Does Information about Legal Migration Pathways Reduce Irregular Migration Aspirations?*

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Abstract

Many high-income countries aim to both increase immigration in line with their labor market needs and decrease irregular arrivals, and some have explicitly promoted efforts to achieve the former as part of a solution to the latter. However, empirical evidence on how information about legal pathways shapes irregular migration intentions as well as self-selection and aspirations for mobility and qualification more broadly remains scant. We contribute new insights with a randomized controlled trial in rural Senegal, providing information and basic assistance on the U.S. Diversity Visa Lottery, which offers medium- and high-skilled migrants access to permanent residence. The intervention significantly increases migration intentions and shifts preferences toward legal pathways. However, currently ineligible individuals, particularly those already contemplating irregular migration, show *increased* interest in migrating irregularly, suggesting an unintended consequence of efforts to broadly promote legal migration options. Education aspirations only increase weakly from a high baseline. These aspirations already mostly surpass the requirements for pursuing a legal migration path, but participants are unable to realize them.

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1 Introduction

Many countries in the Global North struggle with the dual political challenges of trying to identify options to formalize and legalize migration in line with their labor market requirements on the one hand, and reducing the number of irregular arrivals and asylum-seekers on the other. One argument that frequently features in debates around these issues is that these problems do not require entirely separate sets of solutions, but that progress can be made using an integrated approach: Create regular pathways for labor migration, so that potential migrants make those the target of their aspirations and actions while foregoing irregular journey attempts. In fact, this argument has been translated into active policy in several instances, both in the United States and in Europe.[1]

The existing academic literature does not provide a clear-cut answer. Despite research emphasizing the importance of migration policy for the size of migration flows (Mayda 2010, Czaika and De Haas 2013, Hatton 2014, Helbling and Leblang 2019), there is so far little evidence on how legal pathways affect migration preferences and decisions as well as other behaviors. Legal pathways are only available for individuals who meet eligibility criteria, such as formal education or work experience. Academics and policymakers alike also know little about how opening up or closing down legal pathways will affect the self-selection of particular migrant types, and, correspondingly, countries' brain gain or brain drain. These are the knowledge gaps we target.

To identify the impact of legal pathways, we conduct a large-scale randomized controlled trial (RCT) in Senegal with a treatment group that receives information on legal migration routes to the United States via the U.S. Diversity Visa Program (also known as the green card lottery) coupled with the offer of support in preparing basic information for the application. The control group does not receive any information. Through the green card lottery, the U.S. government offers immigrant visas to persons from countries with historically low rates of immigration, and for many aspiring migrants—in Senegal and elsewhere—it is the only realistic chance to gain permission for themselves and their close family members to move to the United States. Since the green card lottery has an education requirement, we can also study whether individuals who meet or fall short of this requirement differ in their responses. If such pathways become salient without being accessible, they may inadvertently shift aspirations toward irregular migration

[1] In the U.S., this was a key argument advanced by the Biden administration when it set up Safe Mobility Offices (SMOs) in several Latin American countries. Under the headline of "Helping Those in Need and Reducing Irregular Migration in the Americas," the Safe Mobility Initiative worked to refer individuals to "lawful migration pathways," including "labor pathways," in order "to avoid dangerous journeys" (U.S. Department of State 2025). In Europe, government-funded migration advice programs deployed in West Africa and elsewhere regularly attempt to convince individuals not to migrate irregularly by providing information about legal migration options. For example, an EU-funded, IOM-implemented Migration Resource Center (MRC) in Nigeria headlined its homepage's hero section "Be informed, migrate legally," noting that one should only migrate "if you have all the necessary travel documents, such as your work permit and visa. You run a lot of dangers when you migrate irregularly, like being exploited or doing unpaid labor" (NELEX 2025). Migration advice centers supported by the German development cooperation agency GIZ similarly engage in the "provision of information on the risks of irregular migration and the possibilities and requirements for regular migration," because youths "resort to irregular means" when they "lack access to information" (GIZ 2025), and the most recent development policy report of the German federal government explicitly links the expansion of legal skilled labor immigration to a potential reduction in irregular migration (German Federal Ministry for Economic Cooperation and Development 2025, 52).

instead of away from it. This possibility remains underexplored in the literature.

We implement our information intervention in a rural setting, in a set of villages that are both nationally and regionally representative, with all households in these villages targeted for interviews in order to collect migration histories and information on migration intentions and other socioeconomic characteristics, and with behavioral outcomes of target subjects and their households being captured after the treatment.

The current paper reports effects immediately after the treatment in fall 2024 and will be extended with one-year follow-up data in late 2025. We find that the treatment significantly increases the intention to migrate internationally but does not alter the preference for the U.S. as a destination. Importantly, awareness of the diversity visa lottery substantially increases the preference for this legal migration route, particularly among those previously unaware of it. However, an unintended consequence emerges: Individuals ineligible for the diversity lottery show an increased interest in irregular migration, potentially due to frustration over their exclusion. This effect is particularly pronounced among less-educated respondents, who have little chance of qualifying for the lottery. Regarding education aspirations, the treatment only has a small impact on education goals, likely because aspirations were already high in the sample. This fact also highlights that there is a binding constraint in terms of capabilities to pursue high education aspirations. Our paper also emphasizes several important descriptive insights, including that the vast majority of people, even in poor rural areas with high emigration rates, do not want to migrate irregularly.

Our paper thus explores not only whether information shifts migration preferences, but whether the response to this information differs between those who can and cannot act on it. We show that for ineligible individuals, legal migration opportunities may backfire, spurring irregular intentions. This highlights an underappreciated risk in the design of migration policies.

Our paper relates to three strands of literature. First, we contribute to the literature on the impact of migration policy, and especially visa policy, on migration intentions and flows. Research in this area was previously mainly based on cross-country data (Mayda 2010, Czaika and De Haas 2013, Hatton 2014, Czaika and Hobolth 2016, Helbling and Leblang 2019) or statistics taken at the border (Amuedo-Dorantes and Bucheli 2023, Clemens 2024). We contribute to this literature by adding micro-evidence, which is very scarce so far (recent exceptions include Detlefsen et al. 2022, Beber et al. 2024). So far, few papers have studied the relationship between policy barriers and regular versus irregular migration flows, such as asylum rejection (Czaika and Hobolth 2016) or limiting access to lawful entry along the border (Amuedo-Dorantes and Bucheli 2023, Clemens 2024). We document heterogeneous effects based on education levels and eligibility, thus creating a linkage to the micro-literature on self-selection into migration (Borjas 1987, Borjas et al. 2018, Patt et al. 2020, Aksoy and Poutvaara 2021, Dustmann et al. 2023, Clemens and Mendola 2024, Abramitzky et al. 2024).

Second, our paper contributes to the literature on migration frictions, which deals with the question of why there is not considerably more migration, given how profitable it can be. Important reasons that have received attention are liquidity and credit constraints (Bryan et al. 2014, Angelucci et al. 2015, Lagakos 2020, Gazeaud et al. 2023), uncertainties (McKenzie and Yang 2022), and knowledge about average income differentials (Baseler 2023, Frohnweiler et al.

2024). Behavioral aspects such as optimism bias may play a role too (Beber and Scacco 2022). Our paper contributes to this literature by documenting that knowledge about existing legal pathways is a relevant friction and that policymakers should not assume that migrants make decisions under full information about migration policy, nor should they assume that frictions concerning one type of migration can be alleviated without effects on other types of migration.

Third, we relate to the substantial literature on brain gain effects. Extant research documents that individuals in sending countries invest more into their human capital if they perceive migration as a viable option (e.g., Mountford 1997, Beine et al. 2008). Recent experimental evidence that is methodologically closest to our paper shows that the prospect of migration can significantly increase educational attainment and skill acquisition among potential migrants (e.g., Batista et al. 2012, Abarcar and Theoharides 2024). We add to this literature the important finding that the brain gain effect may be limited if educational aspirations are already very high, and find evidence in line with Shrestha (2017) that in regions where educational capabilities are a binding constraint, brain gain effects can be much lower.

The rest of the paper commences with a detailed description of the study design (Section 2), which includes the pre-registered hypotheses. We then turn to the discussion of results in Section 3, provide evidence of their robustness in Section 4, before concluding (Section 5). A detailed appendix provides additional evidence as well as documentations of the material used in the experiment.

2 Study design

We study these questions with an RCT conducted in Senegal, with an intervention providing information on the green card lottery. Our intervention targets men aged 18 to 40 from 144 rural villages across Senegal. The sample is based on a set of nationally and regionally representative villages. In the following, we discuss the experimental intervention, survey design, sampling, and treatment assignment.

2.1 The U.S. Diversity Immigrant Visa Program

Through the U.S. Diversity Immigrant Visa Program, the United States offers visas for immigrants from countries with low immigration rates to the United States.[2] For many aspiring migrants who do not qualify for legal migration channels targeted at the highly educated or family reunification, this is the only realistic way to obtain permanent residence in the United States for themselves and their close family members. In addition to the applicant's country of birth, there is a second requirement. Each applicant has to fulfill an education or work experience requirement. Applicants must either have a high school degree (equivalent to twelve years of formal education) or, within the past five years, have obtained at least two years of work experience in an occupation requiring at least two years of training. This experience requirement is determined through the O*Net database.

The diversity visa program is administered as a lottery because it offers at most 55,000 visas globally and is heavily oversubscribed—a testament to its attractiveness. No country can

[2]Countries with more than 50,000 immigrants to the U.S. in the past years are excluded from the program.

receive more than 7 percent of visas. The probability that a respondent who meets the overall requirements gets a visa will thus depend on the total number of eligible applicants.[3] In 2023, a total of 9.57 million qualified entries were entered, from which 119,262 prospective applicants were selected at random (1.25 percent). These received the invitation to proceed with the visa application. This number is higher than the 55,000 spots because many winners will not go through with the application.

2.2 Experimental intervention

The treatment group received truthful information on legal opportunities to migrate to the United States through the U.S. Diversity Visa Program, and we clarified that educational attainment or apprenticeship training are important prerequisites for accessing this migration avenue.

We specifically inform subjects that the program gives people from Senegal, like the respondent, his children, or other members of their household or family, a chance to migrate to the United States legally. We do not recommend registering but phrase the information as potentially helpful for the subject. We emphasize that eligible candidates must have completed secondary school or obtained at least two years of recent work experience in a job that requires at least two years of training or experience.[4] Review questions and discussion points encourage active processing and are designed to keep participants engaged throughout the script.

We also offer respondents assistance with registration for the lottery. If respondents express interest, the study team manages their registration process at no cost to them. Participants need to provide photos of themselves, their spouse, and their children under the age of 21, as well as an email address. If they did not have an email address, we assisted them in creating one, with their consent. We then verify that the photos meet the requirements, collect all required personal details, and enter this information into the online form. After completing the lottery entry, we provide respondents with their confirmation number either via email or WhatsApp. To clarify the probability of being selected, we provide truthful information that the annual number of lottery submissions from Senegal in recent years has ranged from 20,000 to 60,000, with 100 to 400 winning entries. We explicitly mention that each individual and their spouse can make repeated attempts and submit one entry per year.

The treatment script and flyer are included in the appendix (Sections E and F). The flyer contains an abbreviated version of the script. We use icons, in addition to text, to maximize the flyer's usefulness for illiterate participants.

[3] With a population of over 17 million people, Senegal had only 253 winners. By contrast, anglophone Ghana is about twice as large, with a population of about 34 million, and received 3398 winning tickets. However, given that the winning tickets are drawn at random, the chances of winning the lottery are much better in Senegal than in countries where the scheme is more popular, such as Ghana. While the number of qualified lottery entrants for 2023 is not yet published, the figures for 2019–2021 are available and indicate that in these years, the share of qualified candidates who received a visa in Senegal was 0.55 percent, whereas in Ghana, it was only 0.17 percent.

