistance is available. Evidence from the content of replies supports a transaction-cost interpretation. Conditionally positive responses ("yes,

but ...") frequently reference communication concerns, often also inquiring about residency status, expected lease duration, and employment. These concerns appear disproportionately in replies to English and A2 messages, consistent with the view that fluency directly lowers expected hassle and perceived risk at the screening stage,

The beliefs survey highlights an additional friction relevant for social integration. Residents—both natives and foreigners—systematically underestimate the gain from moving from English to fluency in the local language. Roughly three-quarters of respondents predict a smaller fluency premium than the one observed in the field experiment, and on average they underestimate the return to fluency by about 50 percent. This wedge between perceived and actual returns suggests that immigrants may underinvest in language acquisition if they rely on pessimistic beliefs about the benefits of becoming fluent in markets where access is determined at first contact.

Finally, our findings have implications for immigrant welfare, integration policy, and the interpretation of correspondence tests. When limited language proficiency reduces first-stage access, it can distort housing search along several margins: pushing non-fluent newcomers toward market segments where English is more accommodated (potentially at higher rents), increasing reliance on informal networks with implications for residential sorting, and mechanically lengthening search duration, raising short-run costs such as temporary accommodation. From a policy perspective, the results suggest that moving immigrants from partial proficiency to fluency may be particularly important in gatekeeper-mediated markets, and that language instruction may be complemented by clearer information about the magnitude and shape of the returns to proficiency.

Overall, the findings highlight that correspondence-test and audit designs that abstract from realistic language constraints—and from plausible applicant behavior such as persistence—may misstate barriers in markets where communication frictions play a central role. When follow-up behavior is omitted, the native–minority name gap measured at first contact may be overstated, while the barriers faced by minorities with limited local-language proficiency may be substantially understated.

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Tables

Table 1: Balance Test - Listing Characteristics by Treatment

	(1) Fluent, native name	(2) Fluent, foreign name	(3) A2, learning local language	(4) A2	(5) English, learning local language	(6) English	F-test
rent (EUR/month)	791.855	782.989	825.254	789.093	784.363	772.484	0.541
	(35.111)	(25.767)	(32.197)	(28.233)	(26.671)	(23.857)	[0.746]
size (m^2)	96.860	81.605	69.785	96.009	82.205	85.551	1.422
	(21.427)	(14.882)	(3.064)	(16.337)	(8.356)	(10.233)	[0.213]
bedrooms	2.079	2.074	2.096	2.137	2.069	2.093	0.404
	(0.039)	(0.039)	(0.042)	(0.041)	(0.038)	(0.038)	[0.846]
I(elevator)	0.379	0.406	0.424	0.408	0.400	0.423	0.980
	(0.020)	(0.020)	(0.021)	(0.020)	(0.020)	(0.020)	[0.428]
I(balcony)	0.470	0.458	0.467	0.478	0.480	0.497	0.436
	(0.020)	(0.021)	(0.021)	(0.021)	(0.020)	(0.020)	[0.823]
I(furnished)	0.379	0.371	0.387	0.401	0.384	0.364	0.460
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	[0.806]
Observations	609	589	576	588	613	612	3,587

Notes: The dependent variable is an indicator for successfully obtaining an apartment viewing opportunity after sending (1,1') an initial request and (2,2') a follow-up email if no response is received after three business days. The key regressors are indicators for treatment groups, in which foreign-sounding names send messages of varying local-language proficiency: fluent, functional A2 with and without a language-learning signal, and proficient English with and without a language-learning signal; the omitted category is the control group, in which native-sounding names sending fluent local-language messages. Strata fixed effects are defined by city-size groups and the day of the week the message is sent. Controls include rent (EUR), apartment size (m^2), number of bedrooms, and indicators for each of whether the apartment is furnished, includes a balcony, and is located in a building with an elevator. For the 2% of observations with missing covariates, we impute missing values with the sample mean and include missing-value indicators. Observations are not weighted. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Effects of Language Acquisition on "yes" and "not no" Responses

	"yes" replies		"not-no" replies	
	(1)	(2)	(3)	(4)
	I(initial yes)	I(ultimate yes)	I(initial not-no)	I(ultimate not-no)
Fluent, foreign name	-0.0435	-0.0202	-0.0740	-0.0226
	(0.036)	(0.037)	(0.063)	(0.038)
A2, learning local language	-0.159***	-0.141***	-0.239***	-0.179***
	(0.033)	(0.035)	(0.062)	(0.038)
A2	-0.160***	-0.165***	-0.210***	-0.187***
	(0.033)	(0.034)	(0.062)	(0.038)
English, learning local language	-0.139***	-0.152***	-0.213***	-0.204***
	(0.033)	(0.034)	(0.061)	(0.037)
English	-0.175***	-0.203***	-0.218***	-0.227***
	(0.032)	(0.033)	(0.062)	(0.037)
Strata FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3,587	3,587	3,587	3,587

Notes: The dependent variable is an indicator for successfully obtaining an apartment viewing opportunity ("yes") or receiving a response that is not an explicit rejection ("not no"). Columns (1) and (3) are initial responses to the original request and (2) and (4) are ultimate responses after a follow-up email if no response is received within three business days. The key regressors are indicators for treatment groups, in which foreign-sounding names send messages of varying local-language proficiency: fluent, functional A2 with and without a language-learning signal, and proficient English with and without a language-learning signal; the omitted category is the control group, in which native-sounding names sending fluent local-language messages. Strata fixed effects are defined by city-size groups and the day of the week the message is sent. Controls include rent (EUR), apartment size (m^2), number of bedrooms, and indicators for each of whether the apartment is furnished, includes a balcony, and is located in a building with an elevator. For the 2% of observations with missing covariates, we impute missing values with the sample mean and include missing-value indicators. Observations are weighted such that each country receives equal weight. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Hypothesis Test of the Primary Experimental Contrasts

Comparison Groups	Null Hypothesis	Initial "yes"		Ultimate "yes"	
		(1)	(2)	(3)	(4)
Panel A: Ethnic Disadvantage					
Foreign vs. Native	$\beta_2 = 0$	−0.05**	−0.04	−0.043	−0.021
		[0.047]	[0.267]	[0.126]	[0.574]
Panel B: Effects of Language Proficiency					
Fluent English to Functional-Local (A2)	$\beta_4 - \beta_6 = 0$	0.021	0.022	0.039	0.046
		[0.313]	[0.422]	[0.116]	[0.124]
Functional-Local (A2) to Fluent-Local	$\beta_2 - \beta_4 = 0$	0.098***	0.119***	0.14***	0.144***
		[< 0.001]	[< 0.001]	[< 0.001]	[< 0.001]
Fluent English to Fluent-Local	$\beta_2 - \beta_6 = 0$	0.119***	0.142***	0.178***	0.19***
		[< 0.001]	[< 0.001]	[< 0.001]	[< 0.001]
Panel C: Effects of the Language Learning Signal					
For Fluent English	$\beta_5 - \beta_6 = 0$	0.016	0.045	0.033	0.06**
		[0.439]	[0.113]	[0.178]	[0.043]
For Functional-Local (A2)	$\beta_3 - \beta_4 = 0$	0.006	0.001	0.022	0.021
		[0.77]	[0.966]	[0.391]	[0.514]
Strata FE		Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes
Equally Weight Countries		No	Yes	No	Yes

Notes: Table presents hypothesis test of the key contrasts using the estimated regression coefficients in Table 2. The dependent variable is an indicator for successfully obtaining an apartment viewing opportunity ("yes"). Columns (1) and (2) are initial responses to the original request, and (3) and (4) are ultimate responses after a follow-up email if no response is received within three business days. The main entries are estimated differences, and the corresponding p-values are in square brackets. Strata fixed effects are defined by city-size groups and the day of the week the message is sent. Controls include rent (EUR), apartment size (m^2), number of bedrooms, and indicators for each of whether the apartment is furnished, includes a balcony, and is located in a building with an elevator. Observations in columns (2) and (4) are weighted such that each country receives equal weight.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effects of Language Acquisition on "yes" Responses by Country

