

# Impact of Bank Accounts on Migrant Savings and Remittances: Evidence from a Field Experiment\*

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## ABSTRACT

We use a randomized field experiment to estimate the effect of having a United States bank account on Mexican migrants' savings and remittances. With support from the Mexican Consulate and a local bank targeting Hispanic clientele, we randomly assigned assistance in obtaining a *matrícula consular* card, which we call "treatment." This consulate-issued identification card is accepted by many U.S. financial institutions for the purpose of establishing identity for new accounts, and has little other use in the small U.S. city where we conduct the experiment. Migrants in the treatment group were 38 percentage points more likely to open a U.S. bank account, increased their savings as a share of income by 9 percentage points and decreased their remittances to Mexico as a share of income by 6 percentage points. There is heterogeneity of treatment effects by migrants' reported degree of control over how their remittances are allocated in Mexico. Among migrants who report having no control (as opposed to shared or sole control), treatment causes a higher take-up of U.S. bank accounts, a larger increase in total savings, a shift away from Mexico savings toward U.S. savings, and an increase in income. These results suggest that extending bank access can raise savings in a low-income minority population. Additionally, they suggest that issues of control affect intra-household resource allocations.

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## I. Introduction

One in ten households in the United States do not have a checking, savings or money market account.<sup>1</sup> Poor and minority households are much less likely to be banked: Only 1 in 4 households in the bottom income quintile, and only 1 in 5 nonwhite or Hispanic households, have a bank account. Policymakers propose expanding bank access among these less well-off groups, since access may provide a safer way to store money, decrease financial transactions costs, and increase access to credit. But if unbanked households simply have little money or motivation to save, perhaps providing them bank accounts would have no benefit. Thus, it is an empirical question whether providing bank accounts to the unbanked increases savings.

To obtain causally meaningful estimates of the effect of being banked, we conduct a field experiment in which we randomly assign the cost of bank access. Specifically, we randomly assign undocumented Mexican migrants in a small U.S. city to either a control group or a treatment group that is offered assistance obtaining a *matrícula consular* card: Many U.S. banks accept this card—an ID issued by a Mexican Consulate—as sufficient identification for opening accounts. Among undocumented Mexican migrants, it is the most common identification used to open U.S. bank accounts.<sup>2</sup> The treatment, which helps Mexican migrants obtain a valid ID, thus lowers the cost of bank access. Besides enabling holders to open U.S. bank accounts, the *matrícula* card appears to have little other use in the city of our study. Section II provides details about the *matrícula* card, and Section III describes the experimental design and implementation. Our experiment was successful in generating variation in having a U.S. bank account: treatment group migrants were 38 percentage points more likely to open a U.S. bank account than control group migrants. We exploit this to obtain estimates of the effect of having a U.S. bank account on savings, remittances and income.

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<sup>1</sup> This figure is from Bucks, Kennickell and Moore (2006) based on data from the 2004 Survey of Consumer Finances.

<sup>2</sup> Undocumented migrants generally lack more traditional IDs such as passports and driver's licenses.

Various studies correlate bank access with outcomes such as savings, but in general such correlations need not reflect causal relationships due to selection in who is banked.<sup>3</sup> The only randomized experiment linking bank access to savings in the U.S. has been the Individual Development Accounts (IDA) experiment in Tulsa, Oklahoma (e.g., Mills et al. 2008, Sherraden 2009).<sup>4</sup> People from low- and middle-income households were randomly selected to participate in the IDA program, which provided participants with savings accounts and matched their savings.<sup>5</sup> The IDA experiment shows how provision of special accounts that *subsidize each dollar saved* changes savings. Our study only reduces the treated migrant's fixed cost of *obtaining* the conventional bank accounts that private markets provide.<sup>6</sup> Thus we believe we have the first experimental evidence on the causal effects of having conventional bank accounts in a U.S. setting. This is the primary contribution of our study.

Our study also reveals new aspects of migrant workers' remittance behavior. Remittance flows to developing countries were \$338 billion in 2008 (Ratha, Mohapatra and Silwal 2009) and

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<sup>3</sup> Among these studies, the most related to ours is Amuedo-Dorantes and Bansak (2006), who use data from the Mexican Migration Project to study the impact on Mexican migrants of having a U.S. bank account on remittances and the amount of money brought home in person to Mexico. To address the endogeneity of being banked, they use instrumental variables estimation where the identifying instruments are experience with banking prior to migrating to U.S., dummies for Mexican state of origin dummies and dummies U.S. state of residence during last trip to U.S. A concern is that these variables likely affect remittances and amount of money brought home for reasons other than possession of a U.S. bank account, e.g., they may reflect earnings opportunities or preferences for savings.

<sup>4</sup> Several studies provide estimates of the effect of bank access with a causal interpretation in developing country settings. First, Dupas and Robinson (2009) randomly assign self-employed workers in rural Kenya the opportunity to open a savings account at no cost to them at the village bank. Their study, which involved 185 subjects, found that women in the treatment group had significantly higher expenditures and business investment but there were no effects on men. Because the savings accounts in their study had high withdrawal fees, they resemble commitment savings products like those evaluated by Thaler and Benartzi (2004) and Ashraf, Karlan and Yin (2006). Second, Aportela (1999) and Burgess and Pande (2005) use natural experiments—in particular, policy changes that generate region by time variation in access to formal financial institutions—to identify the effect of being banked in Mexico and India, respectively. Also, Banerjee, Duflo, Glennerster and Kinnan (2010) and Crepon, Devoto, Duflo and Pariente (2011) are randomized evaluations of microcredit programs in India and Morocco, respectively.

<sup>5</sup> Matching rates depend on the type of good that a withdrawal from the account is used to purchase. For example, savings that are withdrawn to purchase a house would be matched at 200 percent.

<sup>6</sup> As will be clearer below when we detail our experiment, we did not provide a specific financial product or accounts with a specific bank as part of our study. Rather, we offered migrants randomly assigned to the treatment group with assistance getting documentation used for opening any U.S. bank account. Migrants were free to open any type of account with any bank, though in practice most who opened an account opened a savings account with very low minimum balance at the local bank that supported us in this study. Though in principle the migrants may have chosen this bank because of its association with this study, the most likely reasons are convenience of location (this bank is within walking distance of a day labor center frequented by most of our study participants) and the tailoring of the bank's services to Spanish-speaking working-class clientele.

these flows are often sizable relative to the receiving country's economy.<sup>7</sup> In 2008, \$26 billion of remittances flowed home to Mexico (about 2 percent of Mexican GDP). Despite the magnitude of remittance flows, little is known about how migrants decide how much to remit. In this study, we learn how having a U.S. bank account impacts migrants' remittance behavior.

In addition, our study sheds light on decision-making within migrant households. A U.S. bank account may offer the migrant more control over his earnings and savings. Without a U.S. bank account, the migrant might send his desired saving flows home as part of his remittances (potential theft might make physical cash holding too risky); but he may have only partial control over the actual allocation of his total remittances once they reach home. A U.S. bank account allows the migrant to safely save without his family's help or knowledge, and may therefore be especially valuable for a migrant who has a greater demand for control over how his remittances are allocated. Our treatment lowers the cost of getting a U.S. bank account, and to the extent that issues of control affect decision-making in migrant households, the treatment effects can be expected to differ between migrants with more and less demand for control.<sup>8</sup> Thus we add to a growing empirical literature on intra-household resource allocation (e.g., Udry 1996, Lundberg, Pollak and Wales 1997, Duflo 2003, Aura 2005, Rangel 2006, Qian 2006, Robinson 2008, Ashraf 2009, Bobonis 2009, and de Mel, McKenzie and Woodruff 2009).

To preview our results, we find that migrants randomly assigned to the treatment group ("treated migrants") increased their total savings flow as a share of income by 9 percentage points and reduced their remittances to Mexico as a share of income by 6 percentage points

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<sup>7</sup> Suro, Bendixen, Lowell and Benavides (2002), Suro (2003) and Orozco (2004) provide descriptive information about remittances to Latin America, including about the remitters, recipients and remittance transfer industry.