[4] We did *not* explicitly check the respondents' eligibility and tell them about this during the interview.

2.3 Survey design

Interviews were conducted in person in the fall of 2024 and consisted of a questionnaire that covers personal and household data to elicit important characteristics on topics such as skills, education, and the labor market situation, migration-related questions regarding history, networks, and future plans on migration to urban destinations (Dakar, Ziguinchor, St. Louis, and Kaolack) and abroad. Overall, the in-person interview lasted approximately 60 minutes.

The survey first collects information about respondents' socioeconomic status, background, past migration behavior, current migration intentions, and whether the respondents know any legal migration routes to the United States. Treated subjects then receive a printed flyer, the contents of which the enumerator reviewed in detail with each subject, providing information on the U.S. green card lottery, including the application procedure, requirements, and support available from the study team.

Next, we elicit respondents' interest and intentions regarding international migration, including their preferred routes, destination countries, level of interest in living abroad, planning and preparations, and willingness to pay for clandestine travel or a visa to the United States. We also record aspirations concerning their highest desired educational attainment and interest in undergoing apprenticeship training, as well as how respondents assess the importance of education and training for their children (or future children) and their interest in having their children live in different locations (within Senegal or another country).

In fall 2025, we will again collect survey data on posterior beliefs and aspirations, as well as data on actual migration preparations, cross-border movements, and human capital decisions such as participation in trainings.

2.4 Sampling

The survey sampling is given by the framework implemented for the Senegal Migration Panel (SMP).[5] We implement our study in a total of 144 rural villages, grouped in four samples: A nationally representative sample of rural villages (35 villages); representative samples in three focus departments, Kaolack (30 villages), Matam (29 villages), and Sedhiou (30 villages); and a set of 10 villages in Thies and Diourbel that were targeted by the NGO Eclosio for a separate irrigation intervention as well as 10 matched villages in the same departments (for a total of 20 villages of this type).

Sampling was conducted based on a list of administrative units obtained from Senegal's National Agency for Statistics and Demography, ANSD, with villages defined as settlements containing between 40 and 150 households. We exclude all urban communes and communes in the Dakar region as well as some areas bordering Gambia, Mali, or Guinea for security reasons (specifically, the departments of Bignona, Bounkiling, Medina Yoro Foulah, Saraya, Bakel, Salemata, and the district of Fongolembi), with claims concerning representativeness conditional on these exclusions. In total, the sampling frame covers 3,082 out of 3,463 rural

[5]The sampling details provided here reflect those described in Beber et al. (2023). For general information about the SMP project, see https://www.rwi-essen.de/en/rwi/team/person/projects/detail/the-senegal-migration-panel-understanding-mobility-in-330 and https://www.ifw-kiel.de/de/institut/the-senegal-migration-panel-understanding-mobility-in-a-climate-stressed-population.

villages across all regions and 35 of 41 departments outside of Dakar.[6]

Villages were randomly sampled within location-based strata, namely within the district for focus departments and within the region for the national sample. We then randomly sample households within villages for in-depth surveys, including only those with at least one male member aged 18–40, which is the demographic group most likely to relocate for work from rural Senegal.

Within-village household samples were drawn proportionately to village size, with target sample sizes constrained to range from 15 to 45 households per village. We stratify based on prior migration by target individuals, undersampling those who migrated within the previous year. Within strata, household selection probabilities are proportional to the number of within-household target individuals. Finally, we randomly select a target individual within each sampled household.

2.5 Treatment assignment

We follow a two-step randomization process in which equal shares of our sample are assigned to treatment and control. First, villages are randomly selected for the experiment. We randomly selected 118 out of 144 villages to have subjects receive the treatment, with 26 villages remaining as pure control. We re-randomize to ensure balance across previously measured covariates. Second, within the selected villages, subjects are randomly assigned to the different experimental arms. The control group is divided evenly, with half in treated villages and half in pure control villages. Again, we re-randomize to establish balance in previously measured covariates.

The information treatment was conducted between September and December 2024. The survey targeted 2,732 men aged 18–40 in 144 clusters (villages) that had previously been sampled, including 1,366 subjects assigned to treatment, 698 assigned to the control group across 118 villages with at least some treated subjects, and 668 individuals in 26 randomly assigned pure control villages. Of these, 2,245 completed the interview.[7] The enumeration process was carried out in two phases. In the first phase, the survey team visited the villages and conducted in-person interviews. In the second phase, individuals unavailable for interviews in person during the 2024 village visits were contacted and interviewed by phone. The final sample comprised 1,635 individuals who completed in-person interviews and 610 individuals who completed phone interviews.[8] This includes 1,111 individuals who were assigned to treatment, 559 assigned to the control group in villages that include some treated subjects, and 575 in pure control villages.

[6]Senegal's administrative units are structured so that settlements (quartiers/villages) are nested within communes, then districts, departments, and finally regions.

[7]Interview completion is unrelated to treatment, given that neither subjects nor enumerators were aware of treatment assignment prior to the interview and virtually all interviews were completed once they had begun. In total, the SMP project interviewed 4,097 men in this survey wave, with 1,852 of them randomly assigned to a separate non-control treatment condition that is not part of this study.

[8]In the robustness section, we disaggregate results for in-person and phone samples since respondents and their answer patterns may vary and treatment intensity differs across sample types.

2.6 Treatment balance and sample composition

We check for balance on observable pre-intervention covariates in Table 1. Columns (1) and (2) report the averages for the control and treatment groups, respectively. Columns (4) and (5) show the size and statistical significance of differences between these group averages. The results indicate balance with respect to a large number of socioeconomic characteristics, including age, family characteristics, migration networks, incomes, as well as baseline intentions concerning internal and international migration, both regular and irregular. We observe one difference in the education block, specifically in the share of respondents with professional education. This group is very small, making up only 0.6 and 1.7 percent of the treatment and control groups, respectively. An indicator for this education level is among the control variables in the main specifications but is never selected as a relevant control by the LASSO estimator.[9]

Across treatment and control groups, individuals have a high intent to migrate internationally through regular means, scoring 2.2 out of 3 on average. However, the intent to do so irregularly is low, ranging from 0.70 to 0.79, depending on the route.

In order to qualify for the diversity lottery by fulfilling the education requirement, at least 12 years of schooling are required. In our rural sample, just short of a quarter of respondents (23.8 percent) fulfill this criterion, despite education nowadays being compulsory from age 6 to 16 in Senegal. The remainder of the sample, including 14 percent without formal education, 41 percent that only only completed primary school (6 years in Senegal), and 20.3 percent who finished middle school (10 years with certificate BFEM), might alternatively be able to prove eligibility by documenting relevant qualified work experience. They are, in any case, a key part of the study because we are interested in their education aspirations, the aspirations for their children, and their intentions to pursue other forms of migration.[10]

In Table 2 we provide averages for the outcome variables across treatment and control groups. The table shows that the intention for international migration has increased slightly from the pre-treatment measurement, and so too has the average intention to migrate irregularly. We also observe very high intentions for children to migrate in the future, which on average exceed the already very high migration aspirations that respondents express for themselves.

The preferred destination of 29 percent of respondents is the United States. On average, ten percent of respondents would prefer the lottery as their pathway to the U.S. if they were to migrate there. A little over three-fourths of respondents report an education aspiration for themselves that would be sufficient to clear the 12-year requirement the visa lottery sets. For their children, 97.8 percent of respondents aspire to education levels that would clear the education requirement of the visa lottery, providing little room for a further increase due to the treatment. Training aspirations are also very high, with about 70 percent of respondents reporting they are very interested in apprenticeship training, resulting in an average of 2.5 on a 3-point Likert scale.

[9] We conduct further balance tests for subgroups based on visa program eligibility, prior irregular migration intentions, and control group type in Section C in the Appendix. These do not produce any reasons for concern.

[10] A portion of the sample (around 2%) report prior knowledge of the lottery. Awareness of the lottery varies by education level: 14% of individuals with a university degree are aware of it, compared to 2.5% of those with only a high school education, and 1% among the rest.

Table 1 – Balance across treatment and control groups

	(1) Control	(2) Treatment	(3) Overall	(4) (1) vs. (2)	(5) P-value
Age	29.170	29.200	29.185	-0.030	0.923
	(0.216)	(0.216)	(0.152)	(0.305)	
No education	0.422	0.404	0.413	0.018	0.380
	(0.015)	(0.015)	(0.010)	(0.021)	
Primary school	0.201	0.204	0.203	-0.003	0.848
	(0.012)	(0.012)	(0.008)	(0.017)	
Middle school	0.131	0.149	0.140	-0.017	0.242
	(0.010)	(0.011)	(0.007)	(0.015)	
Secondary school	0.126	0.128	0.127	-0.002	0.903
	(0.010)	(0.010)	(0.007)	(0.014)	
Professional education	0.017	0.006	0.012	0.010	0.021
	(0.004)	(0.002)	(0.002)	(0.005)	
Tertiary	0.095	0.097	0.096	-0.002	0.874
	(0.009)	(0.009)	(0.006)	(0.012)	
Is married	0.512	0.518	0.515	-0.006	0.772
	(0.015)	(0.015)	(0.011)	(0.021)	
Has children	0.471	0.460	0.465	0.011	0.603
	(0.015)	(0.015)	(0.011)	(0.021)	
Is student	0.116	0.114	0.115	0.002	0.877
	(0.010)	(0.010)	(0.007)	(0.013)	
Has family abroad	0.570	0.582	0.576	-0.013	0.543
	(0.015)	(0.015)	(0.010)	(0.021)	
Has family in U.S.	0.126	0.141	0.134	-0.015	0.290
	(0.010)	(0.010)	(0.007)	(0.014)	
Has family in Europe	0.456	0.473	0.465	-0.018	0.405
	(0.015)	(0.015)	(0.011)	(0.021)	
Income last month	60198	64583	62375	-4385	0.371
	(2635)	(4148)	(2450)	(4900)	
Income last 6 months	85564	85902	85733	-338	0.965
	(4555)	(6330)	(3892)	(7786)	
Risk taking	6.948	7.255	7.100	-0.307	0.007
	(0.079)	(0.081)	(0.056)	(0.113)	
Prior knowledge of lottery	0.019	0.026	0.023	-0.007	0.287
	(0.004)	(0.005)	(0.003)	(0.006)	
Prior favorite destination U.S.	0.250	0.273	0.261	-0.023	0.212
	(0.013)	(0.013)	(0.009)	(0.019)	
Prior intent: external migration	2.219	2.188	2.204	0.031	0.524
	(0.034)	(0.035)	(0.024)	(0.048)	
Prior intent: internal migration	1.948	1.908	1.928	0.040	0.448
	(0.037)	(0.038)	(0.027)	(0.053)	
Prior intent: irregular via desert	0.736	0.665	0.701	0.070	0.155
	(0.035)	(0.034)	(0.025)	(0.049)	
Prior intent: irregular via sea	0.804	0.768	0.786	0.035	0.487
	(0.036)	(0.036)	(0.025)	(0.051)	
N	1134	1111	2245	2245	

Table 2 – Descriptive statistics of outcome variables

	Mean	SD	Min	Max
External migration intent	2.321	1.019	0.000	3.000
U.S. is favorite destination	0.294	0.456	0.000	1.000
Favors lottery pathway	0.093	0.291	0.000	1.000
Irregular migration intent	0.793	1.214	0.000	3.000
Log(WtP + 1)	4.629	6.459	0.000	16.118
Education aspiration	0.822	0.383	0.000	1.000
Training aspiration	2.513	0.925	0.000	3.000
Aspiration for children's education	0.977	0.150	0.000	1.000
Aspiration for children's migration	2.476	0.906	0.000	3.000

Notes: For question wording, see Appendix D.1.

