# Social Service Delivery and Access to Financial Innovation

## The impact of Oportunidades' electronic payment system in Mexico[1]

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**Abstract**

This paper follows a quasi-experimental research design to assess the impact of the electronic payment system of Mexico's *Oportunidades* programme. The switch from cash payments to electronic payments delivered via a savings account is found to have medium-term effects on savings decisions, transaction costs, and coping strategies. Overall, the study finds that, following the intervention, a substitution effect emerged between saving portfolio choices, with the poor favouring bank accounts over informal saving arrangements. It also found that the Oportunidades savings account led to an increase in remittance reception, which in turn had important implications for household consumption smoothing and risk management decisions. The study also reveals impact heterogeneity depending on household composition and the rural-urban divide, with important implications for replicability of similar policy innovations in other developing country contexts.

**Keywords:** financial inclusion, social service delivery, Oportunidades, conditional cash transfers, quasi-experimental design, Mexico

**JEL classification:** D04, D14, G21, 012

## 1 Introduction

Social service delivery for the poor remains a major challenge for development effectiveness. While public-private alliances can represent a viable solution to improve the efficacy of social services, rigorous evidence of their impact is scarce throughout the developing world. It is all the more important to fill into this gap given that at the moment a variety of cash transfers programme around the world are implemented a transition from cash to electronic distribution. In this sense, out study is very timely as it contributes to the literature on conditional cash transfer programmes and financial inclusion by examining the impact of a recent electronic payment system introduced by the Mexican government to distribute the Oportunidades programme.

Oportunidades (before known as *Progresa* and more recently renamed as *Prospera*) is Mexico's flagship antipoverty social assistance programme, with the objective of breaking the intergenerational cycles of poverty by enhancing human development through investments in education, health, and nutrition. Oportunidades provides income support to poor families in exchange for regular school attendance of children and periodic health check-ups of household members (Niño-Zarazúa, 2011). Oportunidades was launched in August 1997 to cover 300,700 households in 6,344 rural municipalities. By the end of 2015, the programme supported 6.1 million households living in poverty (28.1 million people), which represents 25 per cent of Mexico's population.

Oportunidades' income support was initially paid in cash at distribution points located in towns. This usually entailed long travelling and queuing times for recipient households. The repercussions were also in terms of opportunity costs for leaving productive activities unattended, and personal safety, as recipients carrying cash were exposed to theft and assault (Klein and Mayer 2011).

An electronic payment system for the delivery of Oportunidades was introduced by the National Savings and Financial Services Bank (BANSEFI), a state-own development bank, in partnership with a network of non-banking institutions known as *L@ Red de la Gente* (People's Network) that includes credit unions, savings and credit associations (SCAs), savings and credit co-operatives (SACCOs), and microfinance institutions. Non-banking institutions in Mexico usually target rural and peri-urban communities, many of which are poor and with limited or no access to banking services.

The fact that L@ Red de la Gente targets communities where the Oportunidades programme also operates provided the opportunity to introduce a pilot project, in which a sub-sample of Oportunidades' beneficiaries received their grant through electronic transfers in savings accounts opened in BANSEFI and non-banking institutions. Most Oportunidades participants continued to receive payments in cash through distribution points located in the nearest town. This study examines the treatment effect of the electronic payments system by taking advantage of the availability of a rich household-level dataset (BANSEFI-SAGARPA Panel Survey 2004-2007) collected by BANSEFI and the Secretariat of Agriculture, Livestock, Rural Development, Fisheries and Food (SAGARPA). The data collection coincided with the phasing-in and roll-out process of the electronic payment programme, which allows us to construct a quasi-experimental evaluation design. More precisely, we exploit as an exogenous rule the fact that the selection of participation in the electronic transfer programme was made by the managers of L@ Red de la Gente and the Oportunidades programme, and not the households themselves. On the one hand, this allows us to rule out any potential endogeneity problem due to household self-selection. On the other hand, however, since the selection into treatment was not random, and most likely influenced by the availability of local infrastructure, we exploit the variation in observables to compute average treatment effect on the treated (ATT) matching estimators.

A scant literature has examined first- and second-order effects of electronic payments of a handful of cash transfer programmes (Duryea and Schargrodsky 2008, Wright, Tekin et al. 2014, Aker, Boumnijel et al. 2016, Muralidharan, Niehaus et al., 2016). In the particular context of Mexico, Seira (2010) and Chiapa and Prina (2014) have examined financial transactions and the propensity to save among a limited sample of Oportunidades' beneficiaries that received payments via electronic payments. Our study contributes, on the one hand, to the literature on cash transfer programmes and financial inclusion, and, on the other hand, it also contributes directly to policy implementation of financial inclusion interventions. With regards to the theoretical contributions: first, our study investigates the four-year, medium term, impact of the Oportunidades' electronic payment system on savings decisions, transaction costs, and coping strategies against idiosyncratic risks. This is particularly relevant in the context of Mexico, where financial inclusion among the poor remains very limited.

Secondly, our analysis identifies the possible medium-term underlying mechanisms through which better access to financial services can have add-on effects on beneficiaries of cash transfer programmes. More specifically, our results indicate that households who received their cash transfer in a BANSEFI savings account decreased their participation in informal saving arrangements, faced less constrains on remittance reception, and as a result, were less likely to reduce consumption or contract loans to cope with idiosyncratic shocks. We also find a degree of outcomes heterogeneity, which seems to be contingent upon the environments that characterize rural and urban areas in

Mexico.[4] Furthermore, our analysis suggests that the small scale local nature of the financial institutions implementing the reforms, the certainty created for the poor by regular income transfers from Oportunidades, and the incentive mechanisms that the intervention generated, played an important role. In particular, the non-banking institutions involved in the pilot phase of the electronic payment system were smaller entities, which the beneficiaries trusted more: they requested no opening or maintenance fees, and were much more densely distributed on the territory than formal commercial banks, indeed they were present even in smaller rural localities.

From the policy implementation point of view, our study highlights the potential welfare benefits from public-private alliances. Cash transfers programmes, in addition to their intended social impacts, can contribute to improve financial inclusion and the risk management portfolios of the poor. An important policy conclusion of our study is that, for cash transfer programmes to effectively encourage financial inclusion, extending financial access per se is not enough. Instead, also providing incentives to get people to use a broader spectrum of financial services is key. The rest of the paper is structured as follows: Section 2 reviews the relevant literature and outlines the theoretical background; Section 3 provides a discussion on the context in which the pilot of the electronic payment system of Oportunidades was introduced; Sections 4 and 5 provide information on the data, identification strategy, and the estimation methods. Section 6 presents the results and a sensitivity analysis, while Section 7 concludes with some reflections on policy.

## 2 Literature review

The importance of financial development and financial inclusion for poverty reduction has long been highlighted in the Economics literature (Deaton 1990; Giné and Townsend 2004; Burgess and

4 We refer to 'urban' areas in contexts of peri-urban and marginalized neighbourhoods. It is, therefore, not uncommon to observe 'urban' dwellers living in houses without concrete floor or walls. See Bazán et al. (2005) for further details.

Pande 2005; Demirgüç-Kunt et al. 2008; Karlan and Morduch 2009). Several studies stress the unconventional forms of savings by the poor, and the need for taking such forms into account when financial inclusion interventions are designed and implemented. For example, Deaton (1990) points out that consumption-smoothing and insurance motives are common reasons behind savings decisions among low-income households. Demirgüç-Kunt et al. (2008) and Demirgüç-Kunt and Klapper (2013) highlight the importance of having access to financial services such as payments and transfers instruments linked to remittances, despite the fact that nearly 80 percent of the global poor remain unbanked.

Over the past 15 years, cash transfer programmes have become one of the most prominent policy instruments against poverty and vulnerability in the developing world. About 100 cash transfer programmes in more than 60 countries currently reach 190 million poor households worldwide (Barrientos and Niño-Zarazúa, 2011). Given the scale and transfer volume of many of these programmes, there has been a growing interest in making cash payments more efficient and financially inclusive. Cash transfer programmes have in recent times begun a transition to deliver cash benefits through electronic payments in savings accounts or prepaid cards.[5]

In Brazil, 85% of Bolsa Familia is distributed through prepaid debit cards whereas the remaining 15% is paid in savings accounts of Caixa Facil Bank. In Colombia, 91% of beneficiaries of Familias en Acción receive their cash benefits through a bank account in Banco Agrario that is accessible through a debit card. In South Africa, 60% of the Child Support Grant and Old Age Pensions are distributed through debit cards linked to mainstream bank accounts and a 'Sekulula' account offered

5 Pickens et al (2009) report that only 25 percent of cash transfer programmes worldwide are distributed through electronic payments that are inclusive in terms of improved accessibility to financial services, Notable examples are Jefes y Jefas de Hogar in Argentina, Bolsa Famılia in Brazil, Familias en Accion in Colombia, Oportunidades in Mexico, and Juntos in Peru.

by AllPay, a subsidiary of ABSA Bank, whereas the remaining 40% of beneficiaries receive their grants via prepaid smarts cards offered by local payment providers (Bold, Porteous et al. 2012). In Mexico, 20% of Oportunidades programme is distributed through savings accounts in BANSEFI bank whereas the remaining 80% is paid through prepaid cards (see Figure 1).

The scant literature that examines the transition from cash to electronic payments generally finds positive demand-side effects, in terms of reducing both transaction costs for recipients households – associated with travel and waiting time and transportation costs–, and risk factors that emerge from carrying cash. In Niger, Aker, Boumnijel et al. (2016) report evidence of higher diet diversity among households receiving a cash transfer programme distributed through mobile money technology, which was partially attributed to time savings associated with lower travel and waiting time to receive the cash benefits.

Limited access to, or long distances to financial institutions are indeed common supply-side constraints for financial inclusion in developing countries. A number of policy strategies have been implemented to tackle these constraints. For example, in Kenya, Jack and Suri (2014) found that the mobile-based transaction system M-PESA, developed by Safaricom in Kenya, considerably reduced transaction costs on risk sharing of users of mobile money, which in turn had an effect on consumption smoothing in the face of negative income shocks, through increases and diversity of sources of remittances. Similarly, Klein and Mayer (2011) found that the M-PESA mobile money system cut transaction and opportunity costs, especially for the poor, while at the same time increasing their exposure and familiarity with financial innovation.

Also in Kenya, Schaner (in press) found that the availability of ATMs increased transactions and saving balances, as clients could avoid having to visit bank branches in order to perform financial operations. In Malawi, Flory (2011) found an increase in the rate of new bank account openings, following the introduction of a 'bank-on-wheel' service. The latter aimed to reach out for the unserved population lacking access to financial services, and the impact of the intervention was stronger the longer the travelling distance faced by households. In Nepal, Prina (2015) reports that access to bank accounts with zero fees led to high take up rates and usage of savings among female household heads, which also improve the ability of women to cope with income shocks. In Thailand, Townsend (2006) reports that small village-based financial institutions play an important role in increasing households' asset ownership.

A major risk factor for recipient households of cash transfer programmes is street crime. Evidence indicates that electronic payments can contribute to tackle this problem. Wright, Tekin et al. (2014) have shown that the transition of delivery of welfare benefits in the US from paper checks to electronic benefits had a negative and significant effect on the overall crime rate, including burglary, assault, larceny, and arrests associated with non-drug offenses. Petty corruption is another endemic risk factor in many developing countries. Muralidharan, Niehaus et al. (2016) study of the biometrical payment system of employment and pension programs in the Indian state of Andhra Pradesh shows that electronic payments were not only more efficient, but they also effectively tackled corrupted payments practices. Similarly, Duryea and Schargrodsky (2008) found that electronic payments of Artgenitna's *Plan Jefes y Jefas de Hogar de Desocupados* reduced bribe rates from 3.6 to 0.3 percent.

Of particular interest for our study is the fact that electronic payments of cash transfer programmes often represent the first entry point for the poor into the financial system. Access to saving accounts can be instrumental in changing in intra-household dynamics and inter-temporal preferences. Intra-household co-operative disequilibria associated with, for example, conflict or disagreement between partners surrounding consumption decisions can influence households' saving decisions. In Kenya, Schaner (2013) found that couples with similar saving attitudes and preferences are much more likely to pursue utility-maximizing saving strategies. Anderson and Baland (2002) find that participation in informal group savings such as rotating savings and credit associations (ROSCAs) is related to women's bargaining power. In particular, they note that women with little bargaining power participate more in ROSCAs to avoid their husbands appropriating their savings for immediate consumption. Inter-household dynamics involving extended family pressure for income and asset sharing, may also discourage the accumulation of savings (Castilla 2013; Platteau 2000). Therefore, electronic payments of cash transfer programmes can be pivotal in shifting intra-household bargaining power for women (Aker, Boumnijel et al. 2011, Aker, Boumnijel et al. 2016).

Access to formal savings instruments can also change inter-temporal preferences by making unbanked poor populations more willing to take risks (Carvalho, Prina et al. 2016). This is relevant since the poor often exhibit 'present-biased time preferences', which hinders their willingness and ability to save. Attaching more importance to present consumption reduces future consumption and results in under-saving (Laibson 1997; Gul and Pesendorfer 2004; and Fudenberg and Levine 2006). These inter-temporal choices are affected by a factor that increases with the length of consumption delays. Present-biased individuals also seem more likely to accumulate higher debts (Meier and Sprenger 2010). This may be due to a 'self-control' problem, whereby immediate needs are perceived as more urgent and relevant; this is likely to be more pronounced the lower the income level (Thaler and Shefrin 1981; Can and Erdem 2013).

'Rational inattention' can also result in lower savings (Karlan et al. 2011; Luo and Young 2010). If individuals fail to plan expenditures, and to smooth consumption accordingly, due to imperfect information, they may be forced to resort to undesired financial responses when resources are needed. When self-control or intra- and inter-household dynamics are an issue, saving products offer the possibility of guarding savings or hiding them away from family members or friends (Ashraf et al. 2006; Ashraf et al. 2010; Brune et al. 2013). In this way, immediate consumption is discouraged, and both partner and family pressures are relieved. Schaner (in press) found that access to low-cost ATM cards in rural Kenya led to a reduction in women's use of cards partly due to the reduction in their control over the cash. Also in Kenya, Dupas and Robinson (2013) showed in their study of expanding access to savings for small informal business owners (mainly women) that, although no interest rates were provided, the small women entrepreneurs chose to hold money in their savings accounts only to avoid appropriation of savings by spouses or relatives.