<sup>8</sup> Ashraf, Aycinena, Martinez and Yang (2010) have a randomized experiment that is explicitly designed to answer the question of how migrant control affects savings and remittances. They randomly assign migrants from El Salvador residing in the Washington D.C. area to receive accounts from a Salvadoran bank with different degrees of migrant control. Our experiment, on the other hand, is designed to answer the question of how having U.S. bank accounts affects savings and remittances, and speaks to the issue of migrant control by using a measure of demand for control from the baseline survey. Both these studies find that issues of control do impact savings and remittance decisions, with migrants who have greater demand for control at the baseline having higher take-up rates on bank accounts that offer more control and greater increases in household savings.

relative to migrants randomly assigned to the control group (“untreated migrants”) five months after our experiment began.<sup>9</sup> Evaluating the effect at the average income for the untreated migrants, savings increased \$364 over the five months, which is three-quarters of the total stock of savings at the baseline. Almost all of this increase is in savings held in the U.S., and comes largely at the expense of remittances. Thus, there are large distributional consequences—between the migrant and his family, and between the U.S. and Mexico—for policies that impact bank access in U.S. (though we emphasize that long-run total welfare effects are unclear).

We also find heterogeneous treatment effects by the migrants’ reported degree of control over how their remittances are spent in Mexico in the baseline survey. Among migrants who report having no control (as opposed to shared or sole control), treatment causes a higher take-up of U.S. bank accounts, a larger increase in total savings, and a shift away from Mexico savings toward U.S. savings. Additionally, these treated migrants “with no control” increased their income during our study period relative to both untreated migrants and treated migrants “with control.”<sup>10</sup> These findings reject the unitary model for characterizing migrant households, and raise some questions about models that assume efficient households. All these estimation results are discussed in Section IV below, and Section V concludes.

## **II. Mexican Immigrants, Banking and the *Matrícula Consular* Card**

Immigration to the U.S. grew dramatically in the last few decades. In 1970, just 4.8 percent of the U.S. population was foreign-born, but by 2005 this figure rose to 12.4 percent.<sup>11</sup>

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<sup>9</sup> Below, we will continue to use this “treated migrants” and “untreated migrants” terminology. There is ambiguity in “control group” in our study, for it could refer to migrants not in the treatment group or to migrants who express having control over how remittances are allocated in Mexico. To avoid ambiguity, we reserve the word “control” for describing migrant control over decision-making in the household. We emphasize that the treatment is being *offered* assistance obtaining a *matrícula* card, which the treated migrant may or may not take up.

<sup>10</sup> We were surprised to find an impact on income; apparently in some households, problems of control are severe enough, and alternative methods of U.S. savings are perceived as unsafe enough, that migrants actually work less when they do not have a U.S. bank account. We discuss this more below.

<sup>11</sup> The 1970 figure is from the 1970 U.S. Census and the 2005 figure is from the 2005 American Community Survey.

Mexicans accounted for 31 percent of the foreign-born population and 4 percent of the total population in 2005. Passel (2005) estimates that over half of the Mexican migrants in the U.S. are here illegally, with undocumented migrants accounting for over three-quarters of Mexican migrants arriving in 1990 or later.

Mexican immigrants are less likely to have a U.S. bank account than other immigrants. Using the 2001 Survey of Income and Program Participation, Osili and Paulson (2009) find that only 52 percent of Hispanic immigrants have a checking or savings account, compared to 67 percent for all immigrants (and 80 percent for natives). Using data from the Mexican Migration Project, which has more coverage of undocumented migrants and migrants who have returned to Mexico, Amuedo-Dorantes and Bansak (2006) find that among Mexican migrant household heads whose last trip to the U.S. was in the 2000s, 19 percent had a bank account during the last trip, with the share banked higher among legal immigrants (30 percent) than undocumented immigrants (7 percent).

Some argue that difficult documentation requirements explain the high unbanked rate among undocumented migrants (e.g., Bair 2004).<sup>12</sup> Yet the Patriot Act, enacted shortly after the 9/11 attacks, inadvertently made it easier for undocumented Mexican migrants to open U.S. bank accounts. To reduce the financing of terrorism, the Patriot Act required financial institutions to establish customer identification programs for all new accounts “to form a reasonable belief that it knows the true identity of each customer” (31 CFR Part 103.121). Prior to the Act, banks had relied on passports, driver’s licenses, social security numbers, and green cards to establish identity (though no Federal law required this at that time), and undocumented migrants generally lacked these forms of ID. The U.S. Treasury, tasked with implementing these new mandates of

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We use tabulations of the 2005 American Community Survey done by the Pew Hispanic Center (Fry and Hakimzadeh (2006)).

<sup>12</sup> In our baseline survey, 54 percent of migrants said not having an acceptable ID was the reason they did not have a bank account in the U.S.; the next most common reason was lack of knowledge about how to get one (20 percent).

the Patriot Act, endorsed foreign-government-issued identification documents (such as the Mexican *matrícula consular* card) as sufficient proof of identity at banks. As of April 2007, 178 U.S. banks accepted the *matrícula* card to open a bank account.<sup>13</sup>

Demand for *matrícula* cards grew dramatically after the Patriot Act made them useful for opening U.S. bank accounts.<sup>14</sup> To obtain a *matrícula* card, applicants must apply in person, present proof of identity, Mexican nationality, and domicile<sup>15</sup>, and pay a \$27 fee. Proof of identity includes any official identification issued by a Mexican or foreign authority such as driver's license, Mexican state ID cards, Mexican voter ID, U.S. green card, U.S. visas, and even official school records with a photo attached. Proof of nationality includes a Mexican birth certificate, Mexican passport, and certification or declaration of Mexican nationality. Utility bills and correspondence from an official U.S. source are accepted as valid proof of domicile. The *matrícula* card is valid for 5 years, and is a laminated, wallet-sized photo ID card with the name, date and place of birth, and the U.S. address of the bearer. The *matrícula consular* does not depend on or state the holder's immigration status. However, in practice, it is demanded more by undocumented migrants, who lack the traditional forms of identification.<sup>16</sup>

Recognition of *matrícula* cards as valid identification is a decision made individually by local businesses such as banks, law enforcement agencies such as police departments, and local governments. Our interviews with officials, service providers, and migrants in the city where we conducted the experiment, and with consular officers at the Mexican Consulate whose

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<sup>13</sup> Interview with Consular Officer, Mexican Consulate, April 2007. In 2001, Wells Fargo (with more than 3,000 branches in 23 states) became the first bank to accept the *matrícula* card, and other banks followed suit.

<sup>14</sup> The *matrícula consular* dates back to 1871, when Mexican consulates began issuing identification documents for Mexicans living outside Mexico. The *matrícula consular* translates to "consular registration" and enables the Mexican government to track its citizens living abroad for tax and consular reasons.

<sup>15</sup> Specifically, applicants must present proof that they live in a U.S. domicile within the area served by the Mexican consulate issuing the card.

