Perceived Social Acceptance and Migrants' Financial Inclusion*

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Abstract

We study how migrants' perceptions of social acceptance in their host country shape their engagement with the financial system. Conducting a survey experiment with 2,115 Venezuelan migrants in Colombia, we find that 66% of participants underestimate the extent to which Colombians are open towards migrants. Providing accurate information about native attitudes corrects these misperceptions and increases migrants' willingness to engage with the financial system. Among individuals who initially underestimated Colombians' acceptance, the intervention increases willingness to visit a bank by 15%, open a digital wallet by 12%, and open a savings account by 18% relative to the control group mean. Effects are concentrated among individuals who have not directly experienced discrimination. Using an instrumental variable strategy, we show these effects are driven by belief updating, and find that corrections persist six months later. Our findings highlight that misperceptions about social acceptance can be an important driver of self-exclusion from the financial system among migrant populations.

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1 Introduction

Migration and forced displacement are pressing global challenges. A key policy concern is how to integrate migrants into host economies effectively (World Bank, 2023). A critical aspect of economic integration is the access to and the use of financial products, which remains particularly challenging for migrants. The case of Venezuelans in Colombia highlights the barriers migrants face in achieving financial inclusion. In 2022, 92.3% of Colombian adults owned at least one financial product, compared to just 27.6% of Venezuelan migrants in the country (SFC, 2022).[1] Various factors can hinder the financial inclusion of vulnerable migrants, including supply-side barriers (e.g., legal requirements, high transaction costs, and unsuitable products) and demand-side barriers (e.g., low trust, fear of rejection and discrimination, and low financial literacy), which can lead to self-exclusion from financial markets (Burgess and Pande, 2005; Dupas and Robinson, 2013; Lusardi and Mitchell, 2014). Migrants' perceptions may systematically underestimate acceptance by host communities and societal integration. Such misconceptions can shape migrants' decisions and behaviors, including their willingness to engage with formal financial systems.

In this paper, we ask whether Venezuelan migrants have accurate perceptions of Colombian public opinion towards them, and whether correcting any misperceptions can improve their engagement with the financial system facilitating their financial inclusion. To address these questions, we conduct a telephonic information provision experiment. First, we elicit migrants' beliefs about Colombian's acceptance of migrants. We proxy Colombians' attitudes toward migrant by their likelihood of supporting birthright citizenship for migrants' children. We find that Venezuelan migrants systematically underestimate the proportion of Colombians who support this policy. Second, using a randomized controlled trial, we show that providing accurate information about the true proportion of Colombians in favor of granting nationality effectively corrects migrants' misperceptions. Third, within the same experiment, we show that correcting these beliefs increases migrants' willingness to engage with the formal financial system, as reflected in their self-reported intentions to visit banks and adopt financial products such as digital wallets and savings accounts. This increased willingness to engage is primarily driven by individuals who initially underestimated Colombian's acceptance of migrants. However, we find no effects on more sophisticated financial services, such as loans and insurance.

Specifically, we conducted a phone survey with 2,115 Venezuelan migrants registered in Colombia's SISBEN system, a proxy means-testing tool used to allocate social benefits. The sample comprises individuals with regularized migration status, drawn from 13 major

[1] This is particularly problematic because Venezuelan migrants cannot rely on financial services from their country of origin. Venezuela's financial system is largely inaccessible to migrants due to severe limitations in cross-border connectivity, institutional instability, and the effects of prolonged hyperinflation, which has eroded the value of local currency and rendered savings and transactions impractical.

cities across Colombia. To assess their baseline beliefs about Colombian's public opinion towards migrants, we asked our subjects: *"Out of 10 adult Colombian citizens, how many do you think agree to provide nationality to the children of migrants born in Colombia?"* According to the 2021 Venezuela Migration Project Observatory Perception Survey, 8 out of 10 Colombians support this policy (Observatorio del Proyecto Migración Venezuela, 2023). However, participants in our sample estimated, on average, that only 6 out of 10 Colombians agreed. Notably, 66% of participants underestimated the actual value.

We then conducted an experiment during the same telephone call, in which participants were randomly assigned to either the treatment or the control group. Both groups received an infographic via WhatsApp in real time, that is, while the enumerator was on the phone with the participant. The treatment infographic included two pieces of information: a reminder of the individual's own response to the question about the proportion of Colombians who support granting citizenship to migrants' children, and the true value of 8 out of 10 Colombians, based on the 2021 Venezuela Migration Project Observatory Perception Survey. The control group received a similar infographic but only saw their own response, without the true value. The enumerator then discussed the infographic with the respondents to ensure they understood the information. At the end of the telephonic survey, we asked respondents a second time about their beliefs regarding Colombians' public opinion and gathered information on our outcomes. Specifically, we asked if, in the following two months, they planned to request a formal financial product, such as a savings account or a loan.

Our main findings can be summarized as follows. First, the treatment effectively corrects participants' beliefs. On average, participants in the treatment group reduced their belief error by 23% relative to the true value, with a learning rate of 0.5, consistent with findings from similar experiments in the literature (Haaland et al., 2023). Second, our reduced-form results show that the treatment increases participants' willingness to engage with the financial system. For individuals who initially underestimated Colombians' openness towards migrants, providing accurate information increases their willingness to visit a bank and request financial information within the next two months by 15% compared to the control group. These individuals also exhibit a 12% increase in willingness to open a digital wallet and an 18% increase in their willingness to open a savings account. Among participants who initially underestimated societal acceptance towards migrants, the treatment significantly increases a standardized index of financial inclusion by 0.11 standard deviations. In contrast, we observe no effects for participants who overestimated societal acceptance towards migrants.

Third, we explore heterogeneous treatment effects across demographic and behavioral subgroups. Among pessimistic individuals, treatment effects are stronger for men, those with lower income and education levels, and older participants. Effects are also concentrated among individuals who plan to stay in Colombia for less than 11 years, those who have not

directly experienced discrimination in Colombia, and those who do not yet hold a savings account. These patterns are consistent with the idea that the treatment is most effective for individuals who are more likely to self-exclude from the financial system due to pessimistic beliefs about societal acceptance, and for whom a belief correction can meaningfully shift their willingness to engage with formal financial institutions.

Fourth, we use the treatment as an instrument for belief updating to compute behavioral elasticities. We find that shifting the belief about Colombian's acceptance of migrants from 0 to 8 leads to a 0.33 standard-deviation increase in our standardized index of Financial Inclusion. This effect is driven by increased reported intentions to visit a bank, request financial information, and adopt savings accounts and digital wallets. Impacts are concentrated among participants who initially underestimated Colombians' acceptance towards migrants, with no significant effects observed among those who overestimated the true level of support.

Fifth, we conducted a follow-up survey six months after the intervention to assess the persistence of belief updating and its impact on financial behaviors. We find that while the effects on beliefs attenuate over time, they remain significant, particularly among participants who were initially more pessimistic. Additionally, although we are underpowered to detect statistically significant behavioral effects for most outcomes at follow-up, the patterns remain consistent with the baseline results. In particular, participants in the treatment group were more likely to report taking steps toward opening a savings account, an effect that is marginally significant when accounting for the reduced sample size through permutation inference.

There are three main channels that can explain our results. First, feelings of unwelcomeness—arising from either direct experiences of discrimination or broader perceptions of societal rejection—can undermine migrants' confidence in their ability to access social and economic opportunities, fostering a "why bother trying" attitude. By receiving accurate information about Colombians' support to granting citizenship to their children, migrants may have felt more accepted in the host country, thereby fostering a desire to engage with Colombian financial institutions. Consistent with this channel, we find that treatment effects are concentrated among individuals who have not directly experienced discrimination in Colombia, suggesting that direct experiences of discrimination create barriers to engagement that belief corrections alone cannot easily overcome.

Second, the intervention might have strengthened migrants' trust in financial institutions. Perceptions of belonging in society are frequently linked to trust in formal systems, and the information shared could have conveyed a message that formal financial institutions are committed to fostering migrant inclusion. Supporting this channel, effects are strongest among individuals who do not yet hold a savings account, consistent with the idea that the treatment encouraged first steps toward engaging with the formal financial system.

Finally, correcting misperceptions of social acceptance may have prompted migrants to envision a longer-term presence in Colombia, motivating them to plan accordingly and interact more actively with the formal financial system. In line with this interpretation, we find stronger treatment effects among individuals who are uncertain about how long they plan to stay in Colombia, suggesting that belief corrections may tip the balance toward greater integration for those whose commitment to staying has not yet been fully determined.

This study has several strengths compared to traditional survey experiments in the belief updating literature. First, the experiment was conducted via a telephone survey, with enumerators ensuring that participants were attentive and engaged, thereby addressing typical attention problems in web-based survey experiments. Second, using WhatsApp to deliver the treatment minimizes the effects of experimenter demand by reducing direct interaction between participants and enumerators, ensuring a less biased elicitation of responses. Third, the experimental design maintains salience across treatment and control groups by providing visually engaging infographics to both groups, enhancing comparability while isolating the treatment's effect. Fourth, the approach is highly cost-effective, leveraging digital communication to implement a large-scale intervention at minimal expense.

This paper contributes to several strands of the literature. First, we add to the growing body of research on how norms—and beliefs about which norms are dominant—affect people's attitudes and choices. The closest work to ours is Field et al. (2021), which examines the role of gender norms in women's use of bank accounts. Unlike most existing studies that focus on correcting the beliefs of key gatekeepers (e.g., husbands reluctant to have their wives employed, as in Bursztyn et al., 2020, or natives limiting opportunities for migrants, as in Alesina et al., 2023), our study explores how changing the beliefs of discriminated populations themselves can positively influence their socio-economic outcomes.

Second, we contribute to the literature on financial inclusion, which explores how to mitigate barriers to financial access for vulnerable populations, including migrants. Prior research primarily focuses on supply-side barriers, such as limited physical access to banks and inadequate infrastructure (Burgess and Pande, 2005; Dupas et al., 2018). Financial products often include features—such as minimum balance requirements, collateral, or overdraft fees—that fail to accommodate individuals with low or irregular incomes, like migrants (Prina, 2015; Cole et al., 2014). Providers, perceiving small and irregular transactions as unprofitable, are hesitant to serve these individuals (Barboni et al., 2022). Efforts to address these supply-side barriers include offering savings accounts (Dupas and Robinson, 2013; Prina, 2015); loans, particularly through microfinance (Banerjee et al., 2015; Barboni et al., 2022); microinsurance (Cole et al., 2014); and financial literacy training (Khan et al., 2022). On the demand side, the literature has documented multiple barriers such as present bias, self-control, and avoidance of social pressures (Thaler and Benartzi, 2004; Baland et al., 2011; Karlan, 2014); fears of rejection by financial institutions; low trust in these institutions;

and limited financial literacy (Lusardi and Mitchell, 2014). Our results indicate that self-exclusion, driven by misperceptions about the natives' acceptance of migrants and potential discrimination, are important drivers of migrants' low engagement with the financial system.

