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## Nudging the agents: Does it reduce discrimination against migrants in the house rental market?

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Inter-American Development Bank  
Country Department Andean Group

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## Abstract

This study examines the impact of a behavioral intervention on reducing discrimination against Venezuelan migrants in the screening of home rental applications conducted by Ecuadorian real estate agents (REAs). Given that Venezuelan migrants represent the second-largest migratory group globally, with over seven million individuals seeking refuge primarily in other Latin American countries, understanding and addressing discrimination against them is of significant importance. Our artifactual field experiment involved providing information to REAs that highlighted the extra efforts Venezuelan migrants must make to achieve the same goals as nonmigrants in host countries. The results demonstrated a meaningful increase of 33.67% in the preference for Venezuelan migrants over native applicants, with this effect mainly driven by changes in male REAs' discriminatory behaviors. The findings suggest that challenging the information value of Venezuelan migrant stereotypes, which often underlie assumptions about their qualities, can effectively diminish discrimination during the rental application process. This research contributes valuable insights to the ongoing efforts to identify effective means to deal with discrimination against migrants.

**Keywords:** Field experiments, Behavioral Interventions, Nudges, Migration, Prejudice

**JEL Codes:** F22, J15, R31

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# 1 Introduction

The socio-political and economic turmoil in Venezuela has triggered a significant migration wave, with millions fleeing the country. As the second-largest migrant population globally after Syria, 8 million Venezuelans have left the country, with a significant portion settling in other Latin American countries, many of which still face fundamental challenges to achieving economic and social development. Local economic concerns and fears of cultural shifts have historically driven resistance to immigrants leading to discriminatory practices (Adida, Lo and Platas, 2019; Bansak, Hainmueller and Hangartner, 2016; Hainmueller and Hiscox, 2010; Hainmueller and Hopkins, 2014, 2015; Konitzer et al., 2019; Hopkins, 2010). While seeking better opportunities, Venezuelan migrants face discrimination, which not only hinders their economic assimilation but also adversely impacts their mental and psychological well-being. Studies by de Freitas et al. (2018) and Hashemi et al. (2019) emphasize the negative effects of discrimination on migrants, particularly on aspects like self-esteem and life evaluation. Such discrimination reduces the potential contributions these migrants can make to their host economies. Addressing this issue is crucial, as the Venezuelan migration trend continues, presenting ongoing economic and social challenges that could influence regional stability and development.

In 2015 Ecuador, a country historically with a sizable outflow<sup>1</sup> of migrants became a net recipient of migrant population<sup>2</sup>. Between 2010 and 2020, the migrant population grew from 2.5% to 4.4% of the total population (Cruces et al., 2023). Today the country hosts slightly over 513 thousand Venezuelans. In parallel to that demographic change, Ecuador grapples with a housing challenge. As of 2022, around 2.7 million Ecuadorian households lived in precarious conditions, a situation now intensified by the migrant influx (Cruces et al., 2023). In what pertains to the housing market, discrimination against Venezuelans in Ecuador possibly mirrors trends observed in Colombia and reflects prevailing social norms (Zanoni and Díaz, 2023). While the 2022 National Urban Policy signifies Ecuador's commitment to addressing housing issues and promoting migrant integration, discrimina-

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<sup>1</sup>Approximately 1.1 million Ecuadorians live abroad; primarily in the USA (40.17%) and Spain (38.27%) (UNDESA, 2015).

<sup>2</sup>Source: Total migrant stock at mid-year by origin and by major area, region, country or area of destination, 2015.

tion poses a persistent obstacle (Cruces et al., 2023). Such discrimination not only impedes Venezuelan migrants from securing housing but also affects their broader integration and welfare. Addressing this discrimination is pivotal, as it directly influences migrants’ access to essential resources, opportunities, and the broader benefits of stable housing. Achieving the global objective of improving migrant welfare, such as that expressed in the UN’s International Convention on the Protection of the Rights of Migrant Workers and their Families, requires addressing housing discrimination against them<sup>3</sup>.

This study examines the impact of a behavioral intervention, a nudge, on reducing discrimination against Venezuelan migrants in the screening of home rental applications conducted by Ecuadorian Real Estate Agents (REAs). In an artifactual field experiment, we engaged REAs to evaluate several pairs of housing rental applications in Quito and Guayaquil, the two biggest cities in the country. These applications were equivalent in all respects, except for the randomly assigned country of origin: Venezuela or Ecuador. To challenge prevailing stereotypes about Venezuelan migrants, the experiment introduced a behavioral economics intervention to a random subset of the REAs. This nudge provided information highlighting the additional challenges Venezuelan migrants face in achieving the same milestones as non-migrants in Ecuador. Our primary objective was to analyze the nudge-induced changes in first, preference patterns of REAs towards Venezuelan versus Ecuadorian applicants, and second, changes in disparities in the quality ratings assigned to applicants from each nationality. By comparing the responses between REAs exposed to this intervention and those who weren’t, we could gauge the nudge’s efficacy in mitigating discrimination.

The nudge was effective in reducing discrimination against Venezuelan migrants. While the REAs rate of choice in favor of Ecuadorian applicants in the control group was 38.91%, that rate was 25.81% for REAs in the treatment group. Similarly, the nudge reduced the mean difference in the assessment of suitability for the property between Ecuadorians and Venezuelans from 0.82 basic points in the control group to 0.39 basic points in the treatment group on a Likert scale from one to ten. Those changes were mainly driven by changes

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<sup>3</sup>The International Convention on the Protection of the Rights of Migrant Workers and their Families (ICRMW) is a UN treaty adopted in 1990 to safeguard migrant workers and their families’ rights. It emphasizes equal rights with nationals in their host countries and promotes family unity.

in male REAs’ discriminatory behaviors. We also found that those REAs with Bachelors degree, and who worked independently (not in a real estate agency) were the ones more likely to be impacted by the nudge than other REAs<sup>4</sup>. Our findings contribute new evidence on effective strategies to deal with discrimination against migrants in the housing market and provide guides on how to tailor those to specific populations. We suggest that a simple intervention, such as providing information about the challenges faced by migrants, can be effective in reducing discrimination when it is rooted in grounds of nationality and the meaning attributed to being Venezuelan.

Understanding effective ways to tackle discrimination in the housing sector against Venezuelan migrants in Ecuador is relevant beyond what we can learn to address the challenges for economic assimilation of this specific population, market, and country. Migrants in OECD countries also face discrimination in the workplace, the housing market, and other areas of life (OECD, 2023, 2020). Research suggests that this type of discrimination, especially in vital sectors like housing, can hinder their integration into host societies, emphasizing the need for policies that address both legal status and social biases (Herrera, 2016; Lee, 2012). With 1 in 10 people foreign-born across the OECD countries, and with increasing migration from the global south, finding effective ways to foster economic assimilation is of paramount importance, and our results hint that nudging market intermediaries could be an actionable policy alternative. Nudges, recognized as promising cost-effective behavioral interventions, have shown potential in various domains, including the reduction of discrimination. A study by Jordan R. Axt (2019) emphasized the importance of understanding how these interventions can influence biases, suggesting that they can offer valuable insights for policymakers aiming to reduce discrimination in the housing market.

Without losing generality to understand discrimination as a systemic problem, the assimilation of Venezuelan migrants in Ecuador presents a unique case. Unlike many other migrant groups, they don’t face linguistic or significant cultural barriers. Moreover, many have family ties in Ecuador, possess higher-than-average educational qualifications, and benefit from active policy efforts that provide formal residency and migratory statuses. Yet, their economic assimilation remains challenging. Most existing research focuses on migrants

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<sup>4</sup>This distinction is made upon the statistically significant difference of the mean of those characteristics.



moving to developed countries, where language, ethnicity, race, and cultural differences significantly influence discrimination. Our study offers a fresh perspective, emphasizing the unique challenges in the south-south migration, especially on economic outcomes.

As it was needed to document the impact of the nudge, our study fills a significant knowledge gap by providing the first experimental measure of discrimination against Venezuelan migrants in the Ecuadorian real estate market. While discrimination is presumed to be of high magnitude, empirical evidence has been lacking. Our research addresses this void, shedding light on the actual extent of this phenomenon. Yet, while our study is rigorous, it isn't without limitations. The external validity of field experiments is often questioned due to concerns about the representativeness of the population studied, REAs in our case. However, we've taken measures to ensure our sample's diversity and its reflection of the broader REA population in Ecuador. Another potential limitation is the study's focus on the formal leasing sector, which might not capture the full spectrum of the rental market. Additionally, the potential for REA behavior to change due to the nudge, influenced by socially desirable response beliefs, remains a concern. Yet, prior research in Colombia and Argentina suggests that our methodology minimizes such biases (Zanoni et al., 2023; Zanoni and Díaz, 2023). While we believe our study captures genuine responses and offers valuable insights, further research might be needed to validate the full scope of our approach.

This paper is organized as follows. Section 2 examines the migration trends of Venezuelans to Ecuador, emphasizing the persistent housing discrimination they face despite various government interventions. Section 3 delves into the methodology, detailing the artificial field experiment design, the selection of real estate agents, and the intricacies of the nudge intervention. Section 4 presents the empirical results, starting with the baseline discrimination findings, followed by the impact of the nudge, and then exploring heterogeneous effects across different subgroups. Section 5 offers robustness checks, including alternative specifications and the results from the placebo trial. Finally, Section 6 concludes the paper, synthesizing the findings, discussing their implications, and suggesting avenues for future research.

## 2 Venezuelan Migrants in Ecuador

Since 2014, migration from Venezuela has experienced a rapid and substantial increase, particularly from 2017 onwards, with asylum claims surpassing 80,000 in 2017 and doubling to 255,000 in 2018, totaling 414,000 between 2014 and 2018 (UNHCR, 2018, 2019). Research on the migration of Venezuelans to other Latin American countries highlights the persistent integration challenges faced by migrants, despite government efforts to regularize their status. Studies such as Gandini, Prieto Rosas and Lozano-Ascencio (2020) emphasize the varying responses of host countries, ranging from inadequate protection to a human rights-oriented regulatory framework. Other studies have evaluated the effectiveness of amnesties granted to illegal Venezuelan migrants (Bahar, Ibáñez and Rozo, 2021; Ibáñez et al., 2022). Similarly, research by Bonilla-Tinoco, Aguirre-Lemus and Fernández-Niño (2020) reveals enduring disparities in health indicators among Venezuelan migrants, despite governments' attempts to address their healthcare needs. Furthermore, the COVID-19 pandemic exacerbated vulnerabilities among approximately 3 million Venezuelan migrants in Colombia, Peru, and Ecuador, which are now significant destinations for asylum seekers in South America (World Food Programme, 2020).

Ecuador currently ranks as the third primary recipient of Venezuelan migrants in the Andean Region and the fourth in Latin America, following Colombia, Peru, and Brazil (Interagency Coordination Platform for Refugees and Migrants from Venezuela, 2023). However, Ecuador's historical migration dynamics were characterized by emigration rather than immigration, largely driven by poverty and inequality issues before 2000 (Herrera, 2022). The shift towards becoming a destination for migrants gained prominence after 2000 when Ecuador experienced a significant increase in emigration and saw an influx of Colombian refugees seeking protection due to escalating violence from the armed conflict.

Ecuador experienced an increasing migration from Colombia between 1990 and 2010, which reached more than 221,500 migrants by 2010 (Herrera, 2022). Some research on this phenomenon has been developed, for instance, the study by Pugh (2018), which examines Colombian displaced people in Ecuador, underscores migrant agency in negotiating identity to influence social hierarchies, coexistence, and human security. Venezuelan migration to Ecuador has increased considerably since the end of 2017 and continued to grow in

the following years. Between 2015 and 2020, approximately 380,000 Venezuelan migrants arrived in Ecuador (Herrera, 2022), and, according to official data, by mid-2023, about 107,500 applied for a temporary visa in Ecuador (Ministry of Foreign Affairs and Human Mobility, 2023).

In Table A2 of the Appendix, we present data from the National Employment, Unemployment, and Underemployment Survey (ENEMDU - in Spanish) for the year 2022. The table reveals that Venezuelans between the ages of 18 and 65 are, on average, 5 and 8 years younger than their Ecuadorian and Colombian counterparts, respectively. Notably, there are significant differences in educational attainment: the average Venezuelan migrant has completed more years of schooling than native Ecuadorians and Colombian migrants (2.12 and 2.9 more years, respectively). Additionally, Venezuelans are more likely to be employed compared to individuals in the other two groups. However, despite these disparities in education, the income of Venezuelans is 11% lower than that of Ecuadorians and 4% lower than that of Colombians.