<sup>16</sup> A Mexican passport, too, can be used by Mexican migrants to establish identity, such as for opening a U.S. bank account. While the documentation needed to obtain a passport is not more stringent, the fee is higher (there is a \$27 fee to obtain the *matrícula* card, compared to \$74 for a three-year passport and \$101 for a five-year passport), and a passport is inconvenient to carry around (a *matrícula* card has the dimensions of a driver's license while a passport is bulkier) and so migrants just wishing to have a portable ID opt for the *matrícula* card.

jurisdiction includes this city, indicate that the main reason migrants in this city obtain a *matrícula* card is to have a valid form of identification (either for the purpose of opening a U.S. bank account, or for establishing identity if asked by police). The *matrícula* card does not appear to have any other use for migrants in this city. For example, the *matrícula* card does not help migrants establish legal status in the U.S., obtain work permits or obtain social security cards; Kossoudji and Cobb-Clark (2002) show that legalization increases migrants' earnings. Second, it does not help this city's migrants obtain a driver's license (though it does in a handful of U.S. states); increased worker mobility might be expected to improve earnings opportunities. Third, by city ordinance, the day labor center is the only place in this city where day laborers may solicit work, and use of the day labor center does not depend on having a *matrícula* card.<sup>17</sup> Fourth, a *matrícula* card cannot be used to claim federal and state government benefits such as food stamps, Medicaid and Medicare, welfare or unemployment insurance. Fifth, in the city where we conduct our experiment, local services such as those provided by hospitals and churches are not dependent on having a *matrícula* card. The first three items mentioned allay concerns that having a *matrícula* card improves earnings opportunities directly, which in turn would affect saving and remittances. The last two items allay concerns that having a *matrícula* card reduces out-of-pocket expenditures, which would affect savings and remittances.<sup>18</sup>

To sum up, *matrícula* cards are pivotal for undocumented immigrants wishing to open a U.S. bank account, but do little else for them in our study site. We take advantage of these

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<sup>17</sup> In principle, feeling safer about looking for jobs because one has a valid identification card could improve earnings opportunities. In fact, it is unlikely that the treatment changed perceptions about the safety of soliciting work. This is because in Boomtown, the day labor center is the only place migrants may gather to look for work, and migrants are safe at this center with or without a *matrícula* card.

<sup>18</sup> Additionally, it does not appear that having a *matrícula* card changes a migrant's access to savings or credit products (other than through having a U.S. bank account). On the savings product side, layaway plans are essentially savings vehicles directed toward a purchase of a particular item, and we would underestimate the treatment effect on U.S. savings if having a *matrícula* card did affect the use of layaway plans (because our survey measures of savings would not include funds in layaway plans). However, our interviews indicate that undocumented migrants tend not to make layaway purchases (if they get deported, which could happen at any time, they would have to forfeit the paid-in funds). On the credit products side, migrants tend to borrow only from family and friends, for which a *matrícula* card is not needed.



institutional features to design an experiment that provides exogenous variation in having a U.S. bank account.

### **III. Experimental Design and Implementation**

Our experiment took place in a small U.S. city with a population between 50,000 and 100,000, which will be called Boomtown to protect both the human subjects and the cooperating people of Boomtown. Our experimental design randomly assigns assistance to get a *matrícula consular* card to Mexican nationals who live and work in or near Boomtown. The experiment was conducted with the support of Boomtown's community leaders, the day labor center in Boomtown, a local bank targeting Hispanic clientele, and the regional Mexican Consulate.

We recruited migrants in January 2007, using flyers posted and distributed at the local day labor center, grocery stores, community centers and churches. Most came from Boomtown's day labor center (we had established a good relationship with the migrants and staff prior to the experiment), and most survey interviews took place there as well. All relevant documents (e.g., flyers, questionnaires, information brochures) were in Spanish, and verbal communication (e.g., interviews, information sessions) was conducted primarily in Spanish. Study participants had to satisfy these requirements: (i) Mexican national age 18 or older; (ii) no valid *matrícula* card; (iii) no U.S. bank account; and (iv) remitted within the past 12 months.<sup>19</sup>

We conducted initial interviews, one migrant at a time, during February and March 2007: These interviews yield our "baseline survey" data. At the end of this interview, the migrant was randomly assigned to either the treated or untreated group. While we informed migrants that we were studying their savings and banking practices, they were not explicitly informed that they were in an experiment: We deliberately hid the purpose of random assignment by means of a

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<sup>19</sup> University regulation regarding the privacy of subjects constrained our ability to inquire directly on the migrant's legal status in the U.S. However, the design of the study eliminated green card holders and U.S. citizens.

cover story about limited resources.<sup>20</sup> After five months, we administered a follow-up survey, once again in individual interviews. A total of 215 male migrants (115 treated and 100 untreated migrants) completed our initial baseline survey.<sup>21</sup> Of these, 184 migrants (99 treated and 85 untreated migrants) also completed the follow-up survey five months later.<sup>22</sup> The overall completion rate is 85 percent, and does not significantly differ by treatment status.<sup>23</sup>

Other things equal, we would prefer to estimate a longer-run treatment effect on outcomes of interest, and longer time interval (say twelve months) between our baseline and follow-up surveys would achieve that. Yet given our transient sampled population, this would entail more attrition. As mentioned above, we experienced a 15 percent attrition rate over our five months. Extrapolating this attrition rate to twelve months exponentially or linearly, we might have expected to lose over 30 percent of our sample. Attrition rates this high would substantially undermine the internal validity of our inferences. We are content, though certainly not delighted, to settle for a relatively short-run treatment effect measurement to avoid this issue.

We provided both treated and untreated migrants with the same information regarding the *matrícula* card application process and U.S. bank accounts. Additionally, we provided both groups with a letter that the Mexican Consulate agreed to accept as proof of domicile. However,

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<sup>20</sup> Specifically, each migrant drew an envelope from a box containing identical-looking envelopes. All envelopes contained a letter on University of Houston letterhead that, if filled out and mailed to the migrant, would be accepted by the Mexican Consulate as proof of domicile for the purposes of obtaining a *matrícula* card. Some of these letters had a code indicating assignment of the migrant into the treated group. Migrants drawing uncoded letters were assigned to the untreated group. At the outset of the study, migrants were informed that the demand for transportation to the Mexican Consulate could exceed the spots available through the study, and a lottery system would be used to establish who would receive transportation to the Consulate. Put differently, the random assignment was represented to migrants as a consequence of our own limited budget, not as a treatment per se.

<sup>21</sup> We designed the experiment in order to get slightly more people in the treatment group because *ex ante* we were concerned about low take-up rates in U.S. bank accounts.

<sup>22</sup> Though the study was open to women, in fact we got only 11 female migrants, so we decided to drop women from the study. In the tables below, we will drop one of the 184 migrants, who was quite an outlier with respect to savings rates (his Mexico savings as a share of income was 149 percent with 40 percent being the next highest value; his U.S. savings were -134 percent with -75 percent being the next lowest value). Including this migrant in the analysis does not change any of the findings, though it does make the results related to savings as a share of income less precise.

<sup>23</sup> Nor do migrants leaving the sample look significantly different (based on observed characteristics) compared to non-attriters.

only the treated migrants were offered the following additional assistance in obtaining a *matrícula* card and U.S. bank account. First, we checked whether their documents met the stated requirements for obtaining a *matrícula* card, and helped them obtain missing documents and/or fix problems with their documents.<sup>24</sup> Once documents were in order, they were instructed to make photocopies and to obtain passport-sized photos. Second, we arranged appointments at the Consulate, provided transportation to the Consulate, and helped fill out application forms for the *matrícula* cards. Third, we paid the \$27 *matrícula* card application fee. Finally, treated migrants were given reminders to open up a U.S. bank account. For example, in the Consulate, bank representatives attempted to recruit new recipients of *matrícula* cards to open accounts.

Table 1 suggests that the randomization successfully produced comparable treated and untreated groups. Demographic characteristics, such as age, marital status, education and years in Boomtown, show no significant differences between the groups. Nor do income, remittances, and savings flows in the baseline survey: This is particularly important, since these are our primary outcomes of interest in the follow-up survey.<sup>25</sup> In some specifications, we will allow treatment effects to vary by the migrant's reported degree of control over how remittances are allocated in Mexico, and Table 1 shows that reported control is similar between untreated migrants (46 percent report having no control) and treated migrants (43 percent).<sup>26</sup>

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<sup>24</sup> The Consulate had provided our interviewers with training to recognize valid documents.

<sup>25</sup> We provide details about the measurement of income, savings and remittances in Section IV.C, when we present estimates of the effect of treatment on income, savings and remittances.

<sup>26</sup> Our measure of migrant's reported control is formed based on the question in the baseline survey, "Do you feel you have total control over or total decision making over how the money you send to Mexico is spent or invested?" The possible responses were: (1) "Yes, I have total control over how money I send is spent or invested"; (2) "The person I send money to has more control than I do over how money I send is spent or invested, but I also have some influence in the decisions"; and (3) "The person I send money to makes all the decisions over how money I send is spent or invested." Migrants who give the first response are referred to in the text as reporting full or sole control, those who give the second response are referred to as reporting joint or shared control and those who give the third response are referred to as reporting no control.