Third, this paper contributes to the literature on migrant integration and the broader implications of Venezuelan migration. The massive influx of migrants has reshaped various aspects of Colombian society, influencing the labor market (Bahar et al., 2021; Bonilla-Mejía et al., 2024), political dynamics (Rozo and Vargas, 2021; Rozo et al., 2023), and public health (Ibáñez et al., 2021). The regularization program implemented in 2016 has played an important role in facilitating integration, yielding significant benefits for migrants, as documented by (Ibáñez et al., 2024). Our study adds to this body of work by highlighting the low levels of financial inclusion among Venezuelan migrants—an essential dimension of economic integration—and showing how self-exclusion can stem from negative perceptions of migrant acceptance. Importantly, migrant acceptance is not static (Chatruc and Rozo, 2024), and well-designed policy interventions can foster greater acceptance. This, in turn, can create a virtuous cycle of inclusion by shifting migrants' perceptions, with important implications for integration, as documented in this paper.

Finally, this paper makes a methodological contribution to the literature on information provision experiments (see Haaland et al., 2023 and Stantcheva, 2023 for a review). We implement a cost-effective telephonic survey experiment that exogenously varies a single piece of information provided to respondents. By tracking pre-treatment and post-treatment beliefs, we assess the impact of information provision on migrants' perceptions of discrimination.

2 Experimental Setting and Design

2.1 Context

The economic collapse in Venezuela has been profound. Between 2013 and 2022, the country's GDP per capita declined by 71.5%, marking one of the most severe economic contractions in recent history. These dire economic conditions triggered mass migration, with millions of Venezuelans leaving the country since 2013. By January 2024, Colombian authorities estimated that approximately 2.8 million Venezuelan migrants were residing in Colombia, accounting for about 5% of the country's population.

In response, the Colombian government implemented multiple policies to regularize the migratory status of Venezuelans. In 2017, the government introduced the Special Stay Permit (*Permiso por Protección Temporal*, PPT) program, which provides regular migratory status and work permits. This permit grants migrants legal access to private services, including financial and digital services, as well as social programs such as subsidized healthcare, public

education, and cash transfers. While these regularization programs have positively affected migrants' well-being, challenges to achieving full integration remain (Ibáñez et al., 2024).

Despite legal regularization efforts, Venezuelan migrants in Colombia continue to face significant barriers to full economic and social integration. Recent data from the *Encuesta Pulso de Migración* by DANE (2023) highlight persistent gaps in access to key services. Only 66.1% of Venezuelan migrants are affiliated with the healthcare system, a significantly lower rate than the general Colombian population, which has nearly universal coverage. Additionally, nearly 30% of migrant households with children report at least one child not attending school, despite a 90%+ coverage rate for Colombian children, depending on the region. Labor market integration also remains limited, with only 58.4% of Venezuelan migrants participating in the workforce. Furthermore, Venezuelan migrant women face a disproportionate burden of household responsibilities, with 39.5% primarily engaged in unpaid domestic work. These statistics suggest that even with formal access granted through legal status, full participation in essential services remains a challenge.

One potential explanation for these disparities lies in the discrimination and social barriers that migrants face. Public perceptions often associate Venezuelan migrants with crime, poverty, and low labor market participation, reinforcing exclusionary attitudes and limiting access to opportunities (Taborda Burgos et al., 2021). Media narratives further stigmatize migration, amplifying fears and biases that translate into everyday discrimination (Namen et al., 2021). Ultimately, these factors may lead to self-exclusion, where migrants, anticipating discrimination or difficulties, choose not to engage fully with available services and opportunities.

In particular, we focus on integration into the financial system. In 2022, financial inclusion rates in Colombia showed a stark contrast between the native population and Venezuelan migrants. While 92.3% of Colombian adults owned at least one financial product, only 27.6% of Venezuelan migrants had access to formal financial services (SFC, 2022). This disparity is particularly relevant given the structural barriers that hinder financial integration for migrants. The financial systems of Colombia and Venezuela have limited interoperability, making it virtually impossible for migrants to use Venezuelan financial services in Colombia. Additionally, Venezuela's prolonged hyperinflation has rendered savings in its local currency nonviable, further necessitating financial inclusion in Colombia.

Access to financial services in Colombia is crucial for multiple reasons. First, financial inclusion facilitates economic stability by enabling individuals to save, make secure transactions, and access credit for entrepreneurship. Second, given that many migrants lack a stable income stream, having a formal financial account can help smooth consumption and provide a buffer against economic shocks. Third, financial access is essential for participation in social programs; for low-income populations, being banked is often a prerequisite for receiving government welfare payments and subsidies.

2.2 Experimental Design

Our sample consists of Venezuelan migrants residing in Colombia who meet the following inclusion criteria: (i) they are over 18 years old, (ii) hold regular migratory status, and (iii) are registered in SISBEN, Colombia's proxy means-testing system for social benefits. Between April and May 2024, we conducted an initial screening survey via WhatsApp.[2] From this pool, we drew a sample of 2,214 individuals across 13 metropolitan areas in Colombia. After excluding 99 observations due to missing information on key variables, the final study sample consisted of 2,115 individuals. Participants were randomly assigned to treatment and control groups in equal proportions, with randomization stratified by gender, ownership of at least one financial product, age, and prior experiences of discrimination.

The telephonic survey began with the enumerator validating the participants' identity and collecting basic demographic information. The enumerator then elicited participants' *baseline beliefs* about the extent to which Colombians welcome migrants. Specifically, participants were asked to provide a second-order quantitative belief by answering the question: *"Out of 10 adult Colombian citizens, how many do you think agree to provide nationality to the children of migrants born in Colombia?"* This question serves as a relevant measure of Colombians' attitudes toward migrants and, more broadly, of hosting countries' views on migrant populations, for two reasons. First, citizenship encompasses a range of additional rights that migrants would be entitled to. While obtaining a permit that may lead to citizenship is possible, it remains non-automatic and discretionary. Second, the issue of citizenship is sensitive in Colombia, one of only two Latin American countries where nationality is not automatically granted by birth on its territory.[3]

Next, we provided participants in the treatment group with an infographic via WhatsApp containing two pieces of information. First, the infographic reminded them of their own response to the question about what proportion of Colombians favor granting citizenship to Venezuelan migrants' children. Second, it provided the actual proportion—8 out of 10 Colombians—based on data from the 2021 Venezuela Migration Project Observatory Perception Surveys, who believe Venezuelans should be granted citizenship (see Figure A1 for an example). During the information provision stage, the enumerator asked questions to ensure that the respondents understood both the questions and the infographic. To address salience concerns, we also provided participants in the control group with an infographic. However, in this case, the infographic only reported their response to the question without including the true proportion.

After the information treatment, the enumerator measured participants' beliefs again by

[2]The initial screening reached 5,999 respondents, representing approximately 226,000 migrants with PPT status in SISBEN. To achieve this sample size, around 51,000 surveys were sent.

[3]For details see: `https://worldpopulationreview.com/country-rankings/countries-with-birthright-citizenship`.

asking the same question about Colombians' attitudes toward granting citizenship to the children of Venezuelan migrants. This constitutes our measure of *posterior beliefs* about Colombian's acceptance of migrants. Next, to obtain our main outcomes, we assessed participants' future plans to engage with the financial system. First, we asked if, in the following two months, they planned to request information about financial products or visit a bank branch or office. Second, we asked if, in the following two months, they intended to request a savings account or a digital wallet, which require minimal approval processes. Finally, we asked if they planned to apply for more complex financial products involving stricter approval processes from financial institutions, such as credit products (credit cards, consumption loans, or business loans) or insurance products. To summarize results across outcomes, we combined them into a financial inclusion index using the methodology proposed by Kling et al. (2007).[4]

Table 1 presents descriptive statistics of the sample at baseline. Sixty percent of participants are female, and the average age is 41 years. Consistent with previous studies, financial access in our population is low: only 33% have a digital wallet, 37% have a savings account, and 1% have a loan. The table also provides evidence that these individual characteristics are balanced across the treatment and control groups, confirming that the randomization process was successful.

Finally, we attempted to recontact participants for a short follow-up survey six months after the initial information experiment to track changes in financial engagement and perceptions over time. All 2,114 migrants from the original sample were invited to complete the survey via WhatsApp, maintaining the same digital approach used in the initial study. The follow-up survey reassessed participants' beliefs to evaluate the persistence of belief updating and asked whether they had taken any steps toward obtaining financial products.

From the original 2,114 participants, we successfully completed follow-up surveys with 1,214 individuals. While we find no evidence of differential attrition across treatment arms, the reduced sample size likely limits our statistical power to detect treatment effects with precision.

3 Results

3.1 Learning Rates

We start by describing migrants' beliefs about societal acceptance of migrants in Colombia. Participants were asked, both before (baseline belief) and after providing the infographic

[4]The financial inclusion index in the main text tables includes visit bank, request information, digital wallet, and savings account. The financial inclusion index reported in the appendix tables additionally includes credit cards, private loans, business loans, and insurance.

(posterior belief), to state the number of Colombians out of 10 they believe are in support of granting nationality to the children of migrants. We refer to individuals with a baseline belief below 8 as *pessimistic* and equal to or above 8 as *optimistic*. We find that 66% of participants are pessimistic. On average, participants estimate that 6 out of 10 Colombians agree to grant citizenship to the children of Venezuelan migrants, with a median guess of 6. Furthermore, the baseline belief underestimates the true value by 25%. Figure 1 shows the baseline and posterior belief across treatment and control groups. The figure highlights a clear shift in beliefs among treated participants, who move closer to the actual value compared to the control group.