Migration policies regarding the Venezuelan population in Ecuador have evolved over the years. Initially, there was no visa requirement for Venezuelans entering the country, and those who arrived between 2010 and 2016 could apply for the 12 XII visa, which provided temporary residency and work authorization, particularly targeting those seeking formal employment opportunities (Herrera, 2022). However, access to the 12 XII visa became limited for migrants arriving after 2017. In January 2017, Ecuador implemented UNASUR visas for the free movement of South American citizens from UNASUR member countries. Still, due to the significant influx of Venezuelan migrants, the government introduced an exceptional humanitarian visa in July 2019, imposing new entry requirements and limitations on their free movement across Ecuadorian borders.

Simultaneously, in 2019, the government initiated a regularization process that benefited approximately 87,932 migrants, a program that extended until mid-2020 due to challenges posed by the COVID-19 pandemic outbreak. In June 2022, Ecuador's government launched a second round of regularization through the Exceptional Temporary Residence Visa for Venezuelan Citizens (VIRTE), providing a two-year migration amnesty to irregular Venezuelan migrants who arrived after 2019. As of August 2023, around 68,000 visas of

this type have been issued (Ministry of Foreign Affairs and Human Mobility, 2023).

Despite these efforts, discrimination against Venezuelan migrants remains a significant obstacle to their economic and social integration in Ecuador. A recent UNDP report indicates that Venezuelans encounter barriers in the labor market, housing access, and to a lesser extent, education and health services (Herrera, 2022). A survey conducted by the International Organization for Migration in 11 Ecuadorian cities revealed that approximately 47.3% of Venezuelan migrants living in Ecuador by 2021 reported experiencing discrimination, with nationality being the primary reason cited by 98% of those who experienced discrimination (International Organization for Migration, 2021). The survey also highlighted instances of violence, with 1 in 10 respondents reporting being victims or witnessing violence directed at other migrants, primarily in the form of verbal and physical aggression.

A study conducted by the World Bank in 2020 revealed that approximately 73% of Ecuadorians held negative perceptions regarding Venezuelan migration and its economic impact (World Bank, 2020). Moreover, the same study found that 4 out of 10 migrants reported experiencing xenophobic attitudes, primarily in public spaces such as streets or public transport. These findings align with the results of a recent survey conducted by Equilibrium SDC in 2023, which employed the Respondent-driven Sampling method. Out of 5,850 Venezuelan migrant respondents, nearly 56.4% reported facing discrimination in public spaces, while approximately 16.5% reported workplace discrimination. Only 3 out of 10 respondents indicated that they had never experienced discrimination based on their nationality.

As previously mentioned, migrants in Ecuador encounter significant obstacles when trying to access the housing market, resulting in unequal renting conditions compared to locals. The World Bank's study in 2020 reported that approximately one-third of Venezuelan families seeking housing experienced discrimination based on their nationality. Furthermore, the study highlighted that women, especially those traveling alone or with children, faced heightened discrimination in housing access, primarily due to concerns about their ability to meet rent payments (World Bank, 2020). Moreover, the research revealed that even after securing housing, migrants often lived in substandard conditions compared to locals.

Around half of the migrant population reported residing in single rooms with multiple occupants, and more than half of the surveyed migrant families indicated living in conditions that posed potential risks to children, such as sharing beds with adults. Official statistics further confirm the precariousness of living conditions. Analysis of the microdata from the ENEMDU survey of 2022 reveals that 94.2% of Venezuelans, while data reveals that 7% have inadequate materials for their roofs, walls, or flooring. Recent reports from UNHCR and R4V continue to validate the challenges faced by migrant families in accessing the housing market, with approximately 2% of migrant families encountering such difficulties (Interagency Coordination Platform for Refugees and Migrants from Venezuela, 2022).

### 3 Experimental design

Our research design falls under the label of an artifactual field experiment as described in Harrison and List (2004), also recognized as a “vignette study,” “Goldberg paradigm study,” or “factorial survey” within the realm of field experiments in social sciences. In our setup, REAs appraise pairs of rental candidates, one self-identifying as a migrant and the other as an Ecuadorian, while considering their qualifications and attributes. Following the evaluation of two such pairs, we randomly allocate half of the REAs to an information intervention (nudge), drawing their attention to the disparities in efforts that minorities may need to exert to attain the same outcomes as nonminorities. By comparing the responses provided to migrants and nonmigrants prior to the nudge, we can gauge the level of baseline discrimination. Comparing the responses between REAs assigned to the treated and control groups, we isolate and quantify the effect of the intervention to answer our main research question.

Our empirical approach is characterized by two distinct features: the engagement of actual market decision-makers, in our case, REAs, and the simulation of genuine market transaction conditions for these REAs to assess rental applications. Regarding the first aspect, we hired REAs from the cities of Quito and Guayaquil, framing the exercise as an authentic job and providing competitive compensation to ensure their commitment. Regarding the second unique element, the properties associated with rental applications, candidate family profiles, and the assessment environment were meticulously crafted fol-

lowing extensive fieldwork involving interviews and focus groups with REAs, real estate chamber representatives, and agency members.

Our approach offers some advantages over correspondence studies, the prototypical method to study discrimination. In typical correspondence studies, the interaction between researchers and agents is restricted to callback responses, which is just the first step in the hiring process, and that is often affected by response rate challenges (Heckman and Siegelman, 1993). In contrast, our method entails repeated interactions with decision-maker agents, enabling us to customize each interaction to mimic varied inquiries. Beyond evaluating agents’ assessments of multiple applicant pairs, our approach incorporates several questions in each evaluation, leveraging multiple outcomes that provide a comprehensive landscape into the discriminatory process by portraying different discrimination dimensions. Additionally, the agents’ job contract allows for profiling across various dimensions like demographics, job experience, socio-emotional factors, and cognitive development. This capacity to delve into the decision-makers’ characteristics empowers us to explore unique mechanisms driving discriminatory behaviors and the nudge’s impact, which is not feasible in correspondence studies.

We developed a custom web platform for the experiment, providing REAs with a user-friendly interface to evaluate rental candidates. The experiment was fully conducted online. We presented the REAs with synthetic rental applications resembling the rental applications in the Ecuadorian market based on information from stakeholder interviews and data from the 2019 household survey in Ecuador. The REAs assessed pairs of rental candidates across ten rounds, each featuring a distinct rental vacancy. REA’s assessment of applicants involved three tasks: ranking each applicant in terms of fit for the corresponding property on a Likert scale from 1 to 10, selecting their preferred applicant, and providing the rent value they thought was appropriate for each candidate. Previous to the ten rounds of evaluation, we collected personal attributes and cognitive and non-cognitive test results on the REAs.

In this study, we analyze the decisions made over three rounds where the difference between the candidates was the migrant condition. Other rounds serve as placebo mitigating potential experimenter demand effects by making the purpose of the research inconspicuous.

Other rounds included comparisons between male and female applicants or applicants with different sexual orientations or gender identity.<sup>5</sup> Additionally, a pure placebo round with no distinguishing attributes reinforced the experiment’s credibility to prevent experimenter demand effects further.

For those rounds analyzed in this study, the migrant identity within pairs of applicant profiles was randomized at the REA level, ensuring that the differences in REA’s choices between Ecuadorians and Venezuelans can be attributed to nationality and not to other characteristics of the applicant. Applicants were comparable in qualifications and attributes by design, differing only in migrant self-identification. In Appendix Table [A1](#), we present a balance test table assessing the disparities in means of key attributes among rental applicants based on nationality. These attributes were intentionally designed to be comparable between the two groups. Columns (1) and (2) display the means and their respective standard errors (in parentheses), while column (3) indicates the mean difference and the accompanying p-value derived from a t-test to assess the differences. We show that the candidates exhibited observational equivalence in all dimensions. Given the equilibrium in observable attributes, we posit that the discrepancies in REAs’ preferences for nonmigrants over migrants reflect instances of discriminatory behavior among REAs.

Half of the REAs were selected randomly to receive an information intervention before the last round of evaluation, allowing us to recover unbiased estimates of the effect of the nudge on the discriminatory behaviors of the REAs. The information intervention constituted a nudge as it followed the principle of inducing a change in behavior without forbidding any options or significantly changing economic incentives ([Thaler and Sunstein, 2009](#)). The nudge was displayed as a randomized pop-up message assessing the impact of the following anti-discrimination statement. “*Welcome to the final section of the activity! Many people are discriminated against for belonging to minorities. Note that individuals who belong to minority groups make extra efforts to achieve the same goals as others. This may be reflected in their behavior as tenants.*”. The rationale behind this nudge is to accentuate the migrant’s adverse circumstances and distinct challenges to update the beliefs of REAs about them. [Kirgios et al. \(2022\)](#) finds that highlighting the marginalized

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<sup>5</sup>Decisions over those rounds are analyzed in a separate study.

identity of women and racial/ethnic minorities activates motivations to avoid prejudiced reactions. [Gomez \(2023\)](#) results also align with these results when analyzing the effect of merit in the selection of disadvantaged applicants.

### 3.1 Experiment sampling strategy

An important characteristic of the population of study that we highlight pertains to the heterogeneous nature of the Real Estate Agent (REA) occupation in Ecuador. The absence of clear regulations for that activity allows individuals to adopt the role of REAs either as their primary or secondary occupation, working independently or affiliating with agencies (formally or informally). Moreover, REAs possess the flexibility to choose their level of effort to engage in full or part-time work, which extends not only at a single point in time but also varies over different phases of the economic cycles, thus responding to shifts in real estate conditions. Because of this inherent diversity, the exact scale, professional traits, and demographic features of the REA population in the country are uncertain. Labor force surveys do not have sufficient resolution to characterize them properly. From a sample design perspective, REAs can be classified as a “hard-to-access” population, posing difficulties in achieving a representative sample.

Acknowledging those limitations, we recruited REAs using the referral-driven sampling method ([Heckathorn, 1997, 2002](#); [Johnston and Sabin, 2010](#)). To gain broad representativeness, we first contacted REAs via LinkedIn, as well as through real estate chambers and individual agencies in Quito and Guayaquil. Out of 477 contacted REAs, 455 registered for the experiment, 181 completed the whole experiment (three trials), 185 completed two trials, and 8 REAs completed just one trial, resulting in 374 REAs and 1842 observations. The sample consisted of 58.25% women and 41.75% men, with an average age of 36.47 years old. REAs held university degrees, averaging seven years of work experience and five years working as REAs. All individuals were screened before participation in the experiment to ensure they worked as REAs. All REAs received competitive compensation not only for completing the experiment but also for their referrals, encouraging robust engagement.

Our sampling strategy aimed at achieving a comprehensive representation of REAs likely improved the sample’s alignment with the broader REA population in Ecuador



strengthening the external validity of the study. This was complemented by examining the stability of key REA attributes with increasing sample size, reducing representativeness worries. Additionally, comprehensive data collection enabled robust control for confounding variables and the analysis of their influence on the REAs responses. Figure 1 illustrates the remarkable stability in key attributes of REAs as the sample size expanded, including gender, age groups, educational level, and work experience. This striking consistency in REA attributes, correlated with the growth in sample size, implies that our approach might have effectively bridged the gap between the sample and the wider population of REAs in Ecuador. However, it's important to acknowledge that the external validity of our study might be limited to REAs with the attributes showcased here, as we lack comprehensive insights into the broader population attributes of REAs in Ecuador (for there is no data available).

In Table 1 we show that REAs revealed some differences in attributes across the stage of the sampling process (initially contacted/ referred). We identify differences in age, employment experience, Rosenberg and Neoffi test scores.<sup>6</sup> Some of the attributes of the agents were also different according to the city they were sampled from. The fact that there are some differences between the REAs by city and sampling method, calls for an empirical model to measure discrimination that holds constant in both of those dimensions.

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<sup>6</sup>Both scores are standardized.