2.7 Treatment engagement

To ensure that participants had sufficient exposure to the treatment to achieve treatment effects, we test treatment engagement in Appendix Table 3. The respondents covered in this table are all part of the treatment group.

Using a knowledge-based item about information shared in the treatment, we find that 46 percent of respondents completely understood the main elements of the information about the visa lottery. Among those with at least secondary education, this share was 59 percent. Participants who gave incorrect responses received immediate feedback to ensure that they understood the information provided. We also asked respondents whether they trusted the information they received. The vast majority do so, resulting in an average of 2.7 on a 3-point scale.

Interest in the visa lottery and the assistance we offered was high. Around 79% of individuals were interested in receiving assistance with the lottery application. In the end, 101 individuals contacted our lottery assistance hotline. Of these, 35 completed all the necessary forms and submitted their applications with our assistance.

2.8 Hypotheses

We pre-registered six hypotheses related to international migration intentions, entry into the lottery, irregular migration intentions, education and training aspirations for respondents and their children, as well as international migration intentions for their children.These are measured using several outcome variables.

First, we expected the treatment to increase knowledge of and interest in the visa lottery. Second, we expected that receiving the information about the green card lottery will increase international migration intentions. The treatment provides information on a possible legal pathway that many have not been aware of, and lifting this informational constraint may, in

Table 3 – Treatment engagement

	Mean
Interested in assistance	0.787
Contacted lottery support	0.089
Understood application procedure	0.670
Understood school requirement	0.687
Understood work experience requirement	0.560
Trustworthiness of received information (0-3)	2.744
N	1,111

Note: Table shows summary statistics for treatment engagement, values are only available for the treatment sample. All variables except trustworthiness are binary variables.

turn, make migration seem more feasible and more desirable.[11] The same mechanism applies to respondents' migration aspirations for their children. Third, we expected that the U.S. will become a relatively more attractive migration destination. Awareness of a structured legal migration pathway may shift respondents' preferences toward the U.S., especially among those who had previously favored destinations where they had family ties, migration networks, or greater familiarity with migration routes. Learning about a specific, accessible legal migration channel could make the U.S. a more salient and feasible option. Additionally, some respondents could hold a latent preference for the U.S., but lack awareness of legal entry pathways, preventing them from seriously considering it. The intervention provides new information that reinforces such existing positive perceptions of the U.S. relative to European destinations, where obtaining legal status is challenging for most Senegalese migrants. Fourth, we expected that the willingness to migrate irregularly and the willingness to pay for clandestine migration to either Europe or the U.S. would on average decrease following the treatment. This is because the availability of a legal migration alternative should, in theory and on average, reduce the perceived need for irregular migration. Fifth, we expected the treatment to increase participation in the visa lottery. In the 2024 round, we measure this by tracking which respondents reach out to our support team for assistance with their applications. In the 2025 round, we will directly ask respondents whether they submitted their names for the lottery. Sixth, we hypothesized that the treatment could increase respondents' education aspirations for both themselves and their children. Learning about a legal migration pathway with an education requirement could raise the perceived returns to education, thereby increasing education and training aspirations.

One might wonder whether it is in principle reasonable to expect information about a low-probability migration option to shape educational aspirations and to matter for actual downstream educational investments. We think it is, for two reasons. First, note that the probability of winning the visa lottery is not especially low. In Senegal, it is about half a percent *per*

[11] The way our survey question is phrased ("How much, if at all, would you like to live in another country, either seasonally or for a longer time?") is not explicitly independent of constraints. By contrast, the widely used question from the Gallup World Poll ("Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?") is designed to minimize the salience of constraints in respondents' answers.

year, or about a one-in-twenty chance over a ten-year time horizon—and spouses can enter a ticket, too.[12] Given the enormous potential gains in (purchasing power-adjusted) income, this should logically fuel aspirations and actions to ensure eligibility. And in their decision-making, subjects may treat their single-year selection probability as being even higher than it actually is, given well-known cognitive biases concerning probabilities (Kahneman and Tversky 1979, Barberis 2013). Second, a substantial body of literature in education economics has shown that educational aspirations and investments can be shifted through information provision (Jensen 2010, Dizon-Ross 2019, Evans and Acosta 2024).

3 Results

3.1 Main results

We use an intent-to-treat approach to estimate treatment effects on our outcomes of interest and to compare subjects who received information on the U.S. green card lottery with those in the control group. For the most basic estimations, we implement a model of the form

$$Y = \alpha + T\theta + X\beta + \varepsilon,$$

where Y is an outcome of interest, T is a treatment indicator (or a set of treatment indicators), θ our estimand, and X a set of covariates. The vector X includes a pre-treatment measure of the relevant outcome variable, if applicable, village indicators, and additional control variables that are double-lasso selected from among those listed above in Table 1.

3.1.1 Treatment efficacy in raising interest in lottery

Our experiment is only likely to affect outcome variables if it did in fact resonate with participants and generate interest in the green card lottery. We show that this is the case in Table 4, with estimates that one could interpret as a first step in our chain of experimental effects. In the main sample, and in different subgroups relevant for our core analysis below, treatment raises the share of individuals reporting the visa lottery as the pathway they would choose to go in order to migrate to the U.S. by 13 percentage points. This effect is highly significant and consistent in magnitude across all subgroups, including those eligible for the treatment.

3.1.2 Effects on migration intentions

In Table 5, we test for effects of the treatment on migration intentions.[13] Panel A estimates treatment effects for the whole sample. Column (1) shows that the treatment raised the intention to migrate internationally significantly by 0.105 points on a three-point scale, roughly interpretable as every tenth respondent choosing a higher category than they would without

[12] For the case of a constant probability of selection p in each of t years, the total probability of selection across years is given by $\sum_{k=0}^{t-1} \left((1-p)^k \cdot p\right)$.

[13] We show alternative codings, specifications, and estimators in the robustness section. The main results are not qualitatively affected by these changes.

Table 4 – Interest in using lottery pathway

	(1) All	(2) Eligible	(3) Non-elig	(4) Prior irreg = 0	(5) Prior irreg > 0
Treated	0.128***	0.145***	0.121***	0.135***	0.124***
	(0.014)	(0.036)	(0.015)	(0.018)	(0.025)
Observations	2245	527	1718	1459	786
Control Mean	0.037	0.085	0.022	0.039	0.034

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are enumerator and village and network size abroad. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

treatment. Compared to a continuous latent migration intent, the estimated effect is likely to be biased downward by top coding, given that about 63 percent of the respondents choose the highest category of the outcome variable even in the control group. Column (2) shows there is a positive effect on the share of respondents giving the United States as their preferred destination if they were to move to another country. The treatment increases the likelihood of naming the United States as the favorite destination by three percentage points. The control mean highlights that the U.S. is the preferred destination of every fourth respondent. Column (3) shows the impact on the preferred pathway of migration to the U.S., which respondents understood and answered in terms of feasible pathways. The increase by 12.8 percentage points compared to the control mean of 3.7 percent represents more than a 3-fold increase in the share of respondents who prefer this pathway.[14]

Comparing this to our registered hypotheses, we find the expected positive effects on external migration intentions and the lottery pathway. We find a statistically significant impact on destination preferences for the US. This suggests that knowledge about the legal pathway offered by the diversity lottery can influence migration decisions and increases the overall attractiveness of the U.S. as a destination.

While the treatment boosts interest in the diversity visa, it is also associated with an increase in irregular migration intentions. This is in contrast to our registered hypotheses. That said, two-thirds of respondents report not being interested in irregular migration at all. There is more interest in irregular migration among the less well-educated, most of whom will not be eligible for the diversity lottery. This pattern may be caused by changing preferences among those who realize they do not have a legal pathway. Being disappointed by the fact that others can potentially migrate legally, but they themselves cannot, these respondents may have resulted in a mentality that can be summarized by the thought, "All the more reason to go irregularly now." Such a response is consistent with the idea of aspiration frustration, which can occur when aspirations are higher than what can be attained (Genicot and Ray 2017, 2020). The response is also consistent with relative deprivation, which can occur if neighbors or acquaintances are better off while the individual's situation does not improve (Luttmer 2005). Both of these mechanisms can result in more risky strategies to make up for the lack of access to the legal opportunity.

[14]The treatment made the diversity visa the second-most preferred pathway after a relatively unspecific employment-based visa. Less than ten percent of respondents are more specific regarding this and know seasonal work visas or visas for specialty occupations.

In column (5), we see that the willingness to pay for irregular migration does not change significantly. This is contrary to our pre-registered expected decrease in the willingness to pay. Looking more closely at the subgroup of individuals who are eligible for the visa lottery but do not harbor irregular migration intentions in Table A.1, we do find the expected decrease in the willingness to pay for irregular migration, but this group only makes up a small proportion of our sample.

Eligibility: In Panels B and C we disaggregate the treatment effects by eligibility for the visa lottery. Note that only 23.6 percent of the respondents are eligible. Hence, the statistical power is considerably lower than in the full sample. That said, effect sizes for external migration intentions in column (1) are similarly sized to those in the entire sample but statistically insignificant among the eligible respondents. For irregular migration intentions in column (4), we find no evidence among the eligible respondent, with the coefficient turning negative. By contrast, among the ineligible in Panel C, we find a strong positive effect. The positive treatment effect on irregular migration intentions in the full sample is completely driven by this group. For them, the treatment increases the irregular migration outcome by 0.092. This can be interpreted as 1 out of 10 ineligible persons who learn about the visa lottery becoming (one level) more interested in irregular migration. Despite this finding that learning about the visa lottery encourages higher migration aspirations and creates more interest in irregular pathways for the ineligible, the results in column (3) show that the visa lottery becomes a more favored route in the U.S. also for the ineligible. Below, we will assess whether this leads to behavioral change in other domains, such as aspirations to become eligible through getting more education.

Information: Panel D shows results in the subpopulation of individuals without prior knowledge of legal pathways to the US. This group, which makes up 75 percent of all respondents, shows the same pattern as the overall sample. Treatment effects on external migration intentions and destination preferences for the U.S. are larger than in the broader sample and in line with our expectations. Effects on irregular migration are similarly sized but only marginally significant.

Prior irregular migration intentions: In Panels E and F of Table 5, we investigate whether having considered irregular migration previously matters for how awareness of the visa lottery moderates the treatment effect. For this, we separately estimate the treatment effects for individuals based on their priors. Panel D covers respondents who had no intent to migrate irregularly during the same survey but before the treatment. In this group, being given the information about the legal channel offered by the green card lottery increases overall migration intentions by an average 0.159 points, which can be interpreted as every sixth respondent choosing a higher category on the four-point scale. By contrast, in Panel E column (1), we can see that among respondents who already had an intention to migrate irregularly, there is no significant increase in overall migration intentions.[15] The treatment thus substantially increases the wish to migrate for those who rule out irregular migration and who are not yet highly aspiring to migrate.

Turning to column (4), we can see that among those previously not considering migrating

[15]Be aware that even before the treatment, 61 percent of respondents across the full sample and 53 percent of those without any irregular intentions had chosen the highest category.