We document belief correction descriptively in Figure 2. To do so, we define the *belief error* as the difference between the true value of eight and the value respondents report. We define these errors for both baseline and posterior beliefs. For example, individuals who state a number below 8, the first time they are asked about Colombian's acceptance of migrants, will have a negative *baseline* belief error. The y-axis of each dot in Panel (a) of Figure 2 shows the average *posterior* belief error among individuals whose *baseline* belief error is given by the value on the x-axis. The blue dots correspond to the treatment group, and the pink dots to the control group. Solid lines represent a linear fit, while the dashed line represents the 45-degree line. Individuals who correct their belief are those whose x value is different from zero and whose y value is equal to zero. The fact that the treatment line (in blue) is above the control line (in pink) indicates that, conditional on the baseline belief error, participants in the treatment group adjusted their beliefs toward the true value more that those in the control. Panel (b) illustrates the belief error at the end of the intervention as a proportion of the true value, disaggregated by treatment arm and the sign of the baseline error, which is negative for pessimists and positive for optimists. We observe significant adjustments in beliefs in both directions. Importantly, participants in the control group who are pessimistic (those with a negative baseline belief error) have a posterior belief error 33.33% smaller than the true value of eight. In contrast, this number is just 5.79% smaller in the treatment group, indicating that pessimistic individuals in the treatment group substantially corrected their belief error.

To provide further evidence that respondents updated their beliefs in response to the randomly assigned information treatment, we estimate different regression specifications. We define E_i^{pre} as the baseline belief error of individual i, expressed as a fraction of the true value of eight. Similarly, we define E_i^{post} as the posterior belief error expressed as a fraction of the true value. We then estimate the following regression specification:

$$\left|E_i^{\text{post}}\right| = \gamma + \beta Treated_i + X_i'\Gamma + \varepsilon_i, \tag{1}$$

where $Treated_i$ is an indicator variable equal to 1 for participants in the treatment group

and 0 otherwise. X_i' is a vector of strata fixed effects and baseline covariates selected through a Lasso procedure, following Belloni et al. (2014). Finally, ε_i denotes the error term, and we compute heteroskedasticity-robust standard errors. The parameter of interest is β, which captures the extent to which the information treatment reduces the magnitude of the belief error as a proportion of the true value, relative to the control group.

We also compute learning rates following the methodology of Haaland et al. (2023). To do so, we estimate the following regression specification:

$$Updating_i = \delta_0 + \delta_1 E_i^{\text{pre}} + \delta_2 Treated_i + \delta_3 Treated_i \cdot E_i^{\text{pre}} + v_i, \tag{2}$$

where $Updating_i$ represents belief updating, defined as the change from baseline beliefs to posterior beliefs, and is equal to the answer to the second question minus the answer to the first one.[5] The parameter of interest is δ_3, which captures the learning rate for the treated group, representing how strongly participants incorporate the signal relative to their initial beliefs.[6] This interpretation facilitates comparisons with other information experiments and provides insights into how participants process new information.

In Table 2, we present regression results for belief correction and learning rates. Columns (1) to (4) report the effects of the information treatment on the absolute value of belief errors, corresponding to estimation results of different versions of equation (1). Column (1) shows that the treatment reduces the absolute value of the belief error as a fraction of the true value by 23 percentage points. This is a substantial effect, considering that the mean of the outcome in the control group is 31%. This result is robust to the inclusion of strata fixed effects (column 2) and Lasso-selected covariates (column 3). Column (4) estimates heterogeneous treatment effects across pessimistic and optimistic individuals by including an interaction term between $Treated_i$ and $Optimistic_i$, where $Optimistic_i$ is a dummy variable equal to 1 for optimistic individuals.[7] Column (4) shows that while both groups significantly reduce their errors, pessimistic individuals exhibit a larger reduction (28 percentage points) compared to optimistic participants (12 percentage points). These results provide robust evidence that the treatment effectively adjusted participants' reported beliefs, with a larger effect observed among pessimistic participants.

Columns (5) and (6) present results on learning rates estimated using equation 2, both

[5] If A_i^{pre} denotes the answer to the first question and A_i^{post} the answer to the second question, with A_i^{pre} and $A_i^{\text{post}} \in \{0, \ldots, 10\}$, then $Updating_i = A_i^{\text{post}} - A_i^{\text{pre}}$. Additionally, under this notation, $E_i^{\text{pre}} = A_i^{\text{pre}}/8$ and $E_i^{\text{post}} = A_i^{\text{post}}/8$. Pessimistic individuals are those for whom $A_i^{\text{pre}} < 8$, and optimistic individuals are those for whom $A_i^{\text{pre}} \geq 8$.

[6] In Bayesian updating models with normally distributed priors and signals, and assuming a quadratic loss function, δ_3 represents the weight participants assign to the signal in updating their beliefs. The complement, $1 - \delta_3$, reflects the weight placed on their prior beliefs; see Cavallo et al. (2017) and Cullen and Perez-Truglia (2022) for details.

[7] In column (4), the treatment effect for pessimistic individuals is given by the coefficient of $Treated_i$, while that for optimistic individuals is given by the sum of the coefficients of $Treated_i$ and $Treated_i \times Optimistic_i$.

with and without strata fixed effects. We calculate a learning rate coefficient (δ_3) of 0.5. Under the structural assumptions mentioned above, this indicates that participants assign equal weight to the signal and their prior beliefs when updating. This learning rate aligns with findings in the literature on information provision experiments. For example, Haaland et al. (2023) report learning rates ranging from 0.30 to 0.70 across various contexts, suggesting that our results are consistent with observed patterns of belief updating in response to information shocks.

3.2 Reduced Form Effects on Financial Inclusion

We now estimate reduced-form effects of the information treatment on financial inclusion outcomes. To do so, we estimate equation (1), replacing the dependent variable with our financial inclusion outcomes. Specifically, we estimate the treatment effects on the KLK Financial Inclusion Index and its individual components. In addition, we explore heterogeneous effects by baseline beliefs, distinguishing between optimistic and pessimistic participants. Table 3 presents the results. Panel A reports average treatment effects, showing that the treatment increases the Financial Inclusion Index by 0.07 standard deviations (s.d.). When examining the individual components of the index, we find that the treatment has significant positive effects on participants' likelihood of visiting a bank and requesting information (columns 2 and 3), increasing these outcomes by 4 and 5 percentage points (p.p.), respectively. We find no significant effects on the other components of the index. These results, however, conceal important heterogeneous effects, which we explore next.

Panel B presents estimates of heterogeneous treatment effects by baseline beliefs, distinguishing between optimistic and pessimistic participants. Consistent with earlier findings, the treatment primarily affects pessimistic participants, significantly increasing the Financial Inclusion Index by 0.11 standard deviations (s.d.). Additionally, we observe positive effects on plans to visit a bank (5 p.p.), request information (5 p.p.), open a digital wallet (4 p.p.), and open a savings account (6 p.p.). Furthermore, no significant effects are observed for optimistic participants, suggesting that the treatment is more effective for individuals who initially underestimated Colombians' willingness to grant citizenship to migrants' children. Results for more sophisticated financial products, such as loans and insurance, are presented in Appendix Table A1.

Correcting these beliefs encourages them to reassess their options, reducing self-exclusion and increasing their willingness to visit banks, seek information, and acquire financial products such as savings accounts and digital wallets. While the treatment encourages engagement with simpler financial products, we find no corresponding increase in willingness to apply for loans or insurance, consistent with the idea that supply-side barriers remain an important obstacle even when demand-side beliefs are corrected. Results for these outcomes

are presented in Appendix Table A1.

To provide evidence on the underlying mechanism of the treatment's impact, we estimate heterogeneous effects of the information treatment on the KLK Financial Inclusion Index across key demographic and behavioral dimensions. These effects are estimated using sample splits for each subgroup, defined by gender, prior loan usage in Venezuela, income, education, and age. Table 4 presents these results. Panel A reports results for the full sample, Panel B focuses on pessimistic respondents, and Panel C examines optimistic respondents. The results in Panel A indicate two clear sources of heterogeneity. First, we estimate a statistically significant treatment effect of a 0.18 standard deviation (s.d.) increase in the KLK Financial Inclusion Index among lower-income individuals, compared to a null effect of 0.03 s.d. for higher-income individuals. Second, effects are stronger among those without prior loan experience in Venezuela (0.08 s.d.) compared to those who did take out loans (0.06 s.d., not significant).

For pessimistic respondents, whose results are reported in Panel B, treatment effects appear stronger among men, those with lower income and education levels, and older participants. Therefore, the effects of the treatment are stronger for pessimistic individuals with lower education, lower income, and those without prior loan experience in Venezuela. This result is consistent with the idea that these individuals are more likely to self-exclude from the financial system because they believe it is unlikely they will be approved for a financial product. When they receive the information treatment, they become more inclined to engage with the financial system. Finally, consistent with the fact that optimistic individuals show no significant treatment effects, we observe noisier heterogeneity estimates in their case (Panel C).

Table 5 explores additional sources of heterogeneity, examining treatment effects by envisioned length of stay in Colombia, prior experience of discrimination, and whether participants already hold a savings account. In Panel A, we find that the treatment effect is concentrated among individuals who plan to stay less than 11 years (0.12 s.d., significant at 10%) and those who are not sure about how long they will stay (0.10 s.d., significant at 10%), while we find no significant effect for those who plan to stay indefinitely. Regarding discrimination, column (5) shows a significant increase of 0.11 s.d. in the KLK Financial Inclusion Index for individuals who had not experienced discrimination. In contrast, those who had directly experienced discrimination exhibit a non-significant effect of 0.02 s.d. Finally, the treatment effects are concentrated among individuals who do not yet hold a savings account (0.12 s.d.), with no significant effect among those who already have one (0.01 s.d.), consistent with the idea that the treatment encourages first steps toward financial inclusion.

For pessimistic respondents in Panel B, these patterns are stronger. The treatment effect is particularly large among those planning to stay less than 11 years (0.25 s.d.), those who have

not experienced discrimination (0.16 s.d.), and those without a savings account (0.15 s.d.). The finding that the treatment only affects pessimistic individuals with no prior experiences of discrimination suggests that direct experiences of discrimination can lead to self-exclusion that cannot be easily overcome through belief corrections alone. Consistent with the absence of significant average treatment effects, Panel C shows no significant heterogeneous effects among optimistic respondents.