	(AT) I(ultimate yes)	(CZ) I(ultimate yes)	(HU) I(ultimate yes)	(SK) I(ultimate yes)
Fluent, foreign name	0.0358	-0.0634*	-0.0476	-0.0126
	(0.070)	(0.035)	(0.069)	(0.105)
A2, foreign name	-0.134**	-0.189***	-0.0720	-0.228**
	(0.055)	(0.030)	(0.059)	(0.089)
English, foreign name	-0.0950*	-0.237***	-0.0788	-0.311***
	(0.056)	(0.029)	(0.057)	(0.090)
Strata FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	544	2,429	344	270

Notes: The dependent variable is an indicator for ultimately obtaining an apartment viewing opportunity, where a follow-up email is sent if no response is received after three business days. The key regressors are indicators for treatment groups, in which foreign-sounding names send messages of varying local-language proficiency: fluent, functional A2 (both with and without a language-learning signal), and proficient English (both with and without a language-learning signal); the omitted category is the control group, in which native-sounding names sending fluent local-language messages. Strata fixed effects are defined by city-size groups and the day of the week the message is sent. Controls include rent (EUR), apartment size (m^2), number of bedrooms, and indicators for each of whether the apartment is furnished, includes a balcony, and is located in a building with an elevator. For the 2% of observations with missing covariates, we impute missing values with the sample mean and include missing-value indicators. Observations are weighted such that each country receives equal weight. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# Figures

Figure 1: Distribution of Listings
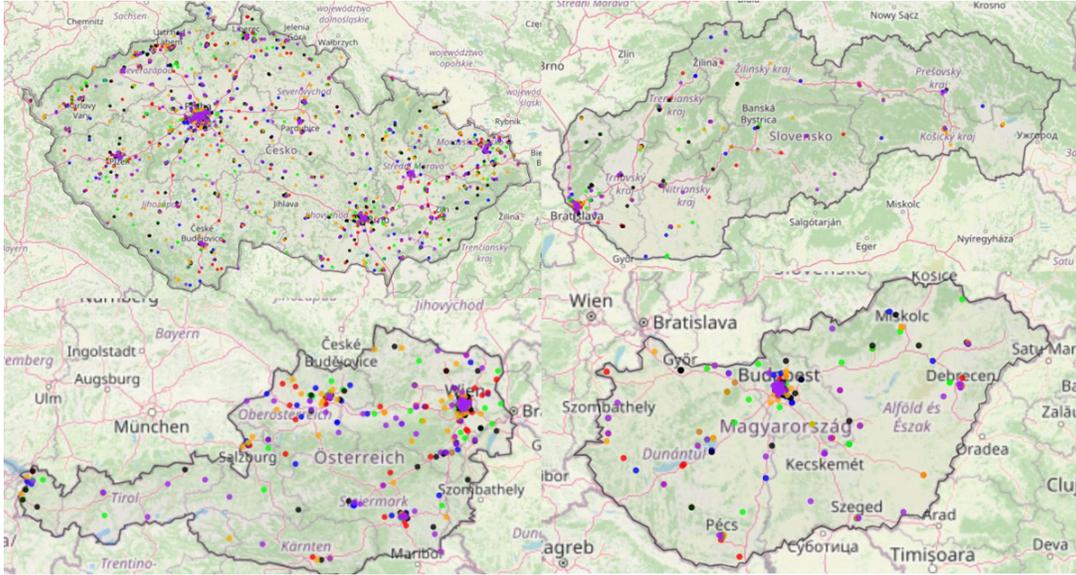
Notes: The figure illustrates the distributions of listings in our sample across Austria (bottom left), Czechia (top left), Hungary (bottom right), and Slovakia (top right). The boundaries of each country are outlined in black. Each dot is a rental listing, plotted using the GPS coordinates that correspond to the address of the rental property. The color of the dot indicates the experimental condition. The control group, native name with fluent local language, is indicated in blue. The treatment groups with foreign names are as follows: fluent (red), A2 with the local-language signal (green), A2 only (black), English with the local-language signal (orange), and English only (purple).

Figure 2: 'Yes' Responses by Treatment
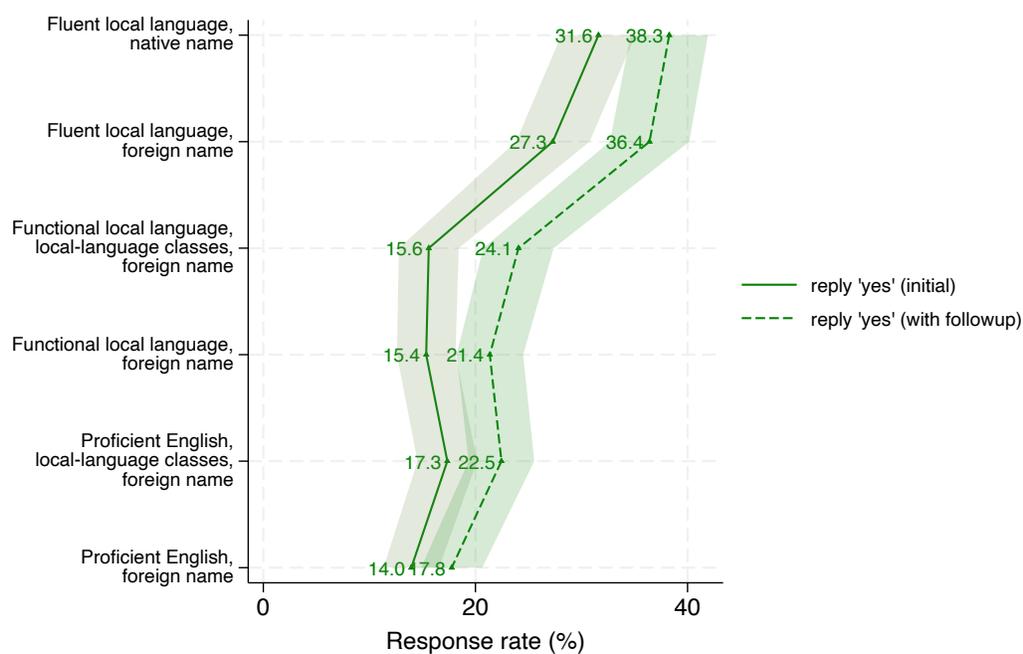
Notes: Actual rates of successfully obtaining an apartment-viewing opportunity by experimental group. The dots represent group means, with each observation weighted such that each country receives equal weight. The solid line represents initial response rates to the original request, and the dashed line represents ultimate response rates after a follow-up email if no response is received within three business days. Shaded bands represent 95% confidence intervals.