Table 5 – Intent to migrate - subgroups

	(1) Ext Mig	(2) Dest U.S.	(3) Irreg Mig	(4) log(WtP +1)
A. All individuals				
Treated	0.105***	0.032***	0.071*	0.155
	(0.040)	(0.012)	(0.039)	(0.246)
Observations	2237	2245	2237	2165
Control Mean	2.296	0.261	0.825	4.909
B. Subsample: Eligible for application				
Treated	0.110	0.054**	-0.023	0.119
	(0.095)	(0.027)	(0.082)	(0.584)
Observations	527	527	527	515
Control Mean	2.333	0.211	0.637	4.435
C. Subsample: Not eligible for application				
Treated	0.084*	0.030**	0.092*	0.220
	(0.047)	(0.015)	(0.047)	(0.289)
Observations	1710	1718	1710	1650
Control Mean	2.284	0.277	0.884	5.060
F. Subsample: Did not know legal migration routes				
Treated	0.129***	0.040***	0.084*	0.387
	(0.047)	(0.014)	(0.045)	(0.288)
Observations	1720	1725	1720	1662
Control Mean	2.263	0.250	0.822	4.983
E. Subsample: No prior irregular migration intentions				
Treated	0.157***	0.042**	0.021	0.328
	(0.058)	(0.018)	(0.045)	(0.291)
Observations	1454	1459	1453	1415
Control Mean	2.072	0.251	0.218	1.615
F. Subsample: Prior irregular migration intentions				
Treated	0.054	0.016	0.171**	-0.288
	(0.050)	(0.019)	(0.080)	(0.499)
Observations	783	786	784	750
Control Mean	2.694	0.279	1.897	10.882

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (1) enumerator and village dummies, and priors on intent to migrate regularly and irregularly; (2) enumerator and village dummies prior on destination preferences and family network in the US; (3) enumerator and village dummies and priors on intent to migrate irregularly; and (4) enumerator and village dummies, priors on intent to migrate regularly and irregularly. We report standardized coefficients in Table A.7. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

irregularly, there is no increase in irregular migration intentions. These individuals seem to remain disinterested in irregular migration. The positive treatment effect in column (4) stems from those who had considered irregular pathways before. Among them, the intent to migrate irregularly increases by 0.171. This effect might be larger if it were not for the 41 percent in this subsample who were already previously at the highest level of the answer scale and thus could not increase their intent. So, a seventh of the overall population we are covering increased their resolve to migrate irregularly. This is a fourth of the total sample, i.e., a substantial part of the population. In Table A.1, we show that the increase in irregular migration intentions is driven in particular by those ineligible who had prior irregular migration intentions. A key implication of these results is that the new knowledge about a legal opportunity is unlikely to induce irregular migration if people had ruled it out before but can spark greater resolve to go irregularly in those who were already considering it.

To summarize, among people who consider irregular migration, learning about a legal pathway that they do not qualify for may increase their determination to migrate irregularly. This is a previously undocumented stylized fact.

We emphasize that our results reflect self-reported intentions immediately post-intervention. While migration intentions are predictive of behavior on average (Docquier et al. 2014, Tjaden et al. 2019, Böhme et al. 2020), actual migration is subject to many constraints in addition to the availability of legal options. Our planned one-year follow-up in the fall of 2025 will allow us to assess whether effects persist and whether respondents moved from aspiring to migrate to planning and executing the move.

3.1.3 Effects on education and training aspirations

We also examine downstream aspirations related to education and children's migration, though these are unlikely to be immediately affected given high baseline aspirations.

In Table 6, we study whether the treatment had an effect on the education and training aspirations for the respondent or their children. In column (1), we test this using as outcome an indicator variable for having an education level that is sufficient to be eligible for the visa lottery.

In our baseline specification, we exclude individuals whose education already meets the eligibility criteria and find no statistically significant effect on educational aspirations. However, in Table A.4, we include these eligible individuals (whose outcome variable is always equal to one) to increase statistical power. This yields a similar effect size but more precise estimate, suggesting a weak increase in educational aspirations, consistent with the expectation that the visa lottery's educational requirement may generate a brain-gain effect within the treatment group.

The scope to increase the education aspirations is limited given that almost half the respondents in both treatment and control groups indicate they would like to have a university degree for themselves, and less than 20 percent are satisfied with a level that is insufficient to qualify for the diversity visa lottery. The second possible education requirement is having a qualifying jobs training. Asking respondents whether they would be willing to undergo apprenticeship training we do not find an effect, which is partly the result of respondents having even higher

education aspirations than that.[16]

Education aspirations in rural Senegal are also very high for children. The aspirations for children's education met the threshold for eligibility in the visa category already in 97% of cases in the control group. It may thus be no surprise that the results in columns (3) and (4) indicate no further increase in education aspirations for children from gaining awareness of the green card lottery pathway. These results imply that if education aspirations are already high, we should not expect vast increases in education aspirations from offering legal pathways with education requirements—not even in countries with relatively low current levels of education. Whether respondents have the capabilities to also pursue these high aspirations will be more relevant (cf. Shrestha 2017). If policymakers want to achieve a brain gain effect by incentivizing higher education levels, ensuring sufficient access to education is a crucial policy that should be pursued in parallel.

In column (5), we find that parents' migration aspirations for their children do not increase statistically significantly. A large majority of parents want their children to migrate, with 67% reporting the highest level of aspiration and 18% reporting the second-highest level in the control group. These results suggest that offering or raising the awareness of a legal pathway does not per se increase parents' migration aspirations for their children - these are already extremely high in the Senegalese context.[17]

Table 6 – Aspirations

	(1) Educ	(2) Training	(3) Educ Child	(4) Train Child	(5) Mig Child
Treated	0.033	-0.023	0.000	-0.004	0.058
	(0.021)	(0.050)	(0.008)	(0.025)	(0.043)
Observations	1635	1997	2146	2233	2231
Control Mean	0.747	2.566	0.973	2.760	2.434

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. The outcome variables in columns (1) and (3) are indicators for an education level that makes the respondent or their child eligible for the lottery, respectively. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (a) enumerator and village dummies and education level; (b) enumerator and village dummies, and priors on intent to migrate regularly; (c) enumerator and village dummies; (d) enumerator and village dummies; and (e) enumerator and village dummies, priors on intent to migrate regularly and irregularly, and risk-taking. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

4 Robustness

Our main results in Table 5 are robust to many specifications we have tested. In Table A.5 we code the aspirations variable differently. Instead of the linear interpretation of the outcome variable, we use an indicator for any aspirations or only high aspirations, respectively. The results show no major differences. In Table A.6, we estimate the effects on migration intentions with an ordered probit, which is more appropriate to the ordinal scale of the outcome variable. Again, the results hold qualitatively.

[16] We construct a human capital index using the first principal component of all five variables. The results, reported in Table A.2, show no significant effects across any of the subgroups.

[17] Looking into subgroups in Table A.3, we find the treatment increased migration intentions for children among eligible respondents.

In Table A.8, we modify the composition of the control group by including individuals who could not be reached in person and were originally assigned to receive an alternative information treatment that could not be delivered by phone. In Table A.11, we combine the alternative codings with the larger control group. Again, no major changes to the main results on intentions.

In Table A.12, we estimate the education aspiration effect using a larger control group, with the effect sizes mirroring those in Table 6. The coefficient in column (1) falls slightly out of statistical significance at the 10 percent level, but there is no reason to change the interpretation of the result as a modest effect at best

In Table 7, we test whether treatment effects differ with the time spent on the treatment. We do not find greater effects on regular and irregular migration for longer treatments, but for education, we do. As column (6) shows, the treatment effect increases by 1.1 percentage points per minute spent on the treatment. Anecdotally, treatment duration increased if people asked for details about the exact requirements of the policy. So, for these crucial elements, it makes sense that greater attention to detail is associated with a greater comprehension of the requirements and, thus, a larger treatment effect.

In Tables A.13 and A.14, we test treatment effects without partial inference. The effects on destination preferences remain statistically significant. Additionally, we find a marginally significant positive effect on external migration and a marginally significant negative effect on willingness to pay for irregular migration.

Table 7 – Treatment Intensity

	Ext Mig.		Irreg. Mig.		Education	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.105***	0.086	0.071*	0.143**	0.033	-0.025
	(0.040)	(0.061)	(0.039)	(0.059)	(0.021)	(0.032)
Treatment time (m)		0.002		-0.011		0.010**
		(0.007)		(0.007)		(0.004)
Observations	2237	2237	2237	2237	1635	1635
Control Mean	2.296	2.296	0.825	0.825	0.747	0.747

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. For (1) and (2) selected covariates are enumerator and village dummies. For (3) and (4) selected covariates are enumerator and village dummies, and priors on regular and irregular migration. For (5) and (6) selected covariates are enumerator and village dummies, and education level.(* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Individuals who could not be reached in person were interviewed by phone, which might affect results. Tables A.15 and A.16 display results split by interview mode, revealing that some effects vary by interview type. There is no observable effect on migration intent or destination preferences in phone interviews (columns (1)–(3) in Panel C), and within this sample, willingness to pay for irregular migration declines significantly.

These differential treatment effects may stem either from selection—since phone interviewees, who were absent during village enumeration, likely represent a distinct subgroup—or from genuine differences inherent to the interview format. To address these possibilities, we employ a two-stage procedure that utilizes multivariate distance matching to account for observable differences between the two respondent groups. In the first stage, we generate weights through

multivariate distance matching that balance a range of covariates (including prior migration intentions, age, marital status, education, number of children, risk-taking, networks abroad and within Senegal, and income) across both interview modes, thereby ensuring comparability between the samples. In the second stage, these weights are applied in weighted OLS regressions that control for village and enumerator fixed effects in addition to the matched covariates, yielding estimates of the average treatment effect.

Table A.17 presents the findings from our two-stage procedure. Panel A reports results for the entire sample. Panel B introduces an interaction between the treatment indicator and an indicator for phone interviews; the interaction coefficient captures how the treatment effect differs between in-person and phone interviews. The treatment effect on destination preferences is significantly smaller among phone respondents, while differential effects by interview type are not statistically significant for migration intentions and willingness to pay, as shown in columns (1), (2), and (4). Panel D focuses exclusively on phone interviewees, with effect sizes interpreted as the average treatment effects that would be observed if this subgroup's composition matched that of the overall sample. Comparing these findings with Panel C in Table A.15, we find that the negative effect on the willingness to pay becomes statistically insignificant, which suggests that differences in sample composition cause at least part of the heterogeneity in treatment effects estimates between in-person and phone interviews.

5 Conclusion

This paper presents new evidence on how knowledge of legal migration pathways influences migration aspirations and preferences, including unintended responses. Our randomized controlled trial in rural Senegal demonstrates that providing information about the U.S. Diversity Visa Lottery significantly increases interest in this legal pathway and leads part of the treatment group to register for the lottery. The treatment increases migration intentions. However, an unintended effect emerges: individuals ineligible for the visa lottery—particularly those with lower education levels—become more interested in irregular migration. These are very short-term impacts measured immediately after the treatment. They will be followed up a year after treatment to analyze whether changes in intentions actually resulted in preparations for or actual migrations, whether treated individuals pursued other strategies, or whether effects on intentions diminish over time. Our short-term results suggest that while expanding legal migration pathways can attract individuals, it must be accompanied by carefully designed policies to mitigate potential adverse effects. Our findings highlight the importance of targeted communication strategies. Simply increasing awareness of legal migration pathways may be insufficient or even counterproductive to reduce irregular migration if it creates greater irregular migration among those who remain ineligible. To avoid backfiring, policymakers should consider pairing information campaigns with accessible education and skills development opportunities, thus creating positive incentive effects.