3.3 Instrumental Variables: Behavioral Elasticities

We now estimate the causal effect of beliefs about acceptance of migrants on migrants' intended financial decisions using an instrumental variable approach implemented with a Two-Stage Least Squares (2SLS) specification. In the first stage, we estimate:

$$B_i^{\text{post}} = \beta_0 + \beta_1 E_i^{\text{pre}} + \beta_2 Treated_i + \beta_3 Treated_i \cdot E_i^{\text{pre}} + X_i'\Gamma + u_i,$$

where B_i^{post} is the normalized *posterior* belief of individual i.[8] E_i^{pre} is the normalized estimation error defined above. $Treated_i$ is the treatment indicator, and X_i' includes strata fixed effects and Lasso-selected baseline covariates. In the second stage, we estimate:

$$Y_i^{\text{post}} = \theta_0 + \theta_1 E_i^{\text{pre}} + \theta_2 \widehat{B}_i^{\text{post}} + X_i'\Gamma + e_i,$$

where Y_i^{post} denotes a financial inclusion outcome measured at the end of the intervention, and $\widehat{B}_i^{\text{post}}$ is the *predicted normalized posterior* belief obtained from the first stage. Both E_i^{pre} and B_i^{post} are normalized by dividing by the true value (equal to 8), which ensures comparability and implies that the coefficients θ_1 and θ_2 can be interpreted as elasticities relative to the true value of our proxy of native's acceptance of migrants. The parameter of interest, θ_2, captures the behavioral elasticity of intended financial decisions to beliefs about native's acceptance of migrants. Also, and given the normalization of B_i^{post}, θ_2 captures the causal effect of updating beliefs from 0 to 8.

Table 6 presents instrumental variable (IV) estimates of the effect of beliefs about native's acceptance of migrants on financial inclusion outcomes, captured by the coefficient of Perception After Treatment. Panel A reports results for the full sample, while Panels B and C explore heterogeneity based on baseline beliefs (pessimistic vs. optimistic respondents). In all panels, we report the F-statistic. In the entire sample, the first-stage F-statistic is 216.44, indicating strong predictive power of the instrument. In other words, the treatment has a substantial effect on posterior beliefs. Panel A shows that, for the whole sample, an increase in beliefs about social acceptance significantly increases the Financial Inclusion Index by 0.33 standard deviations (s.d.). This effect is primarily driven by significant increases in plans to

[8] In the notation of footnote 5, $B_i^{\text{post}} = A_i^{\text{post}}/8$.

visit a bank (15 percentage points, p.p.), open a digital wallet (16 p.p.), and open a savings account (18 p.p.). Results for more complex financial products, such as loans and insurance, are presented in Appendix Table 6.

The results in Panels B and C show that, as expected, the treatment effects are concentrated among pessimistic respondents. For this group, correcting perceptions raises the Financial Inclusion Index by 0.34 standard deviations (s.d.), with significant effects observed for visiting a bank (17 p.p.), requesting financial information (14 p.p.), opening a digital wallet (15 p.p.), and opening a savings account (19 p.p.). In contrast, Panel C shows no significant effects among optimistic respondents across any of the outcomes, highlighting that the impact of perceptions on financial inclusion is primarily driven by those who initially underestimated Colombians' acceptance of migrants.

As noted by Haaland et al. (2023), a limitation of the IV approach is that the exclusion restriction may not hold if the information treatment affects multiple beliefs simultaneously or shifts attention and saliency to certain beliefs. To address this concern, in Table A4 of the appendix, we present alternative estimates of belief updating without relying on the IV approach. Specifically, we follow Bursztyn et al. (2020) by running an OLS regression of financial inclusion outcomes on the potential size of the belief update. The update measure is defined as the negative of the initial belief error for participants in the treatment group and set to 0 for those in the control group. These results are consistent with those obtained from the IV specification.

3.4 Follow-Up Survey

We now turn to the follow-up survey conducted six months after the initial intervention, which allows us to assess the persistence of belief updating and its impact on financial engagement.

Figure 3 presents the distribution of beliefs at baseline, after the intervention, and at follow-up. While belief corrections at follow-up are smaller than immediately post-treatment, we observe statistically significant persistence in the belief updating process, particularly among initially pessimistic individuals. Participants in the treatment group who underestimated societal acceptance at baseline continued to report beliefs closer to the true value than those in the control group. Specifically, the average belief error for pessimistic individuals in the treatment group remained 13.42 percentage points smaller than for those in the control group, a difference that remains statistically significant. Notably, at follow-up both pessimistic and optimistic individuals in both treatment and control groups exhibit negative belief errors, suggesting a general tendency to underestimate Colombians' acceptance of migrants even among those who initially overestimated it. Table 7 further corroborates these findings: the treatment effect on belief updating persists, with a learning rate of 0.57 at follow-up. This rate is higher than the initial learning rate observed immediately post-

treatment, suggesting that individuals who were initially more biased were more likely to incorporate new information over time.

Table 8 presents the reduced-form effects on migrants' financial engagement at follow-up. The table distinguishes between two measures of financial engagement: whether individuals actually opened a financial product (columns 1–3) and whether they took any action toward opening one (columns 4–6). While we are likely underpowered to detect statistically significant treatment effects, the signs of the estimated coefficients are generally consistent with those observed at baseline. In particular, when looking at actions toward financial products, treated individuals were 5 percentage points more likely to take steps toward opening a savings account, an effect that is marginally significant at the 10% level with a permutation p-value of 0.060. Effects on digital wallets and the financial inclusion indices are positive but not statistically significant. For the "opened" outcomes in columns 1–3, we find near-zero effects, suggesting that while treated individuals are more likely to initiate the process, translating intentions into completed products takes more time.

While these follow-up results do not provide conclusive evidence of financial behavior changes, they suggest that initial corrections in perceived social acceptance may have had persistent effects on migrants' willingness to engage with the financial system. However, given the reduced sample size at follow-up, our estimates lack precision, and detecting meaningful changes in financial uptake may require larger samples or longer observation periods.

4 Discussion

Feeling unwelcome—even without directly experiencing discrimination—can trigger a cascade of negative effects on well-being, labor force participation, and access to social and economic opportunities (Avery et al., 2008; Pascoe and Smart Richman, 2009). We hypothesized that such feelings of unwelcomeness reduce migrants' willingness to engage with the financial system. Providing Venezuelan migrants with accurate information about Colombians' support to granting citizenship to migrants' children may help instill a sense of acceptance, thereby positively influencing their demand for financial services and products in the host country. Overall, our results show that the information treatment effectively shifted individuals' second-order beliefs about integration prospects in Colombia. This shift, in turn, led to higher reported willingness to engage with the financial sector. Unsurprisingly, the effects are driven by migrants who initially held pessimistic beliefs about societal integration. The positive impacts on financial inclusion are primarily observed in initial engagement with the financial system and in simpler financial products, such as digital wallets and savings accounts, with no significant effects on debt or insurance products. The lack of effects on these more sophisticated products could be explained by migrants perceiving supply-side barriers as harder to overcome.

While we do not have direct evidence on the mechanisms driving these results, we propose three potential explanations. First, the treatment may have enhanced trust in financial institutions. Beliefs about societal integration are often tied to confidence in formal systems, and the information provided may have signaled that the host economy supports migrant inclusion. Second, the treatment could have lowered participants' expectations of experiencing discrimination when engaging with the financial system. By addressing pessimistic beliefs about societal acceptance, migrants may feel more confident in approaching banks and perceive a higher likelihood of having their applications for financial products accepted. Finally, correcting these beliefs may have encouraged migrants to envision staying in the host economy. Improved perceptions of societal acceptance might prompt participants to make longer-term plans, including deeper engagement with formal financial systems.

While our findings provide strong evidence of the treatment's effectiveness, two main limitations should be considered. First, the effects of experimenter demand are a common concern in this type of experiment. However, we believe the design of our study minimizes this risk. Specifically, the survey was conducted via phone, with no direct contact, and the infographic was delivered through WhatsApp, reducing interaction between participants and researchers. Additionally, as noted by De Quidt et al. (2018), evidence suggests that experimenter demand effects are less pronounced in online and remote surveys. To further address potential biases, we provided an infographic to the control group as well, ensuring consistent interaction across both treatment arms. Another advantage of our design, relative to traditional information provision experiments, is that our enumerators ensured that respondents were paying attention and understood the questions and the information provided. Second, it is important to consider the external validity of our findings. Although the issue of granting nationality to children of migrants is specific to the Colombian context, we believe it serves as a good proxy for social acceptance. Therefore, our results are likely to apply in contexts where migrants underestimate the true level of native's acceptance of migrants.

5 Conclusion

In this paper, we present the results of a telephonic survey experiment with 2,115 Venezuelan migrants in Colombia. We find that migrants systematically underestimate Colombians' acceptance of migrants, as measured by their support for granting citizenship to migrants' children. Correcting these misperceptions with an information treatment increases migrants' reported willingness to engage with the formal financial system, particularly through steps such as visiting banks or opening digital wallets and savings accounts. The effects are concentrated among individuals who initially underestimated the extent of native's acceptance of migrants, with no significant effects observed for those who overestimated

it. These findings highlight the importance of beliefs about social acceptance in shaping financial inclusion outcomes and suggest that simple belief corrections can mitigate barriers to economic participation and reduce self-exclusion from the financial system.

We end with potential avenues for future research. First, our results suggest the need for a deeper understanding of how migrants form and update beliefs about their integration prospects in host economies. Future work could investigate specific dimensions of these beliefs, such as perceived acceptance in different social or institutional domains, trust in public services, or expectations about discrimination. Understanding how these beliefs are shaped by factors such as media narratives, social networks, or direct interactions with host communities could provide valuable insights for designing targeted interventions. Second, while we find that integration prospects significantly influence financial inclusion, these beliefs are likely to affect other critical aspects of migrants' lives, including labor market participation, education, and engagement with social programs. Further research is needed to explore the broader implications of beliefs about integration prospects and to understand their role in facilitating or hindering migrants' overall social and economic integration.