**Figure 3: Heterogeneity in Ultimate Viewing Invitations**
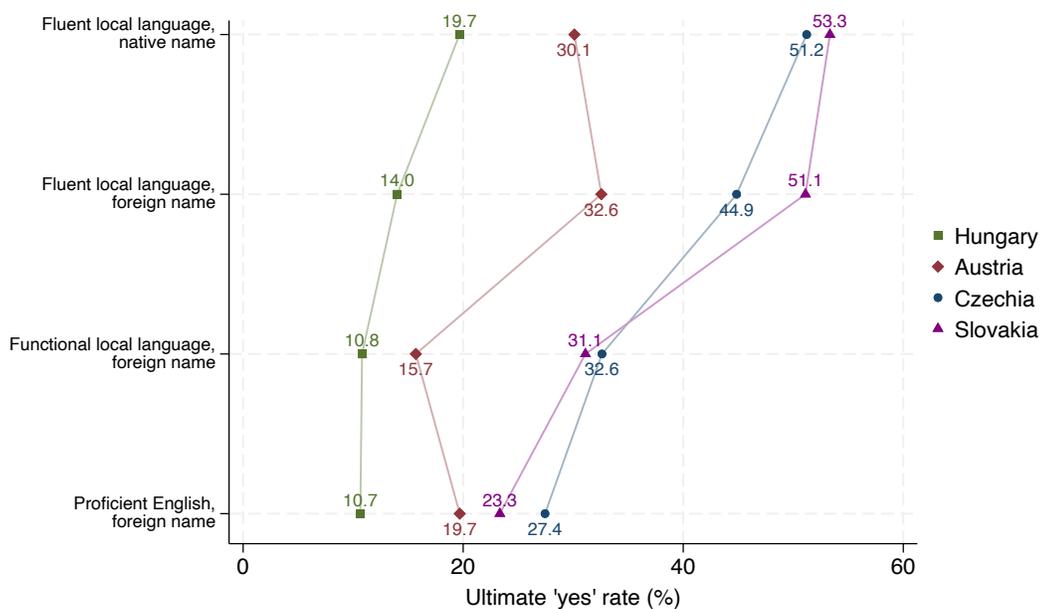

(a) By country
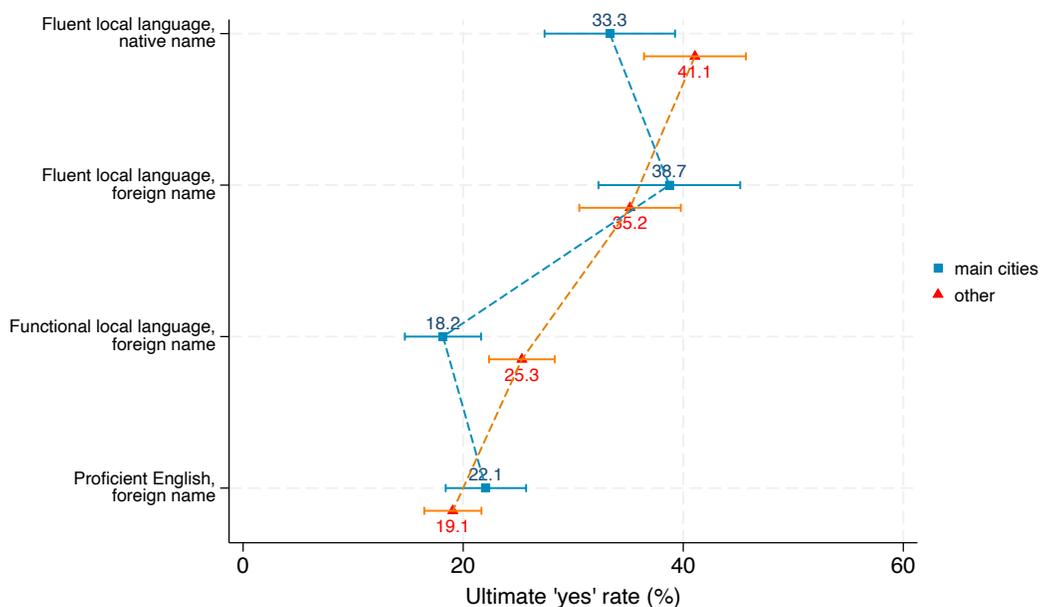
(b) Major cities versus other areas

Notes: Panel (a) reports actual rates of ultimately obtaining an apartment-viewing invitation by experimental group and country. Dots represent group means, color-coded by country: Hungary (green), Austria (red), Czech Republic (blue), and Slovakia (purple). Panel (b) reports ultimate viewing-invitation rates by experimental group in major cities (blue squares) versus the rest of the country (red triangles). Markers represent group means with 95% confidence intervals. In Panel (b), observations are weighted so that each country receives equal weight.

Figure 4: Perceived English-to-Fluent Returns Ga Distribution
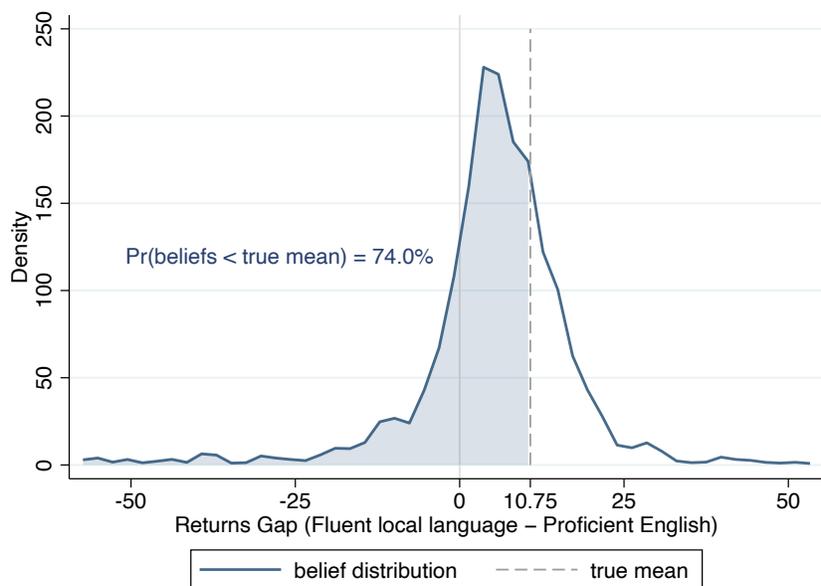

(a) Full sample
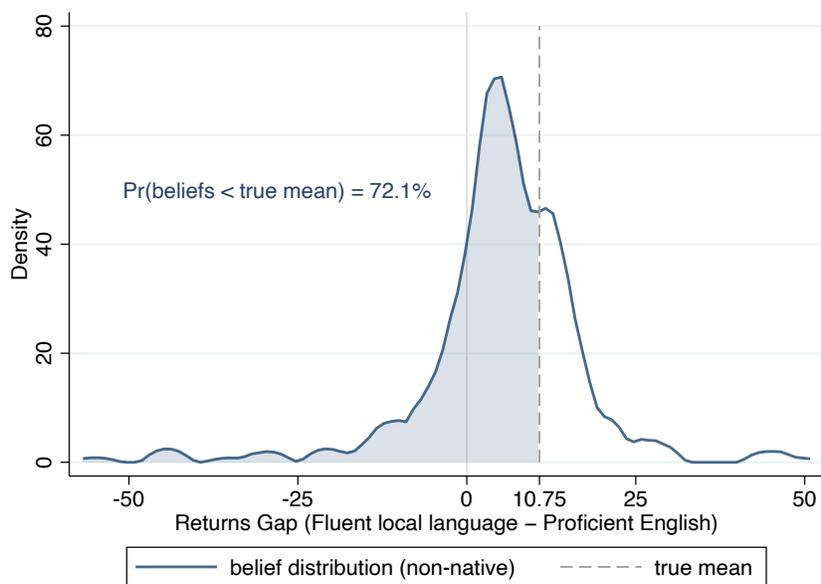
(b) Respondents whose native language is not the local language

Notes: Each panel shows the belief distribution of countries' residents about the difference in initial success rates between sending fluent local-language versus proficient English apartment-viewing requests. Panel (a) reports beliefs for the full sample of respondents, while Panel (b) restricts the sample to respondents whose native language is not the local language. The dashed vertical line indicates the true mean difference estimated in the field experiment. Observations are weighted so that each country receives equal weight. Only respondents who passed all attention checks are included.

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Figure 5: Perceived Returns to Local-Language Fluency Relative to English, by Country
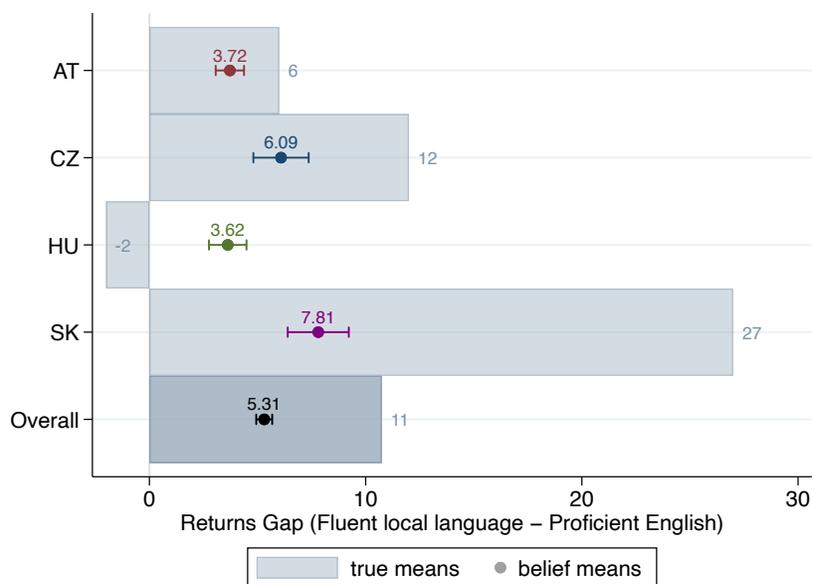
(a) Full sample
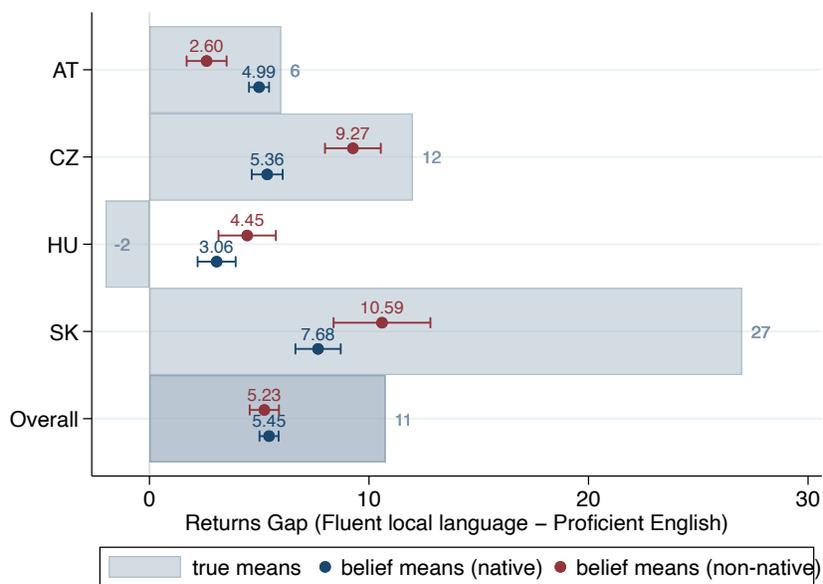
(b) By respondents' local-language fluency