Table 1: REAs attributes

Variable	Both				Guayaquil				Quito			
	Total	Initial Contact	Referred	Difference	Total	Initial Contact	Referred	Difference	Total	Initial Contact	Referred	Difference
<b>Demographics</b>												
REAs Age (years)	36.465 (11.182)	41.837 (11.257)	35.767 (10.99)	-6.07*** (1.287)	35.332 (11.402)	38.188 (9.464)	35.069 (11.54)	-3.119* (1.784)	37.636 (10.842)	44 (11.749)	36.541 (10.309)	-7.459*** (1.701)
REAs Gender (Female == 1)	0.583 (0.493)	0.558 (0.5)	0.586 (0.493)	0.028 (0.057)	0.595 (0.492)	0.688 (0.471)	0.586 (0.493)	-0.101 (0.087)	0.571 (0.496)	0.481 (0.504)	0.586 (0.493)	0.105 (0.074)
REAs Nationality (Ecuadorian == 1)	0.981 (0.136)	0.977 (0.152)	0.982 (0.134)	0.005 (0.017)	0.974 (0.16)	0.938 (0.246)	0.977 (0.15)	0.04 (0.044)	0.989 (0.104)	1 (0)	0.987 (0.112)	-0.013** (0.006)
REAs Employment Experience (Years)	3.83 (4.093)	5.756 (6.093)	3.58 (3.69)	-2.175*** (0.672)	3.684 (4.084)	4.719 (4.872)	3.589 (3.998)	-1.13 (0.888)	3.982 (4.103)	6.37 (6.679)	3.571 (3.321)	-2.8*** (0.928)
Does REA Works Full Time? (Yes == 1)	0.337 (0.473)	0.395 (0.492)	0.329 (0.47)	-0.066 (0.056)	0.279 (0.449)	0.375 (0.492)	0.27 (0.445)	-0.105 (0.09)	0.397 (0.49)	0.407 (0.496)	0.395 (0.49)	-0.013 (0.073)
REAs Work Status (Independent == 1)	0.548 (0.498)	0.512 (0.503)	0.553 (0.498)	0.041 (0.058)	0.511 (0.501)	0.438 (0.504)	0.517 (0.5)	0.08 (0.093)	0.587 (0.493)	0.556 (0.502)	0.592 (0.492)	0.037 (0.074)
Does the REA has a college degree? (Yes == 1)	0.495 (0.5)	0.605 (0.492)	0.48 (0.5)	-0.124** (0.056)	0.479 (0.5)	0.562 (0.504)	0.471 (0.5)	-0.091 (0.093)	0.511 (0.501)	0.63 (0.487)	0.49 (0.501)	-0.139* (0.072)
Education: Primary	0.003 (0.052)	0 (0)	0.003 (0.055)	0.003 (0.002)	0.005 (0.072)	0 (0)	0.006 (0.076)	0.006 (0.004)	0 (0)	0 (0)	0 (0)	0 (0)
Education: Secondary	0.366 (0.482)	0.302 (0.462)	0.375 (0.484)	0.072 (0.053)	0.389 (0.488)	0.312 (0.471)	0.397 (0.49)	0.084 (0.087)	0.342 (0.475)	0.296 (0.461)	0.35 (0.478)	0.054 (0.068)
Education: College Degree	0.436 (0.496)	0.512 (0.503)	0.426 (0.495)	-0.086 (0.058)	0.447 (0.498)	0.562 (0.504)	0.437 (0.497)	-0.126 (0.093)	0.424 (0.495)	0.481 (0.504)	0.414 (0.493)	-0.067 (0.074)
Education: Technical Degree	0.126 (0.332)	0.07 (0.256)	0.133 (0.34)	0.063** (0.031)	0.105 (0.307)	0.062 (0.246)	0.109 (0.312)	0.047 (0.047)	0.147 (0.354)	0.074 (0.264)	0.159 (0.366)	0.085** (0.041)
Education: Masters Degree	0.053 (0.225)	0.07 (0.256)	0.051 (0.221)	-0.018 (0.029)	0.032 (0.175)	0 (0)	0.034 (0.183)	0.034*** (0.01)	0.076 (0.265)	0.111 (0.317)	0.07 (0.256)	-0.041 (0.046)
Education: PhD Degree	0.005 (0.073)	0.023 (0.152)	0.003 (0.055)	-0.02 (0.016)	0 (0)	0 (0)	0 (0)	0 (0)	0.011 (0.104)	0.037 (0.191)	0.006 (0.08)	-0.031 (0.026)
Share of knowledge of Real State Market (%)	33.209 (18.451)	33.953 (16.541)	33.112 (18.694)	-0.842 (1.926)	33.474 (19.695)	28.75 (21.515)	33.908 (19.495)	5.158 (3.944)	32.935 (17.093)	37.037 (11.916)	32.229 (17.752)	-4.808** (1.906)
Observations	374	43	331	374	190	16	174	190	184	27	157	184
<b>Scores in standardized tests</b>												
Score in Wonderlic test (std.)	-0.005 (1.002)	0.089 (0.987)	-0.017 (1.004)	-0.106 (0.113)	0.013 (1.079)	-0.136 (0.95)	0.027 (1.09)	0.162 (0.178)	-0.023 (0.918)	0.223 (0.993)	-0.066 (0.899)	-0.288** (0.144)
Score in Rosenberg test (std.)	0.014 (1.001)	0.543 (0.769)	-0.056 (1.008)	-0.599*** (0.092)	0.07 (0.871)	0.462 (0.956)	0.032 (0.854)	-0.43** (0.175)	-0.042 (1.116)	0.591 (0.639)	-0.152 (1.145)	-0.743*** (0.109)
Score in Neoffi test (std.)	0.01 (1.001)	0.271 (0.803)	-0.024 (1.019)	-0.296*** (0.096)	-0.186 (0.954)	0.468 (0.845)	-0.248 (0.942)	-0.716*** (0.158)	0.217 (1.008)	0.15 (0.759)	0.228 (1.045)	0.078 (0.121)
Score in Neoffi - Neuroticism (std.)	-0.006 (1.002)	-0.398 (0.864)	0.044 (1.008)	0.442*** (0.102)	0.018 (1.019)	-0.39 (0.916)	0.056 (1.021)	0.446** (0.171)	-0.031 (0.985)	-0.403 (0.839)	0.031 (0.995)	0.434*** (0.129)
Score in Neoffi - Extroversion (std.)	0.012 (1.003)	0.289 (0.861)	-0.023 (1.015)	-0.312*** (0.102)	-0.159 (0.973)	0.407 (0.705)	-0.211 (0.978)	-0.619*** (0.135)	0.191 (1.005)	0.216 (0.944)	0.186 (1.016)	-0.03 (0.143)
Score in Neoffi - Openness (std.)	0.009 (1.004)	0.279 (0.959)	-0.026 (1.005)	-0.304*** (0.112)	-0.132 (0.998)	0.434 (1.125)	-0.184 (0.971)	-0.618*** (0.206)	0.156 (0.989)	0.183 (0.838)	0.152 (1.014)	-0.031 (0.13)
Score in Neoffi - Agreeableness (std.)	-0.001 (0.994)	0.322 (0.913)	-0.043 (0.997)	-0.365*** (0.107)	-0.124 (1.014)	0.463 (0.957)	-0.178 (1.004)	-0.641*** (0.178)	0.128 (0.957)	0.236 (0.883)	0.11 (0.969)	-0.126 (0.134)
Score in Neoffi - Conscientiousness (std.)	0.011 (1)	0.23 (0.839)	-0.018 (1.016)	-0.247** (0.1)	-0.096 (0.98)	0.342 (0.819)	-0.137 (0.985)	-0.479*** (0.154)	0.123 (1.01)	0.16 (0.852)	0.116 (1.036)	-0.044 (0.132)
Observations	374	43	331	374	190	16	174	190	184	27	157	184

*Note:* Stars indicate the statistical significance of differences in means across groups at various significance levels: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Each group exhibits the difference between the referred group against the initial contact group. Note that variables bearing the notation of a value followed by ==1 are indicative of the group's proportion.

Figure 1: Changes in the Sample Composition of the REAs (by week)



*Note:* Each one of the four panels illustrates how one key characteristic of the REAs evolves as sample sizes increase in our dataset. This information is succinctly summarized on a weekly basis.

## 3.2 Experiment reliability

As with any research, our study has certain limitations. Specifically, we acknowledge that our artifactual field experiment may not be fully generalizable to real-world situations due to the controlled environment in which it was conducted, which may not capture all the complexities of the rental housing market in Ecuador. In particular, the fact that we focus on formal transactions is important. Despite its limitations, our experimental design allows us to measure discrimination in a controlled environment, providing insights into discriminatory behaviors. Furthermore, it gives us the ability to test the impact of a behavioral intervention and the data collected about decision-making provides an opportunity to gain insights that may not be possible through other research methods.

Another important consideration about the inferences that can be made from this artificial field experiment is the validity of REAs choices. The experimental design minimizes the possibility of strategic behavior or misrepresentation of REAs' preferences for several reasons. First, we provide REAs with clear and concise instructions and set a clear goal: to identify the best candidates. To achieve that goal we validated the correct understanding of the tasks in pilot tests. Second, the inherent structure of the contract enables the best performance of the REAs, providing the opportunity for repeated interactions in future transactions. Finally, the experiment involved real-stakes remuneration.

We address concerns about ethical considerations and experimenter effects as follows. First, we adhere to Experimental Economics' norm against using deception. In this regard, we hired REAs services to provide consulting services over rental candidates. Second, we preserved the anonymity and privacy of the participants. Ethical concerns were meticulously addressed through discussions with an Institutional Review Board (IRB) committee,<sup>7</sup> while the research adheres to the outlined pre-analysis plan<sup>8</sup>.

Another potential concern is the impact of experimenter demand effects over REAs' behavior, as they may have displayed a behavior consistent with their expectations of what we wanted to measure (discrimination). Our experimental design minimizes the risk of experimenter demand effects biasing our results. We included placebo rounds in the experiment and conducted a focus group with some participants after the intervention to make sure the purpose of the experiment was not evident to them. If REAs had realized the purpose of the experiment and changed their behavior systematically, we would expect the direction of biases to be consistent across measures of discrimination in the placebo rounds. However, our results show disparities in the direction of the effects. Specifically, migrant and LGTBI families were ranked less fit for the properties they applied to, while female-headed families were ranked more fit (all results were statistically significant). This finding is not consistent with a condescending direction of bias to hide true preferences. In addition, our study assessed discrimination within a control group comprising quasi-identical individuals lacking a minority attribute, strategically randomized to ascertain a discrimination rate of 0%. This finding augments the assurance of the methodological

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<sup>7</sup>Pearl IRB #22-IADB-104. Approval date: 2022-10-04

<sup>8</sup><https://www.socialscienceregistry.org/trials/10307>

soundness of our research. This observation is visually depicted in Figure 9, wherein the discrimination coefficient values closely approximate zero, with no statistically significant deviation from this baseline.

Furthermore, our findings align with other studies on discrimination against Venezuelan migrants in Ecuador. In particular, there is evidence that migrants face significant barriers to accessing housing. The discrimination faced by Venezuelan migrants, particularly in countries like Colombia and Peru, appears to be influenced by social norms and perceptions. A study titled “Discriminación en silencio” highlights the indirect discrimination experienced by Venezuelan migrants in Colombia, which is often influenced by stereotypes propagated by the media and interactions with the state (Taborda Burgo, Acosta Ortiz and Garcia, 2021). Previous experimental research by Zanoni and Díaz (2023) in Colombia investigated REAs’ choices between migrants and non-migrant candidates in a similar artifactual field experiment. The exercise revealed the REA’s behavioral alignment with prevailing social norms as the elicitation of REAs’ second-order beliefs revealed that discrimination is a social norm REAs are aware of<sup>9</sup>. If this finding proves generalizable, it suggests that social desirability bias is not driving our results, as the social norm in this case is to favor nationals over migrants. REAs may respond genuinely to the nudge as it provides new information that contradicts the existing social norm. Moreover, prior research provides another reason to have confidence in the absence of experimenter effects influencing our results, given the congruence between outcomes obtained from artifactual and correspondence field experiments. (Zanoni et al., 2023; Zanoni, Acevedo and Hernandez, 2022) This comparison rules out the likelihood of experimenter effects, as such effects are generally less of a concern in correspondence studies. Nonetheless, we acknowledge that further research in diverse settings is necessary to assess the validity and effectiveness of

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<sup>9</sup>Research indicates that migrants, especially those from non-European backgrounds, face significant barriers in accessing resources and opportunities, often rooted in societal norms and stereotypes (Taborda Burgo, Acosta Ortiz and Garcia, 2021). In Sweden, despite being similarly qualified, they experience higher unemployment rates and lower wage incomes due to statistical discrimination, network effects, and institutional discrimination (Rydgren, 2004). The dynamics of acculturation can also influence migrants’ perceptions of discrimination and their subsequent reactions to it (Schwartz et al., 2010). Discrimination against migrants manifests individual prejudices and is deeply embedded in societal structures, norms, and beliefs.

nudges in mitigating discrimination.