## IV. Results

### A. Effect of Treatment on Obtaining a *Matrícula* Consular Card

Ultimately we want to know whether treated migrants open more bank accounts, and change their remittance and/or saving behavior, relative to untreated migrants. Yet our treated migrants are offered assistance obtaining a *matrícula* card—we do not directly give them bank accounts. The causal chain is “treatment  $\Rightarrow$  more *matrícula* cards  $\Rightarrow$  more bank accounts and changes in remittances and savings.” It makes sense to begin at the first link in this chain, that is, with the effect of the treatment on the probability of obtaining a *matrícula* card. Because treatment status was randomly assigned, we avoid the selection bias that arises when individuals choose their own treatment status, and thus are able to obtain the causal impact of treatment. The average treatment effect is simply the difference between the mean outcomes of treated and untreated migrants. A simple OLS regression model can compute this difference in means and simultaneously allow for the addition of covariates and/or interactions:

$$(1) \quad y_i = \alpha + \beta T_i + \pi X_i + \varepsilon_i$$

for individual  $i$  where  $y_i$  is some outcome (measured in the follow-up survey),  $T_i$  is the dummy indicating treatment status (=1 for being randomized into the treated group, =0 otherwise), and  $X_i$  is a set of individual characteristics (measured in the baseline survey, before treatment status is known). The coefficient  $\alpha$  gives the mean outcome for untreated migrants and the coefficient  $\beta$  gives the difference between the mean outcomes of treated and untreated migrants.

We begin by estimating eq. 1 with the outcome  $y_i = 1$  if  $i$  obtains a *matrícula* card by the time of the follow-up survey (zero otherwise). Column 1 of Table 2 shows that treated migrants were 81 percentage points more likely to obtain a *matrícula* card than untreated migrants. Column 2 shows that this is robust to the inclusion of a variety of individual characteristics as regressors  $X_i$ —not a surprising result, given the high similarity of treated and untreated migrants

shown in Table 1. Given this insensitivity to regression-adjusting for  $X_i$ , the estimation results we display in subsequent tables will leave out  $X_i$ . The treatment successfully caused people to obtain *matrícula* cards and, as a result, become “eligible” to open U.S. bank accounts. The effect size is huge, potentially improving our power to detect effects of having a U.S. bank account.

### *B. Effect of Treatment on Opening a U.S. Bank Account*

We now estimate eq. 1 with the outcome  $y_i = 1$  if  $i$  obtains a U.S. bank account by the time of the follow-up survey (zero otherwise). Column 5 of Table 2 shows that treated migrants were 38 percentage points more likely to obtain a U.S. bank account than untreated migrants, and column 6 shows that inclusion of a variety of individual characteristics (as regressors  $X_i$ ) hardly changes the estimated treatment effect. The treatment successfully caused a substantial number of people to obtain U.S. bank accounts. The experiment was successful in generating substantial variation in having a U.S. bank account, and we take advantage of this below to study how having a U.S. bank account affects decisions about income, savings and remittances.

### *C. Effect of Treatment on Income, Savings and Remittances*

When migrants gain access to U.S. banks, there may be compositional changes in savings as well as changes in total savings. The old vehicle for U.S. savings was holding cash, and having a U.S. bank account could cause migrants to shift U.S. savings away from cash holdings toward U.S. bank deposits. Mexican savings vehicles could also become less attractive,<sup>27</sup> or the household could simply want more diversification: In either case, the household may shift away from Mexican toward U.S. savings, implying lower remittances to Mexico as well (migrants must remit funds to place them in Mexican savings vehicles). Total savings may increase if the U.S. bank account relaxes certain saving constraints. For example, alternative savings vehicles could have been so unattractive that, in the absence of a U.S. bank account, the household chose

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<sup>27</sup> For example, perceived exchange rate or inflation risks could favor U.S. holdings; access to formal banks may be poorer in Mexico; or the U.S. banking system could be perceived as relatively more trustworthy.

more current consumption (e.g., consuming a dollar today is better than holding the dollar for tomorrow when the risk of theft is high); in this case, total savings may rise.

Panel A of Table 3 shows estimated effects of treatment on income, remittances, U.S. savings, Mexico savings and total savings. All these outcomes are measured as *flows* over the five months leading up to the follow-up survey. We compute the five month savings flows as the difference between savings stocks reported at the times of the follow-up and baseline interviews<sup>28</sup> (a negative number indicates reduced saving). For one set of savings outcomes, we scale savings flows by the migrant's income earned over the same period. For a second set of savings outcomes, we have dummy indicators for whether the five month savings flow is positive; this captures behavior at the extensive margin and is also less sensitive to outliers.

We find that treated migrants remit 6 percentage points less of their income and save 9 percentage points more. Evaluating the treatment effect at the average income for untreated migrants, savings increased \$364 over the five months, which is three-quarters of the total stock of savings at the baseline. Virtually all the increase in savings is in the form of U.S. savings: U.S. savings as a share of income increase 8 percentage points, and treated migrants are 38 percentage points more likely to have increased their U.S. savings over the five-month period. In contrast, there is no significant effect on savings held in Mexico, either on the intensive or extensive margins. Finally, we find that treated migrants earn \$226 more than untreated migrants, but this is not quite significant at conventional levels (the p-value is 0.13).

The significant increase in total savings suggests that a U.S. bank account expands the migrants' choice set in a consequential manner. Access to a U.S. bank account causes migrants

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<sup>28</sup> Savings are hard to measure, and we measured it in an alternative way, by asking migrants to directly report their saving flow (additions to saving) over the five months between the two surveys. This alternative measurement is very similar to the dependent variable used here, which is the difference between the migrant's reported saving stocks in the two separate surveys. Generally, the survey questions were structured to allow for checks of accounting equivalences such as this. In all cases, migrants' responses to income and consumption questions, and saving stock and flow questions, showed a reasonable degree of accounting consistency between the two surveys with no systematic biases.

to keep more earnings in the U.S. (presumably in their U.S. account) and send less back home.

#### *D. Instrumental-Variables Estimates of the Effect of a U.S. Bank Account*

Thus far, we have presented estimates of the effect of treatment. Recall the exact definition of treatment: migrant  $i$  is *offered* assistance obtaining a *matrícula* card. A treated migrant may fail to obtain the card. While these “intention-to-treat” estimates are informative, we may wish to explicitly quantify the effect of being banked rather than the effect of intention-to-treat. Under the assumption that treatment status affects income, remittances and savings *only* through affecting the likelihood of having a U.S. savings account, then it is possible to obtain estimates of the effect of having a U.S. bank account. This assumption is reasonable in the Boomtown context of our experiment: As discussed in Section II, the *matrícula* card has little use in Boomtown other than for establishing identity for new bank accounts.<sup>29</sup>

Thus, we can proceed by using treatment status ( $T_i$ ) as an instrumental variable for having a U.S. bank account in the following equation:

$$(2) \quad y_i = \alpha_2 + \beta_2 \text{USBank}_i + \pi_2 X_i + \varepsilon_{2i}$$

where  $\text{USBank}_i$  is a dummy variable indicating whether individual  $i$  had a U.S. bank account at the time of the follow-up survey (=1 if have an account, =0 otherwise). The corresponding first-stage equation is eq. 1 where the dependent variable is  $\text{USBank}_i$  (see column 5 of Table 2).<sup>30</sup>

Table 4 shows the results of estimating eq. 2 by 2SLS. First, having a U.S. bank account increases total savings as a share of income by 23 percentage points. This increase comes almost entirely from the increase in U.S. savings: U.S. savings as a share of income significantly

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<sup>29</sup> Additionally, when we allow treatment effects to vary by whether the individual opens a bank account, we find that the significant effects on income, savings and remittances are coming from treated migrants who opened a U.S. bank account. Treated migrants who did not open a U.S. bank account—recall many of them did obtain a *matrícula* card through our experiment—have outcomes that are similar to the untreated migrants’ outcomes. This reinforces the institutional information that having a *matrícula* card, and more generally, the treatment we offered, does not affect the outcomes through mechanisms other than having a U.S. bank account.

<sup>30</sup> Here, the two-stage least squares estimate of  $\beta_2$  in eq. 2 is simply the effect of the intention to treat on  $y_i$  divided by the effect of the intention to treat on  $\text{USBank}_i$ . In Table 2, we saw that treated migrants were 38 percentage points more likely to open a U.S. bank account. Then to obtain the effect of having a U.S. bank account on income, remittances or savings, one would multiply the estimated intention-to-treat effect by  $1/.38 \approx 2.6$ .

increases by 21 percentage points while there is no significant impact on Mexico savings. Opening a U.S. bank account basically guarantees having an increase in U.S. savings over the five-month period between the baseline and follow-up period (the coefficient in Column 4 is not statistically different from one). In contrast, there is no change in probability of having an increase in Mexico savings (Column 6). Second, having a U.S. bank account significantly decreases remittances as a share of income by 16 percentage points. Finally, there is a positive effect on income, hovering at the edge of weak significance (p-value is 0.11).

Given these sizable and significant effects of having a U.S. bank account—for example, from Table 4, the increase in income of \$592 is almost 15 percent of the mean income of untreated migrants and total savings as a share of income increases 23 percentage points—it is worth considering why more people in our experiment did not get a U.S. bank account. We discuss the two layers of this question: (1) why didn't more people get a *matrícula* card, and (2) conditional on getting a *matrícula* card, why didn't more people open a U.S. bank account?

We provided *both* treated *and* untreated migrants with verbal and written information about applying for *matrícula* cards and opening U.S. bank accounts. If lack of such information were the crucial barrier, this information provision should have induced many untreated migrants to go to the Consulate on their own, obtain a *matrícula* card and subsequently open a U.S. bank account. In the baseline survey, many migrants claimed that lack of information was the main reason they didn't have a *matrícula* card or U.S. bank account.<sup>31</sup> Yet only 6 percent of the untreated migrants (i.e., 5 out of 85 people) obtained a *matrícula* card, compared to 87 percent of the treated migrants; and only 5 percent of the untreated migrants (4 out of 85 people) obtained a U.S. bank account, compared to 43 percent of the treated migrants. This strongly suggests that information provision alone would fail to significantly reduce the unbanked rate.

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<sup>31</sup> In the baseline survey, asked why they did not have a *matrícula* card, 43 percent of migrants reported “I don't know how to get one.” Asked why they did not have a U.S. bank account, 20 percent reported “I don't know how to open a bank account here.”



Now consider the 85 treated migrants who did obtain a *matricula* card. About half of them, 42 migrants, subsequently obtained a U.S. bank account. Curiously, more than half of the remaining 43 migrants (who obtained the card but not an account) had reported in the *baseline* survey that lack of a *matricula* card was what kept them in the unbanked state! Perhaps inertia and status quo biases (Samuelson and Zeckhauser 1988, Kahneman, Knetsch and Thaler 1991) keep some migrants in an unbanked state. We offered treated migrants several forms of assistance to move from the “no valid ID” state to the “valid ID” state: we helped vet and/or obtain their documents, we arranged Consular appointments, we provided transportation to the Consulate, we helped fill out applications, and we paid application fees. None of these steps seemed overly difficult or costly to us, but collectively these steps were sufficient to make untreated migrants 81 percent less likely to get a *matricula* card relative to treated migrants.<sup>32</sup> However, we did not provide treated migrants with any assistance to move from the “valid ID” state to the banked state. The remaining steps were going to a bank branch, filling out an application and making an initial deposit (which was as low as \$25). These additional steps seem “small” in the Boomtown situation: Several local banks provide service in Spanish, account types tailored to lower-income families, and convenient hours and locations. Nevertheless, the trouble of these remaining steps could be partly responsible for why half of the treated migrants who got a *matricula* card remained unbanked at the time of the follow-up survey. Studies have shown “small situation barriers” like these impede the unbanked from opening a bank account (Bertrand, Mullainathan and Shafir 2004, Mullainathan and Shafir 2009).

Migrants may also underestimate how their earnings, savings and remittances could

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<sup>32</sup> Access to transportation (to travel to the Mexican Consulate) and having all the relevant documents for obtaining a *matricula* card might appear to be major barriers. First, we observed that most migrants appeared to have direct or indirect access to transportation. Migrants either had their own car, or had access to one. One of the board members of the day labor center makes weekly trips to the consulate and occasionally takes workers to the consulate if they request a ride. Second, given the speed and the ease which the people in the treatment group collected their documents from Mexico, we do not believe the lack of documents is a binding constraint either.

change if they become banked. In interviews, some migrants said that they lacked the funds to make a U.S. bank account worthwhile. Although our evidence on this does not quite reach significance, recall that our 2SLS point estimates (Table 4) suggest that having a U.S. bank account increases earnings. Suppose this effect is real. Migrants might underestimate or wholly fail to anticipate this,<sup>33</sup> and so fail to realize that their own changed earnings would generate the very funds needed to maintain an account and make it worthwhile. Possibly, with more time, social interactions among banked and unbanked migrants will transmit more accurate expectations of the effects of having a U.S. bank account, and take-up will increase.

#### *E. Heterogeneity in Treatment Effects by Migrant's Reported Control*

Our reading of the literature on decision-making within households, as well as informal conversations with the migrants, suggest that each migrant's demand for control over remittance spending in Mexico could be an important mediator of the treatment effects that we estimate. Migrants tend to state a greater preference for saving than their family back home.<sup>34</sup> If the migrant lacks confidence that the family back home will abide by his wishes to set aside money for the future; and if the migrant views his own U.S. cash holdings as an unacceptably risky substitute; then a U.S. bank account could provide a solution, resulting in higher U.S. savings as the migrant redirects funds from Mexico to the U.S. This might be the reason for the large (but imprecisely estimated) earnings effect of the U.S. bank account we see in Table 4: With no safe place to store money in the U.S., and lack of control over remittances to Mexico, the migrant may work less than otherwise. It may be that many of the treatment effects we have reported mostly come from migrants with greater demand for control, while these effects are smaller or nonexistent for those migrants who have no significant demand for control.

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<sup>33</sup> Indeed, *we* failed to anticipate this and it surprised us. The only reason we collected information on income was to help us measure savings in alternative ways (i.e., when combined with information we collected on consumption).

<sup>34</sup> Ashraf et al. (2010) ask both U.S.-based migrants and their remittance-receiving households in El Salvador how the recipient households should spend a \$100 raffle winning. Migrants wish for 21 percent to be allocated to savings while the recipient households only wish for 3 percent.

We allow for heterogeneity in treatment effects by reported degree of control over remittance spending using the following equation:

$$(3) \quad y_i = \alpha + \beta_C T_i + \beta_{NC} T_i \times \text{NoControl}_i + \delta \text{NoControl}_i + \pi X_i + \varepsilon_i$$

where  $\text{NoControl}_i = 1$  if migrant  $i$  reports having no control (in the baseline survey, before treatment is assigned) over how his remittances are allocated by his home-country recipients, and is zero otherwise. Thus  $\beta_C$  is the treatment effect among migrants who report *having* control (either sole or shared control),<sup>35</sup> while  $\beta_{NC}$  is the additional treatment effect among migrants who report having *no* control (for these latter people, the total intention-to-treat effect is  $\beta_C + \beta_{NC}$ ).

Column 3 of Table 2 reports the results of estimating eq. 3 with “obtains a *matrícula* card” as the dependent variable. There is no evidence that the propensity to get a *matrícula* card depends on reported degree of control—the estimated  $\beta_{NC}$  does not differ significantly (either statistically or practically) from zero. However, column 7 of Table 2 shows that the propensity to follow through and open a U.S. bank account depends strongly on reported degree of control. Treated migrants “with control” are 21 percentage points more likely to open a U.S. bank account than untreated migrants “with no control.” This effect is 41 percentage points greater for migrants who report no control: Overall the average treatment effect for migrants “with no control” is 62 percentage points. Put differently, treated migrants “with no control” were three times more likely to open a U.S. bank account than treated migrants “with control.”

Of course, reported degree of control is not assigned randomly to migrants, and it may be correlated with other observed or unobserved characteristics. In this case, the interaction  $\beta_{NC}$  will not simply reflect degree of control, but will also reflect those other correlated characteristics. Migrants’ education and reported degree of control are positively correlated in the sample, so the estimates of  $\beta_{NC}$  in Columns 3 and 7 of Table 2 partly reflect differential treatment effects by

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<sup>35</sup> We cannot reject that the effect of treatment on “obtains a *matrícula* card” and “obtains a U.S. bank account” are the same between migrants reporting sole control and migrants reporting shared control, therefore we combine these two categories in the empirical analysis below.

education level.<sup>36</sup> Therefore, we augment eq. 3 with interactions between treatment status and years of schooling. We still find that degree of control does not affect the likelihood that a migrant obtains a *matrícula* card (Column 4 of Table 2), but does significantly affect the likelihood that he opens a U.S. bank account (Column 8 of Table 2).

Panel B of Table 3 reports the results of similar estimations that allow treatment effects on income, savings and remittances to differ by reported degree of control. Treatment caused migrants “with no control” to earn \$575 more, remit 9 percentage points less as a share of income, and save 12 percentage points more as a share of income; treatment also changed the composition of their savings (increasing U.S. savings flows and decreasing Mexican savings flows); and all these effects are significant at the 5 percent level. Treatment also caused migrants “with control” to increase their total savings flows, but only by half as much. However, in contrast to migrants “with no control,” there is no significant impact of treatment on either the income or remittances of migrants “with control,” nor do they shift savings toward U.S. vehicles (their 6 percentage point increase in total savings as a share of income is almost evenly divided between the U.S. and Mexico). The differential effects by reported degree of control are similar qualitatively even after controlling for an interaction between treatment status and years of schooling though the magnitude and precision decrease somewhat (Panel C of Table 3).

These findings soundly reject the unitary model of the household for describing the behavior of the migrant households in our sample. If these households were unitary households, then the introduction of a financial product offering the migrant more control (i.e., the U.S. bank account) should not affect intra-household resource allocation differentially by migrant’s demand for control. In fact, it is the migrants who might have greater demand for control (i.e., migrants reporting no control over remittance allocation) who are especially likely to take up the financial

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<sup>36</sup> We also allowed for other interactions with treatment status—in particular, age, marital status and family composition—and found that the coefficient for the interaction between the treatment dummy and the dummy for reporting no control did not change much.

product offering more control. Additionally, the treated migrants “with no control” show a shift in savings away from Mexico toward the U.S.; there is no shift in the country composition of savings for treated migrants “with control.”<sup>37</sup> This adds to the set of studies that test the unitary model (e.g., Lundberg, Pollak and Wales 1997, Duflo 2003, Aura 2005, Rangel 2006, Qian 2006, Bobonis 2009). These studies tend to reject the unitary model in favor of a more general collective model that allows different household members to influence decision-making.

Most collective models, though, assume that households are Pareto efficient (e.g., Chiappori 1988, Lundberg and Pollak 1993, Browning and Chiappori 1998, Bourguignon, Browning and Chiappori 2009). We find a positive treatment effect on income among migrants who report no control, but no significant treatment effect on income for others. This suggests that in households where the migrant might have a greater demand for control, resource allocation may have been less efficient before the U.S. bank account was available. The migrant works harder as a result of the treatment, leading him to earn \$575 more on average, which is 14 percent of untreated migrants’ income over the same five-month period!<sup>38</sup> The migrant appears to value the income gained from an additional hour of work more than an additional hour of leisure, but before he obtains a U.S. bank account he is unable to ensure that the extra income will be allocated properly. If the migrant expects his family to misallocate it, and feels he cannot safely store it in the U.S.<sup>39</sup>, then he might be better off working less and taking more leisure. If

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<sup>37</sup> We emphasize that the finding that treated households shifts savings out of Mexican vehicles toward U.S. ones is not sufficient to reject the unitary model; the U.S. bank account is a new and potentially superior savings vehicle for the household— regardless of whether it is unitary—so the household may decide to shift savings to the U.S. However, we do not observe that shift among treated migrants “with control.” It is the differential response between treated migrants with and without control that leads us to reject the unitary model.

<sup>38</sup> Unfortunately in our survey, we did not have questions about labor market outcomes. For tractability, we focused our survey on savings and remittances, and in fact the only reason we collected information on income was to help us measure savings in an alternate way (i.e., when combined with information on consumption). Thus, based on our data alone, we cannot distinguish whether the treatment effect on income comes from higher wages or more hours worked. However, at the day labor center where most of our migrants were drawn, workers had much more control over how much they worked than how much they were paid. Daily wage rates were roughly \$80-\$100 around this time, so the \$575 effect implies 5-7 extra days of work over the five-month period.

<sup>39</sup> Just like U.S. bank account holdings, U.S. cash holdings are under the exclusive control of the migrant. Thus, in households with conflicts over resource allocation, the migrant always has the option of holding more in cash in the

the family were able to commit to allocating the income more in alignment with the migrant's wishes, then a Pareto improvement can be achieved—the family should let the migrant work at the optimal level and save the income as he requests in exchange for, say, a lump-sum payment. The U.S. bank account eliminates the need for family commitment by allowing the migrant to safely save money without the family's help or knowledge. The U.S. bank account therefore seems to raise efficiency, implying that migrant households were behaving inefficiently. This adds to a small set of studies that test whether the household is efficient (e.g., Udry 1996, Ashraf 2009, Bobonis 2009, de Mel, McKenzie and Woodruff 2009, Robinson 2008).

It is perhaps surprising that even within a nuclear family—55 percent of the migrants are remitting to their wives, and 41 percent to a parent, in the baseline survey—efficiency is not attained. This contrasts with Bobonis (2009), who also looked at Mexican households.<sup>40</sup> Using two sources of household income shocks (one from rainfall shocks, the other from Progresa transfer payments to mothers), Bobonis rejects the unitary model of the household but cannot reject the collective model. We suspect that one likely reason for our different results is that we are looking at geographically separated migrant households. Geographic separation means that family members interact less frequently, suffer more from asymmetric information problems, and have worse technologies for enforcing agreements (e.g., Chen 2006), all of which can reduce efficiency.

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U.S. The significant positive treatment effects on income, U.S. savings and total savings among migrants “with no control” migrants that are in excess of the treatment effects found for “with control” migrants suggest that migrants strongly prefer to hold U.S. savings in U.S. bank accounts instead of cash. The lack of a safe way to store money in the U.S. hurts not only savings, but also output. It is perhaps surprising that the migrants perceive U.S. cash holdings as so unsafe. On the one hand, U.S. bank account holdings are safe from theft. Undocumented migrants are targeted for crime—from strangers as well as own housemates—because they are likely to have lots of cash and are unlikely to report crimes to the police (e.g., Nossiter 2009, Paulson, Singer, Newberger and Smith 2006 pp. 64-66). On the other hand, they are safer from temptation. In interviews, migrants mentioned self control problems for why they might want a U.S. bank account. While having a U.S. bank account is unlikely to eliminate impulsive spending (considering that free ATMs are near the day labor center, and that it takes self control to get the cash wages deposited in the first place), it should reduce it.

<sup>40</sup> Udry (1996), de Mel, McKenzie and Woodruff (2009) and Robinson (2008) use data from Burkina Faso, Sri Lanka and Kenya, respectively. These studies reject the collective model.

## V. Conclusion

Our experiment provided 215 undocumented Mexican migrants with information on how to obtain a *matrícula* card and how to open a U.S. bank account. We randomly assigned 115 migrants to a treatment group, offered them assistance obtaining a *matrícula* card, and reminded them to consider opening a U.S. bank account. We achieved an 85 percent completion rate at the follow-up survey administered five months after the baseline survey. Our experiment was successful in generating variation in having a U.S. bank account: At completion, treated migrants were 38 percentage points more likely to have a U.S. bank account than untreated migrants.

The treatment caused migrants to increase their savings flows as a share of income by 9 percentage points. Evaluated at the average income of untreated migrants, total savings increased \$364 over the five months, which is three-quarters of the total stock of savings at the baseline. Almost all of this increase is in savings held in the U.S., and comes largely at the expense of remittances to Mexico (which decreased 6 percentage points as a share of income).

Estimated treatment effects vary strongly by the degree of control that migrants report they have over the use of their remittances. Migrants reporting “no control” over their remittances were especially likely to open a U.S. bank account as a result of the treatment; and treatment leads them to earn more income, strongly increase their total savings, and shift savings flows away from Mexican vehicles toward U.S. vehicles. These results suggest that issues of control play an important role in decision-making in migrant households.

Though treatment effects are especially pronounced for migrants reporting lack of control over remittances, we also find a positive effect on savings flows among migrants who report some control over remittances. This suggests that even those migrant households without remittance control issues can benefit from being banked. The benefits may improve over time;

for example, access to credit and other financial services will increase as the individual maintains a U.S. bank account in good standing. This should be relevant for thinking about the effects of banking the unbanked among non-migrant households as well.

Thus, there are large distributional consequences—between the migrant and his family, and between the U.S. and Mexico—for policies that impact bank access in the U.S. though we do not know what the long-run welfare effects are.<sup>41</sup> There are studies showing that remittances have a beneficial effect on the receiving households’ and communities’ well-being; for example, higher remittances raise children’s education, children’s health and business investments (*e.g.*, Cox-Edwards and Ureta 2003, Hildebrandt and McKenzie 2005, Woodruff 2006, and Yang 2008). Expanding U.S. bank access to Mexican migrants could therefore hurt the remittance recipients. The harm might be less than what one might surmise from the reduction in current remittances, though, because some of the U.S. savings will eventually be brought back to Mexico.<sup>42</sup> Moreover, our finding that having a U.S. account raises income for some households (those with remittance control issues) suggests that household behavior was less efficient before they obtained a U.S. account, suggesting a potential Pareto improvement in household welfare.

Experience with this project left us with the impression that—at least for Mexican migrants in the United States—barriers to being banked are relatively easy to remove. It did not take much time to assemble the documents needed to get the *matrícula* card; most migrants had access to transportation to the Mexican Consulate; and several banks (including the local bank

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<sup>41</sup> We have expanded the choice set for the *migrants* by offering assistance obtaining the *matrícula* card to some, so the migrants should be no worse off. By revealed preference, *migrants* who choose to open U.S. bank accounts should be no worse off than without a U.S. bank account. However, it remains unknown what happens to *household* welfare, and social welfare.

<sup>42</sup> Amuedo-Dorantes and Bansak (2006) find using Mexican Migration Project data that upon return migration to Mexico, migrants with a U.S. bank account brought back much larger lump sums. In our own survey, when we asked respondents informally what they intended to do with their U.S. savings, many reported uses that were for the household, not just themselves, such as buying land, appliances or other big items in Mexico. It is debatable whether these allocations make the household better off or worse off than the counterfactual allocation (that would have prevailed without the U.S. bank account), but it leaves open the possibility that the household could be better off by having an improved ability to smooth consumption over time, and accumulate funds for lumpy purchases.



supporting our study) conducted business in Spanish. We might think that a remaining barrier is information about U.S. bank accounts, but as demonstrated by the low take-up of U.S. bank accounts by the untreated migrants—who received the same information about *matrícula* cards and U.S. bank accounts as the treated migrants—it is clear that providing information is not enough. Policies that merely focus on educating the unbanked (on how to become banked, and the benefits of being banked) may not be enough.<sup>43</sup>

There are a number of limitations to our study which may affect its external validity. First, it may be that the migrants in our study are not representative of some broad population of interest, such as immigrants or the unbanked in the United States. While we randomly assigned our recruited migrants to be treated or not, all those recruits are nevertheless a self-selected subset of any putative population of interest (they responded positively to our recruitment). It should be noted, though, that undocumented migrants themselves represent 29 percent of the U.S. foreign-born population, with 57 percent originating from Mexico, so our study participants are an interesting group per se (Passel 2005).

Second, our design exploits the fact that the *matrícula consular* card is pivotal for opening a U.S. bank account among undocumented Mexican migrants. It may be difficult to design similar interventions (that significantly boost the propensity to open a bank account) in other countries or with other migrants or with other lower-income under-banked subpopulations. We recognize that effects estimated using our sample may not apply to these other settings.

Finally, our follow-up survey was conducted only five months after the baseline survey. It is possible that the effects differ in the longer run. Our conversations with consular officials strongly suggest that Mexico is keen to have its migrant nationals open U.S. bank accounts, and their stated reasons are broadly sensible. Safety, that is reducing theft and crime against

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<sup>43</sup> Nevertheless, more intensive financial education treatments might be effective. Table 2, Column 8 suggests that years of schooling has both a positive main effect and a positive interaction effect with the treatment dummy on the probability of obtaining a U.S. bank account.

migrants, is an obvious and understandable reason we have heard from officials: There are press reports that some less savory U.S. citizens refer to migrant day laborers as “Walking ATMs” (Nossiter 2009). More generally, officials said they would like migrants to integrate into the financial mainstream. It may well be that expanding access to banking is unambiguously good for migrants. Yet our study suggests that a policy to encourage migrant banking in host countries might have unintended adverse consequences on those receiving remittances back in the migrant’s home country, particularly when migrants feel they have poor control over the ultimate use of their remittances. As we point out above, even these migrants may ultimately bring their increased savings back home. On the other hand, greater integration into a host country’s financial mainstream could eventually weaken a migrant’s ties to home, and their increased host country savings could become an enabler of a break with the home country. The long-run dynamic consequences of migrant banking remain for future exploration.

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**Table 1. Baseline Characteristics by Treatment Status**

	Untreated Migrants mean (s.d.) (1)	Treated Migrants mean (s.d.) (2)	Difference [p-value of difference] (3)=(2)-(1)
Age	39.32 (9.05)	37.90 (9.98)	-1.42 [.314]
Years since first arrival in U.S.	12.14 (9.79)	10.23 (8.23)	-1.91 [.159]
Years in Boomtown	7.66 (5.85)	7.10 (5.61)	-0.56 [.514]
Number of times return to Mexico since first arrival in U.S.	9.45 (6.50)	8.53 (5.48)	-0.92 [.308]
Married/partnered	0.66 (0.48)	0.65 (0.48)	-0.01 [.935]
There are minors in household	0.73 (0.45)	0.74 (0.44)	0.02 [.814]
Years of schooling	6.92 (2.34)	6.85 (2.56)	-0.07 [.846]
Speaks English	0.51 (0.50)	0.43 (0.50)	-0.08 [.299]
Is sole income earner in household	0.46 (0.50)	0.36 (0.48)	-0.10 [.165]
Has a bank account in Mexico	0.16 (0.37)	0.13 (0.34)	-0.03 [.547]
Person remitting to in Mexico is wife (not parent or other)	0.55 (0.50)	0.54 (0.50)	-0.01 [.870]
Reports no control over allocation of remittances	0.46 (0.50)	0.43 (0.50)	-0.03 [.683]
Reports joint control over allocation of remittances	0.29 (0.46)	0.39 (0.49)	0.09 [.184]
Reports full control over allocation of remittances	0.25 (0.43)	0.18 (0.39)	-0.06 [.303]
<i>Income, savings and remittances flow variables below are measured over the five months preceding the baseline survey:</i>			
Income	3,663.77 (1,523.73)	3,781.52 (1,537.25)	117.76 [.604]
Remittances/Income	0.46 (0.19)	0.46 (0.20)	0.00 [.895]
U.S. savings/Income	0.01 (0.19)	0.02 (0.18)	0.02 [.569]
U.S. savings is positive	0.41 (0.50)	0.38 (0.49)	-0.03 [.639]
Mexican savings/Income	-0.01 (0.12)	-0.03 (0.15)	-0.02 [.211]
Mexican savings is positive	0.18 (0.38)	0.11 (0.32)	-0.06 [.223]
Total savings/Income	0.00 (0.28)	-0.01 (0.21)	-0.01 [.800]
Total savings is positive	0.25 (0.43)	0.17 (0.38)	-0.07 [.227]
Number of observations	85	98	

Notes: "Untreated migrants" refers to subjects randomly assigned to the control group and "treated migrants" refers to subjects randomly assigned to the treatment group. Summary statistics from the baseline survey are reported for the 183 men who completed both the baseline and follow-up surveys. In Column 3, the p-value reported is based on robust standard errors for the difference.

**Table 2. Effect of Treatment on Obtaining a Matricula Card and U.S. Bank Account**

	Obtains a Matricula Card				Obtains a U.S. Bank Account			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.809 *** (0.043)	0.816 *** (0.042)	0.796 *** (0.058)	0.819 *** (0.052)	0.382 *** (0.055)	0.407 *** (0.044)	0.207 *** (0.066)	0.299 *** (0.055)
Treatment×Reports no control			0.032 (0.086)	-0.018 (0.083)			0.409 *** (0.105)	0.206 ** (0.093)
Treatment×Years of schooling				-0.006 (0.022)				0.084 *** (0.023)
Reports no control		0.012 (0.044)	0.033 (0.053)	0.021 (0.048)		0.131 *** (0.046)	0.008 (0.047)	-0.004 (0.043)
Years of schooling		0.030 ** (0.014)		0.042 ** (0.017)		0.104 *** (0.015)		0.040 ** (0.017)
Age		-0.003 (0.005)				0.011 (0.005)		
Years since first arrival in U.S.		0.005 (0.006)				-0.001 (0.006)		
Years in Boomtown		-0.006 (0.009)				-0.011 (0.008)		
Number of times return to Mexico since first arrival in U.S.		-0.001 (0.011)				0.001 (0.010)		
Married/partnered		0.014 (0.056)				0.043 (0.056)		
There are minors in household		-0.027 (0.071)				0.058 (0.073)		
Speaks English		0.039 (0.041)				0.101 ** (0.049)		
Is sole income earner in household		-0.012 (0.044)				-0.033 (0.049)		
Has a bank account in Mexico		0.030 (0.059)				0.066 (0.057)		
Person remitting to in Mexico is wife (not parent or other)		0.003 (0.049)				0.041 (0.057)		
Constant	0.059 ** (0.026)	-0.069 (0.208)	0.043 (0.030)	-0.241 ** (0.103)	0.047 ** (0.023)	-1.203 *** (0.229)	0.043 (0.030)	-0.227 ** (0.103)
Adjusted R-squared	0.651	0.693	0.653	0.687	0.192	0.566	0.313	0.588

Notes: The dependent variable is from the follow-up survey, which is administered five months after the baseline survey. In Columns 4 and 8, "Years of schooling" is demeaned so the estimated coefficient for "Treatment" gives the average effect of treatment for someone with the average education (6.9 years). Robust standard errors are shown in parentheses. Asterisks denote significance levels (\*=.10, \*\*=.05, \*\*\*=.01).

**Table 3. Effect of Treatment on Income, Remittances and Savings**

	Income (1)	Remittances/ Income (2)	U.S. Savings/ Income (3)	U.S. Savings are Positive (4)	Mexico Savings/ Income (5)	Mexico Savings are Positive (6)	Total Savings/ Income (7)	Total Savings are Positive (8)
<b>Panel A</b>								
Treatment	225.7 (148.0)	-0.060 *** (0.023)	0.082 *** (0.020)	0.375 *** (0.060)	0.006 (0.010)	-0.034 (0.074)	0.087 *** (0.019)	0.219 *** (0.068)
Constant	4,157.9 *** (96.1)	0.522 *** (0.016)	-0.029 ** (0.013)	0.094 *** (0.032)	0.045 *** (0.006)	0.565 *** (0.054)	0.016 (0.014)	0.576 *** (0.054)
<b>Panel B</b>								
Treatment	-41.8 (194.0)	-0.035 (0.032)	0.028 (0.027)	0.195 ** (0.078)	0.035 ** (0.014)	0.042 (0.099)	0.063 ** (0.026)	0.145 (0.095)
Treatment×Reports no control	617.1 ** (294.3)	-0.059 (0.045)	0.123 *** (0.037)	0.419 *** (0.114)	-0.067 *** (0.019)	-0.177 (0.149)	0.057 (0.037)	0.172 (0.134)
Effect of treatment on people reporting no control	575.3 *** (221.3)	-0.094 *** (0.031)	0.152 *** (0.026)	0.614 *** (0.084)	-0.032 *** (0.012)	-0.136 (0.111)	0.120 *** (0.026)	0.317 *** (0.095)
<b>Panel C</b>								
Treatment	34.3 (193.1)	-0.041 (0.031)	0.045 * (0.026)	0.278 *** (0.071)	0.029 ** (0.014)	0.011 (0.099)	0.074 *** (0.026)	0.174 * (0.092)
Treatment×Reports no control	450.5 (302.2)	-0.047 (0.044)	0.086 ** (0.037)	0.234 ** (0.106)	-0.053 *** (0.019)	-0.108 (0.153)	0.033 (0.037)	0.108 (0.135)
Treatment×Years of schooling	60.2 (59.3)	-0.002 (0.011)	0.016 ** (0.007)	0.083 *** (0.024)	-0.007 * (0.004)	-0.031 (0.030)	0.010 (0.007)	0.039 (0.027)
Effect of treatment on people reporting no control	484.7 ** (225.2)	-0.088 *** (0.031)	0.131 *** (0.025)	0.513 *** (0.074)	-0.025 ** (0.012)	-0.098 (0.114)	0.107 *** (0.025)	0.281 *** (0.098)

Notes: Income, remittances and savings flow variables are measured for the five months preceding the follow-up survey, and capture the CHANGE in income, remittances and savings over the period. Each panel of each column reports the results of a separate regression. In addition to the reported variables, the regressions in Panel B have a constant and "Reports control", and those in Panel C have a constant, "Reports no control" and "Years of schooling". In Panel C, "Years of schooling" is demeaned so the estimated coefficient for "Treatment" gives the average effect of treatment for someone with the average education (6.9 years). The boxed "Effect of treatment on people reporting no control" is calculated as the sum of the coefficients for "Treatment" and "Treatment×Reports no control". Robust standard errors are shown in parentheses. Asterisks denote significance levels (\*=.10, \*\*=.05, \*\*\*=.01).



**Table 4. 2SLS Estimates of the Effect of Having a U.S. Bank Account on Income, Remittances and Savings**

	<b>Income</b> (1)	<b>Remittances/ Income</b> (2)	<b>U.S. Savings/ Income</b> (3)	<b>U.S. Savings are Positive</b> (4)	<b>Mexico Savings/ Income</b> (5)	<b>Mexico Savings are Positive</b> (6)	<b>Total Savings/ Income</b> (7)	<b>Total Savings are Positive</b> (8)
Obtains a U.S. bank account	591.6 (366.1)	-0.157 *** (0.058)	0.214 *** (0.041)	0.984 *** (0.080)	0.015 (0.027)	-0.089 (0.191)	0.228 *** (0.045)	0.575 *** (0.171)
Constant	4,130.0 *** (106.9)	0.530 *** (0.018)	-0.039 *** (0.013)	0.048 * (0.026)	0.044 *** (0.007)	0.569 *** (0.061)	0.005 (0.014)	0.549 *** (0.059)

Notes: Income, remittances and savings flow variables are measured for the five months preceding the follow-up survey, and capture the CHANGE in income, remittances and savings over the period. Each column reports the results of a separate regression. The identifying instrument is the treatment dummy; the associated first stage is shown in Table 2, Column 4. Robust standard errors are shown in parentheses. Asterisks denote significance levels (\*=.10, \*\*=.05, \*\*\*=.01).