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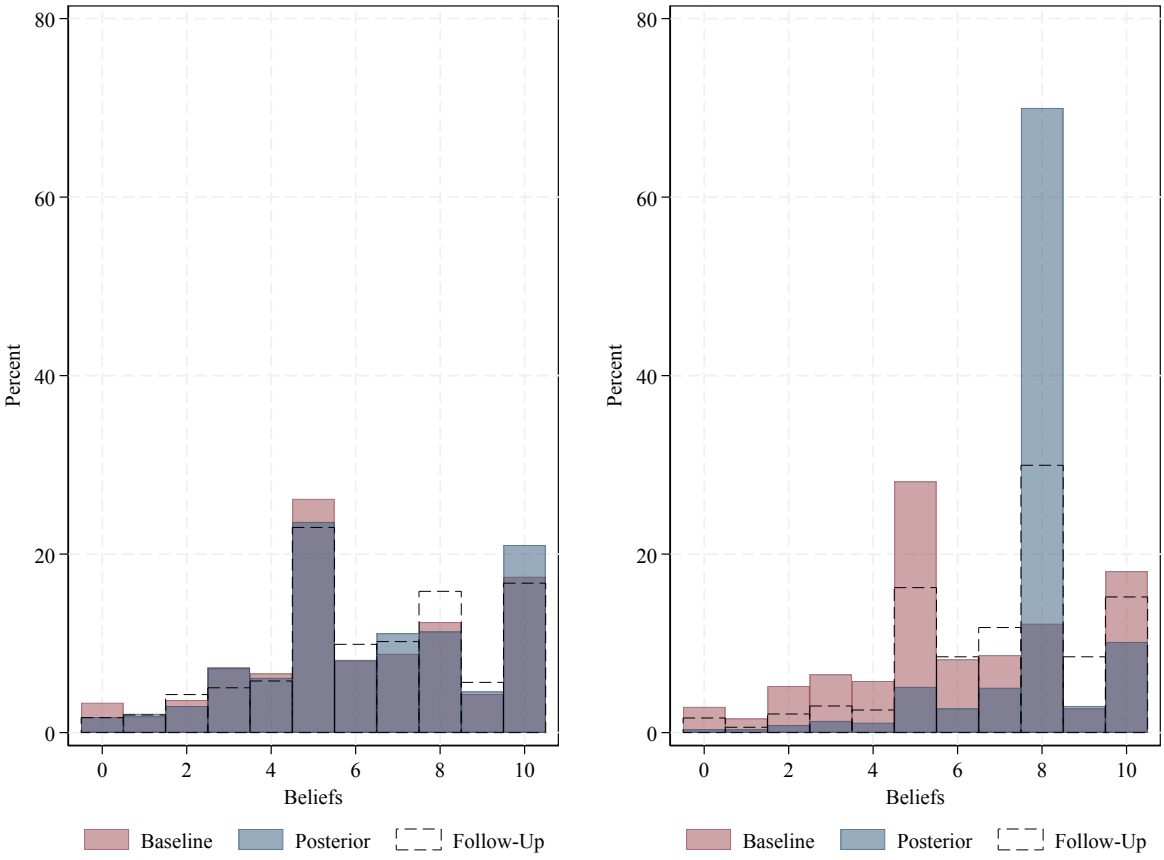
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Figure 1: Raw Belief Distribution Before and After Infographic and at Follow Up

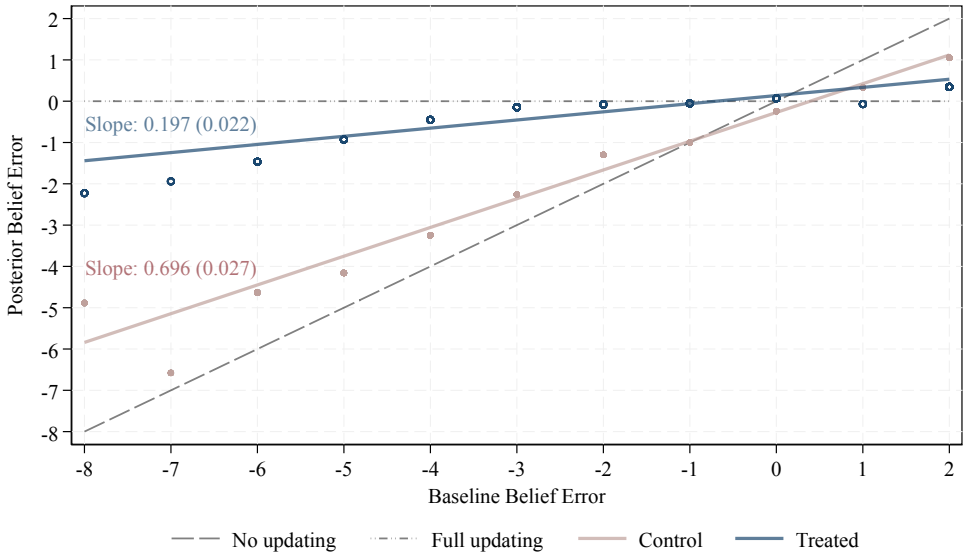


(a) Control Group

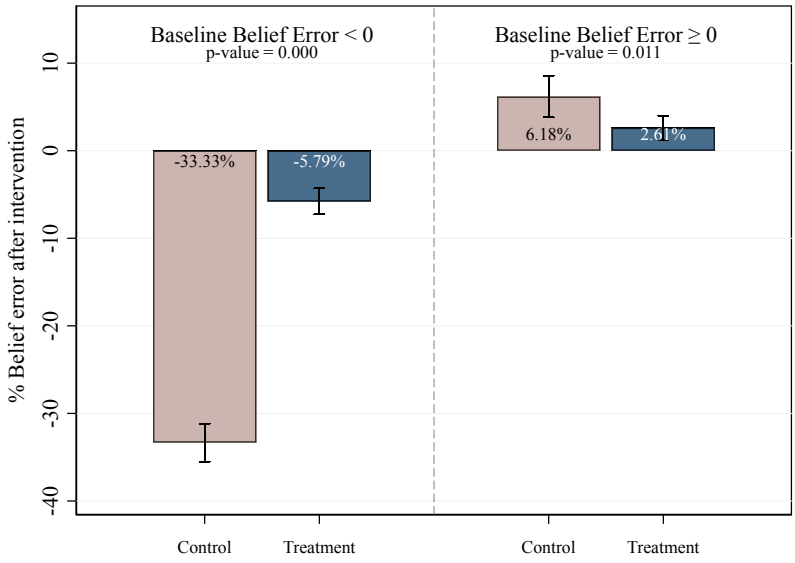
(b) Treatment Group

Notes: This figure shows baseline and posterior beliefs distribution by treatment groups. Panel (a) shows the baseline and posterior belief distributions for the control group, while Panel (b) shows the same for the treatment group. The hollow dotted line represents the belief measured at follow-up. The x-axis represents beliefs (scaled 0–10), and the y-axis indicates the percentage of respondents holding each belief at baseline and after receiving the infographic.

Figure 2: Belief Distribution Before and After Infographic



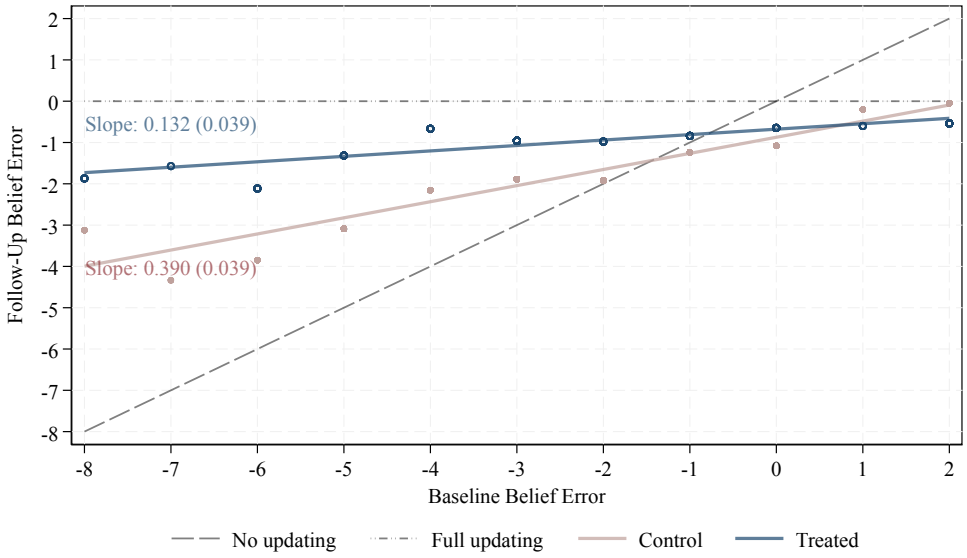
(a) Belief Errors Before and After Intervention



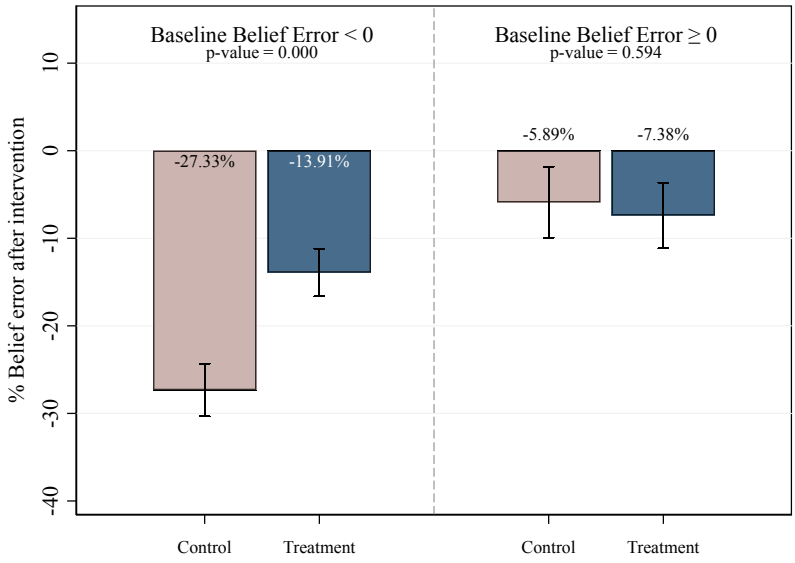
(b) Average Belief Errors by Baseline Error Categories

Notes: This figure presents two visualizations of belief error dynamics before and after the intervention. Panel (a) shows the relationship between belief errors before and after the intervention, with separate regression slopes for treated (blue) and control (red) groups. The 45-degree line represents no updating, and the horizontal line reflects full updating. Panel (b) illustrates the average post-intervention belief errors, grouped by baseline error categories (≤ 0 for pessimistic participants) and > 0 for optimistic participants) for treated and control groups, with corresponding p-values for group differences. Bars represent mean belief errors, and whiskers indicate 95% confidence intervals.

Figure 3: Belief Distribution Before the Intervention and at Follow Up



(a) Belief Errors Before the Intervention and at Follow Up



(b) Average Belief Errors by Baseline Error Categories

Notes: This figure presents two visualizations of belief error dynamics at baseline (before the intervention) and at follow up. Panel (a) shows the relationship between belief errors before and after the intervention, with separate regression slopes for treated (blue) and control (red) groups. The 45-degree line represents no updating, and the horizontal line reflects full updating. Panel (b) illustrates the average post-intervention belief errors, grouped by baseline error categories (≤ 0 for pessimistic participants) and > 0 for optimistic participants) for treated and control groups, with corresponding p-values for group differences. Bars represent mean belief errors, and whiskers indicate 95% confidence intervals.