One conceptual challenge in our experiment is whether to interpret the differential rates of preferences between Venezuelans and Ecuadorians as discrimination. Suppose the applicants’ information is perceived as incomplete to judge the reliability of the tenant. In that case, imputing mean values that are attributable to the group or seeking to find more information fits the label of statistical discrimination. Consequently, our estimates are, in fact, weighted combinations of statistical and taste-based discrimination.<sup>10</sup>

## 4 Results

### 4.1 The Outcome Variables and Their Distributions

As we explained, we examine the impact of a nudge on the discriminatory behavior of the REAs employing an experimental design: we randomly assigned some REAs to a treatment group, where they received a nudge intended to reduce discriminatory behavior, and others to a control group, where they received no intervention. We then compared the behavior of the two groups to assess the effectiveness of the nudge in reducing discrimination.

To gain an initial understanding of the nudge’s impact, we commence this section by analyzing the unconditional distribution of the outcome variables among the untreated observations in the dataset. It’s important to note that the nudge’s influence began during the evaluation of the final trial of the experiment. Consequently, the distributions of outcomes in both the pre-nudge trials and the experimental control trial can be used to portray the size of the baseline discriminatory patterns that the nudge seeks to address.

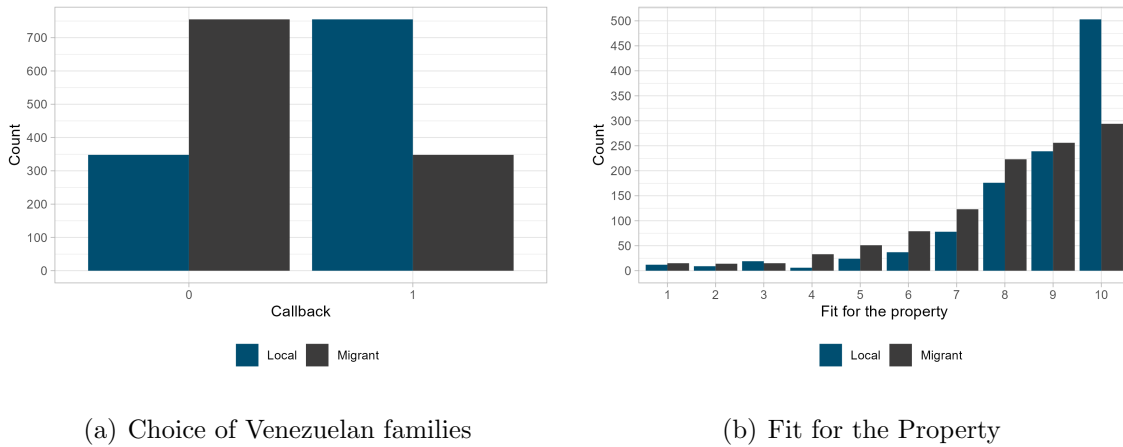
Panel “(a) choice of Venezuelan families” of Figure 2 displays the frequency distribution of how often migrant and non-migrant families were chosen by the REAs (with a value of 1) versus not chosen (with a value of 0) before the nudge took effect. The graph illustrates that among families not selected by the REAs, the frequency of migrants is more than twice that of non-migrants. In contrast, when families are chosen by the REAs as preferred candidates,

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<sup>10</sup>It could also be the case that the assessment about the probability that a migrant will fulfill contracts is more accurate when REAs assess local vs. migrant applicants. This type of differential ”risk” in the assessment is statistical discrimination in the second moment.

migrants constitute less than half of the selected families compared to non-migrant families. In panel B, we depict the frequency distribution of the fit for the property assessments made by REAs to qualify applicant families (the values of that assessment variable range from one to ten). The graph highlights that Venezuelan families were more frequently categorized with lower suitability, compared to local families. In particular, as the values of the suitability scores increase, REAs exponentially tend to favor Ecuadorian families. Compared to Venezuelan families, many more Ecuadorian families were assigned the highest suitability score (10) by the REAs. Given that the pairs of candidates in each trial were observationally equivalent, this disparity in scoring suggests potential discrimination as a factor influencing the differences in choice and assessments.

Figure 2: Distribution of the Outcome Variables by Migratory Status

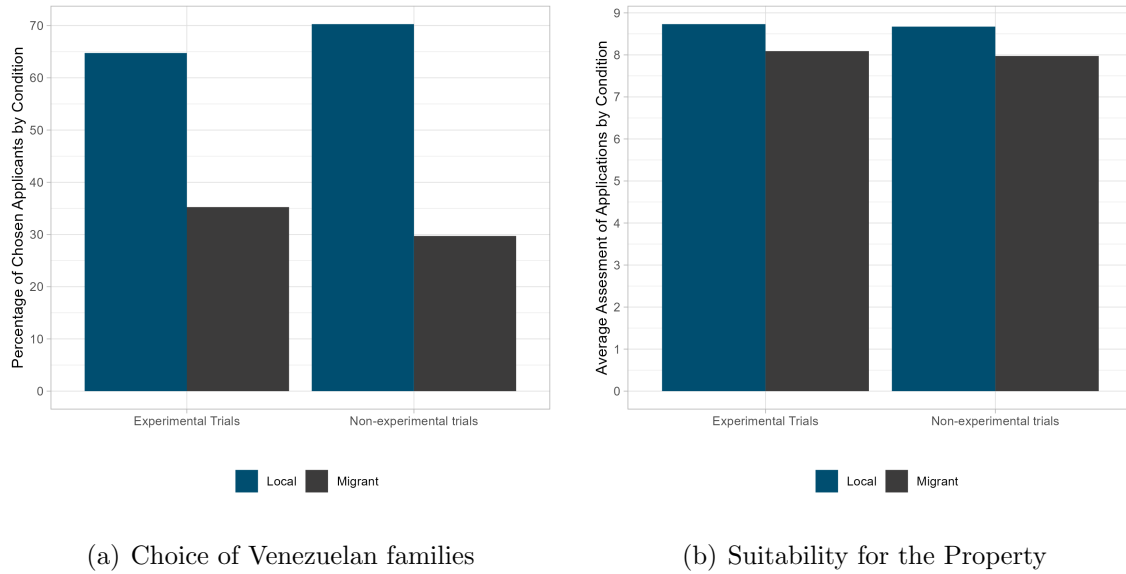


*Note:* Panel (a) reveals that migrant families were more often not selected by REAs and constituted less than half of the preferred selections. Panel (b) indicates that Venezuelan families frequently received lower suitability scores than Ecuadorian families.

In Figure 3, we present the distribution of the two outcome variables: choice and suitability for the property, segmented by migratory status. This is further divided into trials that were exposed to the nudge (Experimental trials) and those that weren't (Non-experimental trials). The bars in Panel "(a) Choice of Venezuelan Families" provides a side-by-side comparison, with the left figure showing the percentage of families chosen by nationality in the Experimental trials, and the right one for the Non-experimental trials. Notably, in the Experimental trials, a larger percentage of Venezuelan families were selected

compared to the Non-experimental trials, with the opposite trend observed for Ecuadorian families. Panel “(b) Suitability for the Property” delves into the suitability scores. Here, we see a correlation between choice and perceived suitability. Specifically, in experiments where REAs received the nudge, Venezuelan families were more frequently assigned higher suitability scores than in the non-nudged trials. Overall, the insights from Figure 3 suggest the nudge might have swayed the REAs’ decisions and assessments, favoring Venezuelan families seeking apartment leasing.

Figure 3: Distribution of the Outcomes: Trials Affected and not by the Nudge



*Note:* Panel (a) contrasts the selection rates of Venezuelan families in Experimental vs. Non-experimental trials. Panel (b) shows how the nudge influenced the suitability scores for Venezuelan families in apartment leasing assessments.

## 4.2 The REAs: characteristics by treatment group

To ensure the validity of our experimental design, we examined the balance of REAs’ attributes across the treatment (affected by the nudge) and control (not affected by the nudge) groups of the experiment using Table 2. The results show that the REAs’ attributes were generally balanced across most dimensions, with two notable exceptions: the attainment of a bachelor’s degree or higher (those in the control group are 12 percentage points less likely) and whether they work independently or for a real estate agency (those who



received the nudge were 9% more likely to work independently).

Despite these differences, the mid panel in Table 2 also shows that, before randomization occurred, the outcome variables measuring discrimination against Venezuelan migrants (choice of Venezuelan families and assessments of fit for the property) were balanced across REAs by treatment status. This balance in the pre-nudge outcome variables suggests that, before the intervention, the treatment and control groups were comparable in terms of their baseline levels of discrimination against Venezuelan migrants, and the imbalance in the two variables that we mentioned should not affect the estimates of the impact of the nudge on discrimination.

Furthermore, in Table 2 we also assess the balance of cognitive and socio-emotional skills across the treatment and control groups. We administered standardized cognitive and socio-emotional tests to the REAs and compared their scores by treatment status<sup>11</sup>. Our findings revealed no mean differences between REAs assigned to treatment and control groups in terms of their scores on these tests. This further supports the soundness of our experimental design and strengthens its internal validity by suggesting that any differences in the outcome variables can be attributed to the nudge intervention rather than to pre-existing differences in cognitive or socio-emotional skills between the treatment and control groups.

### 4.3 Baseline Discrimination Estimates

To gain a deeper understanding of the impact of the nudge, in this subsection, we establish that discrimination against Venezuelan migrants in the rental market does, in fact, exist and proceed to quantify its magnitude. We refer to the measures used to quantify this baseline discrimination as the “discrimination coefficients”. The estimating equation for those discrimination coefficients is as follows:

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<sup>11</sup>In this paper, we implemented three psychological assessments, including the Wonderlic Personnel Test (WPT), a timed cognitive ability test often used in employment contexts; the Rosenberg Self-Esteem Scale (RSE), a 10-item measure gauging global self-esteem; and the NEO Five-Factor Inventory (NEO-FFI), a 60-item instrument evaluating five core personality traits: neuroticism, extraversion, openness to experience, agreeableness, and conscientiousness.

Table 2: Balance Table: REAs by Treatment Status

	(1)	(2)	(3)	(4)
Variable	All	Control	Nudge	Difference (2)-(3)
<b>Demographics and education</b>				
REAs Age (years)	36.465 (11.182)	36.484 (10.987)	36.447 (11.387)	-0.037 (0.818)
REAs Gender (Female == 1)	0.583 (0.493)	0.602 (0.49)	0.564 (0.497)	-0.038 (0.036)
REAs Nationality (Ecuadorian == 1)	0.981 (0.136)	0.984 (0.126)	0.979 (0.144)	-0.005 (0.01)
Sampling method (Referred == 1)	0.885 (0.319)	0.876 (0.33)	0.894 (0.309)	0.017 (0.023)
City: Quito	0.492 (0.5)	0.5 (0.501)	0.484 (0.5)	-0.016 (0.037)
City: Guayaquil	0.508 (0.5)	0.5 (0.501)	0.516 (0.5)	0.016 (0.037)
REAs Employment Experience (Years)	3.83 (4.093)	3.882 (4.219)	3.78 (3.97)	-0.102 (0.3)
Does REA Works Full Time? (Yes == 1)	0.337 (0.473)	0.344 (0.476)	0.33 (0.471)	-0.014 (0.035)
REAs Work Status (Independent == 1)	0.548 (0.498)	0.5 (0.501)	0.596 (0.491)	0.096*** (0.036)
Does the REA has a college degree? (Yes == 1)	0.495 (0.5)	0.559 (0.497)	0.431 (0.496)	-0.128*** (0.036)
Share of knowledge of Real State Market (%)	33.209 (18.451)	33.656 (17.639)	32.766 (19.234)	-0.89 (1.349)
<b>Outcomes (before the nudge)</b>				
Callback	0.474 (0.499)	0.473 (0.5)	0.475 (0.5)	0.001 (0.025)
Fit for the Property	8.13 (2.01)	8.059 (2.027)	8.198 (1.992)	0.138 (0.1)
<b>Scores in standardized tests</b>				
Score in Wonderlic test (std.)	-0.005 (1.002)	-0.016 (0.986)	0.006 (1.019)	0.021 (0.073)
Score in Rosenberg test (std.)	0.014 (1.001)	0.018 (1.05)	0.011 (0.952)	-0.007 (0.074)
Score in Neoffi test (std.)	0.01 (1.001)	0.047 (0.939)	-0.027 (1.058)	-0.074 (0.074)
Score in Neoffi - Neuroticism (std.)	-0.006 (1.002)	-0.063 (0.959)	0.049 (1.04)	0.112 (0.074)
Score in Neoffi - Extroversion (std.)	0.012 (1.003)	0.069 (0.931)	-0.043 (1.067)	-0.112 (0.074)
Score in Neoffi - Openness (std.)	0.009 (1.004)	0.06 (0.962)	-0.041 (1.042)	-0.101 (0.074)
Score in Neoffi - Agreeableness (std.)	-0.001 (0.994)	0.005 (0.912)	-0.007 (1.069)	-0.012 (0.073)
Score in Neoffi - Conscientiousness (std.)	0.011 (1)	0.071 (0.974)	-0.048 (1.023)	-0.118 (0.074)
Observations	374	186	188	374

*Note:* Stars indicate the statistical significance of differences in means by treatment status. Significance levels are: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ . Columns (2) and (3) are means of REAs attributes and outcomes based on whether they received the nudge or not.

$$Y_{itr} = \beta_0 + \beta_1 X_{it} + \beta_k Z_{it} + \epsilon_{itr}. \quad (1)$$

In this model,  $Y_{itr}$  denotes one of the two dependent variables: either an indicator set to

one if the family is chosen by the REA and zero otherwise (for the “Choice of applicants” outcome) or the score in a Likert scale from one to ten (for the “Suitability for the property” outcome). The values of  $Y_{itr}$  reflect those outcomes associated with REA  $r$  when assessing the rental applicant  $i$  during trial  $t$  of the experiment. The variable  $X_{it}$  is an indicator that signifies whether the rental applicant under evaluation was randomly designated as a Venezuelan migrant, and  $\beta_1$  is the discrimination coefficient.  $Z_{it}$  represents a control vector that includes structural variables such as the sampling method, city fixed effects, and applicants’ characteristics. Finally,  $\epsilon_{itr}$  accounts for unobserved heterogeneity.