One such positive effect we had expected and hence pre-registered is an increase in education or training aspirations so that people can fulfill the requirements for the visa lottery. Our results indicate only a weak increase in educational aspirations. Aspirations were already so high that

a large majority wants education that would clear the hurdle and is also very open to training. That they do not have this education and training suggests that structural barriers, rather than a lack of motivation, constrain educational attainment. This underscores the need for parallel investments in education access and skill development to improve the education and skill levels in developing countries. In particular, high-income country policymakers who consider recruiting more skilled workers abroad to fill domestic skills gaps should work more towards overcoming structural gaps in education and training. Better educational opportunities would increase the brain gain effects of providing legal pathways for labor migration.

More broadly, our study contributes to understanding migration frictions and the unintended effects of migration policy. The next phase of this study will extend our analysis to examine these longer-term impacts, providing further insights into how migration policy can be designed to maximize positive outcomes while minimizing unintended consequences.

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A Additional specifications and robustness checks

Table A.1 – Intent to migrate

	(1) Ext Mig	(2) Dest U.S.	(3) Irreg Mig	(4) log(WtP +1)
A. All individuals				
Treated	0.105***	0.032***	0.071*	0.155
	(0.040)	(0.012)	(0.039)	(0.246)
Observations	2237	2245	2237	2165
Control Mean	2.296	0.261	0.825	4.909
B. Subsample: No prior irreg. migration intentions and eligble				
Treated	0.077	0.117***	-0.129	-0.223
	(0.137)	(0.039)	(0.091)	(0.641)
Observations	365	365	365	358
Control Mean	2.124	0.192	0.124	1.359
C. Subsample: No prior irreg. migration intentions and ineligble				
Treated	0.156**	0.034	0.061	0.438
	(0.069)	(0.021)	(0.056)	(0.351)
Observations	1089	1094	1088	1057
Control Mean	2.055	0.270	0.248	1.699
D. Subsample: Prior irreg. migration intentions and ineligble				
Treated	0.043	0.018	0.166*	-0.014
	(0.064)	(0.023)	(0.099)	(0.596)
Observations	621	624	622	593
Control Mean	2.683	0.289	1.981	11.042

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (1) enumerator and village dummies, and priors on intent to migrate regularly and irregularly; (2) enumerator and village dummies prior on destination preferences and family network in the US; (3) enumerator and village dummies and priors on intent to migrate irregularly; and (4) enumerator and village dummies, priors on intent to migrate regularly and irregularly. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.) (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.2 – Human capital index

	(1) All	(2) Ineligible	(3) Eligible	(4) No Know.	(5) In-Pers	(6) Phone
Treated	0.035	0.055	-0.056	0.052	-0.016	0.128
	(0.053)	(0.063)	(0.125)	(0.062)	(0.064)	(0.105)
Observations	1902	1435	467	1452	1322	580
Control Mean	0.025	0.029	0.012	-0.007	0.101	-0.162

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are village and enumerator dummies, priors on migration, and age (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.) (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.3 – Aspirations in subsamples

	(1) Educ	(2) Training	(3) Educ Child	(4) Train Child	(5) Mig Child
A. All individuals					
Treated	0.033	-0.023	0.000	-0.004	0.058
	(0.021)	(0.050)	(0.008)	(0.025)	(0.043)
Observations	1635	1997	2146	2233	2231
Control Mean	0.747	2.566	0.973	2.760	2.434
B. Subsample: Eligible indiv.					
Treated		-0.039	0.015	-0.054	0.201*
		(0.113)	(0.010)	(0.071)	(0.107)
Observations		489	508	524	525
Control Mean		2.569	0.992	2.720	2.472
C. Subsample: Did not know legal migration routes					
Treated	0.023	-0.053	-0.005	-0.029	0.034
	(0.024)	(0.059)	(0.010)	(0.028)	(0.048)
Observations	1346	1520	1645	1717	1716
Control Mean	0.737	2.562	0.973	2.776	2.444

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. The outcome variables in columns (1) and (3) are indicators for an education level that makes the respondent or their child eligible for the lottery, respectively. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (a) enumerator and village dummies and education level; (b) enumerator and village dummies, and priors on intent to migrate regularly; (c) enumerator and village dummies; (d) enumerator and village dummies; and (e) enumerator and village dummies, priors on intent to migrate regularly and irregularly, and risk-taking.priors on intent to migrate regularly and irregularly, and risk-taking. Panel B, displays the same results for the sample of eligible applicants, which are individuals with at least a high school degree or a form of job training. Estimation (1) is not available for this subsample as all individuals already have an education level that makes them eligible. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.4 – Alternative education aspirations specifications

	(1) Dummy	(2) Continuous	(3) OProbit
A. Baseline specification			
Treated	0.033	0.207**	0.188**
	(0.021)	(0.090)	(0.081)
Observations	1632	1632	1635
Control Mean	0.748	3.420	3.418
B. Including question order FE			
Treated	0.044*	0.242**	0.210**
	(0.024)	(0.103)	(0.092)
Observations	1632	1632	1635
Control Mean	0.748	3.420	3.418
C. Including eligible individuals			
Treated	0.032*	0.158**	0.153**
	(0.017)	(0.076)	(0.071)
Observations	2160	2135	2137
Control Mean	0.811	3.669	3.665
C. Including eligible individuals and order FE			
Treated	0.032*	0.158**	0.153**
	(0.017)	(0.076)	(0.071)
Observations	2160	2135	2137
Control Mean	0.811	3.669	3.665

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. The outcome variable in column (1) is an indicator for education that makes the respondent eligible for the lottery. The outcome variables in columns (2) and (3) are the education level as ordinal variables (0 = None, 1 = Primary School, 2 = Middle School, 3 = High School, 4 = Professional education, 5 = University). Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are enumerator and village dummies and education level; ** $p < 0.05$, *** $p < 0.01$.)

Table A.5 – Intent to migrate - alternative coding

	(1) Ext Mig > 0	(2) Ext Mig Lots	(3) Irreg > 0	(4) Irreg Lots	(5) WtP > 0
A. All individuals					
Treated	0.028**	0.027	0.033**	0.018	0.015
	(0.014)	(0.019)	(0.016)	(0.014)	(0.018)
Observations	2237	2237	2237	2237	2245
Control Mean	0.877	0.620	0.338	0.204	0.382
B. Subsample: No prior irreg. migration intentions					
Treated	0.041**	0.049*	0.012	0.005	0.025
	(0.020)	(0.025)	(0.019)	(0.014)	(0.023)
Observations	1454	1454	1453	1453	1459
Control Mean	0.819	0.537	0.096	0.044	0.143
c. Subsample: Prior irreg. migration intentions					
Treated	0.008	0.023	0.061*	0.063*	-0.020
	(0.013)	(0.033)	(0.032)	(0.033)	(0.034)
Observations	783	783	784	784	786
Control Mean	0.980	0.767	0.767	0.485	0.806

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and controls including village and enumerator dummies, a set of priors on migration intentions, and socio-economic controls. The matching is performed using multivariate-distance matching on priors and socio-economic controls, between the phoning and in-person interviews. All results are produced using weightings to estimate average treatment effects. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.) (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.6 – Intent to migrate - ordered probit

	All		No Prior irreg int		Prior irreg int	
	(1)	(2)	(3)	(4)	(5)	(6)
	Ext Mig	Irreg	Ext Mig	Irreg	Ext Mig	Irreg
Treated	0.162**	0.238**	0.231**	0.316	0.173	0.505***
	(0.080)	(0.100)	(0.102)	(0.226)	(0.230)	(0.167)
Observations	2235	2235	1452	1451	783	784
Control Mean	2.296	0.825	2.072	0.218	2.694	1.897

Note: Table shows estimation results from ordered probit, and include all priors, controls, and village and enumerator dummies (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.) (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.7 – Intent to migrate - subgroups (standardized coefficients)

	(1) Ext Mig	(2) Dest U.S.	(3) Irreg Mig	(4) log(WtP +1)
A. All individuals				
Treated	0.103***	0.071***	0.058*	0.021
	(0.039)	(0.027)	(0.032)	(0.038)
Observations	2237	2245	2237	2165
Control Mean	-0.025	-0.072	0.026	0.043
B. Subsample: Eligible for application				
Treated	0.117	0.127**	-0.021	0.023
	(0.100)	(0.063)	(0.076)	(0.096)
Observations	527	527	527	515
Control Mean	-0.041	-0.075	0.038	0.108
C. Subsample: Not eligible for application				
Treated	0.081*	0.065**	0.074*	0.028
	(0.045)	(0.032)	(0.038)	(0.044)
Observations	1710	1718	1710	1650
Control Mean	-0.020	-0.072	0.022	0.024
F. Subsample: Did not know legal migration routes				
Treated	0.124***	0.089***	0.068*	0.056
	(0.045)	(0.031)	(0.037)	(0.044)
Observations	1720	1725	1720	1662
Control Mean	-0.048	-0.062	0.006	0.013
E. Subsample: No prior irregular migration intentions				
Treated	0.139***	0.093**	0.030	0.070
	(0.051)	(0.038)	(0.065)	(0.067)
Observations	1454	1459	1453	1415
Control Mean	-0.058	-0.109	0.007	0.012
F. Subsample: Prior irregular migration intentions				
Treated	0.084	0.035	0.140**	-0.045
	(0.079)	(0.042)	(0.065)	(0.088)
Observations	783	786	784	750
Control Mean	0.037	-0.004	0.002	0.056

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (1) enumerator and village dummies, and priors on intent to migrate regularly and irregularly; (2) enumerator and village dummies prior on destination preferences and family network in the US; (3) enumerator and village dummies and priors on intent to migrate irregularly; and (4) enumerator and village dummies, priors on intent to migrate regularly and irregularly. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.8 – Intent to migrate - subgroups (larger control group)

	(1) Ext Mig	(2) Dest U.S.	(3) Irreg Mig	(4) log(WtP +1)
A. All individuals				
Treated	0.103***	0.026	0.076*	0.135
	(0.039)	(0.023)	(0.039)	(0.244)
Observations	2560	2569	2560	2480
Control Mean	2.299	0.275	0.798	4.680
B. Subsample: Eligible for application				
Treated	-0.044	0.073	-0.049	-0.064
	(0.188)	(0.097)	(0.121)	(1.019)
Observations	289	289	289	283
Control Mean	2.270	0.218	0.391	2.495
C. Subsample: Not eligible for application				
Treated	0.123***	0.033	0.078*	0.136
	(0.042)	(0.025)	(0.043)	(0.263)
Observations	2271	2280	2271	2197
Control Mean	2.303	0.283	0.854	4.982
D. Subsample: No prior irregular migration intentions				
Treated	0.149***	0.055*	0.034	0.362
	(0.056)	(0.031)	(0.044)	(0.282)
Observations	1678	1683	1677	1635
Control Mean	2.096	0.276	0.202	1.511
E. Subsample: Prior irregular migration intentions				
Treated	0.049	-0.050	0.156*	-0.441
	(0.050)	(0.041)	(0.082)	(0.505)
Observations	882	886	883	845
Control Mean	2.679	0.274	1.909	10.738
F. Subsample: Did not know legal migration routes				
Treated	0.132***	0.034	0.090**	0.360
	(0.046)	(0.026)	(0.045)	(0.284)
Observations	1954	1960	1954	1888
Control Mean	2.269	0.261	0.820	4.910

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (1) enumerator and village dummies; (2) enumerator and village dummies, and priors on intent to migrate regularly; (3) enumerator and village dummies; (4) enumerator and village dummies; and (5) enumerator and village dummies, priors on intent to migrate regularly and irregularly, and risk-taking. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.9 – Intent to migrate - age subgroups