Table 1: Summary-Balance Table

	Full Sample	Treatment	Control	Difference (2)-(3)
	(1)	(2)	(3)	(4)
Beliefs				
Belief before treatment	6.10	6.08	6.11	-0.031
	(2.671)	(2.661)	(2.682)	[0.116]
Financial Products Usage				
Savings account (=1)	0.37	0.37	0.37	-0.000
	(0.482)	(0.482)	(0.482)	[0.021]
Loans (=1)	0.01	0.01	0.02	-0.003
	(0.120)	(0.114)	(0.127)	[0.005]
Consumer loan (=1)	0.01	0.00	0.01	-0.003
	(0.072)	(0.061)	(0.082)	[0.003]
Credit loan (=1)	0.01	0.01	0.01	-0.001
	(0.084)	(0.081)	(0.087)	[0.004]
Credit card (=1)	0.01	0.01	0.01	-0.000
	(0.075)	(0.075)	(0.076)	[0.003]
Digital wallet (=1)	0.34	0.34	0.35	-0.012
	(0.475)	(0.473)	(0.477)	[0.021]
Bank App (=1)	0.15	0.16	0.15	0.013
	(0.362)	(0.368)	(0.355)	[0.016]
Socio-demographic Characteristics				
Women (=1)	0.59	0.60	0.59	0.010
	(0.491)	(0.490)	(0.492)	[0.021]
Age	41.06	41.21	40.91	0.301
	(12.386)	(12.465)	(12.308)	[0.539]
Experienced discrimination (=1)	0.40	0.40	0.39	0.009
	(0.489)	(0.490)	(0.488)	[0.021]
Household size	3.89	3.91	3.87	0.045
	(1.660)	(1.650)	(1.671)	[0.072]
Minors in household	1.61	1.62	1.61	0.011
	(1.145)	(1.144)	(1.146)	[0.050]
Risk-taking	5.51	5.46	5.56	-0.093
	(3.012)	(2.981)	(3.043)	[0.131]
Time discounting	6.45	6.38	6.53	-0.142
	(3.164)	(3.203)	(3.123)	[0.138]
Income: 0 - 250,000	0.06	0.05	0.06	-0.010
	(0.236)	(0.226)	(0.245)	[0.010]
Income: 251,000 - 400,000	0.08	0.07	0.09	-0.018
	(0.276)	(0.263)	(0.289)	[0.012]
Income: 401,000 - 600,000	0.14	0.14	0.15	-0.011
	(0.350)	(0.345)	(0.356)	[0.015]
Income: 601,000 - 1,462,000	0.49	0.51	0.47	0.041*
	(0.500)	(0.500)	(0.499)	[0.022]
Income: 1,463,000 - 1,924,000	0.19	0.19	0.20	-0.006
	(0.395)	(0.393)	(0.397)	[0.017]
Income: 2,925,000 - 4,286,000	0.03	0.03	0.02	0.003
	(0.161)	(0.165)	(0.156)	[0.007]
Income: More than 4,287,000	0.00	0.00	0.00	-0.000
	(0.053)	(0.053)	(0.054)	[0.002]
Envision staying: Less than a year	0.01	0.01	0.01	-0.000
	(0.114)	(0.114)	(0.115)	[0.005]
Envision staying: 1-3 years	0.07	0.07	0.07	-0.002
	(0.249)	(0.247)	(0.250)	[0.011]
Envision staying: 4-6 years	0.09	0.09	0.08	0.011
	(0.281)	(0.288)	(0.272)	[0.012]
Envision staying: 7-10 years	0.11	0.11	0.12	-0.003
	(0.318)	(0.316)	(0.321)	[0.014]
Envision staying: Indefinately	0.41	0.42	0.41	0.006
	(0.493)	(0.493)	(0.492)	[0.021]
Envision staying: Not sure	0.31	0.30	0.31	-0.011
	(0.461)	(0.459)	(0.463)	[0.020]
Education level: None	0.06	0.06	0.05	0.003
	(0.229)	(0.232)	(0.226)	[0.010]
Education level: Preschool	0.00	0.00	0.00	-0.000
	(0.043)	(0.043)	(0.044)	[0.002]
Education level: Elementary school (1st - 5th grade)	0.14	0.13	0.15	-0.026*
	(0.348)	(0.335)	(0.362)	[0.015]
Education level: Secondary school (6th - 9th grade)	0.19	0.19	0.19	0.001
	(0.393)	(0.393)	(0.393)	[0.017]
Education level: High school (10th - 13th)	0.43	0.44	0.42	0.020
	(0.495)	(0.496)	(0.494)	[0.022]
Education level: Technical or technological	0.07	0.07	0.07	-0.006
	(0.257)	(0.252)	(0.262)	[0.011]
Education level: University	0.11	0.11	0.10	0.009
	(0.311)	(0.316)	(0.305)	[0.014]
Education level: Graduate	0.00	0.00	0.00	-0.001
	(0.057)	(0.053)	(0.062)	[0.003]
Imputed Covariate (=1)	0.12	0.12	0.12	0.005
	(0.325)	(0.328)	(0.322)	[0.014]
Observations	2,115	1,073	1,042	2,115

Notes: This table presents summary statistics for the full sample (column 1), as well as balance checks between the treatment (column 2) and control (column 3) groups. Column 4 displays the difference between the two groups. Standard deviation in parentheses and standard errors in brackets. Variables include baseline beliefs, financial product usage, socio-demographic characteristics, and income brackets. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 2: Beliefs Correction and Estimated Learning Rates

	Absolute Value Belief Error				Belief Updating	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.23***	-0.23***	-0.23***	-0.28***	-0.05***	-0.05***
	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)
Optimistic				0.03*		
				(0.019)		
Treatment × Optimistic				0.16***		
				(0.015)		
Belief Error					0.30***	0.31***
					(0.027)	(0.027)
Treatment × Belief Error					0.50***	0.50***
					(0.035)	(0.034)
Strata FE		✓	✓	✓		✓
Lasso Selected Covariates			✓	✓		
Control Mean	0.31	0.31	0.31	0.31	-0.04	-0.04
Control Std. Dev.	0.22	0.22	0.22	0.22	0.25	0.25
Observations	2115	2115	2115	2115	2115	2115

Notes: This table reports the effects of the treatment on belief error correction (columns 1–4) and belief updating (columns 5–6). The dependent variable in columns 1–4 is the absolute value of the belief error, while in columns 5–6 it is the belief updating rate. The treatment indicator captures the average effect of the intervention. The interaction terms with Optimistic (=1) and Belief Error show heterogeneity in treatment effects. All regressions include strata fixed effects and lasso-selected covariates when indicated. Robust standard errors are reported in parentheses. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 3: Reduced Form Results on Financial Inclusion

	Financial Inclusion Index	Visit Bank	Request Information	Digital Wallet	Savings Account
	(1)	(2)	(3)	(4)	(5)
	Panel A: Average Treatment Effects				
Treatment (=1)	0.07**	0.04*	0.05**	0.03	0.03
	(0.033)	(0.020)	(0.020)	(0.020)	(0.019)
	Panel B: Heterogeneous Treatment Effects				
Treatment Effect: Pessimistics	0.11***	0.05**	0.05**	0.04*	0.06**
	(0.040)	(0.024)	(0.025)	(0.025)	(0.023)
Treatment Effect: Optimistics	0.01	-0.00	0.05	0.00	-0.03
	(0.057)	(0.036)	(0.035)	(0.036)	(0.035)
Strata FE	✓	✓	✓	✓	✓
Lasso Slected Covariates	✓	✓	✓	✓	✓
Control Mean	0.01	0.33	0.32	0.33	0.33
Control Std. Dev.	0.78	0.47	0.47	0.47	0.47
Observations	2115	2115	2115	2115	2115

Notes: This table presents the effects of the treatment on financial inclusion outcomes. Panel A reports average treatment effects, including the financial inclusion index and individual financial products (columns 2–5). Panel B shows heterogeneous treatment effects for pessimistic and optimistic respondents. The dependent variables include visiting a bank, requesting information, using digital wallets, and accessing credit and insurance products. All regressions include strata fixed effects and lasso-selected covariates. Robust standard errors are reported in parentheses. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 4: Heterogeneous Treatment Effects on Financial Inclusion Index

	Full	Gender		Ven Loan		Income		Schooling		Age	
	Sample	Men	Women	No	Yes	Low	High	Low	High	18-39	39-79
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treatment (=1)	0.07**	0.09	0.06	0.08**	0.06	0.18***	0.03	0.12**	0.05	0.02	0.13***
	(0.033)	(0.054)	(0.041)	(0.037)	(0.071)	(0.060)	(0.039)	(0.052)	(0.042)	(0.044)	(0.048)
Control Mean	0.01	0.12	-0.06	-0.03	0.15	-0.08	0.06	-0.03	0.04	-0.02	0.05
Control Std. Dev.	0.78	0.81	0.75	0.77	0.81	0.74	0.79	0.77	0.78	0.77	0.79
Observations	2115	858	1257	1627	483	604	1511	823	1292	1078	1037
					Panel B: Pessimistics						
Treatment (=1)	0.11***	0.18***	0.06	0.14***	0.04	0.17**	0.09*	0.19***	0.07	0.08	0.13**
	(0.040)	(0.065)	(0.050)	(0.045)	(0.083)	(0.077)	(0.047)	(0.064)	(0.051)	(0.054)	(0.059)
Control Mean	-0.02	0.08	-0.09	-0.08	0.17	-0.08	0.00	-0.07	0.01	-0.08	0.05
Control Std. Dev.	0.78	0.80	0.75	0.75	0.81	0.76	0.78	0.76	0.78	0.76	0.79
Observations	1405	575	830	1056	346	373	1032	521	884	736	669
					Panel C: Optimistics						
Treatment (=1)	0.01	-0.08	0.07	-0.03	0.14	0.23**	-0.11	-0.01	-0.01	-0.13	0.15*
	(0.057)	(0.094)	(0.072)	(0.063)	(0.142)	(0.100)	(0.070)	(0.090)	(0.075)	(0.079)	(0.084)
Control Mean	0.08	0.21	-0.01	0.07	0.11	-0.09	0.17	0.04	0.11	0.10	0.05
Control Std. Dev.	0.79	0.82	0.75	0.78	0.82	0.72	0.81	0.79	0.79	0.78	0.80
Observations	710	283	427	571	137	231	479	302	408	342	368