The discrimination coefficients shown in Table 3 are OLS regression coefficients of  $\beta_1$  from Model 1, calculated using all observations from trials not exposed to the nudge (trials 1-7 in the experiment). In each row panel, we provide overall estimates and estimates computed across relevant subsamples, categorized by the city where the rental application occurred (Guayaquil vs. Quito) and by the gender of the REA, and we test for the statistical significance of the difference in those coefficients using a Wald test.<sup>12</sup> Below the discrimination coefficients, we present standard errors (clustered at the REA level) and display the proportion of non-migrants chosen by the REAs in trials 1-7 to offer a comparison basis to assess the coefficients’ magnitudes.

In a scenario without discrimination, REAs would choose equally qualified applicants in the same manner, regardless of their nationality. Consequently, the proportion of families chosen by nationality would average 50% across all properties evaluated. However, Table 3 reveals that this is not the case, indicating discrimination against Venezuelans. Specifically, there is a 39% difference in the proportion of Venezuelan families compared to Ecuadorian families identified as the best choice candidates. This amounts to 56% fewer Venezuelans than Ecuadorian families chosen across the properties. Reading results across the columns in the top panel, we see that REAs from Quito tend to discriminate more than those in Guayaquil, with a 28 p.p. difference in their discrimination coefficients that is statistically significant. No differences in discrimination measured by the choice of applicants were

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<sup>12</sup>The Wald test assesses whether discrimination coefficients from different OLS models are statistically different. It does this by comparing the difference in estimated coefficients relative to their standard errors. The test statistic, derived from the squared ratio of the coefficient difference to its standard error, follows a chi-squared distribution.

found by gender.

The REAs’ tendency to select families as the best candidates for properties is consistent with their ratings based on nationality: on average, Venezuelans were perceived as less suitable for the properties than Ecuadorians. As indicated by the discrimination coefficient in Table 3, Venezuelans were rated, on average, 0.67 basic points lower in suitability compared to Ecuadorians (on a Likert scale). The data further suggests that discrimination in the rental market is also notably less evident in Guayaquil than in Quito when it comes to rating applicant families. Interestingly, when considering this particular outcome, female REAs seem to demonstrate greater discriminatory behavior than their male counterparts. Those differences in ratings across the city and gender dimensions were statistically significant.

Table 3: Baseline Discrimination

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Both Cities	Guayaquil	Quito	Diff. (2) - (3)	Male REA	Female REA	Diff. (5) - (6)
<b>1. Choice of migrant families :</b>							
Discrimination Coeff.	-0.3938 *** (0.0408)	-0.3253 *** (0.0551)	-0.6064 *** (0.0866)	0.2811***	-0.3709 *** (0.0632)	-0.4092 *** (0.054)	0.0383
Prop. of Locals chosen	0.7028	0.6685	0.7377		0.7206	0.6896	
Observations	1474	742	732		612	862	
<b>2. Fit for the property:</b>							
Discrimination Coeff.	-0.6714 *** (0.0987)	-0.6599 *** (0.1383)	-0.9198 *** (0.1839)	0.2599*	-0.5073 *** (0.14)	-0.78 *** (0.1367)	0.2727*
Average Property Assessment for Locals	8.673	8.3639	8.9863		8.752	8.6357	
Observations	1474	742	732		612	862	

*Note:* In the panels dedicated to each outcome variable, various statistical indicators are presented, including the discrimination coefficient, the mean value of the control group, and the observed data. Column (1) highlights the effect for the full sample (including both cities), while the subsequent two columns (2 and 3) provide city-specific outcomes, and the fourth column shows the differences between the mean values in the second and third columns (the differences in means were assessed using a Wald test). Columns (5), and (6) compare mean outcomes for males, and female REAs; column (7) tests the statistical significance of the difference in those means. Significance levels are: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

In summary, this section shows evidence of baseline discrimination against Venezuelan migrants in the rental market. We quantified this discrimination using “discrimination coefficients,” which were derived from OLS regression coefficients of  $\beta_1$  from Model 1.

Venezuelan families are less likely to be chosen as the best candidates for properties and are rated lower in suitability compared to Ecuadorian families. Specifically, there is a 39% difference in the proportion of Venezuelan families chosen as the best candidates, amounting to 56% fewer Venezuelan families being selected across the properties. This discrimination is more pronounced in Quito than in Guayaquil, with a statistically significant 28 percentage point difference in their discrimination coefficients. Furthermore, female REAs appear to exhibit greater discriminatory behavior in their ratings of applicant families than their male counterparts. These results put in context the importance of testing the effectiveness of policies to address discrimination against Venezuelan migrants in the rental market, which we do in the following section.

#### 4.4 The Nudge Impacts

To examine the effects of the nudge, we turn to Table 4. This table is divided into two horizontal panels, each corresponding to a different outcome of interest. Panel “1. Choice of applicants” relates to the family selected by the REA, while panel “2. Suitability for the property” is self-explanatory. Within each panel, the top row displays estimates of the nudge’s impact, and the bottom row presents estimates of the mean in the control group to provide context for the magnitude of the impact. As in the previous analysis, across the rows of the table, we show estimates for the entire sample as well as for subsamples by city and gender of the REA. We computed those estimates using an OLS regression model with the following model specification:

$$Y_i = \beta_0 + \beta_n N_i + \beta_2 X_i + \beta_3 D_i + \epsilon_i, \quad (2)$$

where  $Y_i$  represents the outcomes of interest for real estate agent  $i$ : a) the choice of a Venezuelan family for apartment leasing, and; b) the difference in the assessment of suitability for the property between an Ecuadorian and a Venezuelan family in the experimental trial.  $N_i$  is an indicator variable that takes the value of 1 if real estate agent  $i$  received the nudge (treatment group) and 0 otherwise (control group).  $X_i$  is a vector of control variables for real estate agent  $i$ , including demographic factors exposed in Table 1.  $D_i$  is a vector of control variables that account for features of the experiment design,

including fixed effects for the experimental trial where the REA evaluated a Venezuelan family (which could be any trial 8 to 10). Furthermore,  $D_i$  also includes fixed effects for the sampling method, city, and property assessed in the experimental trials (there were 8 properties in this section).  $\beta_0$  is the intercept,  $\beta_n$  is the coefficient of interest representing the impact of the nudge on the outcome,  $\beta_2$  and  $\beta_3$  is the vector of coefficients for the control variables for the REAs and design features indicated.  $\epsilon_i$  captures unobserved factors affecting the outcome.

In Model [2](#), the coefficient  $\beta_n$  represents the average difference in the choice of Venezuelan families and suitability scores between REAs affected by the nudge and the control group, holding constant heterogeneity that could be induced by  $X_i$  and  $D_i$ . In Figure [8](#), we show that the coefficient estimates of the nudge impact are consistent across alternative specifications that change the set of variables in  $X_i$  and  $D_i$ , as well as when using different estimators instead of OLS.

Table 4: Experimental Impacts of the Nudge ( $\beta_n$ )

	(1) Both Cities	(2) Guayaquil	(3) Quito	(4) Diff. (2) - (3)	(5) Male REA	(6) Female REA	(7) Diff. (5) - (6)
<b>1. Choice of migrant families :</b>							
Nudge Impact	0.1141 ** (0.052)	0.0918 (0.0729)	0.1545 ** (0.0776)	-0.0627	0.0526 (0.0707)	0.1576 * (0.0799)	0.105
Prop. of Locals chosen	0.3103	0.2588	0.3596		0.3786	0.2113	
Observations	356	173	183		205	151	
<b>2. Fit for the property:</b>							
Nudge Impact	0.3283 * (0.1822)	0.1305 (0.1952)	0.5348 * (0.3141)	-0.4043**	0.3945 (0.2388)	0.2333 (0.2815)	-0.1612
Average Property Assessment for Locals	-0.7759	-0.5176	-1.0225		-0.8155	-0.7183	
Observations	356	173	183		205	151	

*Note:* In the panels dedicated to each outcome variable, various statistical indicators are presented, including the nudge impact coefficient, the mean value of the control group, and the observed data. Column (1) highlights the effect for the full sample (including both cities), while the subsequent two columns (2 and 3) provide city-specific outcomes, and the fourth column shows the differences between the mean values in the second and third columns (the differences in means were assessed using a Wald test). Columns (5), and (6) compare mean outcomes for males, and female REAs; column (7) tests the statistical significance of the difference in those means. Significance levels are: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

The first observation we make is that, for the full sample (i.e. the column labeled “Both Cities”), the nudge had a positive and statistically significant impact on the decision of REAs to choose Venezuelan families. Compared to the mean of the control group, the 0.11  $\beta_n$  coefficient magnitude suggests that, because of the nudge, REAs were 36% more likely to select a Venezuelan family as their preferred choice than what they would have done absent the intervention. The results in the table also suggest that those effects are a weighted average of uneven margins between Quito and Guayaquil with REAs from the former city less affected than those from the latter one, and male REAs also more sensitive to the intervention than female ones. These two latter results have to be taken with caution for they are only suggestive. Wald tests for the differences in the coefficient estimates within the city and gender groups failed to reject the hypothesis that they were different from zero. The lack of statistical significance, however, appears to be due to the lack of power, but further research is needed to validate that hypothesis.

Regarding the fit-for-property outcome, the nudge had a positive effect, with a statistically significant  $\beta_n$  coefficient of 0.2975. The magnitude of the impact of the nudge on the fit-for-property outcome can be calculated by dividing the coefficient of the nudge (0.2975) by the difference between Venezuelans and Ecuadorians (0.7759). This yields an impact of approximately 38.4%. This indicates that the nudge reduced the gap in suitability scores between Venezuelans and Ecuadorians by that much. There were no differences in this impact by city, and women appeared to be more affected by the nudge than men. This suggests that the estimate of the nudge impact is primarily driven by behavioral changes in female REAs.

The findings from this section underscore the potential of nudges as a tool to combat discrimination against Venezuelan migrants in the rental market. The observed 36% increase in the likelihood of REAs selecting a Venezuelan family as their preferred choice, due to the nudge, speaks to the efficacy of such interventions. This is particularly significant in light of the broader literature on discrimination, which highlights the pervasive mental health costs of racial discrimination (Elias and Paradies, 2016), the institutionalization of racial differences through various discourses (Ahn, 2013), and the barriers migrants face in accessing essential services (Rosano et al., 2017). The differential impacts observed across

cities and between genders further emphasize the need for localized and tailored interventions. The findings add to the empirical literature on discrimination in developing countries (Kuang and Liu, 2012; Lin et al., 2011). In addition, they offer actionable insights for policymakers specifically tailorable to design anti-discrimination policies to foster the inclusion of Venezuelans in their host countries.