	(1) Ext Mig	(2) Dest U.S.	(3) Irreg Mig	(4) log(WtP +1)
A. All individuals				
Treated	0.103***	0.071***	0.058*	0.021
	(0.039)	(0.027)	(0.032)	(0.038)
Observations	2237	2245	2237	2165
Control Mean	-0.025	-0.072	0.026	0.043
E. Subsample: Under age 25				
Treated	0.126*	0.040*	0.105	0.176
	(0.071)	(0.023)	(0.082)	(0.484)
Observations	816	820	817	793
Control Mean	2.481	0.255	0.957	5.886
F. Subsample: Over age 25				
Treated	0.088	0.045***	0.079	0.224
	(0.055)	(0.017)	(0.050)	(0.324)
Observations	1342	1346	1342	1295
Control Mean	2.174	0.264	0.722	4.250

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (1) enumerator and village dummies, and priors on intent to migrate regularly and irregularly; (2) enumerator and village dummies prior on destination preferences and family network in the US; (3) enumerator and village dummies and priors on intent to migrate irregularly; and (4) enumerator and village dummies, priors on intent to migrate regularly and irregularly. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.10 – Aspirations in age subgroups

	(1) Educ	(2) Training	(3) Educ Child	(4) Train Child	(5) Mig Child
A. All individuals					
Treated	0.033	-0.023	0.000	-0.004	0.058
	(0.021)	(0.050)	(0.008)	(0.025)	(0.043)
Observations	1635	1997	2146	2233	2231
Control Mean	0.747	2.566	0.973	2.760	2.434
B. Subsample: Under age 25					
Treated	-0.007	-0.006	-0.012	-0.034	0.103**
	(0.049)	(0.085)	(0.015)	(0.049)	(0.077)
Observations	529	723	784	815	815
Control Mean	0.773	2.675	0.980	2.760	2.413
C. Subsample: Over age 25					
Treated	0.062**	-0.014	-0.001	0.022	0.021
	(0.026)	(0.072)	(0.012)	(0.032)	(0.060)
Observations	1049	1202	1287	1339	1338
Control Mean	0.734	2.503	0.967	2.763	2.438

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. The outcome variables in columns (1) and (3) are indicators for an education level that makes the respondent or their child eligible for the lottery, respectively. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (a) enumerator and village dummies and education level; (b) enumerator and village dummies, and priors on intent to migrate regularly; (c) enumerator and village dummies; (d) enumerator and village dummies; and (e) enumerator and village dummies, priors on intent to migrate regularly and irregularly, and risk-taking.priors on intent to migrate regularly and irregularly, and risk-taking. Panel B, displays the same results for the sample of eligible applicants, which are individuals with at least a high school degree or a form of job training. Estimation (1) is not available for this subsample as all individuals already have an education level that makes them eligible. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.11 – Intent to migrate - alternative coding (larger control group)

	(1) Ext Mig > 0	(2) Ext Mig Lots	(3) Irreg > 0	(4) Irreg Lots	(5) WtP > 0
A. All individuals					
Treated	0.028**	0.026	0.032**	0.021	0.015
	(0.013)	(0.019)	(0.014)	(0.014)	(0.018)
Observations	2560	2560	2560	2560	2569
Control Mean	0.887	0.614	0.328	0.195	0.365
B. Subsample: No prior irreg. migration intentions					
Treated	0.039*	0.047*	0.012	0.011	0.030
	(0.020)	(0.025)	(0.016)	(0.014)	(0.022)
Observations	1678	1678	1677	1677	1683
Control Mean	0.838	0.542	0.090	0.041	0.134
c. Subsample: Prior irreg. migration intentions					
Treated	0.007	0.021	0.061**	0.059*	-0.030
	(0.013)	(0.032)	(0.029)	(0.034)	(0.035)
Observations	882	882	883	883	886
Control Mean	0.980	0.750	0.771	0.481	0.797

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and controls including village and enumerator dummies, a set of priors on migration intentions, and socio-economic controls. The matching is performed using multivariate-distance matching on priors and socio-economic controls, between the phoning and in-person interviews. All results are produced using weightings to estimate average treatment effects. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.) (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.12 – Aspirations (larger control group)

	(1) Educ	(2) Training	(3) Educ Child	(4) Train Child	(5) Mig Child
Treated	0.033	-0.019	0.002	-0.011	0.056
	(0.021)	(0.050)	(0.008)	(0.026)	(0.042)
Observations	1856	2320	2449	2554	2544
Control Mean	0.742	2.514	0.973	2.737	2.453

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment and covariates. The outcome variables in columns (1) and (3) are indicators for an education level that makes the respondent or their child eligible for the lottery, respectively. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (a) enumerator and village dummies; (b) enumerator and village dummies, and priors on intent to migrate regularly; (c) enumerator and village dummies; (d) enumerator and village dummies; and (e) enumerator and village dummies, priors on intent to migrate regularly and irregularly, and risk-taking. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.13 – Intent to migrate without partial inference

	(1) Ext Mig	(2) Dest U.S.	(3) Lott Pathw	(4) Irreg Mig	(5) log(WtP +1)
Treated	0.073*	0.034***	0.034***	-0.007	-0.457*
	(0.040)	(0.013)	(0.013)	(0.040)	(0.253)
Observations	1679	1686	1686	1681	1626
Control Mean	2.340	0.243	0.243	0.810	4.786

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (1) enumerator and village dummies, and priors on intent to migrate regularly and irregularly; (2) enumerator and village dummies prior on destination preferences and family network in the US; (3) enumerator and village dummies and priors on intent to migrate irregularly; and (4) enumerator and village dummies, priors on intent to migrate regularly and irregularly. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.14 – Aspirations without partial inference

	(1) Educ	(2) Training	(3) Educ Child	(4) Train Child	(5) Mig Child
Treated	0.019	-0.091*	0.015*	0.006	0.044
	(0.021)	(0.050)	(0.008)	(0.024)	(0.042)
Observations	1224	1516	1614	1677	1676
Control Mean	0.760	2.613	0.967	2.773	2.455

Note:Table shows estimation results without partial interference by comparing treated subjects in treatment villages to subjects in pure control villages. Uses OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (1) enumerator and village dummies; (2) enumerator and village dummies, and priors on intent to migrate regularly; (3) enumerator and village dummies; (4) enumerator and village dummies; and (5) enumerator and village dummies, priors on intent to migrate regularly and irregularly, and risk-taking. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.15 – Intent to migrate by interview type

	(1) Ext Mig	(2) Dest U.S.	(3) Irreg Mig	(4) log(WtP +1)
A. All individuals				
Treated	0.105***	0.032***	0.071*	0.155
	(0.040)	(0.012)	(0.039)	(0.246)
Observations	2237	2245	2237	2165
Control Mean	2.296	0.261	0.825	4.909
B. In-person interviews				
Treated	0.111**	0.032**	0.076	0.379
	(0.047)	(0.014)	(0.049)	(0.310)
Observations	1632	1635	1631	1577
Control Mean	2.314	0.270	0.865	5.326
C. Phone interviews				
Treated	0.070	0.013	0.009	-0.996**
	(0.088)	(0.029)	(0.074)	(0.431)
Observations	605	610	606	588
Control Mean	2.241	0.236	0.706	3.706

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. The selected covariates are: (1) enumerator and village dummies, and priors on intent to migrate regularly and irregularly; (2) enumerator and village dummies prior on destination preferences and family network in the US; (3) enumerator and village dummies and priors on intent to migrate irregularly; and (4) enumerator and village dummies, priors on intent to migrate regularly and irregularly. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.16 – Aspirations by interview type

	(1) Educ	(2) Training	(3) Educ Child	(4) Train Child	(5) Mig Child
A. All individuals					
Treated	0.033	-0.023	0.000	-0.004	0.058
	(0.021)	(0.050)	(0.008)	(0.025)	(0.043)
Observations	1635	1997	2146	2233	2231
Control Mean	0.747	2.566	0.973	2.760	2.434
B. In-person interviews					
Treated	0.029	-0.089	-0.011	-0.020	0.018
	(0.024)	(0.059)	(0.010)	(0.030)	(0.051)
Observations	1215	1394	1572	1631	1630
Control Mean	0.764	2.637	0.976	2.764	2.467
C. Phone interviews					
Treated	0.095**	0.137	0.023	-0.009	0.179**
	(0.045)	(0.099)	(0.018)	(0.051)	(0.090)
Observations	420	603	574	602	601
Control Mean	0.692	2.389	0.966	2.746	2.338

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and covariates. Covariates are double-lasso selected from village and enumerator dummies, a set of priors, and socio-economic controls. For (1) selected covariates are enumerator and village dummies for (2) selected covariates are enumerator and village dummies, and priors on intent to migrate regularly,(3) selected covariates are enumerator and village dummies,(4) selected covariates are enumerator and village dummies,(5) selected covariates are enumerator and village dummies, priors on intent to migrate regularly and irregulary, and risk-taking, Panel B, displays the same results for the sample of eligible applicants, which are individuals with at least a high school degree or a form of job training. (1) is not available for this subsample as all individuals already have an education level that makes them eligible. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

Table A.17 – Intent to migrate PS-matched on interview type

	(1) Ext Mig	(2) Dest U.S.	(3) Irreg Mig	(4)) log(WtP +1)
A. All individuals				
Treated	0.097**	0.038***	0.068*	0.254
	(0.041)	(0.012)	(0.040)	(0.235)
Observations	2230	2236	2230	2158
Control Mean	2.295	0.239	0.820	4.412
B. All individuals (interacted)				
Treated	0.121**	0.054***	0.076*	0.212
	(0.047)	(0.014)	(0.046)	(0.273)
Treated × phoning	-0.067	-0.047**	-0.022	0.119
	(0.069)	(0.021)	(0.067)	(0.396)
Observations	2230	2236	2230	2158
Control Mean	2.295	0.239	0.820	4.412
C. In-person interviews				
Treated	0.126***	0.046***	0.081*	0.473
	(0.046)	(0.014)	(0.048)	(0.299)
Observations	1630	1633	1629	1575
Control Mean	2.310	0.249	0.844	5.050
D. Only phone interviews				
Treated	0.064	0.020	0.026	-0.343
	(0.097)	(0.030)	(0.085)	(0.436)
Observations	600	603	601	583
Control Mean	2.273	0.226	0.788	3.562

Note: Table shows estimation results from OLS regressions of outcome variables on treatment assignment, and controls including village and enumerator dummies, a set of priors on migration intentions, and socio-economic controls. The matching is performed using multivariate-distance matching on priors and socio-economic controls, between the phoning and in-person interviews. All results are produced using weightings to estimate average treatment effects. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.) (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.)