Notes: This table reports heterogeneous treatment effects on financial inclusion across various demographic and behavioral characteristics. The dependent variable is a financial inclusion index. Heterogeneous categories are defined as follows: (i) Sex: male (column 2) and female (column 3); (ii) Venezuelan Loan: those who did not (column 4) and did (column 5) take out loans in Venezuela; (iii) Income: individuals with below 600,000 COP (column 6) and above (column 7); (iv) Schooling: individuals with less than secondary (column 8) and at least secondary (column 9) educational attainment; (v) Age: individuals aged 18–39 (column 10) and 39–79 (column 11). Panel A shows the results for the full sample, Panel B for pessimistic individuals, and Panel C for optimistic individuals. Observations represent the number of respondents in each subgroup. Statistical significance is denoted by * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 5: Heterogeneous Treatment Effects on Financial Inclusion Index: Envision Staying, Discrimination, and Savings Account

	Full	Envision staying			Discrimination		Have Savings Account	
	Sample	Less than 11 years	Indefinitely	Not sure	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment (=1)	0.07**	0.12*	0.03	0.10*	0.11***	0.02	0.12***	0.01
	(0.033)	(0.064)	(0.053)	(0.054)	(0.041)	(0.054)	(0.044)	(0.049)
Control Mean	0.01	0.09	0.06	-0.11	-0.03	0.09	0.04	-0.03
Control Std. Dev.	0.78	0.82	0.80	0.71	0.76	0.81	0.80	0.75
Observations	2115	592	877	646	1277	838	1266	848
				Panel B: Pessimistics				
Treatment (=1)	0.11***	0.25***	0.07	0.05	0.16***	0.05	0.15***	0.04
	(0.040)	(0.081)	(0.063)	(0.065)	(0.052)	(0.062)	(0.054)	(0.059)
Control Mean	-0.02	0.03	0.02	-0.11	-0.08	0.06	-0.01	-0.04
Control Std. Dev.	0.78	0.81	0.79	0.71	0.74	0.81	0.80	0.74
Observations	1405	381	583	441	790	615	815	590
				Panel C: Optimistics				
Treatment (=1)	0.01	-0.10	-0.00	0.16	0.04	-0.05	0.05	-0.05
	(0.057)	(0.106)	(0.093)	(0.102)	(0.068)	(0.106)	(0.075)	(0.088)
Control Mean	0.08	0.19	0.15	-0.13	0.04	0.17	0.13	-0.01
Control Std. Dev.	0.79	0.81	0.81	0.69	0.78	0.81	0.80	0.76
Observations	710	211	294	205	487	223	451	258

Notes: This table reports heterogeneous treatment effects on financial inclusion across various demographic and behavioral characteristics. The dependent variable is a financial inclusion index. Heterogeneous categories are defined as follows: (i) Envision Staying: individuals who plan to stay less than 11 years (column 2), indefinitely (column 3), or are not sure (column 4); (ii) Discrimination: individuals reporting no experience of discrimination (column 5) and those who experienced discrimination (column 6); (iii) Have Savings Account: individuals who do not have a savings account (column 7) and those who do (column 8). Panel A shows the results for the full sample, Panel B for pessimistic individuals, and Panel C for optimistic individuals. Observations represent the number of respondents in each subgroup. Statistical significance is denoted by * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 6: Instrumental Variables Results on Financial Inclusion

	Financial Inclusion Index	Visit Bank	Request Information	Digital Wallet	Savings Account
	(1)	(2)	(3)	(4)	(5)
			Panel A: Full Sample		
Perception After Treatment	0.33**	0.15*	0.13	0.16*	0.18**
	(0.132)	(0.081)	(0.083)	(0.083)	(0.077)
First-stage F-statistic	216.44	216.44	216.44	216.44	216.44
Observations	2115	2115	2115	2115	2115
			Panel B: Pessimistics		
Perception After Treatment	0.34**	0.17**	0.14*	0.15*	0.19**
	(0.135)	(0.083)	(0.084)	(0.084)	(0.079)
First-stage F-statistic	301.37	301.37	301.37	301.37	301.37
Observations	1405	1405	1405	1405	1405
			Panel C: Optimistics		
Perception After Treatment	0.42	-0.02	-0.14	0.43	0.52
	(0.809)	(0.509)	(0.495)	(0.509)	(0.517)
First-stage F-statistic	12.57	12.57	12.57	12.57	12.57
Observations	710	710	710	710	710

Notes: This table presents instrumental variable (IV) estimates of the effect of beliefs after treatment on financial inclusion outcomes. Panel A reports results for the full sample, Panel B focuses on pessimistic respondents, and Panel C focuses on optimistic respondents. The dependent variables include a financial inclusion index and individual financial plans to visiting a bank, requesting information, obtaining a digital wallet, and obtaining a savings account. We report the F-statistic of the first stage. All regressions include strata fixed effects and control variables selected via lasso. Robust standard errors are reported in parentheses. Statistical significance is denoted by * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 7: Beliefs Correction and Estimated Learning Rates at Follow-Up

	Absolute Value Belief Error				Belief Updating	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.08***	-0.09***	-0.09***	-0.11***	-0.04***	-0.04***
	(0.013)	(0.013)	(0.012)	(0.016)	(0.013)	(0.013)
Optimistic				0.04		
				(0.026)		
Treatment × Optimistic				0.07***		
				(0.025)		
Belief Error					0.26***	0.27***
					(0.033)	(0.033)
Treatment × Belief Error					0.57***	0.57***
					(0.043)	(0.043)
Strata FE		✓	✓	✓		✓
Lasso Selected Covariates			✓	✓		
Control Mean	0.30	0.30	0.30	0.30	-0.04	-0.04
Control Std. Dev.	0.23	0.23	0.23	0.23	0.24	0.24
Observations	1214	1214	1214	1214	1214	1214

Notes: This table reports the effects of the treatment on belief error correction (columns 1–4) and belief updating (columns 5–6). The dependent variable in columns 1–4 is the absolute value of the belief error, while in columns 5–6 it is the belief updating rate. The treatment indicator captures the average effect of the intervention. The interaction terms with Optimistic (=1) and Belief Error show heterogeneity in treatment effects. All regressions include strata fixed effects and lasso-selected covariates when indicated. Robust standard errors are reported in parentheses. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 8: Reduced Form Results on Financial Inclusion at Follow-Up

	Financial Inclusion Index Opened	Opened Savings Account	Opened Digital Wallet	Financial Inclusion Index Action	Action Savings Account	Action Digital Wallet
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Average Treatment Effects			
Treatment (=1)	0.02	0.02	-0.01	0.06	0.05*	0.01
	(0.040)	(0.024)	(0.025)	(0.043)	(0.026)	(0.024)
p-value under N=2,115	0.605	0.242	0.762	0.059	0.019	0.439
Permutation p-value	0.710	0.377	0.811	0.140	0.060	0.541
			Panel B: Heterogeneous Treatment Effects			
Treatment Effect: Pessimistics	0.04	0.04	-0.00	0.05	0.05	0.00
	(0.049)	(0.029)	(0.031)	(0.053)	(0.031)	(0.029)
Treatment Effect: Optimistics	-0.03	-0.01	-0.02	0.09	0.04	0.04
	(0.069)	(0.039)	(0.045)	(0.073)	(0.044)	(0.042)
Pessimistics Perm. p-value	0.349	0.123	0.968	0.250	0.054	0.952
Optimistics Perm. p-value	0.558	0.622	0.576	0.507	0.521	0.508
Strata FE	✓	✓	✓	✓	✓	✓
Lasso Selected Covariates	✓	✓	✓	✓	✓	✓
Control Mean	0.01	0.38	0.63	0.01	0.56	0.74
Control Std. Dev.	0.79	0.49	0.48	0.82	0.50	0.44
Observations	1214	1214	1214	1214	1214	1214

Notes: This table presents the effects of the treatment on financial inclusion outcomes. Panel A reports average treatment effects, including two financial inclusion indices (columns 1 and 4) and individual financial products (columns 2–3 and 5–6). The financial inclusion index and outcomes in columns 1–3 capture whether individuals opened a product, while columns 4–6 capture whether individuals took any action toward opening a product. Panel B shows heterogeneous treatment effects for pessimistic and optimistic respondents. We report permutation p-values to account for the reduced sample size at follow-up. All regressions include strata fixed effects and lasso-selected covariates. Robust standard errors are reported in parentheses. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

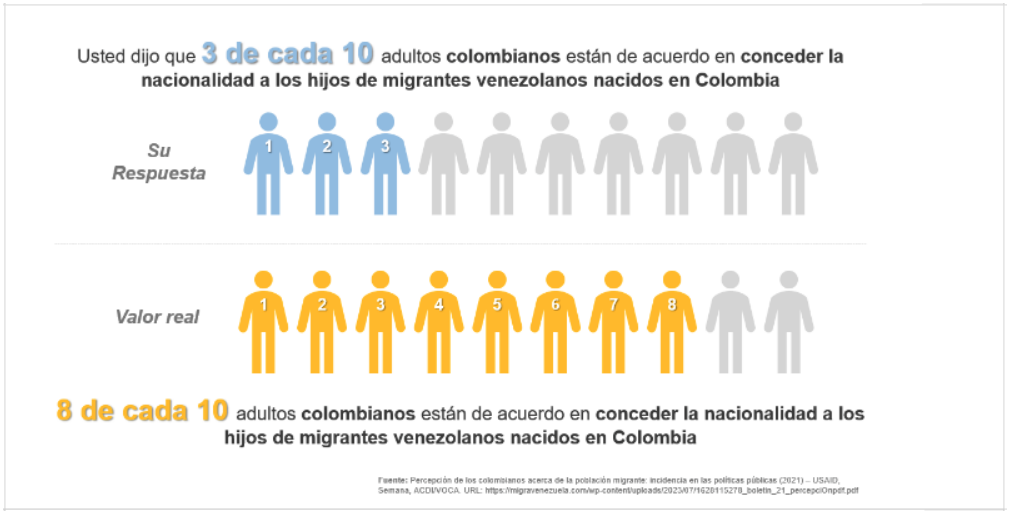
APPENDIX
(for online publication)