## 4.5 Heterogeneous effects of the nudge

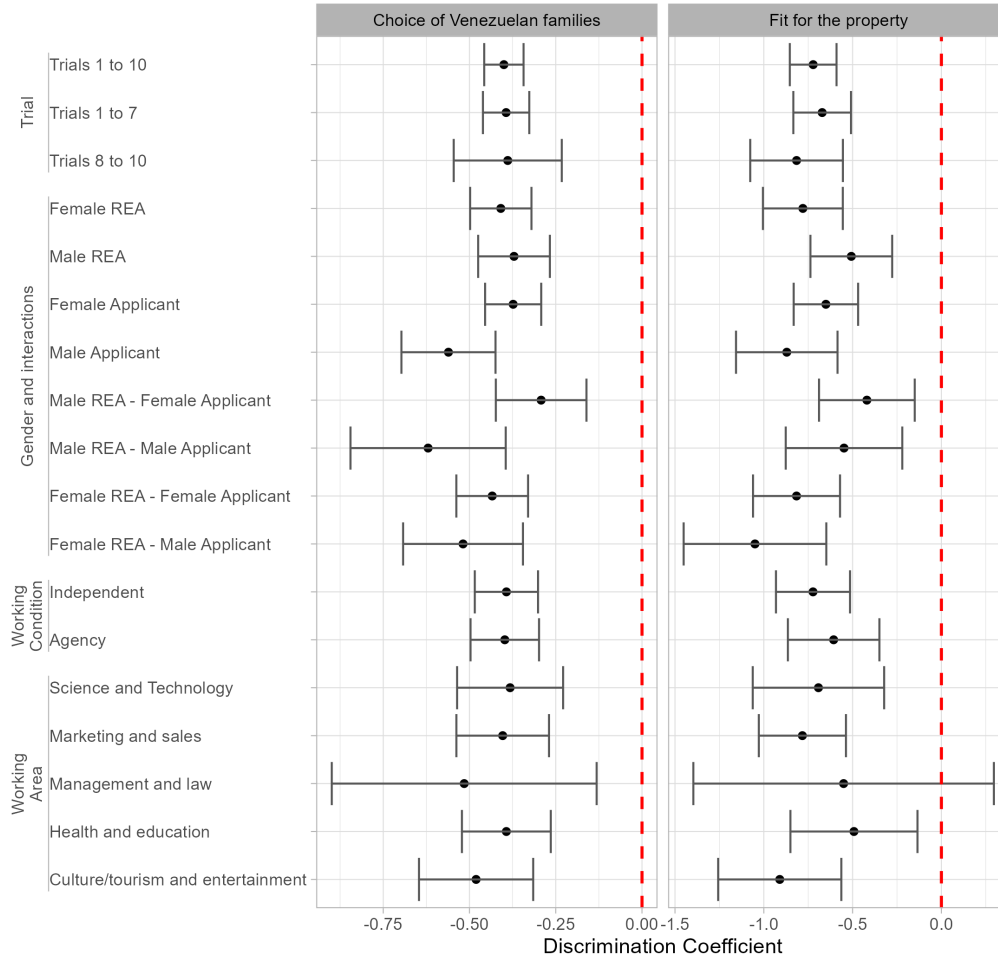
Before proceeding to describe heterogeneity in the impact of the nudge, we first explore salient patterns of heterogeneity in the baseline discrimination coefficient estimates. Understanding the baseline variations in discrimination across different groups is essential to accurately assess the effects of the nudge. This insight not only guides more tailored interventions but also enriches academic discourse on the complexities of bias against migrants in the rental market.

In Figure 4, we provide a graphical representation with estimates of the discrimination coefficients across several subgroups (alongside 90% confidence intervals). Across the diverse dimensions under scrutiny—spanning city, REA gender, applicant gender and their interactions, applicant occupation, and the employment status of the REA (independent versus affiliated with a real estate agency)—all estimates consistently manifest as negative and achieve statistical significance. The magnitude of these estimates varies, ranging from -0.25 to -0.625. Notably, the most pronounced discrimination is evident when male REAs evaluate male-headed applicant families. In terms of occupational variance, the “Management and Law” category exhibits the greatest variance in the discrimination coefficients. Conversely, the least discrimination is discernible when male REAs assess female-headed applicant families.

In Figure 5, we turn attention to the heterogeneous impacts of the nudge, delving into the same variations and outcomes explored in Figure 4. The figure presents the nudge impact estimates alongside 90% confidence intervals. While the data exhibits considerable noise, primarily due to large standard errors that preclude definitive statistical significance, a discernible pattern emerges. The prevalent positive sign across coefficient estimates indicates a pervasive positive effect of the nudge across the majority of subgroups. This



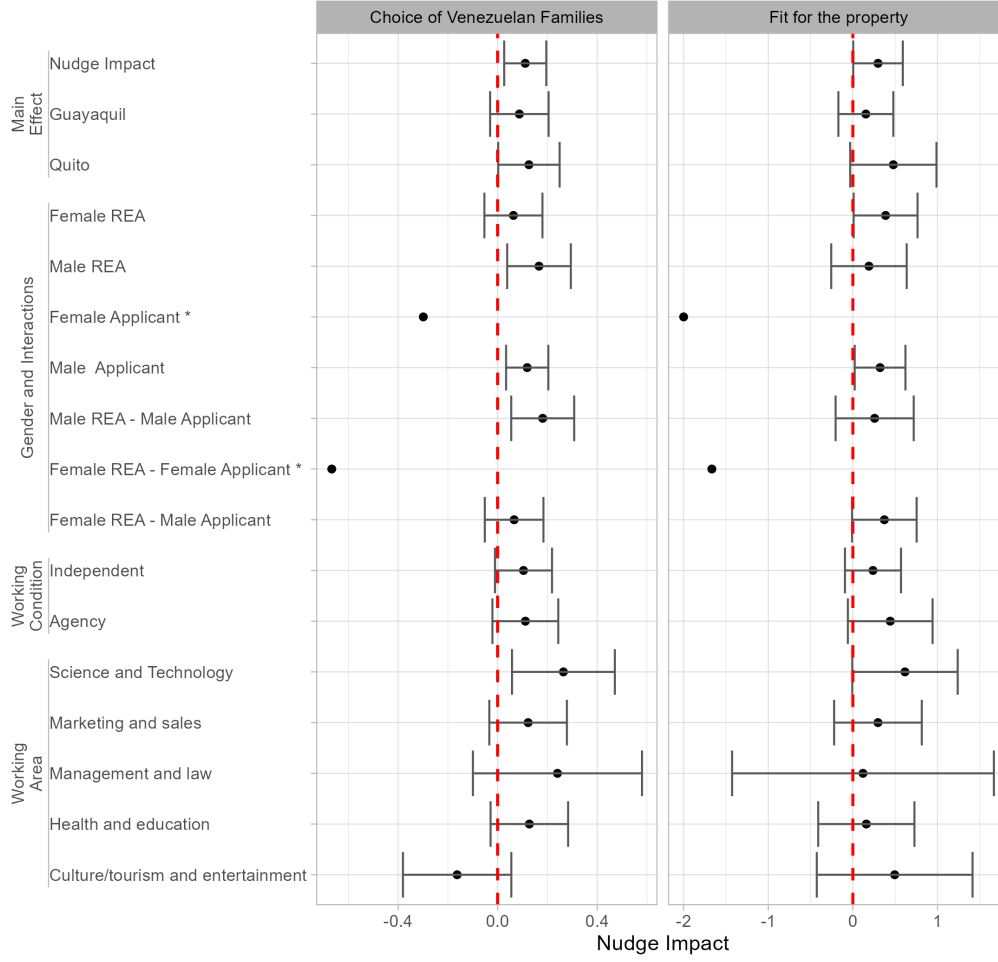
Figure 4: Heterogeneity in the Discrimination Coefficients



*Notes:* This figure presents estimates of discrimination coefficients for various subgroups, accompanied by 90% confidence intervals. Dimensions analyzed include city, REA gender, applicant gender and their interactions, applicant occupation, and REA employment status. *Working Condition* refers to the work modality of the REA, i.e. as an independent worker or working for an Agency. *Working Area*, instead, relates to the fictitious applicants and refers to the industry in which they work in.

suggests that the nudge intervention consistently influenced REA behavior in a favorable direction across diverse contexts and demographics. That consistent positive effect of the nudge across diverse REA characteristics underscores the external validity of the intervention, indicating that such nudges could be effective across a broad spectrum of REAs, irrespective of their background or working conditions, as well as across job positions.

Figure 5: Heterogeneity in the Nudge Impacts



*Notes:* This figure presents estimates of the nudge impact coefficients for various subgroups, accompanied by 90% confidence intervals. Dimensions analyzed include city, REA gender, applicant gender and their interactions, applicant occupation, and REA employment status. *Working Condition* refers to the work modality of the REA, i.e. as an independent worker or working for an Agency. *Working Area*, instead, relates to the fictitious applicants and refers to the industry in which they work in. In some of the estimates, we omitted the confidence intervals that were too large, which was due to the fact that there were too few observations; these estimates are followed by an \*.

## 5 Robustness checks

Next, we present a series of analyses to further validate our research design and provide additional insights into the discriminatory behaviors observed in our study. First, we study how estimates of the discrimination coefficients and the nudge impacts evolved as the experiment unfolded and sample sizes grew toward our projected sample size. This exercise

shows that our findings aren’t simply byproducts of variations in the data-generating process through time, but rather are rooted in stable trends observed throughout the study’s duration. Then, we assess whether the discrimination coefficients across trials of the experiment are constant among the untreated observations, including those in the control group. This test serves to validate two key features of our design: the absence of fatigue effects and the consistency of discrimination coefficients across trials. Next, we explore the non-linearity in the fit-for-property measure, revealing that migrant families were more often rated with lower suitability levels than non-migrant families. We then extend our analysis to examine the experimental effects of the nudge on discrimination against other groups, including women-led families and LGBTQ+ families. Finally, we present estimates of the discrimination coefficients derived from the placebo trial, which further supports our research design and the assumption that families are observationally equivalent. Together, these analyses provide a comprehensive understanding of the discriminatory behaviors observed in our study and the impact of the nudge intervention.

## 5.1 Stability of estimates as the experiment unfolded

In this section, we delve into the temporal dynamics of our experiment, shedding light on the evolution of both baseline discrimination and the impact of the nudge over time, until we reach the planned sample size ( $N=370$  REAs). As the experiment progressed, we observed that the initial variability in our estimates began to stabilize. This stabilization can be attributed to the increasing sample sizes, which naturally led to a reduction in the variance of our estimates. To visually represent this evolution, we present a series of four graphs that capture the weekly variations in the magnitude of the discrimination coefficients and the impact of the nudge.

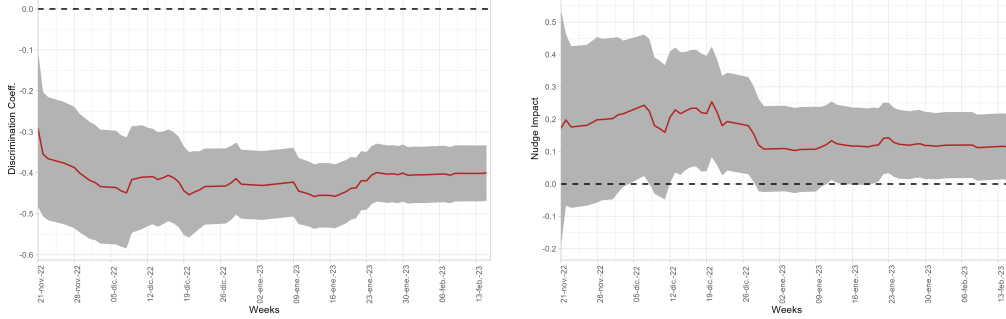
Figure 6<sup>13</sup> provides a comprehensive view of these dynamics. The subfigures (a) and (c) depict the evolution of baseline discrimination for the outcomes “choice of Venezuelan family” and “Fit for property”, respectively. As can be seen, the magnitude of discrimina-

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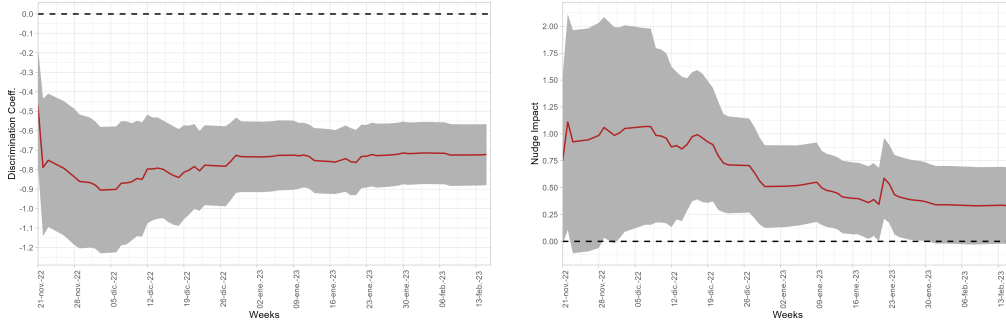
<sup>13</sup>Experiment started on October 31, 2022, but the dynamics are presented from November 14, 2022. The big confidence intervals at the start of the project distort the visual appreciation of convergence to a constant mean.

tion fluctuated at the outset but began to stabilize as the experiment continued. Similarly, subfigures (b) and (d) illustrate the temporal evolution of the nudge’s impact on the same outcomes. Here too, the initial variability in the nudge’s impact reduced over time, converging towards more stable estimates as the sample size grew.