B Additional descriptives

Table B.1 – Descriptives - eligibility

Group	**Level**	**Share**	*N*
Ineligible	Missing	0.008	21
	None	0.407	1 045
	Primary	0.200	515
	Middle School	0.143	367
	Subtotal Ineligible	**0.756**	**1 927**
Eligible	Secondary	0.129	332
	Professional	0.013	33
	University	0.100	256
	Subtotal Eligible	**0.244**	**621**

Table B.2 – Descriptives - eligibility by irregular migration intentions

		Irreg. mig = 0		**Irreg. mig > 0**	
Group	Level	Share	N	Share	N
Ineligible	Missing	0.007	12	0.014	2
	None	0.424	702	0.343	49
	Primary	0.186	309	0.224	32
	Middle School	0.124	205	0.203	29
	Subtotal	**0.739**	**1,216**	**0.780**	**110**
Eligible	Secondary	0.127	210	0.147	21
	Professional	0.019	31	0.007	1
	University	0.113	188	0.063	9
	Subtotal	**0.261**	**429**	**0.220**	**31**

Table B.3 – Descriptives - eligibility by migration intentions

		Intent ext. = 0		**Intent ext. > 0**	
Group	**Level**	**Share**	**N**	**Share**	**N**
Ineligible	Missing	0.015	6	0.009	2
	None	0.480	194	0.398	86
	Primary	0.210	85	0.194	42
	Middle School	0.097	39	0.088	19
	Subtotal	**0.799**	**318**	**0.687**	**147**
Eligible	Secondary	0.097	39	0.148	32
	Professional	0.030	12	0.042	9
	University	0.072	29	0.120	26
	Subtotal	**0.201**	**80**	**0.313**	**67**

C Additional balance checks

Table C.1 – Balance checks in subgroup ineligible

	(1) 0	(2) 1	(3) Overall	(4) (1) vs. (2)	(5) p-value
Age	30.148	30.042	30.095	0.106	0.766
	(0.252)	(0.251)	(0.178)	(0.355)	
No education	0.554	0.526	0.540	0.029	0.234
	(0.017)	(0.017)	(0.012)	(0.024)	
Primary school	0.264	0.266	0.265	-0.002	0.928
	(0.015)	(0.015)	(0.011)	(0.021)	
Middle school	0.172	0.193	0.183	-0.021	0.266
	(0.013)	(0.014)	(0.009)	(0.019)	
Secondary school	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Professional education	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Tertiary	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Is married	0.595	0.573	0.584	0.022	0.349
	(0.017)	(0.017)	(0.012)	(0.024)	
Has children	0.547	0.518	0.533	0.030	0.215
	(0.017)	(0.017)	(0.012)	(0.024)	
Is student	0.028	0.032	0.030	-0.004	0.640
	(0.006)	(0.006)	(0.004)	(0.008)	
Has family abroad	0.541	0.555	0.548	-0.015	0.546
	(0.017)	(0.017)	(0.012)	(0.024)	
Has family in US	0.109	0.132	0.120	-0.024	0.134
	(0.011)	(0.012)	(0.008)	(0.016)	
Has family in Europe	0.420	0.439	0.430	-0.019	0.427
	(0.017)	(0.017)	(0.012)	(0.024)	
Income last month	55027.921	60104.087	57553.234	-5076.166	0.326
	(2501.174)	(4539.229)	(2584.380)	(5168.882)	
Income last 6-months	88875.212	92138.835	90509.978	-3263.623	0.726
	(4801.071)	(7956.966)	(4649.300)	(9301.078)	
Risk taking	6.994	7.324	7.158	-0.329	0.012
	(0.092)	(0.094)	(0.066)	(0.132)	
Prior knowledge of lottery	0.006	0.013	0.009	-0.007	0.126
	(0.003)	(0.004)	(0.002)	(0.005)	
Prior favorite destination US	0.256	0.282	0.269	-0.026	0.217
	(0.015)	(0.015)	(0.011)	(0.021)	
Prior intent ext	2.180	2.179	2.180	0.000	0.994
	(0.039)	(0.040)	(0.028)	(0.056)	
Prior intent int	1.934	1.825	1.880	0.109	0.078
	(0.043)	(0.044)	(0.031)	(0.062)	
Prior irreg desert	0.781	0.721	0.751	0.060	0.302
	(0.042)	(0.040)	(0.029)	(0.058)	
Prior irreg sea	0.829	0.832	0.831	-0.003	0.961
	(0.042)	(0.042)	(0.030)	(0.059)	
N	864	854	1718	1718	

Table C.2 – Balance checks in subgroup eligible

	(1) 0	(2) 1	(3) Overall	(4) (1) vs. (2)	(5) p-value
Age	26.041	26.401	26.216	-0.360	0.479
	(0.352)	(0.367)	(0.254)	(0.508)	
No education	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Primary school	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Middle school	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Secondary school	0.530	0.553	0.541	-0.023	0.599
	(0.030)	(0.031)	(0.022)	(0.043)	
Professional education	0.070	0.027	0.049	0.043	0.022
	(0.016)	(0.010)	(0.009)	(0.019)	
Tertiary	0.400	0.420	0.410	-0.020	0.638
	(0.030)	(0.031)	(0.021)	(0.043)	
Is married	0.248	0.339	0.292	-0.090	0.023
	(0.026)	(0.030)	(0.020)	(0.040)	
Has children	0.226	0.268	0.247	-0.043	0.258
	(0.025)	(0.028)	(0.019)	(0.038)	
Is student	0.400	0.389	0.395	0.011	0.799
	(0.030)	(0.030)	(0.021)	(0.043)	
Has family abroad	0.663	0.673	0.668	-0.010	0.804
	(0.029)	(0.029)	(0.021)	(0.041)	
Has family in US	0.181	0.171	0.176	0.010	0.758
	(0.023)	(0.024)	(0.017)	(0.033)	
Has family in Europe	0.570	0.588	0.579	-0.017	0.690
	(0.030)	(0.031)	(0.022)	(0.043)	
Income last month	88299.320	89529.930	88903.979	-1230.610	0.930
	(9849.375)	(9940.145)	(6984.602)	(13995.426)	
Income last 6-months	75153.992	65114.458	70271.484	10039.535	0.455
	(11331.808)	(6856.677)	(6705.603)	(13422.009)	
Risk taking	6.799	7.027	6.911	-0.228	0.290
	(0.149)	(0.155)	(0.108)	(0.215)	
Prior knowledge of lottery	0.063	0.070	0.066	-0.007	0.745
	(0.015)	(0.016)	(0.011)	(0.022)	
Prior favorite destination US	0.230	0.241	0.235	-0.012	0.754
	(0.026)	(0.027)	(0.018)	(0.037)	
Prior intent ext	2.346	2.219	2.284	0.127	0.185
	(0.065)	(0.070)	(0.048)	(0.096)	
Prior intent int	1.996	2.187	2.090	-0.191	0.067
	(0.075)	(0.072)	(0.052)	(0.104)	
Prior irreg desert	0.590	0.478	0.536	0.111	0.216
	(0.065)	(0.061)	(0.045)	(0.090)	
Prior irreg sea	0.721	0.553	0.640	0.168	0.082
	(0.070)	(0.066)	(0.048)	(0.096)	
N	270	257	527	527	

Table C.3 – Balance checks in subgroup no prior irregular migration intention

	(1) 0	(2) 1	(3) Overall	(4) (1) vs. (2)	(5) p-value
Age	29.810	29.925	29.867	-0.115	0.767
	(0.278)	(0.273)	(0.195)	(0.389)	
No education	0.450	0.414	0.432	0.035	0.180
	(0.019)	(0.018)	(0.013)	(0.026)	
Primary school	0.176	0.197	0.187	-0.021	0.308
	(0.014)	(0.015)	(0.010)	(0.021)	
Middle school	0.123	0.124	0.124	-0.001	0.976
	(0.012)	(0.012)	(0.009)	(0.017)	
Secondary school	0.112	0.138	0.125	-0.026	0.142
	(0.012)	(0.013)	(0.009)	(0.017)	
Professional education	0.024	0.010	0.017	0.014	0.038
	(0.006)	(0.004)	(0.003)	(0.007)	
Tertiary	0.108	0.107	0.107	0.001	0.963
	(0.012)	(0.012)	(0.008)	(0.016)	
Is married	0.552	0.556	0.554	-0.005	0.864
	(0.019)	(0.019)	(0.013)	(0.026)	
Has children	0.508	0.503	0.506	0.005	0.852
	(0.019)	(0.019)	(0.013)	(0.026)	
Is student	0.112	0.114	0.113	-0.002	0.905
	(0.012)	(0.012)	(0.008)	(0.017)	
Has family abroad	0.528	0.559	0.544	-0.031	0.238
	(0.019)	(0.019)	(0.013)	(0.026)	
Has family in US	0.112	0.152	0.132	-0.040	0.027
	(0.012)	(0.013)	(0.009)	(0.018)	
Has family in Europe	0.403	0.448	0.426	-0.044	0.089
	(0.018)	(0.019)	(0.013)	(0.026)	
Income last month	64730.833	69184.305	66957.569	-4453.472	0.473
	(3384.845)	(5198.977)	(3101.249)	(6203.752)	
Income last 6-months	91988.338	89616.330	90795.459	2372.008	0.827
	(6662.229)	(8533.539)	(5418.932)	(10841.790)	
Risk taking	6.733	6.976	6.855	-0.244	0.093
	(0.102)	(0.103)	(0.072)	(0.145)	
Prior knowledge of lottery	0.025	0.025	0.025	0.000	0.983
	(0.006)	(0.006)	(0.004)	(0.008)	
Prior favorite destination US	0.238	0.291	0.264	-0.053	0.024
	(0.016)	(0.017)	(0.012)	(0.023)	
Prior intent ext	1.997	1.958	1.978	0.039	0.554
	(0.046)	(0.047)	(0.033)	(0.066)	
Prior intent int	1.787	1.749	1.768	0.037	0.589
	(0.049)	(0.049)	(0.035)	(0.069)	
Prior irreg desert	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Prior irreg sea	0.000	0.000	0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
N	714	719	1433	1433	

Table C.4 – Balance checks some prior irregular migration intention

	(1) 0	(2) 1	(3) Overall	(4) (1) vs. (2)	(5) p-value
Age	28.083	27.870	27.980	0.213	0.655
	(0.334)	(0.340)	(0.238)	(0.477)	
No education	0.376	0.385	0.381	-0.009	0.792
	(0.024)	(0.025)	(0.017)	(0.034)	
Primary school	0.243	0.217	0.230	0.026	0.379
	(0.021)	(0.021)	(0.015)	(0.030)	
Middle school	0.145	0.194	0.169	-0.049	0.065
	(0.017)	(0.020)	(0.013)	(0.026)	
Secondary school	0.150	0.110	0.131	0.040	0.089
	(0.017)	(0.016)	(0.012)	(0.024)	
Professional education	0.005	0.000	0.002	0.005	0.172
	(0.003)	(0.000)	(0.002)	(0.003)	
Tertiary	0.074	0.079	0.076	-0.005	0.778
	(0.013)	(0.014)	(0.009)	(0.019)	
Is married	0.445	0.449	0.447	-0.004	0.915
	(0.024)	(0.025)	(0.017)	(0.035)	
Has children	0.407	0.380	0.394	0.027	0.431
	(0.024)	(0.025)	(0.017)	(0.034)	
Is student	0.124	0.115	0.119	0.009	0.693
	(0.016)	(0.016)	(0.011)	(0.023)	
Has family abroad	0.640	0.625	0.633	0.015	0.648
	(0.023)	(0.024)	(0.017)	(0.034)	
Has family in US	0.150	0.122	0.137	0.028	0.254
	(0.017)	(0.017)	(0.012)	(0.024)	
Has family in Europe	0.545	0.520	0.533	0.025	0.479
	(0.024)	(0.025)	(0.018)	(0.035)	
Income last month	52337.598	56291.291	54276.596	-3953.693	0.619
	(4150.973)	(6863.613)	(3973.268)	(7952.409)	
Income last 6-months	74656.683	79207.792	76877.440	-4551.109	0.646
	(4765.848)	(8841.683)	(4954.148)	(9916.137)	
Risk taking	7.312	7.767	7.531	-0.455	0.009
	(0.120)	(0.127)	(0.088)	(0.175)	
Prior knowledge of lottery	0.010	0.028	0.018	-0.019	0.050
	(0.005)	(0.008)	(0.005)	(0.009)	
Prior favorite destination US	0.269	0.240	0.255	0.029	0.340
	(0.022)	(0.022)	(0.015)	(0.031)	
Prior intent ext	2.597	2.611	2.604	-0.015	0.795
	(0.041)	(0.038)	(0.028)	(0.056)	
Prior intent int	2.208	2.197	2.203	0.011	0.890
	(0.055)	(0.058)	(0.040)	(0.080)	
Prior irreg desert	2.007	1.921	1.966	0.086	0.289
	(0.056)	(0.058)	(0.040)	(0.081)	
Prior irreg sea	2.200	2.218	2.208	-0.018	0.786
	(0.048)	(0.047)	(0.034)	(0.067)	
N	420	392	812	812	