Perceived Social Acceptance and Migrants' Financial Inclusion

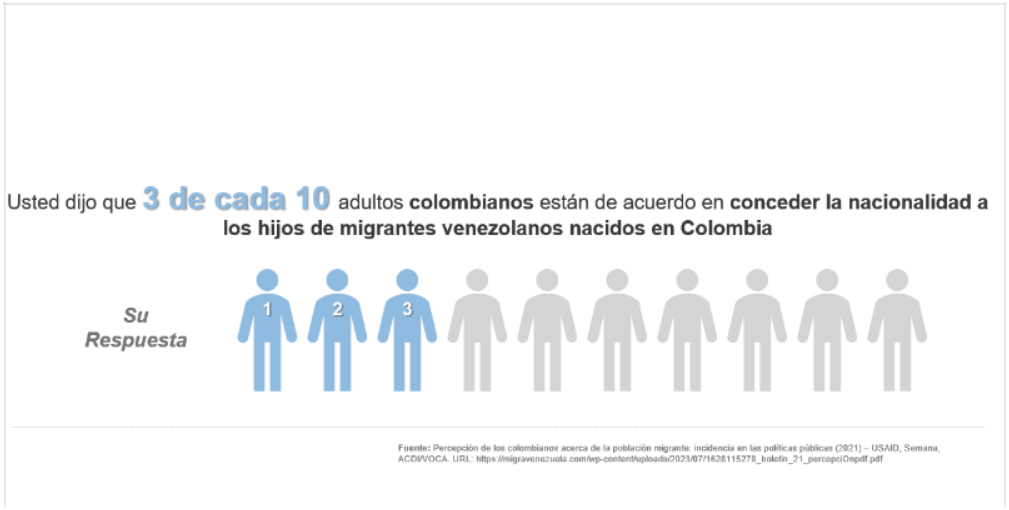
Authors: Giorgia Barboni, Nicolás de Roux, and Santiago Perez-Cardona

Appendix A Additional Figures and Tables

Figure A1: Information Treatment Infographic Example



(a) Treatment Example



(b) Control Example

Notes: This figure illustrates the infographics provided to participants. Panel (a) shows an example for the treatment group, where participants are reminded of their own response and provided with the actual proportion of Colombians who support granting citizenship to the children of Venezuelan migrants born in Colombia. Panel (b) shows an example for the control group, where participants are only reminded of their own response without receiving information on the true value.

Table A1: Reduced Form Results on Financial Inclusion

	Financial Inclusion Index	Credit Card	Private Loan	Business Loan	Insurance
	(1)	(2)	(3)	(4)	(5)
	Panel A: Average Treatment Effects				
Treatment	0.04	0.01	0.00	0.01	0.00
	(0.029)	(0.018)	(0.016)	(0.018)	(0.014)
	Panel B: Heterogeneous Treatment Effects				
Treatment Effect: Pessimistics	0.07**	0.03	-0.01	0.02	0.01
	(0.035)	(0.022)	(0.019)	(0.021)	(0.016)
Treatment Effect: Optimistics	-0.01	-0.03	0.02	0.00	-0.01
	(0.054)	(0.032)	(0.030)	(0.033)	(0.025)
Strata FE	✓	✓	✓	✓	✓
Lasso Selected Covariates	✓	✓	✓	✓	✓
Control Mean	0.01	0.23	0.17	0.21	0.11
Control Std. Dev.	0.71	0.42	0.38	0.41	0.31
Observations	2115	2115	2115	2115	2115

Notes: This table presents the effects of the treatment on financial inclusion outcomes. Panel A reports average treatment effects, including the financial inclusion index (column 1) and individual financial products (columns 2–5). Panel B shows heterogeneous treatment effects for pessimistic and optimistic respondents. The financial inclusion index in column 1 is constructed over all outcomes, including visit bank, request information, digital wallet, savings account, credit cards, private loans, business loans, and insurance. Columns 2–5 report results for credit cards, private loans, business loans, and insurance individually. All regressions include strata fixed effects and lasso-selected covariates. Robust standard errors are reported in parentheses. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table A2: Heterogeneous Treatment Effects on Financial Inclusion Index

	Full	Gender		Ven Loan		Fin. Inclusion		Income		Schooling		Age		Discriminated	
	Sample	Men	Women	No	Yes	Low	High	Low	High	Low	High	18-39	39-79	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Treatment	0.04	0.07	0.03	0.05	0.03	0.05	0.05	0.14***	0.00	0.07	0.03	-0.01	0.09**	0.08**	-0.01
	(0.029)	(0.050)	(0.036)	(0.033)	(0.065)	(0.035)	(0.052)	(0.054)	(0.035)	(0.049)	(0.037)	(0.039)	(0.044)	(0.037)	(0.048)
Observations	2115	858	1257	1627	483	1343	772	604	1511	823	1292	1078	1037	1277	838
							Panel B: Pessimists								
Treatment	0.07**	0.12**	0.03	0.10***	-0.03	0.10**	0.03	0.14**	0.05	0.11*	0.05	0.05	0.08	0.13***	-0.01
	(0.035)	(0.058)	(0.043)	(0.039)	(0.075)	(0.042)	(0.061)	(0.068)	(0.041)	(0.060)	(0.044)	(0.046)	(0.053)	(0.046)	(0.054)
Observations	1405	575	830	1056	346	892	513	373	1032	521	884	736	669	790	615
							Panel C: Optimists								
Treatment	-0.01	-0.05	0.02	-0.05	0.22	-0.05	0.11	0.20**	-0.11	-0.00	-0.01	-0.15**	0.14*	-0.01	0.00
	(0.054)	(0.095)	(0.065)	(0.059)	(0.142)	(0.062)	(0.100)	(0.092)	(0.067)	(0.086)	(0.071)	(0.074)	(0.079)	(0.064)	(0.099)
Observations	710	283	427	571	137	451	259	231	479	302	408	342	368	487	223

Notes: This table reports heterogeneous treatment effects on financial inclusion across various demographic and behavioral characteristics. The dependent variable is a financial inclusion index. Heterogeneous categories are defined as follows: (i) Sex: male (column 2) and female (column 3); (ii) Venezuelan Loan: those who did not (column 4) and did (column 5) take out loans in Venezuela; (iii) Financial Inclusion: individuals with low (column 6) and high (column 7) pre-intervention financial inclusion levels in Venezuela; (iv) Income: individuals with below 600,000 COP (column 8) and above (column 9); (v) Schooling: individuals with less than secondary (column 10) and at least secondary (column 11) educational attainment; (vi) Age: individuals aged 18–39 (column 12) and 39–79 (column 13); and (vii) Discrimination: individuals reporting no experience of discrimination (column 14) and those who experienced discrimination (column 15). Panel A shows the results for the full sample, Panel B for pessimistic individuals, and Panel C for optimistic individuals. Observations represent the number of respondents in each subgroup. Statistical significance is denoted by * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table A3: Instrumental Variables Results on Financial Inclusion

	Financial Inclusion Index	Credit Card	Private Loan	Business Loan	Insurance
	(1)	(2)	(3)	(4)	(5)
			Panel A: Full Sample		
Perception After Treatment	0.21*	0.06	-0.05	0.06	0.02
	(0.114)	(0.073)	(0.063)	(0.070)	(0.053)
First-stage F-statistic	217.67	217.67	217.67	217.67	217.67
Observations	2115	2115	2115	2115	2115
			Panel B: Pessimistic		
Perception After Treatment	0.21*	0.06	-0.04	0.06	0.02
	(0.116)	(0.075)	(0.064)	(0.071)	(0.053)
First-stage F-statistic	304.85	304.85	304.85	304.85	304.85
Observations	1405	1405	1405	1405	1405
			Panel C: Optimists		
Perception After Treatment	0.66	0.68	-0.17	0.34	0.36
	(0.783)	(0.465)	(0.433)	(0.478)	(0.379)
First-stage F-statistic	12.61	12.61	12.61	12.61	12.61
Observations	710	710	710	710	710

Notes: This table presents instrumental variable (IV) estimates of the effect of beliefs after treatment on financial inclusion outcomes. Panel A reports results for the full sample, Panel B focuses on pessimistic respondents, and Panel C focuses on optimistic respondents. The financial inclusion index in column 1 is constructed over all outcomes, including visit bank, request information, digital wallet, savings account, credit cards, private loans, business loans, and insurance. Columns 2–5 report results for credit cards, private loans, business loans, and insurance individually. We report the F-statistic of the first stage. All regressions include strata fixed effects and control variables selected via lasso. Robust standard errors are reported in parentheses. Statistical significance is denoted by * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table A4: Effect of Belief Update on Financial Inclusion

	Financial Inclusion Index	Visit Bank	Request Information	Digital Wallet	Savings Account	Credit Card	Private Loan	Business Loan	Insurance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A: Full Sample					
Update (-Belief Error × Treatment)	0.12*	0.09*	0.06	0.10**	0.13***	0.04	-0.03	0.03	0.01
	(0.066)	(0.047)	(0.048)	(0.048)	(0.046)	(0.042)	(0.037)	(0.041)	(0.031)
Observations	2115	2115	2115	2115	2115	2115	2115	2115	2115
				Panel B: Pessimists					
Update (-Belief Error × Treatment)	0.11*	0.10**	0.07	0.09*	0.12**	0.02	-0.03	0.03	0.01
	(0.068)	(0.049)	(0.050)	(0.049)	(0.048)	(0.044)	(0.038)	(0.042)	(0.032)
Observations	1405	1405	1405	1405	1405	1405	1405	1405	1405
				Panel C: Optimists					
Update (-Belief Error × Treatment)	0.21	0.02	-0.11	0.13	0.25	0.26	-0.08	0.10	0.12
	(0.292)	(0.194)	(0.188)	(0.190)	(0.197)	(0.171)	(0.163)	(0.180)	(0.143)
Observations	710	710	710	710	710	710	710	710	710

Notes: Column (1) reports estimates from an OLS regression of the financial inclusion index and individual financial outcomes on belief updating. Belief updating is measured as the reduction in normalized belief errors following the treatment, scaled by the true value of our social acceptance proxy. Columns (2) to (9) examine specific components of financial inclusion, including visiting a bank, requesting information, opening a digital wallet, a savings accounts, and credit or insurance products. Panel A shows results for the full sample, while Panels B and C explore heterogeneity based on baseline beliefs, focusing on pessimistic and optimistic respondents, respectively. Statistical significance is denoted by * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.