Notes: Each panel compares actual versus perceived benefits of fluent local-language versus proficient English when sending apartment-viewing requests in terms of initial success rates—by country and overall. Bars represent the true mean differences estimated in the field experiment. Dots represent belief means elicited from resident surveys, with 95% confidence intervals. Panel (a) reports beliefs for the full sample of respondents. Panel (b) further distinguishes belief means for respondents who are fluent in the local language (navy) and those who are not fluent (red). For the overall row, observations are weighted so that each country receives equal weight. Only respondents who passed all attention checks are included.

A   Conceptual Framework and Hypothesis

In this section, we will use economic reasoning, features of the institutional background, and prior literature to formulate hypotheses for the field experiment. The prior literature already documents a native-name-based advantage in social settings, including the housing market. The native name and fluent local language serve as the natural control group, and we hypothesize the following.

Hypothesis 1 (Ethnic Differential): For fluent inquiries, native-sounding names will receive a higher viewing invitation rate than foreign-sounding names.

The core focus of our field experiment is to assess the effects of local language proficiency on foreign-name applicants' access to rental housing. Realtors and landlords reasonably screen applicants based on expected stability and anticipated transaction costs. Language proficiency is a salient signal in this screening process because it affects the ease of communication, intentions for longer-term tenancy, and the expected hassle of contracting and tenancy management. For a prospective tenant with a foreign-sounding name, writing fluently in the local language can credibly signal that key interactions—arranging viewings, clarifying terms, and signing the lease—will proceed smoothly without communication barriers. Beyond mitigating communication barriers, local-language fluency (and, potentially, language learning) may also serve as a proxy of cultural integration and longer-term residence plans, which landlords may associate with stability. Although limited proficiency may not meaningfully reduce these frictions, it can signal willingness to integrate.

Hypothesis 2 (Language proficiency): Greater local-language proficiency will increase the probability of receiving a positive response (invitation to view) relative to sending the inquiry in English. Fluent inquiries receive the highest response rates, followed by functional A2, and then English.

Signaling ongoing language learning can further affect beliefs, but clearly more weakly than demonstrated fluency. A stated commitment to learning may indicate motivation, willingness to invest in integration, and improved future communication. It may, however, be discounted if landlords prioritize immediate transaction ease.

Hypothesis 3 (Learning signal): Conditional on proficiency, signaling ongoing local-language learning will weakly increase the probability of receiving a positive response.

Finally, non-response need not reflect outright rejection. Landlords and realtors often face limited attention and inbox congestion, and some inquiries may be missed or deprioritized. A follow-up message can mitigate these attention frictions by resurfacing the inquiry and also signaling contin-

ued interest. As a result, persistence may raise response rates broadly.

Hypothesis 4 (Persistence / follow-up): Sending a follow-up message will increase the probability of receiving a positive response across all treatments.

All hypotheses stated above were pre-registered on the Social Science Registry (ID 12827) before beginning the field experiment.

B Robustness

In this section, we investigate the robustness of our main results to different specifications of the outcome and conditioning on whether the email was opened.

B.1 Examining alternative outcomes

Conditional positive response ("not-no").   Our primary outcome is an unconditional positive response: a viewing invitation without further questions. In practice, many replies are *conditionally* positive, in the sense that the landlord or realtor requests additional information (e.g., employment status) before offering a viewing. We classify these responses as "not-no" rather than "yes." Although these replies are not immediate invitations, they plausibly represent meaningful progress in the screening process and may convert into viewings once the requested information is provided.

Figure 10 (and Table 2) reports "not-no" response rates before and after the follow-up for each experimental condition. By construction, "not-no" rates exceed "yes" rates in both the initial and ultimate windows. Importantly, however, the qualitative treatment patterns closely mirror the primary analysis. English and functional A2 local language perform similarly, while fluent local-language inquiries generate a large increase in positive engagement for foreign-sounding names. For example, after the follow-up, moving from functional A2 to fluent local language increases the "not-no" rate by about 16 percentage points (from 36% to 52%).

Receiving any reply and negative responses.   Figure 10 also reports the overall reply rate (any response, regardless of content) before and after the follow-up. The gap between the overall reply rate and the "not-no" rate within a condition captures the share of explicitly negative replies. Two patterns emerge. First, overall reply rates shift upward after the follow-up across all conditions, consistent with the follow-up mitigating inattention or inbox congestion. Second, the difference between overall replies and "not-no" replies is relatively small and does not vary substantially across conditions, suggesting that treatment effects are driven primarily by changes in positive engagement rather than by large changes in explicit rejections. As a result, treatment patterns for receiving any reply closely resemble those for the primary outcome.

B.2 Conditioning on opening the email

An advantageous feature of our email-based deployment is that we can observe whether an inquiry is opened. Figure 10 reports open rates by condition. Open rates increase with language proficiency, which may reflect differential attention to the subject line and preview text, or differential filtering of non-fluent messages (e.g., spam or junk folders). We interpret open-rate patterns cautiously, as the open rates are subject to measurement error.

As a robustness check, we re-estimate the main specifications conditioning on whether the email is opened. Although opening is not an ideal control (since it can itself be affected by the treatment), the results in Table B.5 show that the main effects are unchanged when restricting attention to opened emails: fluency continues to yield large gains relative to English and A2, with A2 providing no additional benefit.

C Additional Heterogeneity Analysis

By rent. Figure 11 examines heterogeneity by whether a listing is above- or below-average rent within its country. Two patterns emerge. First, for above-average rent listings, the English–A2 difference is not statistically detectable. In contrast, for below-average rent listings the proficiency gradient is much more monotonic: responses increase more clearly from English to A2 to fluent local language, implying that English-only newcomers face a particularly steep access penalty in the more affordable segment of the rental market. Second, overall positive response rates are higher for above-average rent listings, suggesting that the initial screening may be less stringent in the higher-rent segment.[15] Taken together, these patterns imply that even when an immigrant can obtain help drafting an inquiry in the local language (e.g., through a native speaker or translation tools), access may be relatively easier in more expensive parts of the market.

By the gender of the sender. Figure 12 examines heterogeneity by whether the inquiry is sent under a male- or female-sounding name. Across both the control and treatment conditions, we find no detectable differences by sender gender, suggesting that signals about gender does not materially shape first-stage access in rental housing.

By origin of the sender. We next turn to heterogeneity by the sender's origin group. Figure 15 reports the native–foreign gap separately for the three foreign groups considered in each country. We interpret these subgroup estimates cautiously, since splitting the sample by the three origin groups reduces precision. With this caveat in mind, the patterns are broadly consistent with our pooled results: in most cases, native-sounding names have a small advantage relative to each

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[15]Realtors may also prioritize renting more expensive properties that are likely to result in higher commission.

foreign group, but the differences are not statistically distinguishable from zero. One notable exception arises in Czechia, where inquiries with Vietnamese-sounding names receive an ultimate viewing-invitation rate that is about 9 percentage points lower than that of Czech-sounding names. Overall, the similarity of the estimated native–foreign gaps across most origin groups suggests that nationality-specific penalties are limited in this setting and that the primary barriers operate through language-related screening frictions rather than distinctions (e.g., cultural distance) across foreign origins.

We also examine heterogeneity by whether the sender's origin group was historically part of the Austro-Hungarian Empire. Table B.6 reports treatment effects separately for foreign groups that were part of the former empire (e.g., Poland or Slovakia) and those that were not (e.g., Vietnam or China). Three patterns emerge. First, among origin groups from within the former empire, we do not detect a native–foreign name gap for fluent inquiries, whereas for origin groups outside the empire a sizeable and statistically significant gap of roughly 9–12 percentage points remains. Second, consistent with the main results, the returns to local-language fluency are large for both sets of origin groups, indicating that fluency substantially improves access regardless of historical ties. Third, while functional A2 proficiency performs similarly to English for origin groups within the former empire, A2 inquiries perform significantly better than English for origin groups outside the empire, suggesting that limited proficiency may be interpreted differently depending on historical proximity.

By the origin of the poster. Lastly, we examine whether treatment effects vary with the inferred origin of the listing poster. We classify posters as native- versus foreign-origin based on their full names using an automated chatGPT (v5.2) name-classification procedure. We interpret these results with caution because name-based classification is inevitably measured with error, which can attenuate differences across groups. Figure 13 shows that the qualitative treatment pattern is similar regardless of poster origin: for foreign-sounding applicants, fluency in the local language substantially increases the probability of receiving a positive response whether the poster's name is inferred to be native or foreign. One difference stands out, however. Listings posted by foreign-name posters are about 10 percentage points more likely to invite a fluent foreign-name applicant to a viewing than a native-name applicant, although the estimates are imprecise because foreign-name posters represent a smaller share of the sample. A natural interpretation is that some foreign-origin posters may be more willing to engage with foreign applicants, but the overall pattern underscores that local-language fluency remains a key determinant of first-stage access even when the poster is plausibly foreign themselves.

D   Beliefs Survey Outline

This section will describe the different components of the belief survey and list the survey questions. After agreeing to participate in the study through the consent form, the survey began with some demographic questions.

Demographics.   There were three demographic questions (all questions had options: Yes, No, and I prefer not to say):

1. Were you born in [Country]?

2. Are you a citizen of [Country]?

3. Is [local language] your native language?

Purpose.   Next, the participants were presented with the purpose of the survey. Here is an example from the Czech Republic:

Figure 6: Beliefs Survey - Purpose of Study in Czech Republic

Background.   Then, the participants in the Czech Republic were given the following background information together with attention check questions:

Researchers studied how learning Czech affects a foreigner's success in booking apartment viewings.

They found that emails in **fluent Czech** from **native-names** received a viewing appointment **33%** of the time.

Using a **foreign-name**, they sent hundreds of emails with different language levels:

- **Fluent Czech**,
- **A2 Czech** (broken but understandable),
- **Fluent English**

Figure 7: Beliefs Survey - Background information in Czech Republic

How many language levels are there?

○ 1

○ 2

○ 3

○ 4

What is the success rate for viewing a property for those with a **native name** emailing in **fluent Czech**?

| 0 | 10 | 20 | 30 | 40 | 50 | 60 | 70 | 80 | 90 | 100 |

Native name and **fluent Czech**

○

Figure 8: Beliefs Survey - Background Questions for Attention Check

Bonus. Participants were given the following incentives "You can earn a bonus if you can guess the researchers' findings. An exact guess earns £1. Continue to the next page to start guessing."

Forecasts. Finally, the survey participants would be presented randomly with one of three language groups (fluent, English, or functional A2) to make their forecast. Here is an example in Czech Republic for the English forecast:

Figure 9: Beliefs Survey - Forecast for English Message

E Additional Tables

Table B.1: Distribution of Foreign Groups by Country

	Top 3 Foreign Group		
Country	**Group 1**	**Group 2**	**Group 3**
Austria	Romania	Slovakia	Turkey
Czech Republic	Poland	Ukraine	Vietnam
Hungary	China	Slovakia	Ukraine
Slovakia	Germany	Hungary	Ukraine

Notes: Table shows the three foreign groups used to construct the foreign names for the study in each country. The foreign groups were chosen based on their high prevalence and likelihood of not speaking the local language. There are 9 distinct foreign groups in the study.

Table B.2 Experiment names and survey results for Austria and Czechia

	Austria (n=39)		**Czechia** (n=38)	
	Name	Sounds foreign (in %)	Name	Sounds foreign (in %)
Native-sounding female names	Julia Huber	0	Tereza Dvořáková	0
	Lisa Weber	0.08	Martina Novotná	0
	Melanie Winkler	0.08	Michaela Svobodová	0
	Katharina Gruber	0.03	Nikola Procházková	0
	Stefanie Wagner	0	Kristýna Veselá	0
Foreign group 1	Ana Maria Ionescu	0.95	Anna Kamiński	0.97
	Mihaela Popescu	0.95	Maria Wiśniewski	1
Foreign group 2	Jelena Đorđević	0.95	Yulia Bondarenko	0.95
	Marija Marković	0.95	Anna Kovalenko	0.97
Foreign group 3	Fatma Çelik	0.95	Anh Hoang	1
	Ayşe Şahin	0.95	Linh Nguyen	1
Native-sounding male names	Lukas Huber	0.05	Jan Dvořák	0
	Michael Weber	0.10	Tomáš Novotný	0
	Daniel Winkler	0.05	Martin Svoboda	0
	Florian Gruber	0	Jakub Procházka	0
	Alexander Wagner	0	Michal Veselý	0

Table B.2 Experiment names and survey results for Austria and Czechia

	Austria (n=39)		**Czechia** (n=38)	
	Name	Sounds foreign (in %)	Name	Sounds foreign (in %)
Foreign group 1	Ştefan Ionescu	0.95	Jakub Kamiński	0.97
	Gabriel Popescu	0.90	Mateusz Wiśniewski	1
Foreign group 2	Nikola Đorđević	0.95	Artem Bondarenko	0.97
	Aleksandar Marković	0.95	Denys Kovalenko	0.97
	Mehmet Çelik	0.95	An Hoang	1
Foreign group 3	Mustafa Şahin	0.95	Ba Nguyen	1
	Discontinued names			
	Anna Schmidt	0.08	Mária Balážová	0.75
	Christina Müller	0.10	Anna Molnárová	0.24
	Maximilian Schmidt	0.03	Ján Baláž	0.75
	Daniel Müller	0.10	Jozef Molnár	0.34
	Marija Blažević	0.95	Sofiya Smirnova	0.97
	Ana Pavlović	0.90	Alexandra Ivanova	0.86
	Ivan Blažević	0.92	Alexander Smirnov	0.95
	Marko Pavlović	0.95	Artyom Ivanov	0.97

Notes: Table reports the percentage of Prolofic respondents who identified each name as sounding-foreign in Austria and Czechia. Both native and foreign names are included. There is an almost equal distribution of male and female names. Only respondents who passed all attention checks are included.

Table B1.3 Experiment names and survey results for Hungary and Slovakia

	Hungary (n=39)		**Slovakia** (n=35)	
	Name	Sounds foreign (in %)	Name	Sounds foreign (in %)
Native-sounding female names	Andrea Nagy	0	Mária Balážová	0
	Krisztina Kovács	0	Anna Molnárová	0
	Katalin Tóth	0	Zuzana Vargová	0
	Mónika Szabó	0	Katarína Kovácová	0.18
	Szilvia Horváth	0	Helena Balogová	0.03
Foreign group 1	Yulia Bondarenko	1	Andrea Nagy	0.68
	Anna Kovalenko	0.95	Krisztina Kovács	0.91
Foreign group 2	Yu Yan Chen	1	Yulia Bondarenko	1
	Hui Yin Liu	1	Anna Kovalenko	0.91
Foreign group 3	Mária Balážová	1	Anna Schmidt	1
	Anna Molnárová	0.83	Christina Müller	1
Native-sounding male names	Ádám Nagy	0	Ján Baláž	0.03
	Dávid Kovács	0	Jozef Molnár	0.03
	Levente Tóth	0	Peter Varga	0
	Máté Szabó	0	Štefan Kovác	0.18
	Bence Horváth	0	Michal Balog	0.06
Foreign group 1	Artem Bondarenko	1	Ádám Nagy	0.91
	Denys Kovalenko	1	Dávid Kovács	0.79
Foreign group 2	Wei Chen	1	Artem Bondarenko	1
	Jie Liu	1	Denys Kovalenko	1
Foreign group 3	Ján Baláž	0.97	Lukas Huber	0.8
	Jozef Molnár	0.79	Michael Weber	1

	Discontinued names			
	Anna Schmidt	0.62	Erika Lakatošová	0.10
	Christina Müller	1	Sandra Berková	0

Table B1.3 Experiment names and survey results for Hungary and Slovakia

	Hungary (n=39)		**Slovakia** (n=35)	
	Name	Sounds foreign (in %)	Name	Sounds foreign (in %)
	Maximilian Schmidt	1	Koloman Lakatoš	0.26
	Daniel Müller	0.92	Zoltán Berky	0.49
	Ana Maria Ionescu	1	Tereza Dvořáková	0.68
	Mihaela Popescu	1	Martina Novotná	0.09
	Ştefan Ionescu	1	Jan Dvořák	0.72
	Gabriel Popescu	1	Tomáš Novotný	0.06

Notes: Table reports the percentage of Prolific respondents who identified each name as sounding-foreign in Hungary and Slovakia. Both native and foreign names are included. There is an almost equal distribution of male and female names. Only respondents who passed all attention checks are included.

Table B.4: Email Bodies (Initial, Follow-up, and Thank You) in Austria

Treatment	Initial Email	Follow-Up Email	Thank You Email
Fluent local language	Hallo, Ich bin an ihrer Wohnungsanzeige interessiert. Ist die Wohnung noch verfügbar? Mein Zeitplan für eine Besichtigung ist flexibel. Vielen Dank!	Hallo, Ich bin noch auf der Suche nach einer Wohnung. Ist die Wohnung noch verfügbar? Mein Zeitplan für eine Besichtigung ist flexibel. Vielen Dank!	Vielen Dank für Ihre Antwort, ich brauche keine Wohnung mehr.

Treatment	Initial Email	Follow-Up Email	Thank You Email
Functional (A2), Language Class	Hallo, Ich mag dein angebot für Wohnung. Es ist noch frei? Ich kann komme alle Zeit, aber nicht Samstag am morgens da ich gehe zu einem Deutsch Kurs. Vielen Dank!	Hallo, Ich noch suche nach eine Wohnung. Es ist noch frei? Ich kann komme alle Zeit, aber nicht Samstag am morgens da ich gehe zu einem Deutsch Kurs. Vielen Dank!	Vielen Dank für Ihre Antwort, ich brauche keine Wohnung mehr.
Functional (A2)	Hallo, Ich mag dein angebot für Wohnung. Es ist noch frei? Ich kann komme alle Zeit. Vielen Dank!	Hallo, Ich noch suche nach eine Wohnung. Es ist noch frei? Ich kann komme alle Zeit. Vielen Dank!	Vielen Dank für Ihre Antwort, ich brauche keine Wohnung mehr.
Fluent English, Language Class	Hello, I am interested in your advertisement about the rental property. Is it still available to rent? My schedule is flexible for viewing the property, except Saturday morning because I am taking a German language class. Thank you!	Hello, I am still looking for an apartment. Is it still available to rent? My schedule is flexible for viewing the property, except Saturday morning because I am taking a German language class. Thank you!	Thank you so much for your response, I no longer need an apartment.

Treatment	Initial Email	Follow-Up Email	Thank You Email
Fluent English	Hello,	Hello,	Thank you so much for your response, I no longer need an apartment.
	I am interested in your advertisement about the rental property. Is it still available to rent?	I am still looking for an apartment.	
		Is it still available to rent? My schedule is flexible for viewing the property.	
	My schedule is flexible for viewing the property.		
		Thank you!	
	Thank you!		

Table B.5: Responses Conditional on Opening

	"yes" replies		"not-no" replies	
	(1) I(initial yes)	(2) I(ultimate yes)	(3) I(initial not-no)	(4) I(ultimate not-no)
Fluent, foreign name	-0.00607	0.00117	-0.0205	0.0312
	(0.050)	(0.050)	(0.077)	(0.047)
A2, foreign name	-0.185***	-0.161***	-0.270***	-0.186***
	(0.043)	(0.044)	(0.076)	(0.044)
English, foreign name	-0.160***	-0.165***	-0.262***	-0.217***
	(0.044)	(0.044)	(0.075)	(0.044)
Strata FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	2,388	2,388	2,388	2,388

Notes: The dependent variable is an indicator for successfully obtaining an apartment viewing opportunity ("yes") or receiving a response that is not an explicit rejection ("not no"). Columns (1) and (3) are initial responses to the original request and (2) and (4) are ultimate responses after a follow-up email if no response is received within three business days. The key regressors are indicators for treatment groups, in which foreign-sounding names send messages of varying local-language proficiency: fluent, functional A2 (both with and without a language-learning signal), and proficient English (both with and without a language-learning signal); the omitted category is the control group, in which native-sounding names sending fluent local-language messages. Strata fixed effects are defined by city-size groups and the day of the week the message is sent. Controls include rent (EUR), apartment size (m^2), number of bedrooms, and indicators for each of whether the apartment is furnished, includes a balcony, and is located in a building with an elevator. For the 2% of observations with missing covariates, we impute missing values with the sample mean and include missing-value indicators. All regressions are restricted to sent messages that were opened by the recipient. Observations are weighted such that each country receives equal weight. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Responses by Foreign Groups Historical Participation in Austro-Hungarian Empire

	Austro-Hungarian Empire		Non-Austro-Hungarian Empire	
	(1)	(2)	(3)	(4)
	I(initial yes)	I(ultimate yes)	I(initial yes)	I(ultimate yes)
Fluent, foreign name	-0.0313	-0.00322	-0.0875**	-0.124***
	(0.029)	(0.031)	(0.035)	(0.039)
A2, foreign name	-0.154***	-0.180***	-0.129***	-0.159***
	(0.023)	(0.025)	(0.027)	(0.030)
English, foreign name	-0.153***	-0.194***	-0.179***	-0.227***
	(0.022)	(0.025)	(0.025)	(0.028)
Strata FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	2,576	2,576	1,620	1,620

Notes: The dependent variable is an indicator for successfully obtaining an apartment viewing opportunity after sending (1,3) an initial request and (2,4) a follow-up email if no response is received after three business days—where foreign groups are the subset from the former Austro-Hungarian empire or not, respectively. The key regressors are indicators for treatment groups, in which foreign-sounding names send messages of varying local-language proficiency: fluent, functional A2 with and without a language-learning signal, and proficient English with and without a language-learning signal; the omitted category is the control group, in which native-sounding names sending fluent local-language messages. Strata fixed effects are defined by city-size groups and the day of the week the message is sent. Controls include rent (EUR), apartment size (m^2), number of bedrooms, and indicators for each of whether the apartment is furnished, includes a balcony, and is located in a building with an elevator. For the 2% of observations with missing covariates, we impute missing values with the sample mean and include missing-value indicators. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: Inquiry Prevalence – Conditional on Replying

	Types of Inquiry (%)					
Groups	Any	Language	Residency or Nationality	Lease Duration	Employment	Other
Fluent, native name	11.9	0.0	0.7	1.9	1.4	11.5
Fluent, foreign name	14.2	0.0	1.0	1.8	2.0	13.8
Non-fluent, foreign name	15.9	1.2	2.5	3.5	3.5	14.6

Notes: Prevalence of different inquiry types, among responses received. Rows correspond to experimental groups, where foreign names that are non-fluent (functional A2 or English) are collapsed into the same category. Values are expressed in percentages. Any inquiry is when the response contains at least one inquiry; language inquiries are about knowledge of the local language; residency or nationality inquiries seek to clarify the prospective tenant's residency status or nationality; lease duration clarify the length of the rental agreement requested; employment inquiries clarify employment status and occasionally employment type and income level; other inquiries include requesting phone number and various additional questions, such as whether there will be children, pets, and smokers.

# F   Additional Figures

Figure 10: Responses by Treatment
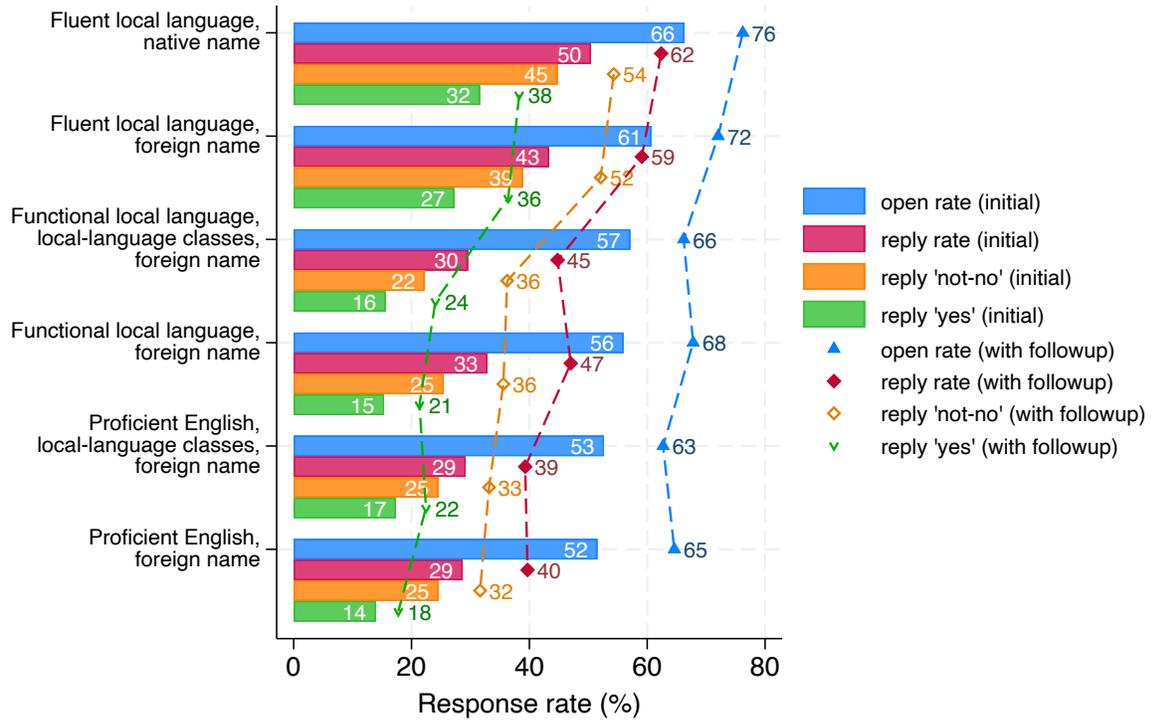
Notes: Actual response rates to apartment-viewing requests by experimental group—for proportions opened (red), replied to (blue), "not-no" responses exclude explicit rejections (orange), and "yes" responses grant apartment-viewing opportunities (green). The bars represent response rates to the initial request, while the points indicate response rates to a follow-up message if no response is received within 3 business days. Each observation is weighted such that each country receives equal weight.
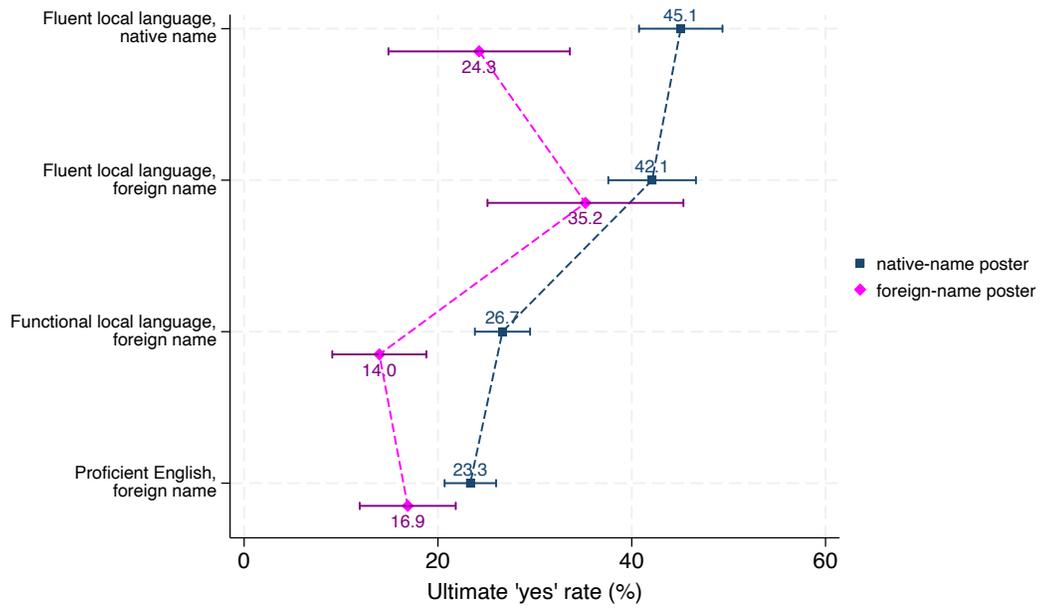
Figure 11: Heterogeneity—above/below-average rent
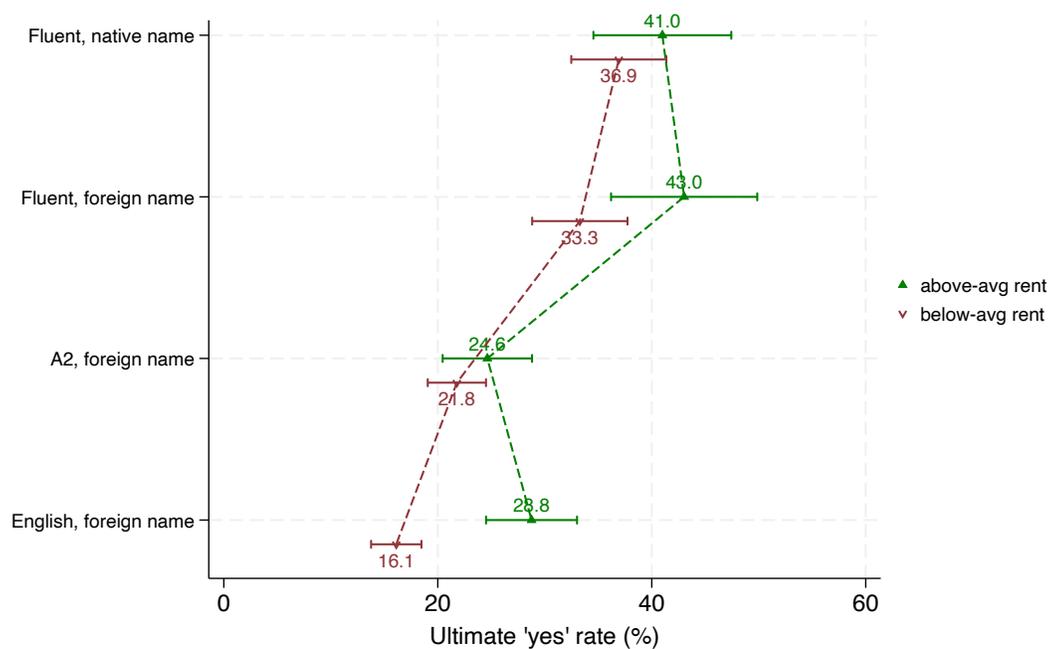
Notes: Actual rates of ultimately obtaining an apartment-viewing opportunity by experimental group for above-average rent (solid green triangles) versus below-average rent (downward red arrows) apartments. The markers represent group means with 95% confidence intervals. Each observation is weighted such that each country receives equal weight.

Figure 12: Heterogeneity—by sender gender
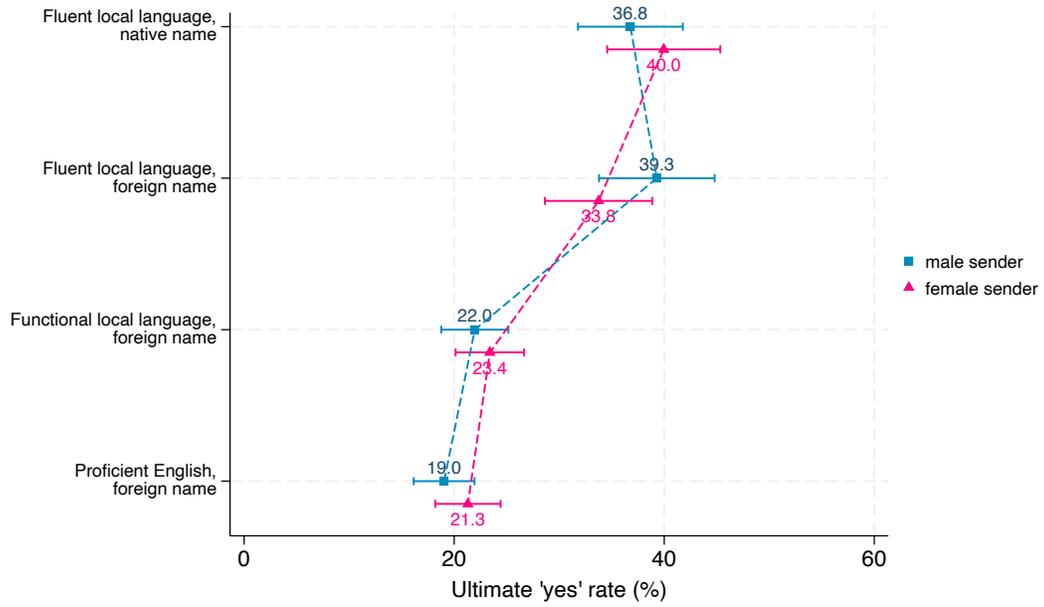
Notes: Actual rates of ultimately obtaining an apartment-viewing opportunity by experimental group for male (blue squares) versus female (pink triangles) apartment searchers. The markers represent group means with 95% confidence intervals. Each observation is weighted such that each country receives equal weight.

Figure 13: Heterogeneity—by poster name ethnicity

Notes: Actual rates of ultimately obtaining an apartment-viewing opportunity by experimental group from native-sounding (navy squares) versus foreign-sounding (magenta diamonds) poster names. The markers represent group means with 95% confidence intervals. Each observation is weighted such that each country receives equal weight.

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Figure 14: Returns Gap Heterogeneity by Nationality
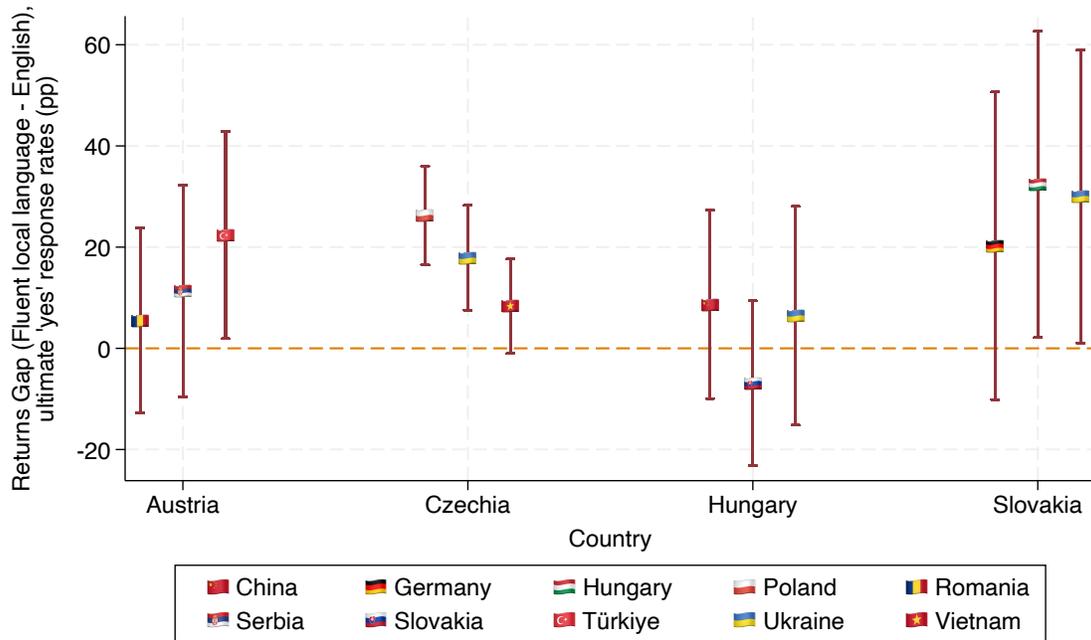
Notes: Improvements in ultimate success rates of obtaining an apartment-viewing opportunity from sending requests in fluent local language versus English for foreign-sounding names. Regression coefficients for the three largest foreign groups of each country are displayed by their respective flags, with 95% confidence intervals based on robust standard errors. Results are obtained by regressing, for each country and foreign group, an ultimate "yes" response indicator over local-language proficiency, omitting English group, and plotting the the coefficient of the fluent local language group.

Figure 15: Ethnic Disadvantage—among ultimate "yes" responses
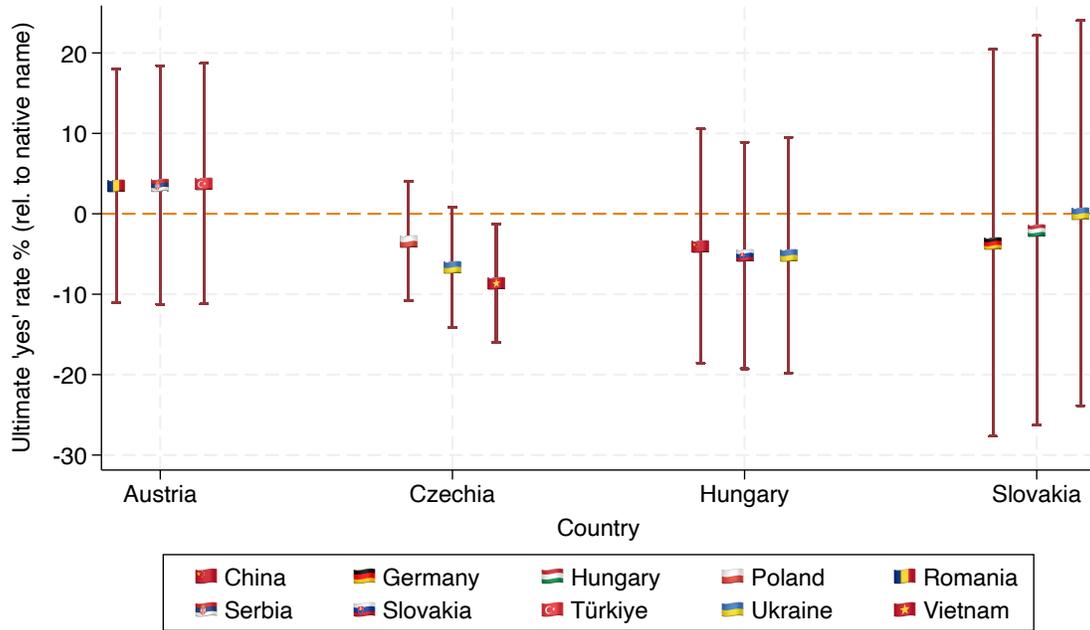
Notes: Differences in ultimate success rates of obtaining an apartment-viewing opportunity for foreign-sounding versus native-sounding names, for message requests made in fluent local language. Regression coefficients for the three largest foreign groups of each country are displayed by their respective flags, with 95% confidence intervals based on robust standard errors. Results are obtained by regressing, for each country, an ultimate "yes" response indicator over ethnic group indicators, omitting the country's native group, and local-language proficiency indicators, omitting fluency, and controlling for sender gender, rent (EUR), apartment size (m^2), number of bedrooms, and indicators for each of whether the apartment is furnished, includes a balcony, and is located in a building with an elevator. For the 2% of observations with missing covariates, we impute missing values with the sample mean and include missing-value indicators.