Table C.5 – Balance checks using larger control group

	(1) 0	(2) 1	(3) Overall	(4) (1) vs. (2)	(5) p-value
Age	28.969	29.200	29.069	-0.231	0.419
	(0.187)	(0.216)	(0.141)	(0.285)	
No education	0.409	0.404	0.407	0.005	0.813
	(0.013)	(0.015)	(0.010)	(0.020)	
Primary school	0.198	0.204	0.200	-0.007	0.670
	(0.010)	(0.012)	(0.008)	(0.016)	
Middle school	0.139	0.149	0.143	-0.010	0.475
	(0.009)	(0.011)	(0.007)	(0.014)	
Secondary school	0.130	0.128	0.129	0.003	0.851
	(0.009)	(0.010)	(0.007)	(0.013)	
Professional education	0.018	0.006	0.013	0.012	0.010
	(0.003)	(0.002)	(0.002)	(0.004)	
Tertiary	0.102	0.097	0.100	0.004	0.719
	(0.008)	(0.009)	(0.006)	(0.012)	
Is married	0.499	0.518	0.507	-0.020	0.320
	(0.013)	(0.015)	(0.010)	(0.020)	
Has children	0.460	0.460	0.460	-0.000	0.983
	(0.013)	(0.015)	(0.010)	(0.020)	
Is student	0.116	0.114	0.115	0.002	0.900
	(0.008)	(0.010)	(0.006)	(0.013)	
Has family abroad	0.569	0.582	0.575	-0.014	0.484
	(0.013)	(0.015)	(0.010)	(0.020)	
Has family in US	0.133	0.141	0.137	-0.008	0.546
	(0.009)	(0.010)	(0.007)	(0.014)	
Has family in Europe	0.459	0.473	0.465	-0.015	0.463
	(0.013)	(0.015)	(0.010)	(0.020)	
Income last month	66303.635	64582.618	65555.402	1721.016	0.707
	(2439.733)	(4147.536)	(2269.562)	(4579.177)	
Income last 6-months	86142.918	85902.440	86038.164	240.478	0.973
	(3877.158)	(6330.032)	(3519.408)	(7099.355)	
Risk taking	7.025	7.255	7.125	-0.229	0.030
	(0.069)	(0.081)	(0.052)	(0.106)	
Prior knowledge of lottery	0.024	0.026	0.025	-0.002	0.736
	(0.004)	(0.005)	(0.003)	(0.006)	
Prior favorite destination US	0.264	0.273	0.268	-0.009	0.623
	(0.012)	(0.013)	(0.009)	(0.018)	
Prior intent ext	2.219	2.188	2.206	0.030	0.502
	(0.029)	(0.035)	(0.022)	(0.045)	
Prior intent int	1.968	1.908	1.941	0.060	0.236
	(0.033)	(0.038)	(0.025)	(0.050)	
Prior irreg desert	0.718	0.665	0.695	0.053	0.256
	(0.031)	(0.034)	(0.023)	(0.046)	
Prior irreg sea	0.792	0.768	0.782	0.024	0.616
	(0.032)	(0.036)	(0.024)	(0.048)	
N	1458	1111	2569	2569	

D Pre-registered hypotheses and phrasing of survey items

- **Hypothesis A (international migration intent)**: Subjects who receive the U.S. green card lottery treatment will express a greater desire to move internationally, to the United States, via the green card lottery.
- **Hypothesis B (pursuing lottery):** Subjects who receive the U.S. green card lottery treatment will get in touch to benefit from the offered support for entering the lottery.
- **Hypothesis C (irregular migration intent):** Subjects who receive the U.S. green card lottery treatment will express lower interest in migrating irregularly, and a lower willingness to pay to migrate irregularly to Europe or the United States.
- **Hypothesis D (educational/training aspirations):** Subjects who receive the U.S. green card lottery treatment who do not yet have a high school/training degree will express a greater desire to get a high school degree/training to fulfill U.S. green card lottery requirements.
- **Hypothesis E (educational/training aspirations for children):** Subjects who receive the U.S. green card lottery treatment will express a greater desire for their children to get a high school degree/training (to fulfill U.S. green card lottery requirements).
- **Hypothesis F (external migration aspirations for children):** Subjects who receive the U.S. green card lottery treatment will express a greater desire for their children to migrate internationally.

Table D.1 – Phrasing of key outcome variables

Hypothesis on	**Description**	**Name in survey form**	**Survey question**
External migration intentions	Indicator: Aspirations	post_intent_external	How much, if at all, would you like to live in another country, either seasonally or for a longer time?
	Indicator: Destination choice	post_intent_ext_destination	If you were to move to another country, where would you most want to live?
	Indicator: Route choice	know_us_visa_lottery	If you would like to migrate to the U.S., which routes would you use?
Pursuing lottery	Indicator: Pursuing lottery	green_card_help	Indicator whether respondent got in touch for help with green card lottery entry
Irregular migration	Indicator: Interest	post_irregular	How interested would you say you are in migrating clandestinely?
	Amount: WTP for irregular relocation	wtp_irreg	How much money would you be willing to pay in order for someone to bring you clandestinely to Europe (the U.S.)?
Educational aspirations	Indicator: Aspirations	asp_educ	No matter whether and which school you are currently attending and how good the grades are: What is the highest school degree you want for yourself?
Training aspirations	Indicator: Aspirations	asp_train	How much, if at all, would you like to undergo apprenticeship training?
Educational aspirations for children	Indicator: Aspirations for children	asp_educ_child_min	No matter whether you have a child or which school your children are currently attending and how good the grades are: What is the highest school degree you would want for him/her?
Training aspirations for children	Indicator: Aspirations for children's education/training	asp_educ_child	No matter whether you have children or not: How important is education and training for your children or future children?
	Indicator: Aspirations for children's apprenticeships	asp_train_child	No matter whether you have children or not: How much, if at all, would you like your children to undergo apprenticeship training?
External migration aspirations for children	Indicator: Aspirations for children	asp_mig_child_ext	No matter whether you have children or not: How much, if at all, would you like for your children to live in another country, either seasonally or for a longer time?

E Example treatment script

Now, we would like to share some information with you about a program that gives people from Senegal like you, your children, or other members of your household or family a chance to migrate to the United States of America legally. All of the information we will tell you is true. You can follow along on this handout, which includes links to official websites where you can verify that what we are telling you is accurate. [Give flyer.] We are not from the government, and we are not telling you what to do, but we are sharing these facts with you because they might be helpful for you to know.

The U.S. Diversity Visa Program, also called the Green Card Lottery, allows people from countries like Senegal to apply for permission to immigrate to the United States. Each year, tens of thousands of people get a chance to live and work in the U.S. through this program, together with their immediate family members.

To enter the lottery, you need to meet one of these requirements: you must have finished secondary school OR have two years of recent work experience in a job that needs at least two years of training or experience. The handout explains how to check if your job qualifies. Entries to the Green Card Lottery need to be submitted using an online form at dvprogram.state.gov. The form is usually available from early October until early November. Only one entry per person is permitted. There is no fee charged by the U.S. government for entering the lottery. On the handout, we list all of the information that applicants will need to enter. Importantly, applicants must enter an email address that they can access, and they must provide photos of themselves, their spouse, and any children under the age of 21. Photos must meet strict requirements, which are listed on the handout.

After you enter the lottery, you will get a confirmation number. Keep this number to check if you are selected in May of the next year. If you are selected, you must complete the process to get a U.S. visa. This includes having passports for all family members, filling out more paperwork, attending a visa interview, and having money to pay visa fees, travel, and initial expenses in the U.S. The handout has a link with more details on this process. In recent years, between 20,000 and 60,000 Senegalese entered the Green Card Lottery, and between 100 and 400 of these lottery entries were selected. The chance of being selected is not high, but you can apply once every year and it is a way to get a chance to move legally to America for anyone who finishes secondary school or works in an occupation that requires extensive training.

F Flyer for treatment

GREEN CARD LOTTERY

Programme américain de visas de diversité/Loterie de la carte verte:
Une chance de vivre et de travailler aux États-Unis d'Amérique

Étape 1

Pour participer à la loterie, il faut aller sur le site **https://dvprogram.state.gov/** entre début octobre et début novembre.

Vous ne pouvez **participer à la loterie qu'une seule fois par an.** L'inscription à la loterie est **gratuite.**

Des informations complémentaires détaillées sont disponibles à l'adresse suivante:

EXIGENCE CLÉ:
diplôme de fin d'études secondaires OU deux ans d'expérience professionnelle au cours des cinq dernières années dans une profession nécessitant au moins deux ans de formation ou d'expérience.

Étape 2

Si vous n'avez pas de diplôme d'école secondaire, déterminez l'expérience admissible: Visitez **https://www.onetonline.org/**. Recherchez une profession. Vérifiez l'échelle SVP (listée sous Job Zone). Vous devez avoir acquis, au cours des cinq dernières années, deux années d'expérience dans une profession dont l'échelle SVP est de 7,0 ou plus.

Étape 3

Pour participer à la loterie, remplissez le formulaire en ligne sur **https://dvprogram.state.gov/** en fournissant les infos suivantes:

- Nom et sexe
- Date, ville et pays de naissance
- Adresse postale
- Adresse électronique
- Niveau d'éducation le plus élevé
- État civil actuel et, si vous êtes marié, nom, sexe et date, ville et pays de naissance de votre conjoint
- Nombre d'enfants vivants, non mariés, âgés de moins de 21 ans et, pour chaque enfant, le nom, le sexe et la date, la ville et le pays de naissance.

Étape 4

Vous devez présenter des photos individuelles de vous-même, de votre conjoint et de tous vos enfants inscrits sur la liste.

Les photos doivent être:

- En couleur, avec une bonne mise au point et sur un fond blanc uni
- Prises au cours des six derniers mois
- Prises de face, en regardant directement l'appareil photo
- Avec une expression faciale neutre et les deux yeux ouverts
- Prises dans des vêtements que vous portez au quotidien
- Pas d'uniformes ou de couvre-chefs, sauf s'ils sont portés quotidiennement à des fins religieuses
- Pas de lunettes, d'écouteurs, etc.

GREEN CARD LOTTERY

Étape 5

Après avoir envoyé votre bulletin de participation à la loterie, **vous recevrez un numéro de confirmation.**
Conservez-le précieusement et imprimez l'écran de confirmation, si vous le pouvez. Vous avez besoin du numéro de confirmation pour savoir si vous avez été sélectionné(e)! **Vous DEVEZ vérifier vous-même si vous avez été sélectionné, à partir du mois de mai de l'année suivante.** Vous ne serez pas contactés pour être informé(e)!

Si vous êtes sélectionné, vous devrez accomplir rapidement la procédure d'obtention d'un visa américain.

Vous trouverez plus d'informations sur cette procédure à l'adresse suivante:

Ces dernières années, entre 20 000 et 60 000 Sénégalais se sont inscrits à la loterie de la carte verte, et entre 100 et 400 d'entre eux ont été sélectionnés.

Si vous avez souhaitez que l'équipe d'enquête fasse pour vous votre inscription à la loterie de la carte verte, alors veuillez nous envoyer toutes les informations et les photos par WhatsApp à [WHATSAPP NUMBER] ou par courriel à [EMAIL ADDRESS]. Vos informations seront traitées de manière confidentielle.

Bonne chance!

Fourni par le projet Sénégal Migration Panel (SMP)