Figure 6: Baseline Discrimination and Nudge Impacts



(a) Choice of Venezuelan family: baseline discrimination (b) Choice of Venezuelan families: nudge impact



(c) Fit for property: baseline discrimination (d) Fit for property: nudge impact

*Notes:* This figure visualizes the evolution of baseline discrimination and the impact of the nudge on the outcomes “choice of Venezuelan family” and “Fit for property.” Subfigures (a) and (c) track baseline discrimination, while (b) and (d) chart the temporal change in the nudge’s effect. Both measures display initial fluctuations before stabilizing as the experiment progresses. The representation starts from November 14, 2022, due to initial wide confidence intervals.

The visual representations in Figure 6 not only offer a perspective of the experiment’s progression but also serve as a robustness check of our findings. The stabilization of the estimates, as depicted in the graphs, reinforces the idea that our results are not mere artifacts of initial fluctuations or anomalies but are grounded in consistent patterns observed over time. This consistency is particularly crucial in field experiments like ours, where the

validity of the findings hinges on the stability of observed effects. Moreover, the diminishing variance as sample sizes grew provides further confidence in our conclusions. It suggests that as we gathered more data, the precision of our estimates improved, reducing the likelihood of spurious results. In essence, the plotted trends not only validate the reliability of our design but also emphasize the potential generalizability of our findings in broader contexts.

## 5.2 Are the discrimination coefficients constant across trials?

At the top of Figure 4 we show discrimination coefficient plots within subsamples of the data restricted to the first seven trials of the exercise, and then for trials 8-10 (only for the control group). We do so to evaluate whether the discrimination coefficients across trials of the experiment are constant, with emphasis on comparing those estimates among the pre-nudge trials (1-7), and the experimental trials (8-10) in the control group. This test validates two features. On the one hand, we show that there are no fatigue effects that could lead to the discrimination coefficients in the first trials of the experiment being distinct from those in the last trial (in which case they would not be the right comparison basis to dimension the nudge effects). On the other hand, by verifying that the discrimination coefficients in no-treated observations are consistent across trials, we can also rule out the possibility of learning effects among the REAs. If REAs were learning over time and adjusting their behavior based on previous trials, this could introduce bias in our estimates of the nudge effects.

The mean estimates portrayed in the top plots in Figure 4 confirm that the discrimination coefficients are stable across trials. As expected (due to the smaller sample size), the variance of the estimates in trials 8-10 is bigger, yet there are no statistically significant differences in the values of the discrimination coefficients between the pre-and post-nudge trials (again: with the latter coefficient calculated for the control group only). This finding reinforces our confidence that the observed effects of the nudge are not confounded by fatigue or learning effects among the REAs.

### 5.3 Estimates with alternative specifications

In this subsection, we show that estimates of the discrimination coefficients and the impact of the nudge are both largely unaffected by the incorporation of covariates, and the choice of estimators.

In Figure 7, we assess the robustness of the *discrimination coefficients* by examining their magnitude across alternative specifications that modify the set of covariates included. For the outcome related to the choice of Venezuelan families, the original LPM model is presented, complemented by marginal effects derived from both logit and probit models across three distinct specifications: “Model 1” includes indicators for whether the REA was contacted directly vs. referred in the context of the RDS, and also adds city fixed effects; “Model 2” adds applicant demographic characteristics (age, income, work experience, gender, and indicators for whether the applicant has a college degree, a partner, children, and legal documents) to Model 1. Model 3 introduces occupation fixed effects to Model 2. When considering the fit for the property outcome, which is categorical, we replicate the OLS models and, for that outcome variable is categorical, we incorporate marginal effects computed from Poisson and negative binomial models. As can be seen in the Figure, the estimates’ magnitudes for both outcomes remain consistent across different estimation methods, even with increasing model structure.

This consistency across diverse specifications and models underscores the robustness of our findings. The three models—ranging from the inclusion of basic indicators and city fixed effects in “Model 1” to the comprehensive addition of applicant demographics in “Model 2” and occupation fixed effects in “Model 3”—all converge on a consistent narrative. Furthermore, the congruence of estimates between the original LPM model and its counterparts in logit, probit, Poisson, and negative binomial models, reinforces the reliability of our conclusions.

In Figure 8, we assess how sensitive are the estimates of the nudge effects to the choice of variables that define the specifications. We estimate the impacts of the nudge with seven distinct specifications. In Model 1, we control for fixed effects for the sampling method, city, property assessed, trial, and profiles of applicants<sup>14</sup>. Progressing, Model 2 expands upon

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<sup>14</sup>There were 8 profiles of observationally equivalent pairs of rental applicants.

this by controlling for the characteristics of REAs that exhibited statistically significant differences between the Control and Nudge groups (an indicator for college degree, and another for whether the REA works for a real estate agency or independently). With Model 3, we encompass all REAs characteristics for a comparative perspective with regard to Model 3<sup>15</sup>. Additionally, Models 4 through 7 branch from the core of Model 1. Model 4 withdraws property fixed effects, Model 5 omits trial fixed effects, Model 6 removes the profile of applicants fixed effects, while Model 7 drops both property and profile of applicants fixed effects.

The figure evidences a consistent pattern in the estimates: they are pretty robust to the introduction of alternative model structures. This consistency underscores the reliability and validity of our findings. The observed effects do not result from arbitrary model choices or the inclusion/exclusion of particular controls but reflect the impact of the nudge. The persistent pattern across models increases our confidence in the intervention’s effectiveness and its potential applicability in real-world scenarios.

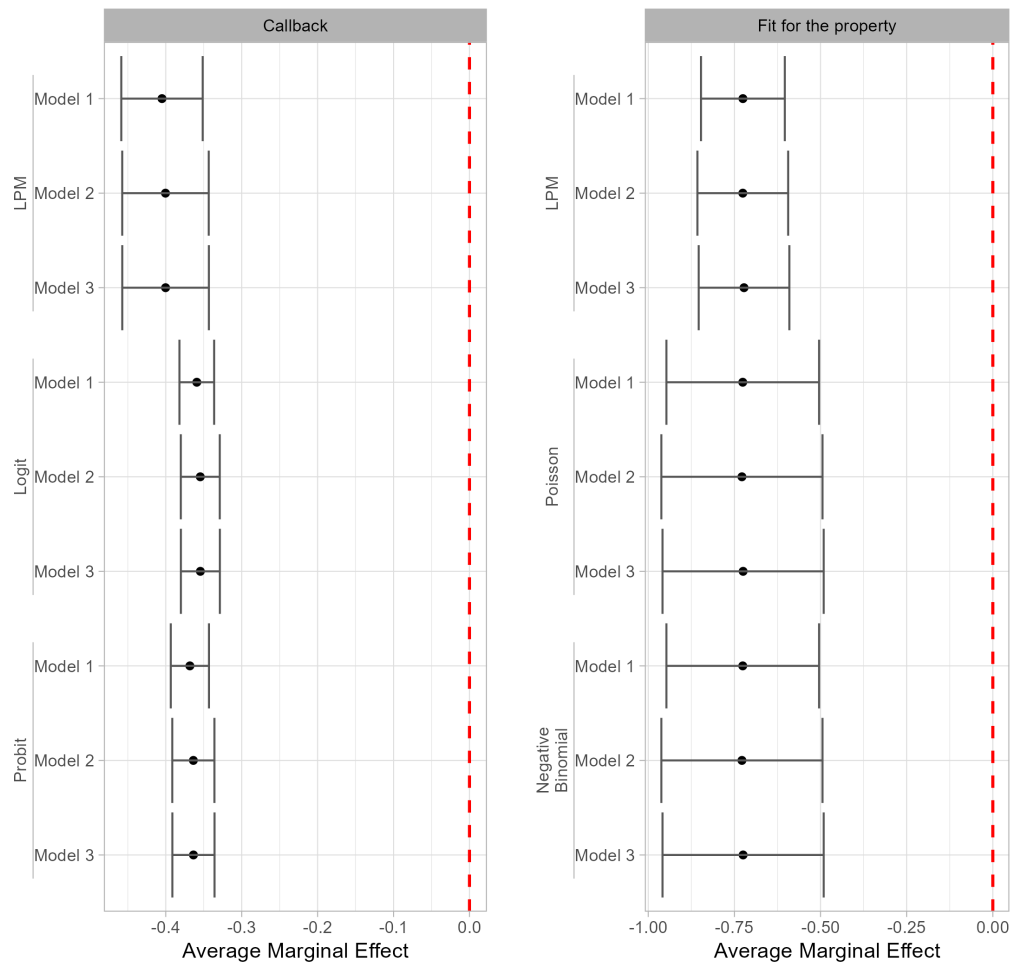
## 5.4 How did the nudge affect discrimination against other groups?

Our study explores whether the observed discriminatory behaviors towards Venezuelan migrants and the effects of the nudge on REAs are specific to this group or if REAs discriminate differently based on other prejudiced attributes. Besides studying discrimination against migrants, we also studied discriminatory behaviors and nudge impacts against people with other characteristics often subject to prejudice. In three of the ten trials, we compared discrimination between women-led and men-led families, and in another three trials, we compared households whose heads self-identified as LGBTQ+ with those who did not. Preliminary results (to be detailed in a forthcoming companion paper) indicate positive discrimination towards women-headed families in both choices of families and fit-for-property assessments, with the nudge having no effect on this pattern. For LGBTQ+ families competing with straight families for tenancy, we found some evidence of negative discrimination against the former group in the fit-for-property outcome, but the nudge

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<sup>15</sup>Those characteristics are age, gender, work experience, and an indicator for full-time work along with the percentage of correct answers to the local real estate market test.

Figure 7: Discrimination Coefficients Estimated with Alternative Estimators and Specifications



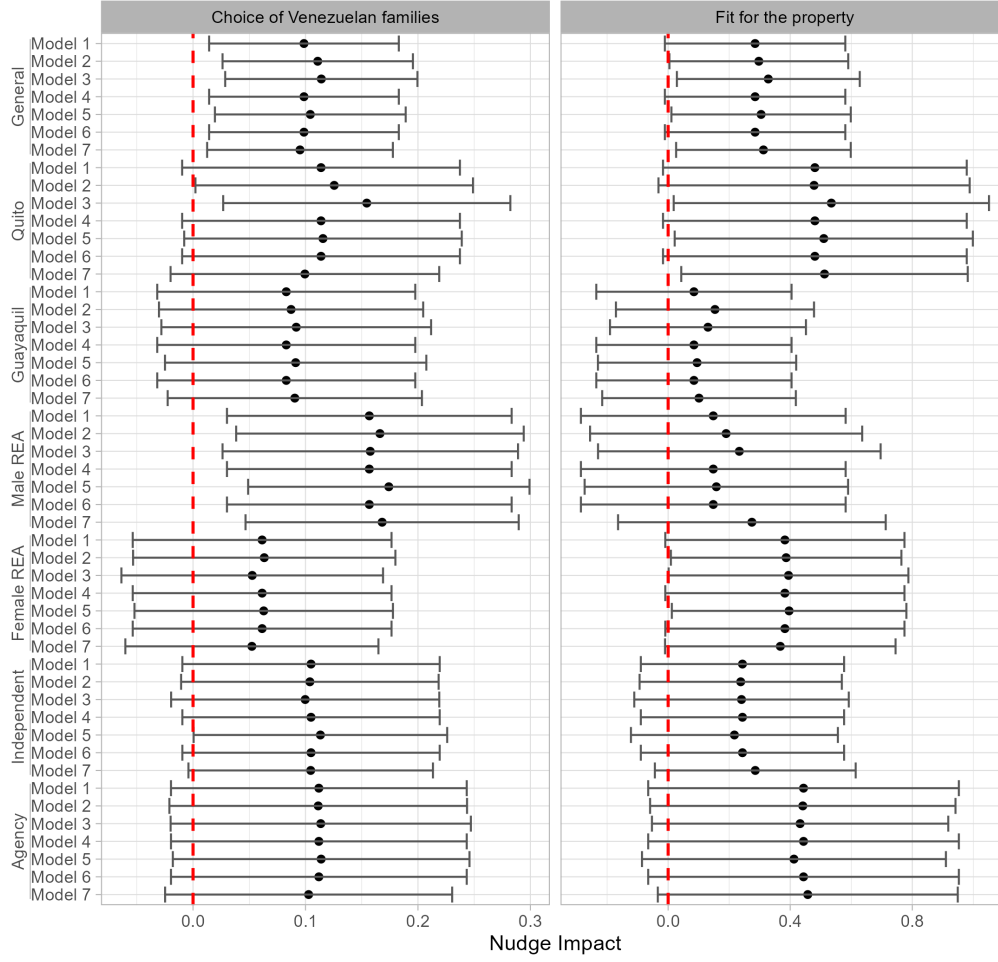
Notes: This figure presents estimates of discrimination coefficients for various subgroups, accompanied by 90% confidence intervals. Furthermore, the different specifications are stated for each outcome variable.

effect had no statistically significant effects.