In Mexico, Aportela (1999) reports evidence from a natural experiment involving the opening of new branches of the National Savings Trust Fund (PAHNAL), a state-owned savings bank, between 1993 and 1994. The expansion used post offices to reach out to the unbanked in Mexico, and it was successful in raising savings levels, especially among lower income households, although Aportela did not find substitution effects on various informal saving instruments, despite their associated risks and inefficiencies. Similarly, Bruhn and Love (2014) analysed the opening of Banco Azteca, which, with over 800 branches in 2002, specifically targeted low-income households. The opening of Banco Azteca promoted the creation and survival of informal businesses. The bank was able to act as a lender for informal businesses after the introduction of alternative collateral requirements which were more suited to low-income clients. Also in Mexico, Woodruff and Martinez (2008) analysed 'Program to Strengthen the Popular Credit and Saving Sector', launched in 2004. They found an increase in the penetration of non-banking financial institutions between 2004 and 2007. This took place, however, mainly among households with relatively higher incomes, leading to the conclusion that the diffusion of financial services among the poor still represents a pressing development concern in Mexico.

More closely related to our study are Seira's (2010) and Chiapa and Prina's (2014) studies of the transaction flows related to the Oportunidades electronic payment system. Seira's transaction flow data was recorded only for the subset of beneficiaries who already received payments via debit cards, whereas Chiapa and Prina's (2014) study focused on Oportunidades' beneficiaries living in urban and peri-urban areas. Both studies show that a fraction of the banked poor did not withdraw the whole sum corresponding to their Oportunidades transfer and saved part of it in their saving account, suggesting that low income households do save when appropriate financial instruments are available to them. These results are in line with the earlier findings by Gertler, Martinez et al. (2012), who reported that beneficiaries of Oportunidades saved about a tenth of their grant for investment, which led to increases in consumption in the medium-term.

Thus, electronic payments of cash transfer programmes can widen the portfolio of financial services to the poor. Remittances are distinctly important in the context of Mexico, where they act as an insurance mechanism that protects household consumption against income shocks (Skoufias and

Quisumbing 2005, Amuedo-Dorantes and Pozo 2006).[6] Recent studies, notably Jack and Suri (2014) and Blumenstock et al. 2016) have underscored the role of remittances as one of the underlying mechanisms that explains why access to electronic payments can help the poor to smooth consumption. The study by Arestoff and Venet (2011) supports this line of argument. They analysed the introduction of 'Orange-money' in Madagascar, which provided mobile-based deposit and transfer services and found a sizable effect on the frequency of remittances for 'Orange-money' clients. The results of our study (presented in Section 6) speaks directly to this transmission mechanism, and highlights the potential contribution of electronic payments of cash transfers in widening the portfolios of the poor to manage risks.

## 3 Context and intervention

Oportunidades programme (recently renamed as Prospera) is the largest national-wide antipoverty policy in Mexico, currently reaching 28.1 million people or a fourth of Mexico's population. It is centrally run by a federal agency –Oportunidades National Co-ordination Unit–that identifies the eligible population based on a rigorous targeting method in two steps. First, it involves a 'spatial' selection procedure that identifies poor localities using a census-based marginality or 'social gap' index. The second step involves categorical criteria and a proxy means test (sistema unico de puntaje or SUP) that identifies the poor using survey and census data.[7] Oportunidades income support is distributed every two months and is primarily given to women. The monthly average transfer size is about 130 USD –or 20 per cent of household income among the targeted population–, which can vary significantly depending on household composition (ECLAC 2016). The fact that Oportunidades provides regular and predictable income support to beneficiaries is critical to

6 Migrant workers, 25% of which were poor, sent in 2015 nearly US $25 billion to their families in Mexico, which contributed to 2.3% of the country's GDP.

7 For a detailed description of Oportunidades' targeting method, see Orozco and Hubert (2005)

understand consumption smoothing, risk management and savings decisions made by the poor (Hoddinott and Skoufias 2004, Angelucci and Attanasio 2009, Gertler, Martinez et al. 2012)

Oportunidades' income support was initially paid in cash at distribution points located in towns. This usually entailed long travelling and queuing times for recipient households. The repercussions were also in terms of opportunity cost for leaving their economic activities unattended, as well as endangered personal safety, as collectors carrying cash were exposed to the risk of theft and assault (Klein and Mayer 2011). In 2001, the Mexican Congress passed a new law –*Ley de Ahorro y Crédito Popular*– as part of a wider reform of the financial system, with the aim of transforming non-banking financial institutions into fully regulated and monitored entities, legally authorised to receive deposits. The law also transformed the National Savings Trust Fund (PAHNAL) into a development bank, the National Savings and Financial Services Bank (BANSEFI) with the mandate of deepening financial intermediation and inclusion among low-income households (Niño-Zarazúa 2009).

A census conducted by BANSEFI in 2002 found that the non-banking sector - composed by about 630 institutions with nearly four million clients - had a market penetration rate of only 17 per cent (Gavito 2002).[8] Data on financial inclusion collected a bit later, in 2006 by the National Banking and Securities Commission showed that financial penetration in rural and peri-urban communities remained very limited. Financial inclusion rates were slightly higher in the urban sector, with just 26 per cent of urban households being banked (Honohnan 2008). This was in line with early findings

8 Non-banking institutions in Mexico include credit unions, savings and credit associations (SCAs), savings and credit co-operatives (SACCOs), microfinance institutions. Credit unions have formally operated in Mexico since the creation of the National Banking Commission in 1924. Their original objective was to form syndicates of producers and small firms to distribute direct credits and technical assistance from development banks and other governmental agencies. SCAs are non-profit organizations with open membership. As in the case of credit unions, financial operations within SCAs are constrained to receive deposits and give credits to their members. SACCOs are organizations that operate under a set of simple principles: i) one person, one vote; ii) no returns on capital, and iii) the use of profits for social purposes. SCAs as well as SACCOSs usually operate in rural and peri-urban areas.

by Caskey et al. (2006), who reported that only 24 per cent of households in Mexico City had access to formal financial services provided by either banking or non-banking institutions.

The pilot of the electronic payment system analysed in this study is the result of a joint effort that began in 2003 by the Secretariat of Social Development (SEDESOL), the Oportunidades National Co-ordination Unit, BANSEFI and non-banking institutions affiliated to L@ Red de la Gente. Two central objectives guided the policy: first, to make the delivery of Oportunidades grants more efficient, by cutting transaction and opportunity costs to beneficiary households, and second, to broaden the limited financial inclusion in the country. The pilot intervention involved the opening of savings accounts for Oportunidades beneficiaries in nearby BANSEFI branches and non-banking institutions that formed part of L@ Red de la Gente. Oportunidades accounts were free of opening and maintenance fees. The design of the intervention meant that transactions and opportunity costs were reduced for treated households. Indeed, Seira (2010) reports that, as the result of the electronic payment system, rural households' opportunity and financial costs associated with the collection of Oportunidades decreased by 77 per cent and 98.5 per cent, respectively.[9]

The inclusion of BANSEFI branches and affiliates to L@ Red de la Gente into the pilot depend on on two criteria: first, the institutional quality of financial intermediaries, whereby only those non-banking institutions that had been transformed into fully regulated entities, as defined in the *Ley de Ahorro y Crédito Popular,* were eligible to distribute Oportunidades payments, and second, the availability of financial infrastructure. During the pilot phase, more than 90 percent of Oportunidades beneficiaries continued to receive the grant in cash (see Figure 1). BANSEFI's

9 When transfers had to be collected in cash at the nearest distribution point, the figures for rural beneficiaries indicated an average time allocation of four hours, corresponding to an opportunity cost of 17 pesos, and an average travelling expense of 30 pesos. These costs go down to half an hour of time allocation, corresponding to an opportunity cost of 2.22 pesos, and 0.5 pesos for travelling expenses, on average, after the pilot implementation.

potential distribution network expanded over time to achieve a broad national coverage, thanks to its nearly 500 branches across the country and the partnership with L@ Red de la Gente. The network specifically targeted rural and peri-urban localities, with limited access to financial services, and where many Oportunidades' beneficiaries live. At the end of the pilot intervention in 2008, L@ Red de la Gente had already served more than 700 municipalities, and by 2009, it had covered nearly 2000 municipalities with 80 per cent of national coverage. The same year, BANSEFI began a rapid transition from cash to electronic payments, which was completed in 2011. It involved the expansion of Oportunidades payments in savings accounts and prepaid smartcards with biometric technology.[10] Our study covers the pilot stage that only included savings accounts.

## 4 Data and identification strategy

In 2004, BANSEFI and SAGARPA began the collection of a household panel survey in 25 of Mexico's 32 federal states that coincided with the pilot phase of Oportunidades electronic payment system, which had started operating one year earlier, in 2003. The survey sampling frame was designed to be representative of three regions: north, centre, and south, and of both users and non-users of financial services. A sample of non-banking institutions was randomly selected with a probability proportional to their number of clients (Woodruff 2006). Then, for each of the selected branches, between 20 and 30 households were randomly selected from a listing of clients, while an equal number of households with no recorded use of formal financial services in the previous five years to the survey were also included in the survey. The survey was repeated for four rounds, in 2004, 2005, 2006, and 2007. This gives us an overall sample of 3003 observations corresponding to Oportunidades beneficiaries, who, between 2004 and 2007, received their income support either in cash (1,197) or in a savings account (1,806).

[10] For a detailed description of the payment modalities of Oportunidades program, see CGAP (2011).

For the identification strategy, we exploit as a strictly exogenous rule the fact that the selection of households into the electronic transfer programme was made by the managers of L@ Red de la Gente and Oportunidades programme, and not the households themselves. More specifically, selection into treatment was based on the proximity of the household to a BANSEFI branch, or the nearest affiliate of L@ Red de la Gente, generally within a radius of 10 kilometres. This criterion was adopted to reduce the travelling and opportunity costs to the recipient households.

This identification strategy allowed us to rule out any potential endogeneity problem due to household self-selection. However, since the selection into treatment was not random, and influenced by the availability of local infrastructure, we exploit the variation in observables to compute ATT matching estimators. This was done despite of the relative homogeneity in the Oportunidades sample attributable to the targeting method that relies on the marginality index and the proxy-means test described above, and which are used to identify the poor who are eligible to receive Oportunidades. Given the data requirements for the implementation of the ATT matching estimators presented in Section 5, we employ the pooled sample over the four available years.

Since we are interested in the treatment effects of the electronic payments on savings decisions, transaction costs, and risk management decisions, we considered four outcome variables, which are summarized in Table 1: the first is a binary variable, *tandas*, taking the value of one for households that participate in informal rotating saving associations (ROSCAs), known in Mexico as tandas. Only slightly more than 10 per cent of our sample used tandas in the 12 months prior to the survey. This is explained by the socio-economic profile of the sampled households. Theft risks and budget constraints are likely factors underpinning the low participation of Oportunidades beneficiaries in tandas, which require a commitment to a fixed sum of money for a given period. *Homesavings* is a binary indicator that takes the value one if the household keeps part or all its savings at home. Table 1 shows that, on average, 30 per cent of the households kept money at home in the 12 months prior to the survey.

*Remittances* measures the frequency with which households receive remittances during the year. This variable has been transformed into log-form although it is presented in Table 1 on a linear scale for informative purposes. *ShockCoping* is a binary indicator recording whether a household has used its own savings to cope with idiosyncratic shocks; 15 per cent of the sample reported to have used their savings as a coping strategy. Idiosyncratic shocks include calamities associated with injuries, illness, or death of a household member, the job loss experienced by a household's member, a drop in either the price or the quantity of the produce sold by the household, and the loss or damage of tools and machinery used for economic activity.

[INSERT TABLE 1 ABOUT HERE]

The survey questionnaire provided data on a number of household- and location-covariates. For all of them, Table 1 reports summary statistics on the mean difference between the treatment and control groups. Only the covariates associated with statistically significant differences have been included in the estimation of the Mahalanobis distance metric and propensity score discussed in section 5.

As discussed earlier, we cannot completely rule out the presence of sample selection due to non-random placement of the pilot phase. There may exist systematic heterogeneity in terms of available infrastructure and services *within* the locations, and *between* the areas—being these rural, peri-urban or urban—where the treatment and control groups reside. Indeed, Table 1 indicates that treated households are significantly more likely to be located in urban and peri-urban areas, measured by the *LocalType* variable, and in northern regions, measured by *North_Mexico*. The Southern and Central regions exhibit a higher prevalence of rural areas, while northern Mexico, despite being less densely populated, has a higher urbanization rate. At the same time, it is worth noting that the probability of being treated is marginally lower in larger localities; here the size of the locality is measured by *LocalSize* which takes the value zero for small and very small localities and one for medium-size or big localities. This reflects the socio-economic profiles of Oportunidades beneficiaries, who are less likely to reside in large urban agglomerations.

A number of household-level covariates also displayed statistical difference from zero. For example, the variables indicating the presence of piped water in the house and whether the house floor was made of concrete suggest that treated households enjoyed better infrastructure. Similarly, treated household were less likely to have experienced idiosyncratic shocks of the types described above, which may again be associated with differences in environmental conditions. Finally, dependency ratios were only marginally lower in treated households.[11] Heads of treated households were marginally older and more educated, although the difference is only statistically significant at the 10 per cent level. They were, however, more likely to speak an indigenous language. Overall, the covariate distribution between the two groups suggests that there may be sources of upward or downward bias, with the direction of the bias depending on the outcome analysed. For these reasons, we decided to adopt a methodology that allows to control for these sources of bias. The next section discusses this methodology in more detail.

11 The dependency ratio estimated here is adjusted to treat as dependents household members who did not contribute to household income. For example, adults who reported to be students and had no other occupation were classified as dependents; but adults aged 65 and older who reported to work, were not.

## 5 Methodology

For comparative purposes, it is useful to begin our exposition by considering the case of a simple linear ordinary least squares (OLS) model, in which control variables along with the impact variable are regressed on the outcomes of interest. The linear OLS specification is depicted as follows:

$$(1) \qquad y_i = \alpha + D_i\beta + X_i\gamma + \varepsilon_i$$

where $D_i$ is a dummy variable taking the value one for households receiving Oportunidades via electronic payments and zero for households receiving the grant in cash, whereas $X_i$ is a vector of household- and location-level characteristics as described in Table 1. OLS estimates simply compare average outcomes between treatment and control groups after controlling for the effect of covariates. Shortcomings of this approach arise from model misspecification as well as from the risk of overlooking the potential effect of observed and unobserved heterogeneity affecting the outcomes of interest. A partial step towards addressing observable heterogeneity is the estimation of a fully interacted linear model (FILM), which relaxes the homogeneity assumption and allows for interactions of all control variables with the treatment status:

$$(2) \qquad y_i = \alpha + D_i\beta + X_i\gamma + (X_i * D_i)\delta + \varepsilon_i$$

If statistically significant interaction terms are found, impact heterogeneity can be regarded as an issue. In such cases, only comparable individuals should be considered to estimate treatment effects. For that purpose, matching estimators based on the propensity score or other distance metrics can be used to construct a synthetic quasi-experimental counterfactual. More formally, if we let $y_{i1}$ denote the outcome of household $i$ when treatment occurs ($D_i = 1$) and $y_{i0}$ the outcome of a control household, ( $D_i = 0$), the average treatment effect on the treated (ATT) corresponds to $\bar{y}_1 - \bar{y}_0$, where each outcome is averaged over the respective population. Under such a setting, the vector of covariates in $X$ allows us to balance the distribution of those determinants across treated and control groups using matching estimation methods.

Rosenbaum and Rubin (1983) show that this can be achieved by matching directly on the covariates, or by matching on the propensity score, which is calculated as the probability of treatment given a set of $X$ covariates. They indicate that while propensity score methods provide the coarsest balancing score, covariate matching provides the finest balancing score. Zhao (2004) points out, however, that, while matching on covariates removes all covariate differences and bias directly, such approach is impractical when there are many covariates because of the curse of dimensionality. Typically, in these situations a metric is needed to combine the multiple covariates into a scalar.

A metric that is often adopted for its desirable properties is the Mahalanobis distance metric. As discussed in Rubin (1980), the Mahalanobis metric matching is an equal per cent bias reducing (EPBR) technique; where by bias we refer to the difference between the covariate mean of the treated and that of the control group. EPBR techniques reduce per cent bias equally on all covariates, while no covariate's bias increases due to matching. The Mahalanobis metric minimizes the distance between treated unit $i$ and control unit $j$ as follows:

$$d(i,j)_M = \left(X_{ik} - X_{jk}\right)' D^{-1} (X_{ik} - X_{jk}) \tag{3}$$

where $X$ identifies $k$ matching covariates and $D^{-1}$ is the variance covariance matrix of $X$. The Mahalanobis metric assigns weights to each co-ordinate of $X$ in inverse proportion to the variance of that co-ordinate. By applying the mahalanobis distance metric, the control unit with the minimum distance $d(i,j)_M$ is chosen as a match for each treated unit. Following this procedure, matches are found for each treated unit, which are similar under all other respects (i.e. covariate characteristics) but for their treatment status. As a result, it is possible to attribute any measured difference in the outcomes of interests to the treatment itself. The estimation is only performed within the boundaries of the common support region, defined as the region within which comparable treatment and control units lie. All treated units for which $d(i,j)_M$ cannot be minimized fall outside of the common support, and are thus excluded from the matching. In this setting, the ATT corresponds to:

$$ATT = E[y_1|T = 1, d(i,j)_M] - E[y_0|T = 0, d(i,j)_M] \tag{4}$$

or

$$ATT = E[y_1 - y_0|d(i,j)_M] \tag{5}$$

In order to test for the sensitivity of our results, three different matching algorithms are estimated and presented in Tables 3 to 5. In all cases, the standard errors are calculated according to Abadie and Imbens' (2006) analytical asymptotic variance formula. The first set of results is that of a nearest neighbour matching estimation in which treated observations are only matched to the closest untreated neighbour. Here, the size of the caliper is the key parameter measuring proximity (Cochran and Rubin 1973). Results are also presented for a weighted smoothed kernel-based matching, where, rather than relying on the closest match, the counterfactual estimation is based on the whole data distribution on which a weighting structure is imposed. Such weighting structure corresponds to the kernel function (the Epanechnikov, in our case), which attributes progressively lower weights the larger the distance between the matched observations. In practice, the choice of the kernel is often unimportant; the bandwidth, instead, plays a role similar to the caliper described above.

After estimating the nearest neighbour and kernel matching described above, we verified the covariate distribution balancing and the mean bias reduction achievement, with the post-estimation routines detailed in Leuven and Sianesi (2003). It is important to note that we calibrate caliper and bandwidth restrictions according to such bias reduction performance. There is always a trade-off between bias elimination and the amount of observations retained. Although with closer matches better balancing is achieved, this comes at the expense of external validity. In all instances, we choose the least restrictive caliper and bandwidth which allow us to get rid of all bias. Figures A1-A24 in the Appendix provide some helpful visual representation of the bias reduction achieved. While both the nearest neighbour and kernel based estimators relied on the Mahalanobis distance metric described earlier, the last set of results presented in Tables 3 to 5 use the nearest neighbour bias-adjusted Abadie and Imbens' (2011) estimator. Here, the distance metric corresponds to:

$$d(i,j)_{AI} = \left(X_{ik} - X_{ij}\right) diag(D^{-1})(X_{ik} - X_{ij}) \tag{6}$$

This metric is similar to the Mahalanobis distance, except for the weighting matrix adopted. In fact, while $d(i,j)_M$ is weighted by the inverse of the variance-covariance matrix of $X$, $d(i,j)_{AI}$ is weighted by a diagonal matrix, with the inverse of the variances of the $X$'s as its elements. The bias-correction algorithm proposed in Abadie et al. (2004) and Abadie and Imbens (2011) allows to overcome the finite sample bias deriving from non-exact matching. The correction adjusts the difference between the matches with the differences in their covariate values, without affecting the asymptotic variance. We use a propensity score-based adjustment. In addition to this, to improve overlap, we follow Crump et al. (2009) and Abadie and Imbens (2011) and restrict the matching region to the subset of observations with $0.1 < p(Z) < 0.9$; where $p(Z)$ denotes the propensity score. Crump et al. (2009) calculate the percentage propensity score distribution ($\alpha$) to be dropped according to a condition based on the marginal distribution of the propensity score. They establish a rule of thumb for the parameter $\alpha$ to be fixed at 0.1.

Before moving to the results, we recall that any potential violation of the Conditional Independence Assumption (CIA) due to household self-selection was ruled out, although local-level heterogeneity remains an issue. To address this limitation, we follow two strategies. First, we include in the set of matching covariates all those for which a statistically significant difference between treatment and control groups exists. This includes the geographical location variables and the rural/urban location identifier. Second, we separate rural from urban localities and re-estimate the model by matching only households within each area separately. As explained in List et al. (2003), this is the matching analogy to the fixed effects estimator, which removes any location-related unobservable not already controlled for by the distance metric. In addition, such estimator satisfies an important condition set out in Smith and Todd (2005), namely, that, for treated and non-treated units to be comparable they should reside in the same local markets. Once this further condition is imposed, the ATT in (5) becomes:

(7) $$ATT = E[y_1|T = 1, d(i,j)_M, loc] - E[y_0|T = 0, d(i,j)_M, loc]$$

or

(8) $$ATT = \mathrm{E}[\mathrm{y}_1 - \mathrm{y}_0|\mathrm{d}(\mathrm{i},\mathrm{j})_\mathrm{M}, \mathrm{loc}]$$

where $loc$ corresponds to the rural-urban identifier.

## 6 Results

### 6.1 OLS and FILM estimation

The OLS and FILM results obtained from equations (1) and (2), respectively, are presented in Table 2 for exposition purposes only. The OLS estimates are likely to be bias due to concerns related to the common support and heterogeneity in observables. A first step towards correcting for this bias is to estimate a FILM regression. As it is apparent for all outcome variables in Table 2, apart from the case in which we consider coping mechanisms against idiosyncratic shocks, there is significant impact heterogeneity. This is signalled by the significance of some of the elements contained in the interaction vector.

More specifically, there is evidence of regional heterogeneity, with treated households living in southern regions being more likely to participate in tandas but less likely to save at home. Households living in central and southern regions also show a lower frequency of remittance reception. The same holds for households in urban areas, those belonging to indigenous groups, and those who suffer idiosyncratic shocks.

[INSERT TABLE 2 ABOUT HERE]

Locality size only seems to be associated with impact heterogeneity in the case of remittance reception, where more densely-populated areas received remittances more often when treated. Additional heterogeneity affects the frequency of remittance reception depending on households' demographic characteristics such as the age of the household's head and the dependency ratio. In fact, households with younger heads and those with lower dependency ratios seem to benefit comparatively more in that they receive remittances more often when treated.

### 6.2 Matching estimation

Once these diverse heterogeneity patterns are uncovered, to ensure that the ATT is only calculated over the common support, and that all bias in the covariate distribution is eliminated, we resort to matching methods. We rely on a Mahalanobis distance metric approach derived above to identify households with similar treatment probabilities, conditional on the set of covariates reported in

Table 1. Table A4 in the Appendix presents the results from the probit estimation. The findings broadly conform to our expectations in terms of significance and direction.

Tables 3-5 present for each outcome, the results obtained with the different matching algorithms described in Section 5. In Table 3, the whole sample is considered, while in Tables 4 and 5, the fixed effect matching estimators are presented. The post-estimation routines proposed by Leuven and Sianesi (2003) allow us to assess the imposed common support, for which details are presented in each table, under the ATT result panel. Furthermore, the balancing of the covariate distribution in the treated and control groups, and the overall mean bias reduction achieved can also be assessed. These details are presented separately in Tables 6-8.

[INSERT TABLES 3 TO 5 ABOUT HERE]

Starting with Table 3, matching on the whole sample indicates that electronic payments of Oportunidades decreases the propensity to participate in tandas by between 3.3 per cent and 4.8 per cent, depending on the estimator. Opportunity and financial costs associated with informal saving arrangements, both in terms of time allocation to peer-monitoring of savings groups, and the implicit risks of losing the funds, are the factors most likely underpinning the results. Interestingly, the propensity to save at home, which is our second outcome of interest, does not seem to be affected by the provision of a bank account. The fact that households only partly substitute informal group savings with bank savings seem to indicate that transaction costs are, indeed, influencing the results. The possibility of saving in a bank account provides an alternative to group savings when transaction costs are high. However, when transaction costs are minimal, as in the case of keeping money 'under the mattress', no substitution takes place. Our findings are in line with those of

Aportela (1999) who reports insignificant crowing out effects from the expansion of Pahnal, the preceding institution of BANSEFI, on home savings.

The magnitude of the substitution effect between saving portfolio choices is, however, small and it increases when the sample of urban beneficiaries is considered on its own, arguably due to the fact that tandas are much more common among urban dwellers in Mexico. It must, however, be noted that, due to data limitations, we were unable to establish with certainty whether lower transaction costs and associated risks were the only underlying mechanisms driving the substitution effect between savings accounts and tandas. It may be the case that intra-household dynamics also influenced savings decisions. Administrative data reported in González Rosas (2008) indicates that the median saving account balance among Oportunidades beneficiaries at the end of the pilot was about 22.40 USD, more than twice than the one observed among non-Oportunidades users of BANSEFI saving products. This is relevant since 96 per cent of Oportunidades beneficiaries are women. Previous studies have underscored the role of savings accounts in enabling women to hide income from family members and neighbours, particularly in traditional rural settings (Dupas and Robinson 2013, Jakiela and Ozier, 2015). The benefits of anonymity from savings accounts may in this regard well be outweighing the benefits of tandas as a saving commitment device (Anderson and Baland, 2002; Gugerty, 2007), although we cannot confirm this transmission channel.

The third outcome of interest is the frequency of remittance reception, which does not seem to be influenced by the electronic payment system of Oportunidades. However, when we computed the fixed effects estimators (see Tables 4 and 5), the results turn out to be positive for rural households. We return to this issue in subsection 6.2.1.

Finally, we find that households who received Oportunidades in a bank account were 6-8 per cent more likely to use their savings to cope with idiosyncratic shocks. The increased reliance on savings implies, in this case, that resorting to contracting loans or reducing consumption become less frequent. Karlan et al. (2011) point out that when unexpected events arise, failure to smooth consumption as a consequence of inadequate financial planning can result in households resorting to contracting new debt, or defaulting on existing loans. These are undesirable consequences, particularly when considering that, for Oportunidades beneficiaries who live near the subsistence level, any reduction in consumption can drastically impact health status, schooling, work productivity, and also future consumption and income levels.

Furthermore, as social and financial sanctions usually accompany loan defaults, the improved portfolio of copying strategies is a desirable result of electronic payments. Administrative data on the balance of savings among Oportunidades' beneficiaries (see González Rosas 2008) suggest that the shift from debt accumulation and cuts in consumption to usage of savings reflects an increase in the levels of savings, which in turn may be partly attributed, at least in the rural context as we discussed below, to increases in the frequency of remittances reception.

### 6.2.1 Fixed effect estimates

We turn now to the results from the location fixed-effects estimates presented in Tables 4 and 5. First, it appears that the decreased participation in tandas is concentrated in the urban sector, with a larger impact magnitude ranging from 8-14 per cent depending on the estimator. This is not surprising. Rural areas are often scarcely populated and the distance between villages can be considerable. This increases transaction and monitoring costs, as well as financial risks. In fact, in Mexico, tandas seem to be predominantly an urban phenomenon. This is confirmed by Klaehn et al. (2006) who report that only 7 per cent of the rural population use tandas as a saving instrument.

The impact of the electronic payments on the frequency of remittance reception and coping strategies is concentrated in the rural sector. As the frequency of remittance reception is expressed in log-form, we take the antilog of the ATT estimate and compute $(e^{\gamma}-1)\times 100$ (Halvorsen and Palmquist 1980). This calculates the percentage change of the median of remittance reception of treatment households relative to the control group. Two of the three matching algorithms reported in Table 5 indicate that the frequency of remittance reception increases by 90 per cent in the rural sector, as a result of the bank account provision. To gauge the extent of the impact, consider the hypothetical case of a household that receives remittances six times a year before the intervention. Following the provision of a bank account, the same household would be now able to receive remittances on a monthly basis.

Travelling distance to branches of BANSEFI and non-banking institutions is likely to be much lower than other money transfer providers that are usually located in the nearest town. Indeed, BANSEFI and non-banking institutions affiliated to L@ Red de la Gente achieved a very extensive territorial coverage, specifically targeting localities with limited or no access to banking services. As a result, transaction and opportunity costs were reduced; which translated into an increase in the frequency of remittance reception among those poor households that were dependent on money transfers.[12]

Interestingly, we find that the impact of electronic payments on coping strategies is concentrated in the rural sector. Rural households receiving Oportunidades in a bank account exhibited a higher propensity to use their own savings as a coping mechanism against idiosyncratic shocks than rural

[12] Due to data limitations, we were unable to assess whether the size of remittances changed over time.

households receiving Oportunidades in cash. The impact is in the order of 8 to 10 per cent. The electronic payments of Oportunidades seem to have enabled the poor to diversify their risk management portfolios. In the context of rural Mexico, it is plausible to argue that better, and more frequent, access to remittances, together with the certainty that regular, predictable and reliable income transfers from Oportunidades provide to the poor, are the underlying incentive mechanisms that permitted the poor to save and better cope with shocks. Our results are in line with those of Jack and Suri (2014) who report that the availability of a mobile-based transaction system in Kenya translated into consumption smoothing in times of income shocks, whereby remittances reception where the mechanism underlying these effects. The absence of significant impacts in urban areas may reflect the higher incidence of idiosyncratic shocks in rural areas as indicated by the sample size differential. This may also reflect a more pronounced impact on the households that were more economically disadvantaged to begin with.

### 6.2.2 Impact heterogeneity

Our analysis also reveals important dimensions of impact heterogeneity. Older Oportunidades beneficiaries, with a lower educational background, displayed a higher propensity to substitute savings in tandas for savings in BANSEFI accounts. These households were also less likely to have saved at home in the previous year (see Table A1 in the Appendix). While not saving 'under the mattress' could simply mean that these groups were poorer and therefore unable to save at all, it is more likely, based on existing evidence, that they store their savings elsewhere (Seira, 2010; Chiapa and Prina, 2014, and Gertler et al. 2012). This distinction is important because if the absence of home savings was due to other savings modalities such as tandas, the observed substitution effect between informal saving mechanisms and the BANSEFI saving accounts may pinpoint the benefits of anonymity from formal savings arrangements discussed above in Section 6.2.

Interestingly, we find that the impact of the electronic payment is more pronounced among those with higher dependency ratios (see Table A2). This is not surprising. Families with children, and thus with more liquidity constraints, are likely to be in more pressing need to receive remittances from adult family members living abroad. Age of the household head also appears to influence the frequency of remittance reception, as a result of treatment, until the age of 55; point after which no further impact is detected. This could be linked to the life cycle of economic migration that is reported by Moulaert and Deryckere (1984) and Massey (1987). Moreover, the more educated beneficiaries were also more likely to receive remittances more frequently. To interpret this, one must consider the rural context under which migration decisions take place. To begin with, it is reasonable to assume that for the illiterate and poorly educated, it is harder to take full advantage of the financial products made available to them through the BANSEFI savings account, including remittances. Thus, they may simply stick to the usual method of receiving remittances. Furthermore, previous studies have indicated that migrant workers usually come from relatively better off households within rural villages (Lipton 1980, López-Córdova, Tokman R et al. 2005, McKenzie and Rapoport 2007). This can be partly attributed to the fact that migration decisions, especially to the United States, involve risks and high financial costs to the household. Therefore, it is not surprising that the impact of the electronic payment system on remittance reception is concentrated among those households with better educational profiles. Finally, we find that households with lower dependency ratios, higher schooling levels, and lower propensity to save informally were more prone to resort to their savings to cope shocks as a result of treatment –as opposed to contracting debt or reducing consumption (see Table A3).

## 6.3 Matching quality

To assess the quality of the ATT matching estimators and the sensitivity of the results, Tables 6-8 report the mean bias reduction achieved after matching, as well as likelihood-ratio test statistics, for all specifications presented in Tables 3-5. The mean bias reduction, in practice, verifies how much of the pre-matching imbalance existing between controls and treated, has been reduced following the matching procedure. Because the aim of matching is to identify the controls and treated who are the most comparable, and determine the ATT by only comparing the outcome values for comparable matches, bias reduction is an intrinsic property of matching estimator. The more mean bias reduction is achieved, the higher the quality of the matching procedure implemented. Table 6 indicates that mean bias was reduced by over 98 per cent in the whole sample estimation. For the last outcome, shock coping strategies, smaller average bias exists in the unmatched sample to start with, however, also the amount of bias reduced via matching was lower.

The comparison of the likelihood-ratio test statistics and their corresponding p-values for the unmatched and matched sample confirms that in the matched sample no explanatory power is left to the covariates. In other words, matching gets rid of the imbalances in the matching covariates of treated and controls by only comparing similar treated and controls. In turn, if all covariates have similar values for treated and control, this allows us to attribute the differences in outcomes between the two groups to the intervention itself. Tables 7 and 8 report similar findings with regard to urban and rural samples, respectively. In particular, the post-estimation bias reduction for urban areas indicates a 95 per cent average bias reduction for the first three outcomes, and a bias reduction of 80-90 per cent for the fourth outcome. In the rural sample, more than 99 per cent of the bias was eliminated by matching, in all cases. All of the above results are confirmed by a comparison of the pseudo-$R^2$ in the unmatched and matched samples.

[INSERT TABLES 6 TO 8 ABOUT HERE]

In nearest neighbour matching control households are matched to the closest treated household. However, this incurs problems in the regions of the overlap distribution where probability density is low. In the more peripheric areas of the overlap, lower density means there will be more distance between matching control and treated observations. Because of this, matched units might still be somewhat different even after the matching. To avoid such bias, it is possible to allow for control observations to be matched more than once to different treated units. This option, however, is not exempt from risks. Substantial precision losses can occur from certain control observations being used too often. This is typically the case for control observations that have very similar characteristics, on average, to most treated units. An indicator of matching quality that is illustrative of such trade-off is the weight concentration ratio where weight captures the number of treated observations each control observation is matched to. The concentration ratio is computed as the sum of weights in the first decile of the weight distribution divided by the total sum of weights in the comparison sample (Lechner 2002).

Table 9 reports the percentage of concentration ratios for all nearest neighbour estimations. For the first three outcomes, in both the whole and rural samples, around 70 per cent of the control observations are matched to either one or at most two treated units. Slightly over 50 per cent of the control units have only one or two treated matches, in the urban sample. These results show that the matching quality is high. The last outcome performs slightly worse, just as it did in the mean bias reduction case. Here, over 50 per cent of the control observations are matched once or twice in the whole and rural samples, but the figure goes down to 20 per cent in the urban sample. Note, however, that in the latter instance, the maximum amount of repeated matched pairs corresponds to six. So, despite a low concentration ratio, it would be misleading to interpret this as an indication of poor matching quality.

[INSERT TABLE 9 ABOUT HERE]

### 6.4 Sensitivity analysis

Although self-selection into treatment is not a source of concern in our study, it is important to test the robustness of our quasi-experimental set-up in the presence of any potential unobserved confounder. In this section, we test the robustness of our results to possible deviations from the main assumption upon which matching estimators are based, i.e. the Conditional Independence Assumption (CIA).

To do this, we apply the test developed by Ichino et al. (2008), whereby the ATTs estimated via nearest neighbour estimation are reproduced by repeated simulations of the underlying models, where a confounder variable is included among the matching covariates.[13] The inclusion of the confounder simulates a violation of the CIA, and the comparison of the results obtained with and without matching on the simulated confounder is an indication of the extent to which the baseline results would change if indeed a violation of the CIA existed.

Various combination sets can be tested by varying the parameter values of the test, with the aim to find the one that drives the ATT to zero. The test developed by Ichino et al. (2008) gives the possibility to test two types of confounders: a confounding factor that has a positive impact on the untreated outcome $Y_0$, which would then reduce the observed difference between treated and

13 An ad hoc routine was developed using as a basis the readily available Stata programme developed by Ichino et al. (2008). This was done is order to adapt the sensitivity test to our own estimation analysis. A drawback is that the simulation of the ATTs estimated via kernel-weighted matching methods is too cumbersome. This is why only ATT simulations based on nearest neighbour baselines are reported.

controls, and a confounding factor that has a positive effect on treatment assignment, thus creating selection bias.

The estimation adopts a grid-search approach, and table 10 reports the corresponding ATTs simulated when a confounding factor $U$, defined by each of the selected combination sets, is included in the model.[14] Following Nannicini (2007), the combinations sets are specified so as to represent an increasingly dangerous confounder, with the first rows of the table presenting results for a less dangerous confounder and the last row presenting results for a combination that models a large outcome effect. In all instances, outcomes are remarkably stable. We conclude, therefore, that unobservable factors do not pose a threat to our results.

[INSERT TABLE 10 ABOUT HERE]

## 7 Conclusions

This study analyses a case of financial innovation in social service delivery, which was implemented in Mexico in the context of the conditional cash transfer programme Oportunidades. Currently, all Oportunidades beneficiaries receive the transfer electronically. The possibility of using a rich household-level dataset representative of the three main regions of the country, which was collected during the phase-in and roll-out of the electronic payment system of Oportunidades, gave us the opportunity to evaluate its impact on a number of outcomes. We find that the programme produced positive effects, although with a degree of heterogeneity in terms of impact between rural and urban areas. Access to a savings account led to a reduction in the rate of participation in tandas, although the effect was limited to urban areas where these informal savings instruments are recurrent. Arguably, transaction costs in terms of peer-monitoring, organization effort, and risks of insolvency,

[14] Further details on the test and on the construction of the combination sets can be found in the Appendix.

are the most likely underlying transmission mechanisms explaining the substitution effect between formal and informal savings, although we cannot rule out the possibility that intra-household dynamics also influencing savings decisions.

Electronic payments into savings accounts also helped the poor to access other financial services that are vital to improve their risk management portfolios. Specifically, our results indicate that the intervention reduced remittance reception costs, and it highlights the contribution of remittances in smoothing consumption and mitigating the catastrophic effects of income shocks. Moreover, the fact that treated households were more likely to resort to their own savings as a shock coping strategy is in itself a desirable outcome of the intervention.

As mentioned previously, our findings have clear implications for policy, especially at a time when various other cash transfer programmes around the world are undertaking the transition from cash to electronic payments. Indeed, our study underscores the potential welfare benefits from public-private alliances, and that cash transfers programmes can also contribute to improving financial inclusion among the poor, in addition to their intended social impacts. Firstly, the fact that beneficiaries of cash transfers programmes can access financial services opens the possibility to improve risk management decisions and possibly accumulate and protect vital assets. Secondly, we argued that for cash transfer programmes to effectively encourage financial inclusion, extending financial access per se is not enough. Instead, also providing incentives to get people to use a broader spectrum of financial services is key.

Further research is needed to improve our understanding of the longer-term and second-order effects of electronic payments of cash transfer programmes, and their link to other financial services such as insurance and credit.

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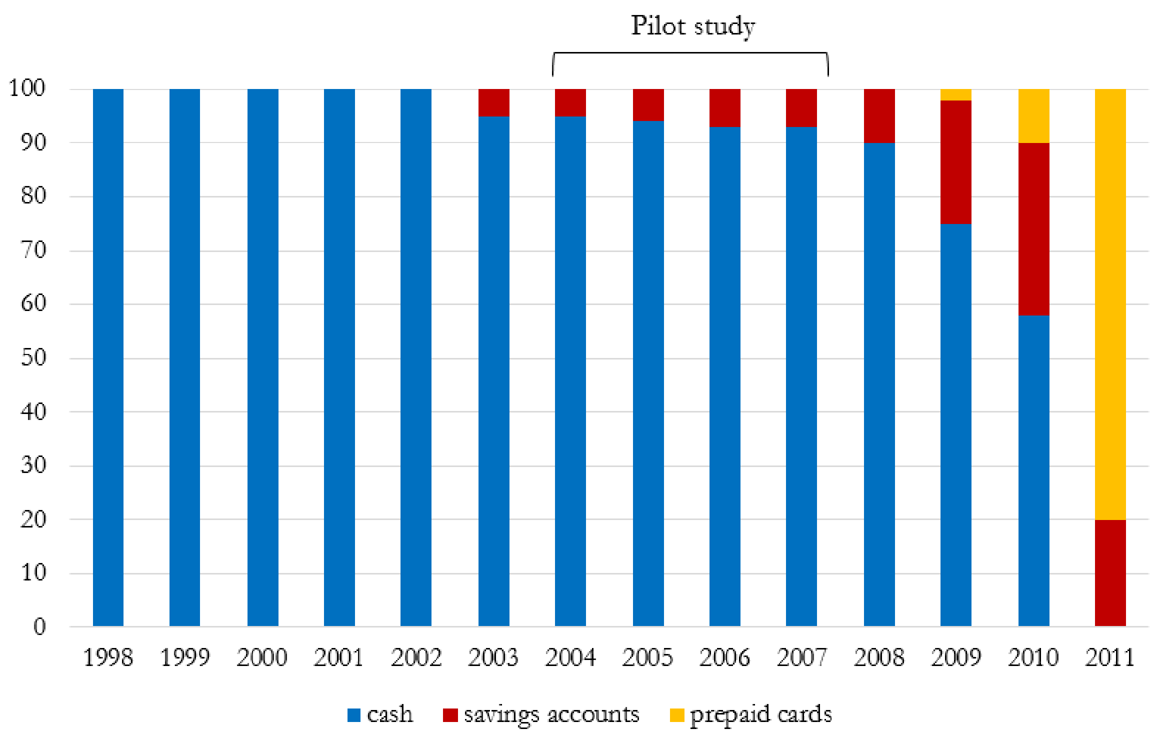
## TABLES AND FIGURES

Figure 1. Phase-in and roll-out of Oportunidades electronic payment system




Source: Authors' with data from Oportunidades (2012)

Table 1: Covariate balance and summary statistics

| Variable | Mean (C) | St. Dev. (C) | Mean (T) | St. Dev. (T) | *Pval* T=C | Obs. Total |
|---|---|---|---|---|---|---|
| *Outcomes* | | | | | | |
| Tandas | 0.112 | 0.315 | 0.108 | 0.311 | 0.772 | 2997 |
| HomeSavings | 0.307 | 0.461 | 0.306 | 0.46 | 0.923 | 2995 |
| Remittances | 0.748 | 2.817 | 0.592 | 2.618 | 0.119 | 2997 |
| ShockCoping | 0.152 | 0.36 | 0.14 | 0.348 | 0.936 | 629 |
| | | | | | | |
| *Covariates* | | | | | | |
| LocalType | 0.286 | 0.452 | 0.408 | 0.491 | 0.000*** | 2997 |
| LocalSize | 0.103 | 0.304 | 0.06 | 0.238 | 0.000*** | 2637 |
| North_Mexico | 0.115 | 0.319 | 0.227 | 0.419 | 0.000*** | 2997 |
| South_Mexico | 0.644 | 0.478 | 0.58 | 0.493 | 0.000*** | 2997 |
| Centr_Mexico | 0.239 | 0.427 | 0.191 | 0.393 | 0.002*** | 2997 |
| HouseProperty | 0.814 | 0.388 | 0.8 | 0.399 | 0.355 | 2996 |
| HouseFloor | 0.724 | 0.446 | 0.818 | 0.385 | 0.000*** | 2997 |
| PipedWater | 0.79 | 0.407 | 0.857 | 0.349 | 0.000*** | 2997 |
| DepRatio | 1.167 | 0.954 | 1.067 | 0.886 | 0.005*** | 2810 |
| Age | 47.86 | 14.7 | 48.97 | 15.54 | 0.05** | 2994 |
| Sex | 0.118 | 0.389 | 0.119 | 0.401 | 0.394 | 2997 |
| Education | 1.18 | 0.385 | 1.206 | 0.404 | 0.082* | 2988 |
| MaritalStatus | 0.814 | 0.388 | 0.798 | 0.401 | 0.279 | 2995 |
| Indigenous | 0.262 | 0.44 | 0.44 | 0.496 | 0.000*** | 2982 |
| IdioShock | 0.25 | 0.427 | 0.193 | 0.394 | 0.002*** | 2996 |

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2: OLS and FILM estimation

| | Tanda | | Home Saving | | Remittances | | Shock Coping | |
|---|---|---|---|---|---|---|---|---|
| | (1) OLS | (2) FILM | (3) OLS | (4) FILM | (5) OLS | (6) FILM | (7) OLS | (8) FILM |
| Treatment (D) | 0.01 | 0.01 | -0.029 | -0.029 | -0.008 | -0.008 | 0.05 | 0.05 |
| | (0.014) | (0.014) | (0.02) | (0.02) | (0.142) | (0.142) | (0.034) | (0.034) |
| LocalType | 0.036*** | 0.036*** | 0.006 | 0.006 | -1.045*** | -1.045*** | -0.014 | -0.014 |
| | (0.014) | (0.014) | (0.02) | (0.02) | (0.128) | (0.128) | (0.032) | (0.032) |
| LocalSize | 0.001 | 0.001 | 0.194 | 0.194 | 0.194 | 0.194 | 0.096** | 0.096** |
| | (0.027) | (0.027) | (0.28) | (0.28) | (0.28) | (0.28) | (0.046) | (0.046) |
| South_Mexico | 0.034** | 0.034** | -0.021 | -0.021 | -0.268 | -0.268 | 0.111** | 0.111** |
| | (0.017) | (0.017) | (0.027) | (0.027) | (0.178) | (0.178) | (0.049) | (0.049) |
| Centr_Mexico | 0.076*** | 0.076*** | -0.139*** | -0.139*** | 0.444* | 0.444* | 0.049 | 0.049 |
| | (0.022) | (0.022) | (0.031) | (0.031) | (0.256) | (0.256) | (0.051) | (0.051) |
| HouseFloor | 0.036*** | 0.036*** | 0.042* | 0.042* | 0.6*** | 0.6*** | -0.065 | -0.065 |
| | (0.013) | (0.013) | (0.024) | (0.024) | (0.151) | (0.151) | (0.043) | (0.043) |
| PipedWater | 0.034*** | 0.034*** | 0.01 | 0.01 | -0.109 | -0.109 | 0.018 | 0.018 |
| | (0.013) | (0.013) | (0.025) | (0.025) | (0.184) | (0.184) | (0.045) | (0.045) |
| DepRatio | -0.001 | -0.001 | -0.009 | -0.009 | 0.113 | 0.113 | -0.013 | -0.013 |
| | (0.006) | (0.006) | (0.01) | (0.01) | (0.081) | (0.081) | (0.016) | (0.016) |
| Age | -0.001*** | -0.001*** | -0.003*** | -0.003*** | 0.009* | 0.009* | -0.0009 | -0.0009 |
| | (0.0004) | (0.0004) | (0.0007) | (0.0007) | (0.005) | (0.005) | (0.001) | (0.001) |
| Education | 0.042** | 0.042** | -0.019 | -0.019 | -0.74*** | -0.74*** | 0.046 | 0.046 |
| | (0.018) | (0.018) | (0.025) | (0.025) | (0.154) | (0.154) | (0.045) | (0.045) |
| IdioShock | 0.064*** | 0.064*** | 0.029 | 0.029 | 0.198 | 0.198 | | |
| | (0.017) | (0.017) | (0.023) | (0.023) | (0.165) | (0.165) | | |
| Indigenous | -0.057*** | -0.057*** | -0.004 | -0.004 | -1.078*** | -1.078*** | -0.007 | -0.007 |
| | (0.014) | (0.014) | (0.022) | (0.022) | (0.133) | (0.133) | (0.04) | (0.04) |
| LocalType*D | | -0.008 | | -0.048 | | -0.918*** | | -0.052 |
| LocalSize*D | | 0.022 | | 0.06 | | 0.937* | | -0.171 |
| South_Mexico*D | | 0.08** | | -0.123** | | -1.574*** | | 0.154 |
| Centr_Mexico*D | | 0.035 | | 0.007 | | -0.957** | | 0.119 |
| HouseFloor*D | | -0.021 | | 0.049 | | -0.052 | | -0.092 |
| PipedWater*D | | 0.043 | | 0.092* | | -0.023 | | -0.01 |
| DepRatio*D | | 0.006 | | -0.046** | | 0.179 | | -0.024 |
| Age*D | | -0.0002 | | -0.003** | | -0.008 | | -0.002 |
| Education*D | | -0.055 | | -0.11** | | 0.278 | | 0.042 |
| IdioShock*D | | -0.013 | | 0.03 | | -0.555* | | |
| Indigenous*D | | -0.048 | | 0.052 | | -0.582* | | -0.1 |
| Obs. | 2456 | 2456 | 2454 | 2454 | 2456 | 2456 | 510 | 508 |
| $R^2$ | 0.045 | 0.05 | 0.03 | 0.04 | 0.08 | 0.094 | 0.03 | 0.045 |

Notes: heteroskedasticity-robust standard errors are reported in parenthesis.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Mahalanobis distance metric and bias-adjusted nearest neighbour matching estimators

| | Tanda | | | Home Saving | | | Remittances | | | Shock Coping | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | NN mahal | kernel weighted | NN bias_adj | NN mahal | kernel weighted | NN bias_adj | NN mahal | kernel weighted | NN bias_adj | NN mahal | kernel weighted | NN bias_adj |
| ATT | -0.048** | -0.033* | -0.046** | -0.05 | -0.031 | -0.053 | 0.114 | 0.03 | 0.106 | 0.08** | 0.08** | 0.06* |
| | (0.024) | (0.018) | (0.021) | (0.037) | (0.02) | (0.035) | (0.238) | (0.16) | (0.129) | (0.038) | (0.033) | (0.033) |
| Obs. | 2456 | 2456 | 2456 | 2454 | 2454 | 2454 | 2456 | 2456 | 2456 | 510 | 510 | 510 |
| Treated | 1200 | 1097 | 1399 | 1198 | 1095 | 1099 | 1200 | 1200 | 1413 | 224 | 224 | 264 |
| Controls | 1043 | 1043 | 1043 | 1043 | 1043 | 1043 | 1043 | 1043 | 1043 | 246 | 246 | 246 |
| Comm Supp | 2243 | 2140 | 2442 | 2241 | 2138 | 2052 | 2243 | 2243 | 2456 | 470 | 470 | 510 |
| Off sup | 213 | 316 | 14 | 213 | 316 | 402 | 213 | 213 | 0 | 40 | 40 | 0 |

Notes: Abadie and Imbens (2006)'s heteroskedasticity-robust analytical standard errors are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Mahalanobis distance metric and bias-adjusted nearest neighbour matching estimators (urban sector)

| | Tanda | | | Home Saving | | | Remittances | | | Shock Coping | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | NN mahal | Kernel weighted | NN bias_adj | NN mahal | kernel weighted | NN bias_adj | NN mahal | kernel weighted | NN bias_adj | NN mahal | kernel weighted | NN bias_adj |
| ATT | -0.1* | -0.077* | -0.14*** | -0.026 | -0.016 | -0.071 | -0.712 | -0.485 | -0.427 | 0.036 | 0.024 | 0.024 |
| | (0.053) | (0.046) | (0.05) | (0.057) | (0.057) | (0.052) | (0.49) | (0.3) | (0.26) | (0.064) | (0.062) | (0.062) |
| Obs. | 896 | 896 | 896 | 896 | 896 | 896 | 896 | 896 | 896 | 196 | 196 | 196 |
| Treated | 456 | 433 | 466 | 456 | 433 | 467 | 444 | 444 | 346 | 83 | 87 | 107 |
| Controls | 293 | 293 | 293 | 293 | 293 | 293 | 293 | 293 | 293 | 78 | 78 | 78 |
| Comm Supp | 749 | 717 | 759 | 749 | 717 | 759 | 737 | 717 | 639 | 161 | 175 | 175 |
| Off sup | 147 | 170 | 137 | 147 | 170 | 136 | 159 | 159 | 257 | 35 | 21 | 11 |

Notes: Abadie and Imbens (2006)'s heteroskedasticity-robust analytical standard errors are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Mahalanobis distance metric and bias-adjusted nearest neighbour matching estimators (rural sector)

| | Tanda | | | Home Saving | | | Remittances | | | Shock Coping | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | NN mahal | Kernel weighted | NN bias_adj | NN mahal | Kernel weighted | NN bias_adj | NN mahal | kernel weighted | NN bias_adj | NN mahal | kernel weighted | NN bias_adj |
| ATT | -0.019 | -0.008 | -0.017 | -0.035 | -0.05 | -0.044 | 0.642*** | 0.327 | 0.644** | 0.089** | 0.079** | 0.097** |
| | (0.021) | (0.02) | (0.021) | (0.044) | (0.033) | (0.043) | (0.239) | (0.25) | (0.257) | (0.041) | (0.031) | (0.042) |
| Obs. | 1560 | 1560 | 1560 | 1558 | 1558 | 1558 | 1560 | 1560 | 1560 | 314 | 314 | 314 |
| Treated | 810 | 752 | 810 | 808 | 750 | 808 | 810 | 752 | 810 | 146 | 134 | 146 |
| Controls | 750 | 750 | 750 | 750 | 750 | 750 | 750 | 750 | 750 | 168 | 168 | 168 |
| Comm Supp | 1560 | 1502 | 1560 | 1558 | 1500 | 1558 | 1560 | 1502 | 1560 | 314 | 302 | 314 |
| Off sup | 0 | 58 | 0 | 0 | 58 | 0 | 0 | 58 | 0 | 0 | 12 | 0 |

Notes: Abadie and Imbens (2006)'s heteroskedasticity-robust analytical standard errors are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Matching quality – testing the of % bias reduction achieved by matching and its pseudo $R^2$ (whole sample)

| | Tanda | | Home Saving | | Remittances | | Shock Coping | |
|---|---|---|---|---|---|---|---|---|
| | NN mahal | Kernel weighted | NN mahal | kernel weighted | NN mahal | kernel weighted | NN mahal | kernel weighted |
| Unmatched | 17.35 | 17.35 | 17.35 | 17.35 | 17.35 | 17.35 | 12.94 | 12.94 |
| Mean \|bias\| | (9.92) | (9.92) | (9.94) | (9.94) | (9.92) | (9.92) | (10.07) | (10.07) |
| Matched | 0.5 | 0.226 | 0.47 | 0.23 | 0.5 | 0.668 | 2.14 | 1.65 |
| Mean \|bias\| | (1.55) | (0.506) | (1.46) | (0.515) | (1.55) | (1.51) | (4.27) | (4.29) |
| Unmatched | 0.101 | 0.108 | 0.102 | 0.108 | 0.101 | 0.108 | 0.078 | 0.069 |
| Pseudo $R^2$ | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Matched Pseudo | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.001 | 0.005 | 0.004 |
| $R^2$ | (0.996) | (1.000) | (0.998) | (1.000) | (0.996) | (0.999) | (0.978) | (0.993) |

Table 7: Matching quality – testing the of % bias reduction achieved by matching and its pseudo $R^2$ (urban sample)

| | Tanda | | Home Saving | | Remittances | | Shock Coping | |
|---|---|---|---|---|---|---|---|---|
| | NN mahal | Kernel weighted | NN mahal | Kernel weighted | NN mahal | kernel weighted | NN mahal | kernel weighted |
| Unmatched | 24.01 | 24.01 | 24.01 | 24.01 | 24.01 | 24.01 | 21.76 | 21.76 |
| Mean \|bias\| | (13.51) | (13.51) | (13.52) | (13.52) | (13.51) | (13.51) | (13.32) | (13.32) |
| Matched | 0.178 | 0.178 | 0.178 | 0.178 | 0.178 | 0.178 | 0.178 | 0.178 |
| Mean \|bias\| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Unmatched | 1.22 | 1.52 | 1.22 | 1.52 | 1.04 | 1.69 | 2.18 | 4.95 |
| Pseudo $R^2$ | (2.96) | (3.32) | (2.96) | (3.32) | (2.35) | (3.63) | (5.94) | (9.11) |
| Matched Pseudo | 0.002 | 0.003 | 0.002 | 0.003 | 0.001 | 0.003 | 0.011 | 0.019 |
| $R^2$ | (0.984) | (0.977) | (0.984) | (1.000) | (0.998) | (0.955) | (0.956) | (0.857) |

Table 8: Matching quality – testing the of % bias reduction achieved by matching and its pseudo $R^2$ (rural sample)

| | Tanda | | Home Saving | | Remittances | | Shock Coping | |
|---|---|---|---|---|---|---|---|---|
| | NN mahal | Kernel weighted | NN mahal | Kernel weighted | NN mahal | Kernel weighted | NN mahal | kernel weighted |
| Unmatched | 14.01 | 14.01 | 14.06 | 14.06 | 14.01 | 14.01 | 12.42 | 12.42 |
| Mean \|bias\| | (13.05) | (13.05) | (13.03) | (13.03) | (13.05) | (13.05) | (8.37) | (8.37) |
| Matched | 0.084 | 0.084 | 0.084 | 0.084 | 0.084 | 0.084 | 0.046 | 0.046 |
| Mean \|bias\| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.018) | (0.018) |
| Unmatched Pseudo | 1.97 | 0.965 | 1.99 | 0.968 | 1.97 | 0.965 | 5.11 | 4.14 |
| $R^2$ | (1.46) | (2.49) | (1.48) | (2.50) | (1.46) | (2.49) | (5.98) | (6.19) |
| Matched Pseudo $R^2$ | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.012 | 0.011 |
| | (0.951) | (0.971) | (0.948) | (0.97) | (0.951) | (0.971) | (0.858) | (0.897) |

Table 9: Matching quality – % concentration ratio (nearest neighbour estimation)

| | Tanda | Home Saving | Remittances | Shock Coping |
|---|---|---|---|---|
| whole sample | 69 | 68.5 | 69.5 | 52.6 |
| urban sample | 56 | 56 | 56 | 22 |
| rural sample | 71.3 | 73 | 72 | 57.5 |

Table 10: Sensitivity analysis – ATT obtained when allowing for violation of CIA by introduction of a confounder

| | Tanda | | | Home Saving | | | Remittances | | | Shock Coping | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ATT | Whole | Urban | Rural | whole | Urban | rural | whole | Urban | rural | Whole | urban | rural |
| Baseline | -0.048** | -0.1* | 0.019 | -0.05 | -0.026 | -0.035 | 0.114 | -0.712 | 0.642*** | 0.08** | 0.036 | 0.089** |
| | (0.024) | (0.053) | (0.021) | (0.037) | (0.057) | (0.044) | (0.238) | (0.49) | (0.239) | (0.038) | (0.064) | (0.041) |
| $p11, p10 = 0.7$ $d = 0.2$ | -0.048** | -0.127* | 0.019 | -0.049 | -0.026 | -0.036 | 0.115 | -0.712 | 0.642*** | 0.08** | 0.036 | 0.089** |
| | (0.024) | (0.088) | (0.02) | (0.037) | (0.057) | (0.045) | (0.236) | (0.49) | (0.24) | (0.038) | (0.064) | (0.041) |
| $p11, p10 = 0.8$ $d = 0.3$ | -0.048** | -0.127* | 0.02 | -0.049 | -0.026 | -0.036 | 0.125 | -0.713 | 0.642*** | 0.08** | 0.036 | 0.089** |
| | (0.024) | (0.088) | (0.02) | (0.037) | (0.057) | (0.045) | (0.24) | (0.49) | (0.24) | (0.038) | (0.064) | (0.042) |
| $p11, p10 = 0.8$ $d = 0.5$ | -0.048** | -0.101* | 0.02 | -0.049 | -0.026 | -0.036 | 0.124 | -0.713 | 0.642*** | 0.08** | 0.036 | 0.089** |
| | (0.024) | (0.053) | (0.02) | (0.037) | (0.057) | (0.045) | (0.24) | (0.49) | (0.24) | (0.038) | (0.064) | (0.042) |

Notes: standard errors are reported in parentheses.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

## APPENDIX

**Sensitivity Analysis**

The confounder used in the sensitivity analysis is specified as a binary variable U, setting treatment status equal to $T_0, T_1 \in \{0,1\}$ and assuming for simplicity a binary outcome $Y_0, Y_1 \in \{0,1\}$.[15] The distribution of U is fully defined by a set of four probability parameters:

$$p_{ij} \equiv Pr\ (U = 1|T = i, Y = j) = Pr\ (U = 1|T = i, Y = j, W)$$

with $i, j \in \{0, 1\}$, which represents the probability that a confounder *U* exists in each of the four groups defined by treatment and outcome status. In the above, conditional independence of *U* with respect to *W* is assumed. By adopting a grid-search approach, various configuration sets of the $p_{ij}$ probability parameters can be tested, with the aim to find the one that drives the ATT to zero. Ichino et al. (2008) show, first, that if $d = p01 - p00 > 0$, that is, if $Pr(Y_0 = 1|T = 0, U = 1, W) > Pr(Y_0 = 1|T = 0, U = 0, W)$, a confounding factor that has a positive impact on the untreated outcome $Y_0$ (conditioning on $W$) is simulated. Second, they show that, when $s = p1. - p0. > 0$, that is, when $Pr(T = 1|U = 1, W) > Pr(T = 1|U = 0, W)$, the simulated confounding factor has a positive effect on treatment assignment (conditioning on $W$).

As the choice of probability parameters is discretionary, we follow Nannicini (2007) and fix the value of the difference $p11 - p10$, while varying $d$ and $s$ to identify what combination represents a real threat to the ATT. Following Nannicini (2007), the various sets of combinations are specified so as to represent an increasingly dangerous confoundert So that the first rows of table 10 adopt set is characterized by relatively smaller $d$ and $s$ differences with $p11$ and $p10$ equal to 0.7 and $d$=0.2, while the last represents a large outcome effect with $d$=0.5.

15 The discussion extends to continuous treatment cases.

Table A1 : Tanda (whole sample)

| | | DepRatio | | Education | | Age | | | | | Suffered a Shock | | Home Saving | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | benchmark | High (>0.55) | Low (<0.55) | Low | High | >25 | >35 | >45 | >55 | >60 | Shock | No shock | yes | No |
| ATT | -0.048** (0.024) | -0.037 (0.028) | -0.067 (0.051) | -0.048* (0.026) | -0.041 (0.054) | -0.04 (0.026) | -0.047 (0.029) | -0.068* (0.035) | -0.07* (0.04) | -0.09* (0.055) | 0.009 (0.051) | -0.052* (0.027) | 0.008 (0.038) | -0.054* (0.029) |
| Obs. | 2456 | 1632 | 824 | 1969 | 487 | 2384 | 1873 | 1108 | 674 | 404 | 515 | 1941 | 768 | 1686 |
| Treated | 1200 | 933 | 480 | 1116 | 297 | 1368 | 1098 | 635 | 414 | 252 | 267 | 1049 | 365 | 977 |
| Controls | 1043 | 699 | 344 | 853 | 190 | 1016 | 775 | 473 | 260 | 152 | 248 | 795 | 334 | 709 |
| Com Sup | 2243 | 1528 | 698 | 1884 | 381 | 2175 | 1657 | 923 | 560 | 302 | 460 | 1844 | 699 | 1545 |
| Off sup | 213 | 104 | 126 | 85 | 106 | 209 | 216 | 185 | 114 | 102 | 55 | 97 | 69 | 141 |

Notes: Abadie and Imbens (2006)'s heteroskedasticity-robust analytical standard errors are reported in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%

Table A2 : Remittances (rural sector sample, as no ATT effect is detected in the whole even in the benchmark)

| | | DepRatio | | Education | | Age | | | | | Suffered a Shock | | Home Saving | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Benchmark | High (>0.55) | Low (<0.55) | Low | High | >25 | >35 | >45 | >55 | >60 | Shock | No shock | yes | No |
| ATT | 0.642*** (0.239) | 0.52* (0.27) | 0.1 (0.57) | 0.62*** (0.266) | 0.742* (0.386) | 0.712*** (0.24) | 0.68*** (0.29) | 0.81* (0.046) | 0.997* (0.513) | 0.277 (0.53) | -0.65 (0.6) | 0.991*** (0.36) | -0.113 (0.66) | 0.735** (0.36) |
| Obs. | 1560 | 1021 | 539 | 1287 | 273 | 1523 | 1207 | 810 | 446 | 255 | 317 | 1243 | 490 | 1068 |
| Treated | 810 | 536 | 274 | 663 | 147 | 735 | 622 | 403 | 236 | 132 | 148 | 662 | 251 | 557 |
| Controls | 750 | 485 | 265 | 624 | 126 | 788 | 585 | 407 | 210 | 123 | 169 | 581 | 239 | 511 |
| Com Sup | 1560 | 1021 | 539 | 1287 | 273 | 1523 | 1162 | 721 | 446 | 235 | 317 | 1243 | 455 | 1068 |
| Off sup | 0 | 0 | 0 | 0 | 0 | 0 | 145 | 89 | 0 | 20 | 0 | 0 | 35 | 0 |

Notes: Abadie and Imbens (2006)'s heteroskedasticity-robust analytical standard errors are reported in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%

Table A3: ShockCoping (whole sample)

| | | DepRatio | | Education | | Home Saving | |
|---|---|---|---|---|---|---|---|
| | Benchmark | High (>0.55) | Low (<0.55) | Low | high | yes | No |
| ATT | 0.08** (0.038) | 0.077* (0.045) | 0.104* (0.06) | 0.028 (0.04) | 0.181* (0.09) | 0.09 (0.06) | 0.072* (0.04) |
| Obs. | 510 | 333 | 177 | 415 | 95 | 167 | 343 |
| Treated | 224 | 176 | 88 | 215 | 49 | 87 | 177 |
| Controls | 246 | 157 | 89 | 200 | 46 | 80 | 166 |
| Com Sup | 470 | 299 | 166 | 415 | 79 | 167 | 304 |
| Off sup | 40 | 34 | 11 | 0 | 16 | 0 | 39 |

Notes: Abadie and Imbens (2006)'s heteroskedasticity-robust analytical standard errors are reported in parentheses. * Significant at 10%; ** significant at 5%; *** significant at 1%

Table A4. Probit regressions, marginal effects

| | Tanda | Home Saving | Remittances | Shock Coping | Tanda | Home Saving | Remittances | Shock Coping | Tanda | Home Saving | Remittances | Shock Coping |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | whole sample | | | | urban sample | | | | rural sample | | | |
| LocalType | .136*** | .138*** | .136*** | .107** | | | | | | | | |
| | (.02) | (.02) | (.02) | (.045) | | | | | | | | |
| LocalSize | -.111*** | -.111*** | -.111*** | -.128* | -.256*** | -.256*** | -.256*** | -.287*** | -.116** | -.117** | -.116** | .044 |
| | (0.036) | (0.036) | (0.036) | (0.075) | (.049) | (.049) | (.049) | (0.108) | (0.032) | (0.032) | (0.032) | (0.113) |
| South_Mexico | -.336*** | -.336*** | -.336*** | -.336*** | -.478*** | -.478*** | -.478*** | -.642*** | -.268*** | -.268*** | -.268*** | -.184** |
| | (.025) | (.025) | (.025) | (.059) | (0.042) | (0.042) | (0.042) | (.082) | (.032) | (.032) | (.032) | (.077) |
| Centr_Mexico | -.275*** | -.276*** | -.275*** | -.218*** | -.477*** | -.477*** | -.477*** | -.378*** | -.192*** | -.193*** | -.192*** | -.157* |
| | (.031) | (.031) | (.031) | (.066) | (.054) | (.054) | (.054) | (.114) | (.038) | (.038) | (.038) | (.084) |
| HouseFloor | .159*** | .159*** | .159*** | .135** | .137*** | .136*** | .137*** | .281*** | .159*** | .174*** | .175*** | .095 |
| | (.025) | (.025) | (.025) | (.053) | (.046) | (.046) | (.046) | (.096) | (.025) | (.029) | (.025) | (.066) |
| PipedWater | .1*** | .101*** | .1*** | .111* | .053 | .054 | .053 | .1 | .1*** | .119*** | .117*** | .173** |
| | (.027) | (.027) | (.027) | (.061) | (.05) | (.05) | (.05) | (.142) | (.027) | (.031) | (.027) | (.067) |
| DepRatio | -.043*** | -.043*** | -.043*** | -.011 | -.058*** | -.058*** | -.058*** | -.063 | -.016 | -.016 | -.016 | .015 |
| | (.011) | (.011) | (.011) | (.024) | (.016) | (.016) | (.016) | (.04) | (.015) | (.015) | (.015) | (.032) |
| Age | .001** | .001* | .001** | .001 | .004** | .004** | .004** | .005* | .000 | .000 | .000 | -.002 |
| | (.0008) | (.0008) | (.0008) | (.001) | (.001) | (.001) | (.001) | (.003) | (.001) | (.001) | (.001) | (.002) |
| Education | .071*** | .07** | .071*** | .003 | .14*** | .14*** | .14*** | .21** | .012 | .012 | .012 | -.1 |
| | (.027) | (.027) | (.027) | (.06) | (.044) | (.044) | (.044) | (.103) | (.036) | (.036) | (.036) | (.08) |
| IdioSock | -.07*** | -.07*** | -.07*** | | -.1** | -.1** | -.1** | | -.05* | -.053* | -.055* | |
| | (.024) | (.024) | (.024) | | (.004) | (.004) | (.004) | | (.031) | (.031) | (.031) | |
| Indigenous | .245*** | .245*** | .245*** | .19*** | .164*** | .166*** | .164*** | .3*** | .29*** | .29*** | .29*** | .162*** |
| | (.022) | (.022) | (.022) | (.05) | (.04) | (.04) | (.04) | (.09) | (.026) | (.026) | (.026) | (.063) |
| Obs. | 2694 | 2691 | 2694 | 566 | 1024 | 1023 | 1024 | 223 | 1670 | 1668 | 1670 | 343 |
| LR $\chi^2$ | 378.38 | 379.37 | 378.38 | 61.05 | 225.03 | 224.94 | 225.03 | 63.85 | 205.1 | 204.95 | 205.1 | 24.95 |
| $p > \chi^2$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Psuedo $R^2$ | 0.101 | 0.102 | 0.101 | 0.078 | 0.164 | 0.164 | 0.164 | 0.207 | 0.088 | 0.088 | 0.088 | 0.053 |

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%.

Figure A1: Tanda participation: nearest neighbour matching – bias reduction

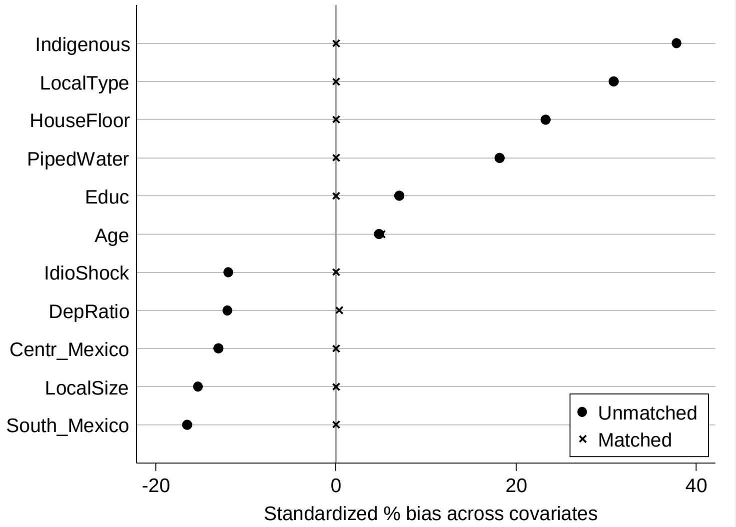



Figure A2: Tanda participation: kernel matching – bias reduction

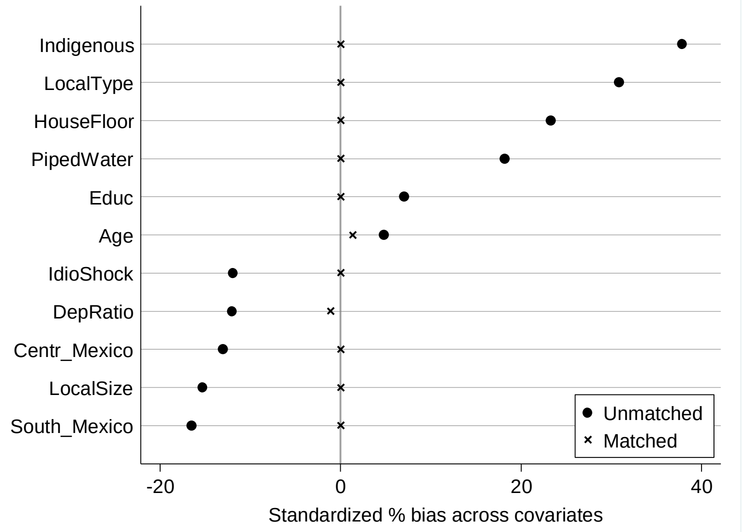



Figure A3: Tanda participation (urban): nearest neighbour matching – bias reduction

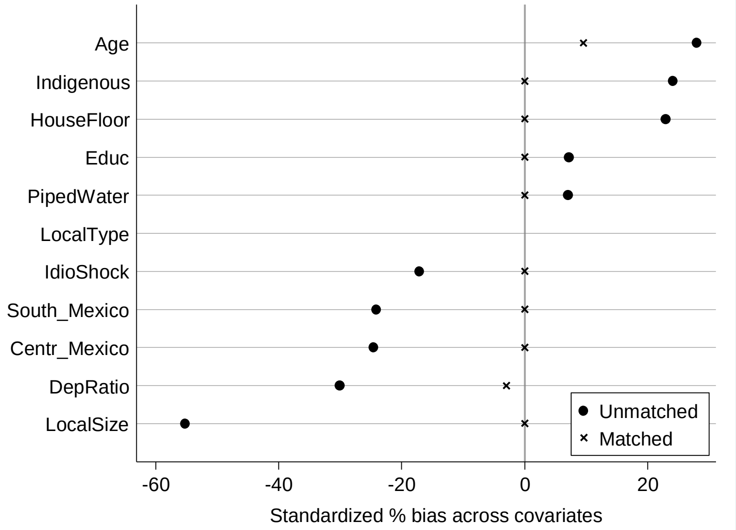



Figure A4: Tanda participation (urban): kernel matching – bias reduction

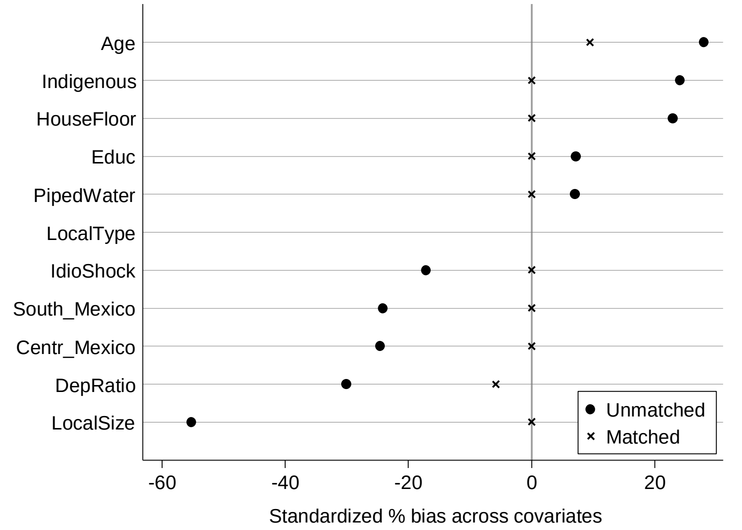


Figure A5: Tanda participation (rural): nearest neighbour matching – bias reduction

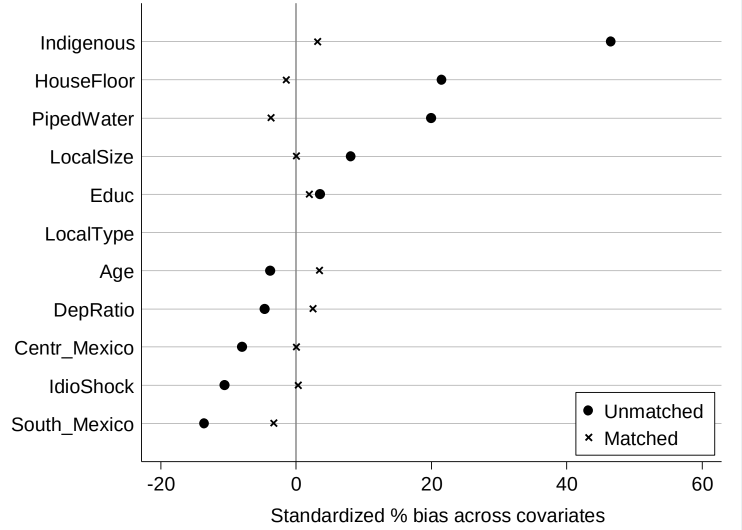



Figure A6: Tanda participation (rural): kernel matching – bias reduction

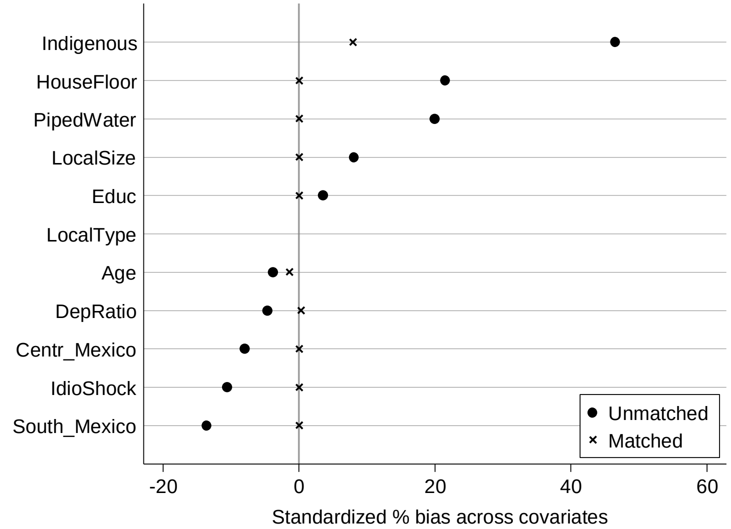



Figure A7: Home saving: nearest neighbour matching – bias reduction

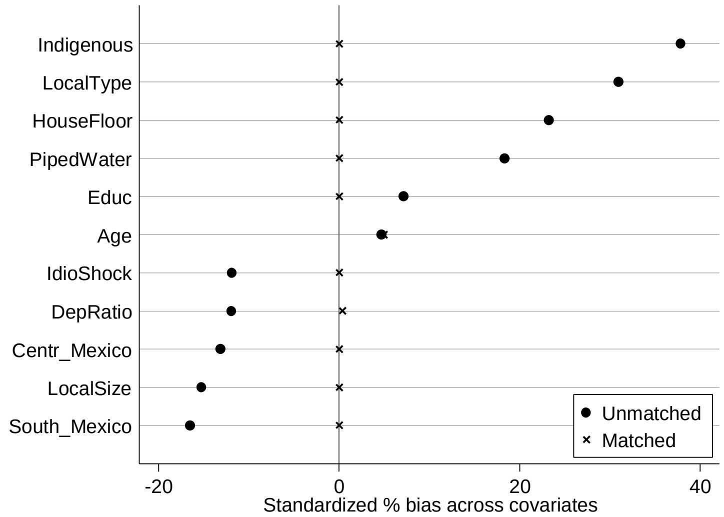



Figure A8: Home saving: kernel matching – bias reduction

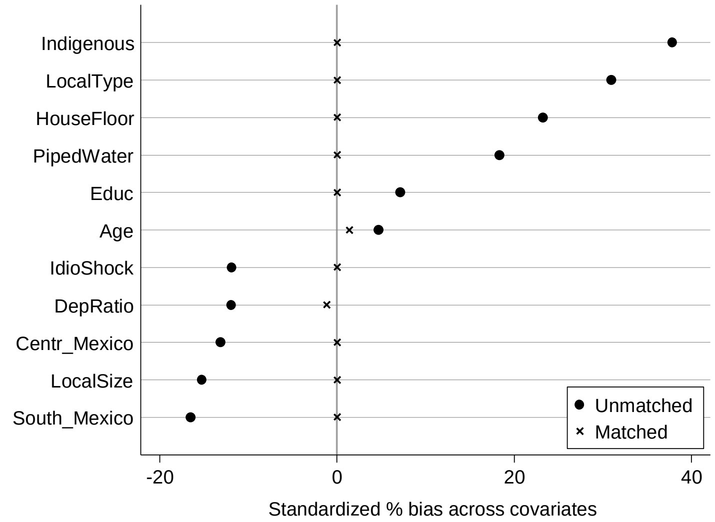


Figure A9: Home saving (urban): nearest neighbour matching – bias reduction

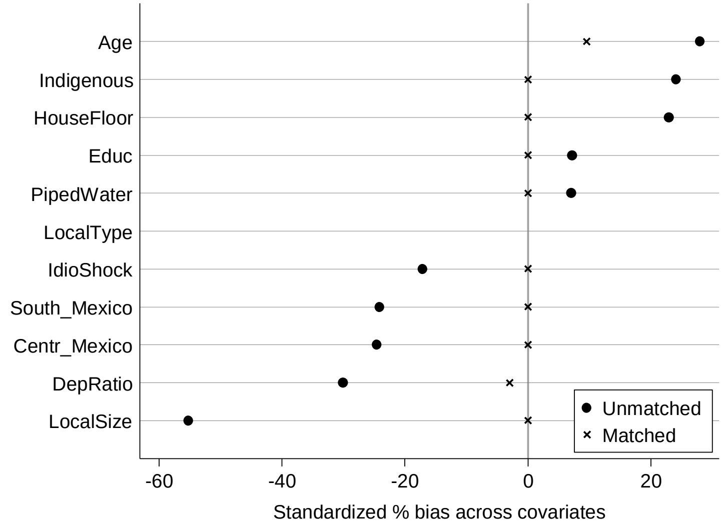



Figure A10: Home saving (urban): kernel matching – bias reduction

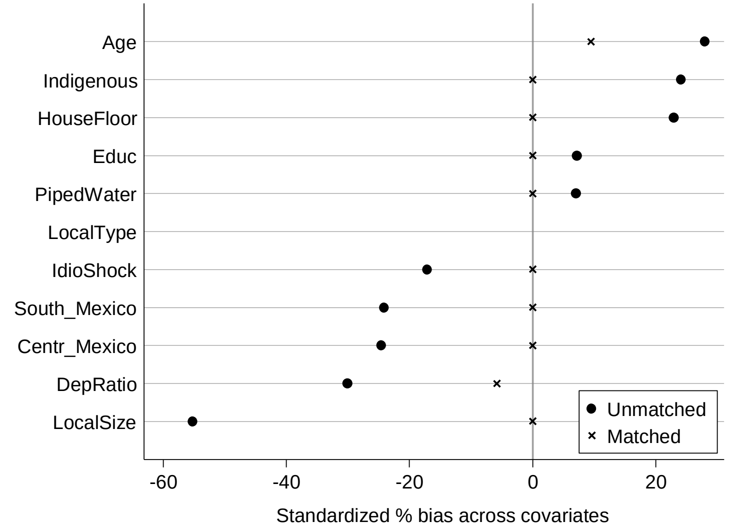



Figure A11: Home saving (rural): nearest neighbour matching – bias reduction

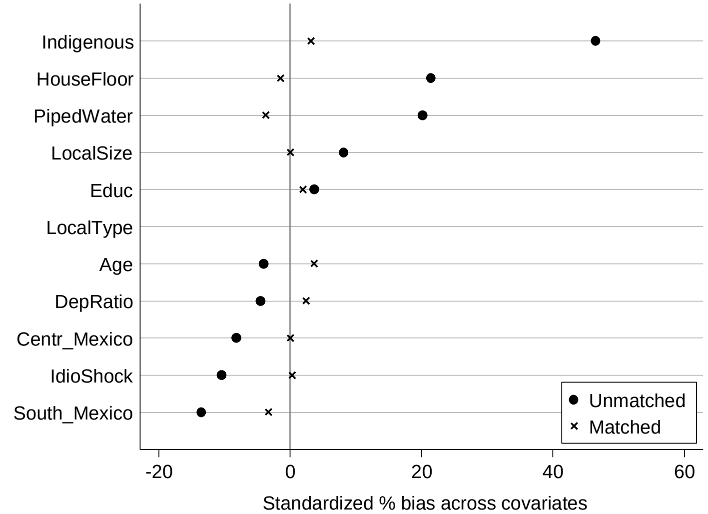



Figure A12: Home saving (rural): kernel matching – bias reduction

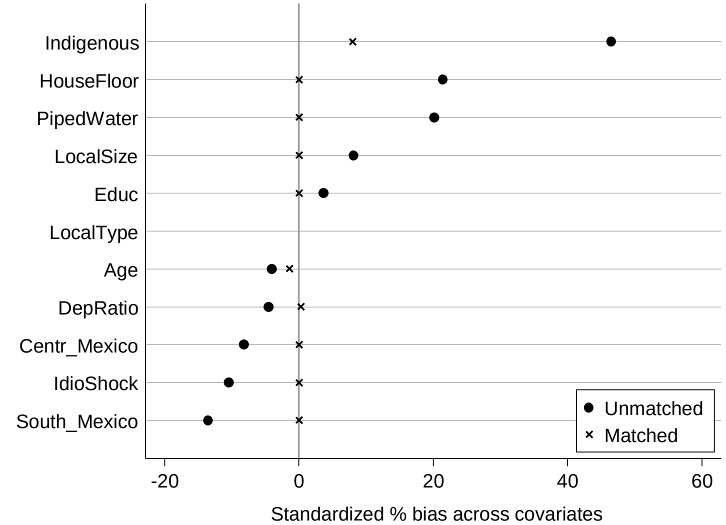


Figure A13: Remittances: nearest neighbour matching – bias reduction

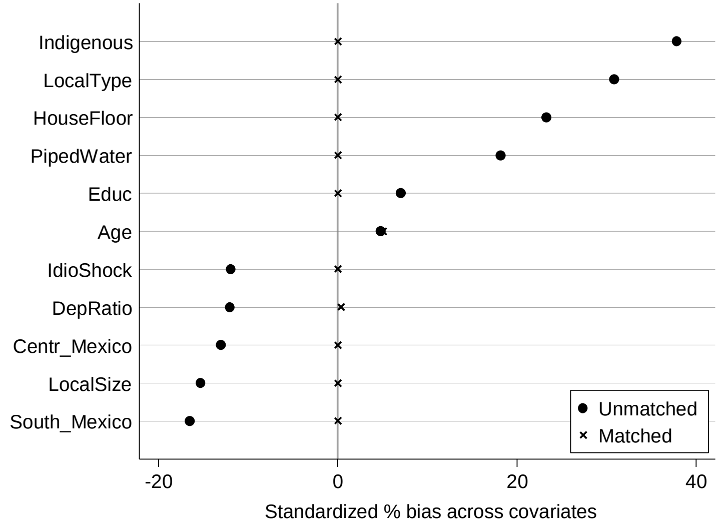



Figure A14: Remittances: kernel matching – bias reduction

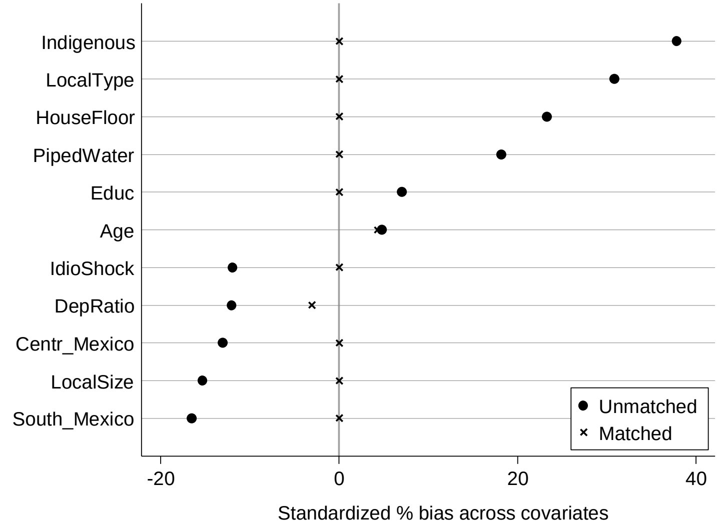



Figure A15: Remittances (urban): nearest neighbour matching – bias reduction

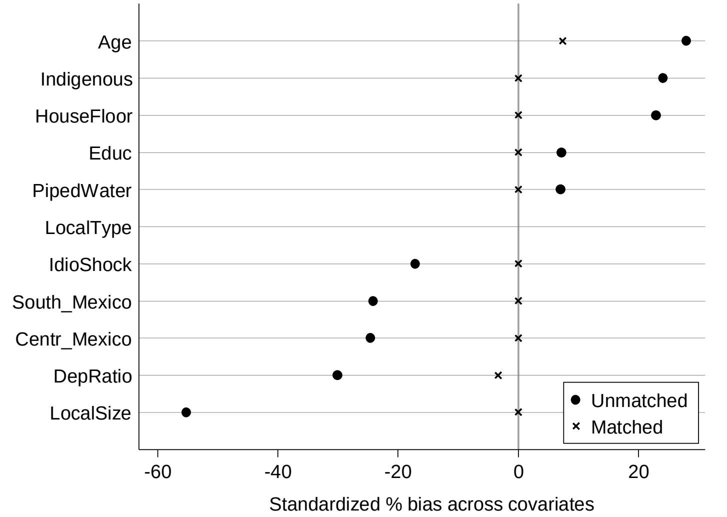



Figure A16: Remittances (urban): kernel matching – bias reduction

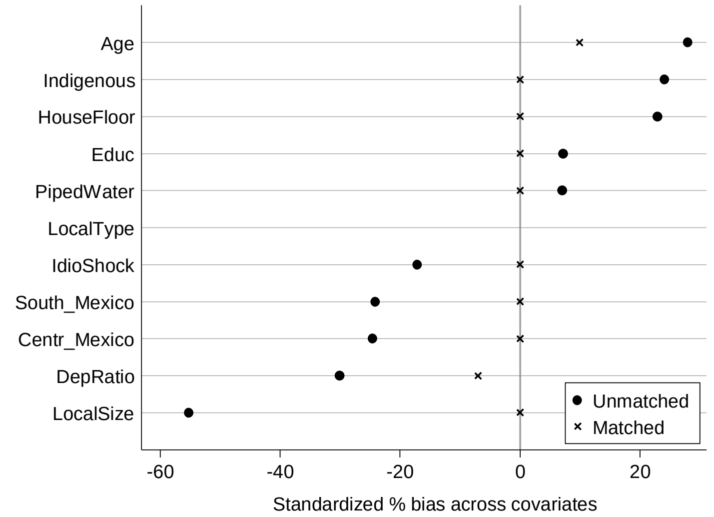


Figure A17: Remittances (rural): nearest neighbour matching – bias reduction

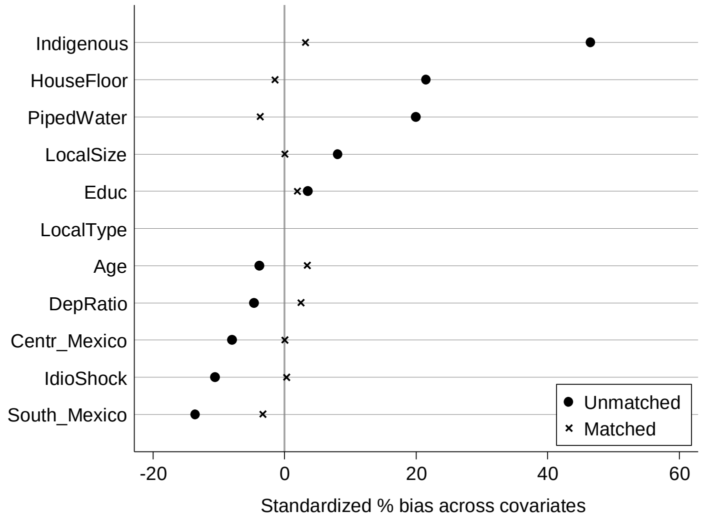



Figure A18: Remittances (rural): kernel matching – bias reduction

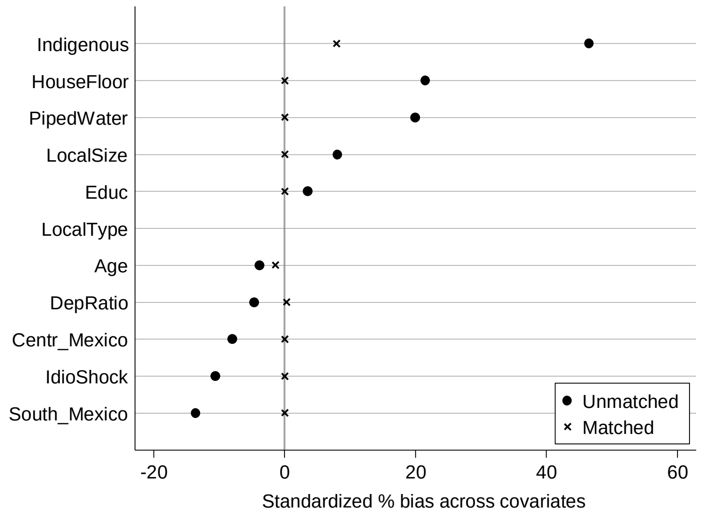



Figure A19: Shock Coping: nearest neighbour matching – bias reduction

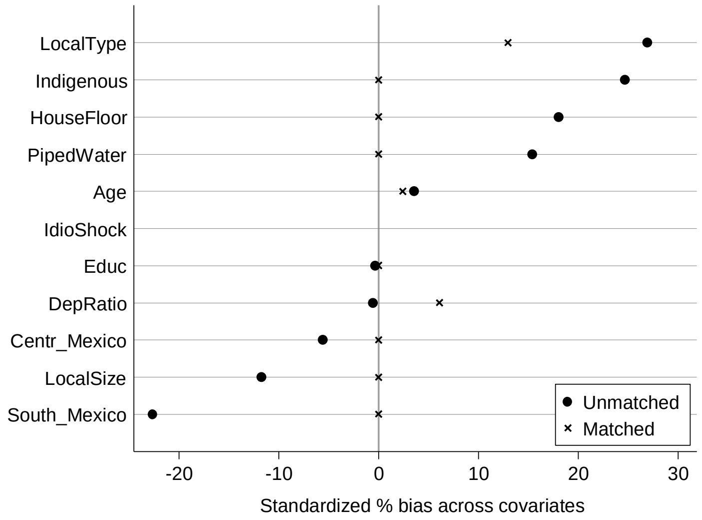



Figure A20: Shock Coping: kernel matching – bias reduction

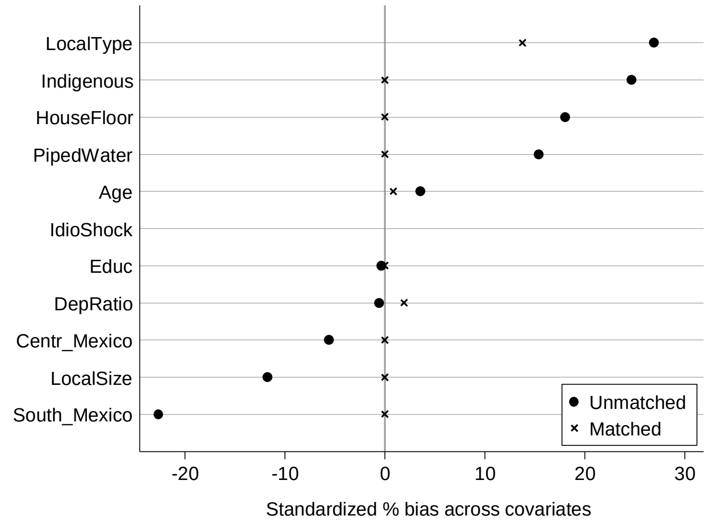


Figure A21: ShockCoping (urban): nearest neighbour matching – bias reduction

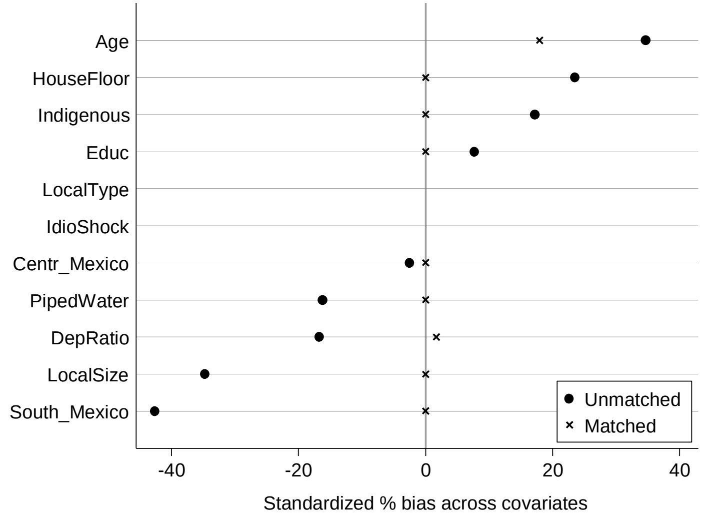



Figure A22: ShockCoping (urban): kernel matching – bias reduction

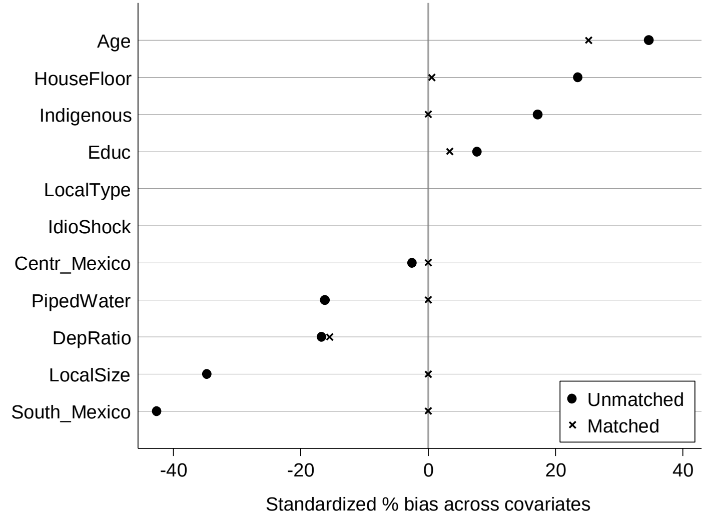



Figure A23: Shock Coping (rural): nearest neighbour matching – bias reduction

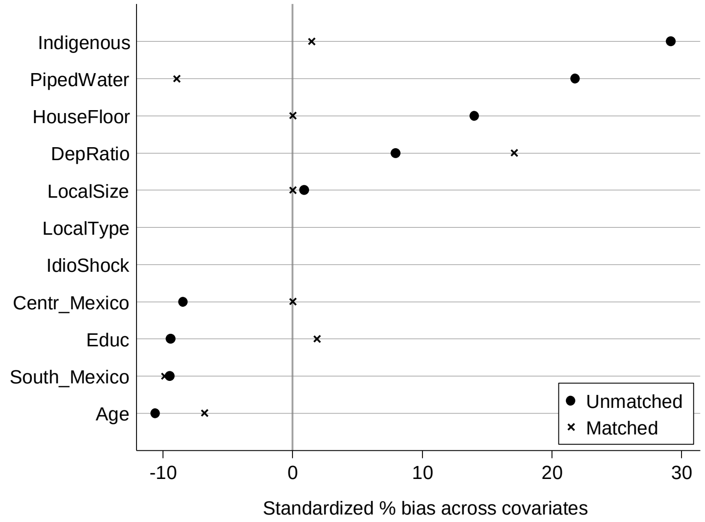
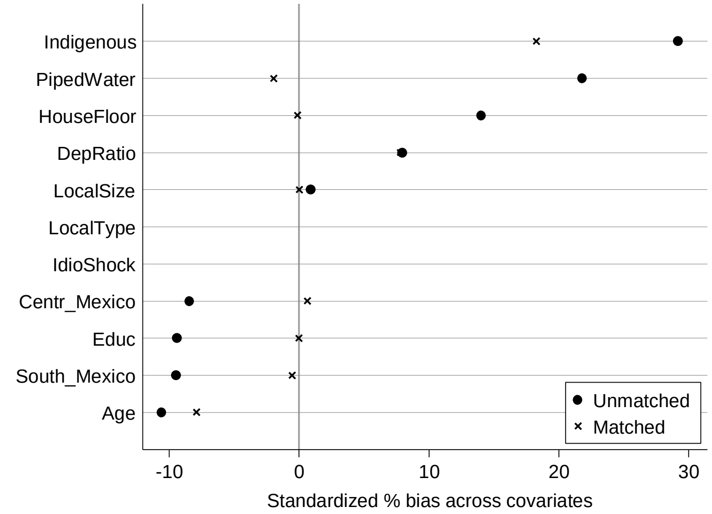



Figure A24: Shock Coping (rural): kernel matching – bias reduction




Source: Authors.