Studying discrimination against LGBTQ+ and women, in addition to migrants, gives our research a broader perspective on housing market biases. The varied results indicate that biases aren't solely directed at migrants but reflect wider societal tendencies. By comparing outcomes across these groups, we strengthen the credibility of our findings and emphasize the complexity of discrimination. The distinct impact of the nudge on migrant discrimination suggests it specifically addressed biases against migrants rather than general prejudices towards all minority groups.



Figure 8: Nudge Impact Estimated with Alternative Specifications



*Notes:* This figure presents estimates of the nudge impact coefficients for various subgroups, accompanied by 90% confidence intervals. Dimensions analyzed include city, REA gender, and REA employment status.

## 5.5 Discrimination coefficients and nudge effects in the placebo group

To increase confidence in the validity of our research design and affirm the hypothesis that families are observationally equivalent, we incorporated a placebo trial within our set of ten trials. In this specific trial, the Venezuelan migrant attribute was randomized only after the REA had selected a family. Given this design, we anticipated a zero coefficient for discrimination since the REA's choice was uninfluenced by knowledge of the migrant status. This expectation is corroborated by Figure 9, where the majority of the coefficients,

both for the selection of Venezuelan families and their fit for the property, are statistically indistinguishable from zero.

The inclusion of this placebo trial is pivotal for validating our research method. By introducing the Venezuelan migrant attribute post-decision, we ensured the migrant status had no bearing on the REA’s choice. The observed zero coefficient for discrimination, as depicted in Figure 9, aligns with our anticipations and attests to the robustness of our findings. This placebo result strengthens our hypothesis that families were viewed as observationally equivalent by REAs, confirming that any detected biases genuinely stemmed from the randomized migrant status.

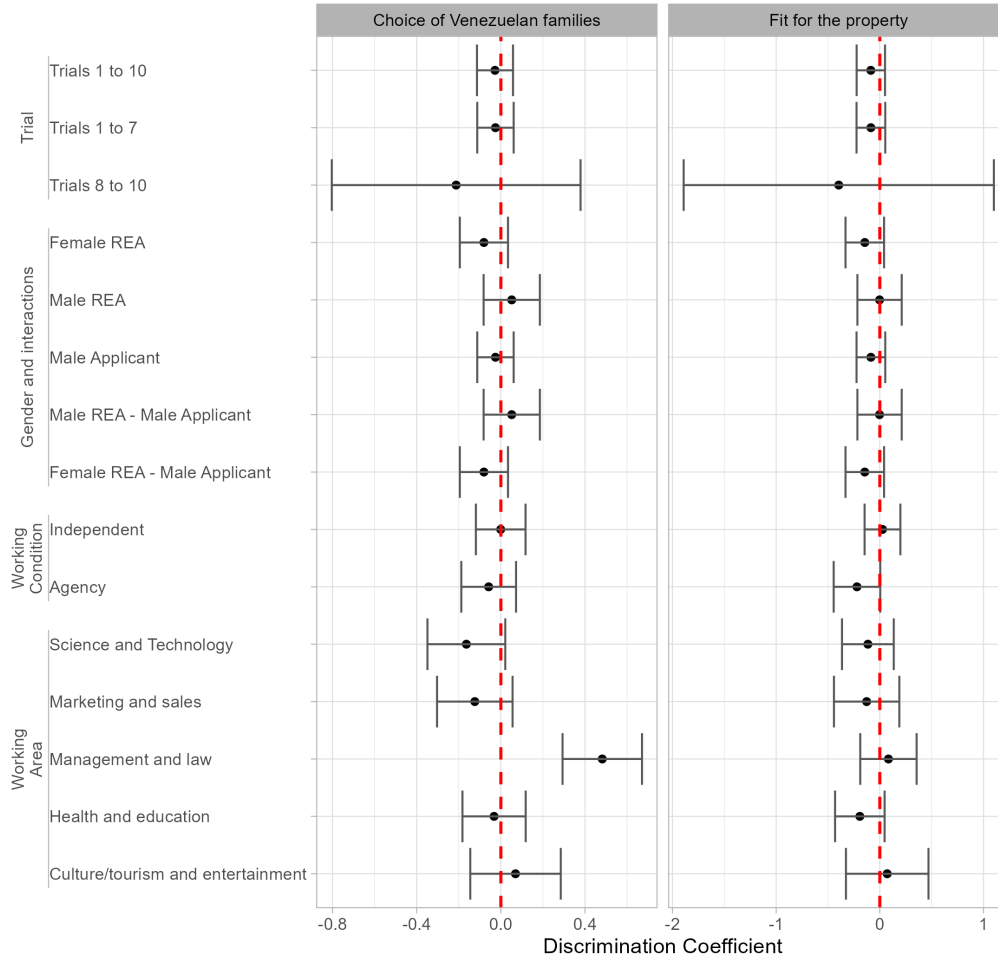
## 6 Conclusion

Housing markets play a crucial role in migrants’ integration, reflecting broader socio-economic dynamics beyond just wealth accumulation. Our study delves into the discrimination faced by Venezuelan migrants in Ecuador’s rental market. Through a detailed field experiment, we found that REAs tend to favor Ecuadorian families over Venezuelan ones in both selection and perceived property suitability.

However, our research also introduced a behavioral nudge, specifically designed to counteract these biases. This intervention successfully influenced REAs, leading to a higher selection rate of Venezuelan families and reducing the suitability assessment gap between the two groups. While we also examined discrimination against other demographics, such as families led by women and the LGBTQ+ community, the nudge was most effective for Venezuelan migrants. This suggests that biases in the rental market vary based on different attributes, and interventions need to be tailored accordingly.

The challenges Venezuelan migrants face in housing markets, especially in Latin American countries where 6.5 millions reside, are significant. These biases represent broader obstacles in their journey toward integration and stability. Yet, our nudge intervention offers an actionable policy tool. By addressing these biases, we can create a more inclusive housing market for migrants where decisions are based on merit, not prejudice. If such behavioral strategies are expanded and adapted, they can greatly improve migrant integration, allowing them to establish themselves and contribute to their new communities.

Figure 9: Discrimination Coefficients for the Placebo Group



*Notes:* This figure presents estimates of discrimination coefficients for the Placebo group, accompanied by 90% confidence intervals. Dimensions analyzed include city, REA gender, applicant gender and their interactions, applicant occupation, and REA employment status.

Our research contributes to the discourse on discrimination in developing nations. It not only sheds light on housing market biases against migrants but also provides empirical insights for policy-making. The success of our nudge emphasizes the potential of behavioral economics in devising strategies that promote fairness and equity in housing decisions. While the housing market might operate grounded on biases, they are not insurmountable. With well-researched and tailored interventions, we can pave the way for a more equitable housing market for migrants.

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## 7 Appendix

Table A1: Applicants Characteristics

Variable	(1) Ecuadorians	(2) Migrants	(3) Difference (1) - (2)
<b>Demographics</b>			
Candidates Age (years)	35.693 (3.724)	35.526 (4.007)	-0.167 (0.165)
Candidates Gender (Female == 1)	0.336 (0.473)	0.336 (0.473)	0 (0.02)
Candidates Laboral Experience (Years)	2.431 (0.938)	2.412 (0.929)	-0.019 (0.04)
How higher is the candidate's income in relation to rent?	3.021 (0.443)	3.026 (0.44)	0.006 (0.019)
Does the candidate has extra documentation?	0.498 (0.5)	0.498 (0.5)	0 (0.021)
Does the candidate has a college degree? (Yes == 1)	0.455 (0.498)	0.455 (0.498)	0 (0.021)
Candidate is single or married/with partner? (Yes == 1)	0.831 (0.375)	0.831 (0.375)	0 (0.016)
Does candidate have kids? (Yes == 1)	0.5 (0.5)	0.5 (0.5)	0 (0.021)
<b>Job of the applicant:</b>			
Working Area: Culture/Tourism and Entertainment	0.15 (0.358)	0.15 (0.358)	0 (0.015)
Working Area: Health and Education	0.301 (0.459)	0.301 (0.459)	0 (0.02)
Working Area: Management and Law	0.092 (0.29)	0.092 (0.29)	0 (0.012)
Working Area: Marketing and Sales	0.267 (0.443)	0.267 (0.443)	0 (0.019)
Working Area: Science and Technology	0.189 (0.391)	0.189 (0.391)	0 (0.017)
Observations	1103	1103	2206

*Note:* Stars indicate the statistical significance of differences in means across groups at various significance levels: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ . Columns (1) and (2) display the attributes of applicants based on whether they identify as part of the LGBTQ+ community.

Table A2: Ecuadorian vs. Migrant population Characteristics

Variable	(1) Ecuadorian (1)	(2) Venezuelan (2)	(3) Colombian (3)	(4) Difference (1)-(2)	(5) Difference (1)-(3)	(6) Difference (2)-(3)
Age (years)	38.524 (13.575)	33.387 (10.279)	41.091 (12.368)	-5.137*** (0.246)	2.566*** (0.379)	-7.704*** (0.338)
Gender (Female=1)	0.525 (0.499)	0.539 (0.499)	0.547 (0.498)	0.013 (0.009)	0.022 (0.014)	-0.009 (0.015)
Years of education (years)	11.241 (4.360)	13.370 (3.165)	10.464 (4.674)	2.128*** (0.079)	-0.777*** (0.122)	2.906*** (0.113)
Employment <sup>(a)</sup>	0.713 (0.452)	0.760 (0.427)	0.729 (0.445)	0.047*** (0.008)	0.015 (0.013)	0.032** (0.013)
Adequate employment <sup>(b)</sup>	0.387 (0.487)	0.384 (0.486)	0.370 (0.483)	-0.003 (0.010)	-0.017 (0.016)	0.014 (0.017)
Informal job <sup>(c)</sup>	0.503 (0.500)	0.478 (0.500)	0.509 (0.500)	-0.025** (0.011)	0.006 (0.017)	-0.031* (0.019)
Income (US Dollars)	450.426 (386.313)	404.407 (283.388)	423.889 (341.987)	-46.019*** (8.441)	-26.537** (13.442)	-19.482* (11.400)
Observations	10,033,514	145,168	60,691			

*Note:* The data come from the Household Survey of Ecuador (ENEMDU) of 2022. The table compares the characteristics of the Venezuelan and Colombian migrant population with Ecuadorians' characteristics; therefore, units of analysis are people aged between 18 - 65 years old. "Observations" row shows the estimated population for each group, which is calculated considering the expansion factor of the survey. (a) Binary variable that takes values equal to 1 if the person is employed. The category of employment considered includes either formal or informal jobs. (b) A binary variable that takes values equal to 1 if the person is employed in an adequate job. According to the survey, adequate employment is defined as jobs where employees work 40 hours or more weekly and receive an income equal to the minimum salary (by 2022 of USD 450) or superior. (c) A binary variable that takes values equal to 1 if the person is employed in an informal job. The survey defines informality as jobs not registered in the Single Taxpayer Registry. Stars indicate the statistical significance of differences in means across groups at various significance levels: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .