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Social service delivery and access to financial innovation

The impact of Oportunidades' electronic payment system in Mexico

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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 bank account is found to have implications in terms of reallocation between saving portfolio choices, transaction costs, and coping strategies. The study shows that, following the intervention, participation in informal saving arrangements was reduced, the frequency of remittance reception increased and, when hit by idiosyncratic shocks, beneficiaries of bank accounts were more likely to use savings rather than contracting loans or reducing consumption to cope with the events. The study also reveals impact heterogeneity between rural and urban areas, with important implications for policy and replicability of similar financial 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

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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 efficiency of social services, rigorous evidence of their impact is scarce throughout the developing world. This study contributes to the literature on social service delivery by examining the impact of a recent electronic payment system introduced by the Mexican government to distribute the flagship *Oportunidades* cash transfer programme.

The electronic payment system was implemented 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 (SAPs), 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 income entitlements through electronic transfers in banks accounts in non-banking institutions. Most Oportunidades participants continued to receive payments in cash through distribution points located in the nearest town. This study takes advantage of the availability of a rich household-level dataset (BANSEFI-SAGARPA Panel Survey 2004-2007) that was collected by BANSEFI and the Secretariat of Agriculture, Livestock, Rural Development, Fisheries and Food (SAGARPA), during the phasing-in, and roll-out process of the electronic payment programme, 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, to rule out potential endogeneity problems, and use the variation in observables to carry out a matching-based impact analysis. In particular, we investigate the four-year impact of the electronic transfer programme on savings decisions, transaction costs, and coping strategies against idiosyncratic risks. To the best of our knowledge, this is the first study of financial innovation in social service delivery to the poor in Mexico.

The results indicate that households who received their transfer in a bank account decreased their participation in informal saving arrangements, increased the frequency of remittance reception, and were more likely to use their savings to cope with idiosyncratic shocks. We also find a degree of outcomes heterogeneity, which seems to be contingent upon the environments that characterize rural vs. urban areas in Mexico.[1]

The rest of the paper is structured as follows: Section 2 outlines the theoretical background; Section 3 provides contextual details and describes the electronic payment system; Sections 4 and 5 contain information on the data and the estimation methods. Section 6 presents both a

1 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.

discussion on the results and a sensitivity analysis, while Section 7 concludes with some reflection on policy.

2 Background

The importance of financial development and financial inclusion for growth and poverty reduction has been explored extensively in the Economics literature (see Deaton 1990; Giné and Townsend 2004; Burgess and 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) explains that consumption-smoothing and insurance motives are very often the reasons behind the savings accumulated by low-income households. Demirgüç-Kunt et al. (2008) highlight the importance of having access to financial services such as payments and transfers instruments linked to remittances.

Financial innovation can also play a key role in promoting financial development and broadening financial inclusion. While the traditional view links the poor to a limited savings capacity due to resource constraints, more recent evidence also suggests that there are other important factors associated with inter alia, household intertemporal preferences, information asymmetries, transaction costs, intra- and inter-household dynamics and resource constraints. These factors require a different set of policy tools and tailored financial instruments to respond more effectively to the needs of the poor (Karlan et al. 2013).

It is not unusual for the poor to 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 seem to be more likely to have higher credits and 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 levels (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. These responses may involve debt contraction, debt default, or consumption reduction.

Intra- and inter-household allocation dynamics can play an important role too. Intra-household co-operative disequilibria associated with e.g. conflict or disagreement between partners surrounding consumption decisions can influence households' saving decisions. In Kenya, for example, 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).

When self-control or intra- and inter-household dynamics are an issue, saving commitments or incentive instruments have proved to be an effective solution (Ashraf et al. 2006; Ashraf et al. 2010; Brune et al. 2013). They facilitate financial planning, and offer the possibility to guard savings or hide them away. In this way, immediate consumption is discouraged, and both partner and family pressures are relieved. Instruments of this type can be explicitly tied to an incentive and retribution mechanism, or they can involve an implicit encouragement element. For example, Dupas and Robinson (2013) showed that, although no interest rate was provided, the street vendors targeted by their intervention chose to hold money in a savings bank account only to avoid appropriation by spouses or relatives.

Resource constraints are another important factor to consider, with the co-existence of supply-side and demand-side considerations. On one hand, limited access to, or long distances to branches of financial institutions represents a supply-side constraint which translates into transaction and opportunity costs for clients. On the other hand, low-income households' inability to pay the fees attached to the use of formal financial services limit the demand for such services. A number of policy strategies have aimed to tackle these constraints. For example, Townsend (2006) reports that village-based small scale financial institutions play an important role in increasing households' asset ownership in Thailand. Klein and Mayer (2011), found that the mobile-based transaction system M-PESA, developed by Safaricom in Kenya, considerably cut transaction and opportunity costs for the poor, while at the same time increasing their exposure and familiarity with financial innovation.

Also in Kenya, Schaner (2011) 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. Arestoff and Venet (2011) analysed the introduction of 'Orange-money' in Madagascar; which provided mobile-based deposit and transfer services. They found no impact on deposit rates, but showed that the frequency of remittances considerably increased for 'Orange-money' clients. 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 the specific case of Mexico, Aportela (1999) reports evidence from a natural experiment involving the opening of new branches of a government banking institution, Panhal, 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. Aportela finds, however, that no substitution or displacement of informal saving mechanisms occurred. Bruhn and Love (2009) 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.

More closely related to our study is the study where Woodruff and Martinez (2008) considered the impact of the Mexican 'Program to Strengthen the Popular Credit and Saving Sector', launched in 2004. They find an increase in the penetration of non-banking financial institutions between 2004 and 2007. The latter, however, took place 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. Finally, Seira (2010) analyse the transaction flows related to the *Oportunidades* electronic payment intervention considered in our study. Seira's transaction flow data was recorded only for the subset of beneficiaries who already received payments via debit cards at the time. The study shows that the banked beneficiaries did not withdraw the whole sum corresponding to their *Oportunidades* transfer and saved part of it in the account, suggesting that low income households in Mexico do save when appropriate financial instruments are provided to them.

3 Context and intervention

Oportunidades (before known as *Progresa*) is Mexico's flagship antipoverty social protection programme. It was launched in August 1997 to cover 300,700 households in 6,344 rural municipalities. By 2013, the programme supported nearly six million households living in 109,852 marginalized rural, peri-urban and urban localities (Oportunidades 2013). Its objective is to break the intergenerational cycle of poverty by enhancing the development of human capabilities through education, health, and nutrition. Oportunidades provides income supplements to poor families in exchange for certain commitments, such as regular school attendance and periodic health clinic visits (Niño-Zarazúa 2011).

Oportunidades' income supplements were 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). 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 for beneficiary households, and second, to broaden the limited financial inclusion in the country.

Data on financial inclusion collected by the National Banking and Securities Commission in 2006 showed a very limited financial penetration in rural and peri-urban communities. 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 by Caskey et al. (2006) that 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. A census conducted by BANSEFI in 2002 also found that the non-banking sector—integrated by about 630 institutions with nearly four million clients—had a market penetration rate of only 17 per cent (Gavito 2002).[2]

BANSEFI's distribution network achieved 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 most of Oportunidades' beneficiaries live. At the end of the pilot intervention, *L@ Red de la Gente* had already served more than 700 municipalities, and by 2010, it had achieved 80 per

2 Non-banking institutions in Mexico include credit unions, savings and credit associations (SAPs), 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. SAPs are non-profit organizations with open membership. As in the case of credit unions, financial operations within SAPs are constrained to receive deposits and give credits to their members. CAPs 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. SAPs as well as CAPs usually operate in rural and peri-urban areas.

cent of national coverage. The pilot intervention involved the opening of bank accounts for Oportunidades beneficiaries in 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 travelling distance and associated costs were substantially reduced for treated households. Seira (2010) has reported that as the result of the electronic payment system, the opportunity and financial costs for rural households associated with the collection of Oportunidades decreased by 77 per cent and 98.5 per cent, respectively.[3]

4 Data

In 2004, BANSEFI and SAGARPA began a household panel survey in 25 of Mexico's 32 federal states. The survey covered 5,768 households, clients and non-clients of non-banking institutions. The sampling frame was designed to be representative at three regions: north, centre and south, from which a sample of non-banking institutions was randomly selected with a probability proportional to their number of clients (Woodruff 2006). For each of the selected branches, about 20 to 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. The results showed that 2,647 households had savings accounts while the remaining 3,141 households had not.

The survey was then repeated for another three rounds, in 2005, 2006, and 2007, for a total number of 17,680 observations. For the purpose of this study, we retain only those households that between 2004 and 2007 were *Oportunidades* beneficiaries. This is a subset of the overall sample, and it amounts to 6,218 observations when pooled over the four years. Of these, 211 observations were discarded due to inconsistencies in the reported modalities of cash transfer delivery, which left us with 6,007 observations.[4]

An additional 2,746 observations were excluded, at this stage, because they identified households who switched from cash to electronic transfers only after the beginning of the intervention in 2003. While in principle it is interesting to investigate the impact of the intervention on this subset, for the purpose of our analysis we will only focus on the group of 'always compliers'. This is in order to retain comparability, given that the year of switching differs from household to household, and the differential in exposure to treatment would bias the impact estimate. Retaining only the households that receive *Oportunidades* via either of the same delivery method, whenever surveyed, leaves us with 3,261 observations in total.

In addition, we dropped an additional 258 households that appeared only in the baseline survey. This was due to the decision by BANSEFI to substitute these with new households, clients of microfinance institutions. This leaves us with a final sample of 3,003 observations. Of these, 1,806 received the Oportunidades grant via electronic transfers whereas 1,197 did it in cash. Given the data requirements for the matching-based quasi-experimental impact evaluation presented below, we employ the pooled sample over the four years. By doing this, we obtain

3 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.

4 In particular, throughout the length of this pilot, Oportunidades beneficiaries could receive their income transfer via two delivery modalities only: either in cash or via a bank account. Some households reported payments in kind, though a cheque, or mentioned other methods. When these inconsistencies could not be resolved by cross-checking with other questions that helped detect mistakes attributable to enumerators' transcription, the observations were dropped.

robust estimates although at the cost of being unable to exploit the time dimension, the impact of which we are not able to disentangle.

Four outcome variables are considered, 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, 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 profiles of the sampled households. Budget constraints are likely to partly explain 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.

Table 1: Pre-intervention characteristics 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%.

Source: Authors.

The survey questionnaire provides data on a number of household- and location-covariates. For all of them, Table 1 reports both 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.

The ability to compare outcomes between the treatment and control groups critically depends on the selection process of the electronic transfer programme. Since the main criterion for participation in the programme was that the *Oportunidades* beneficiaries lived within the catchment area served by a branch of the non-banking institution, and given that the decision to join L@ Red de la Gente was made by the managers of the non-banking institutions, and not the households themselves, who had no power to accept, reject, or request the bank account, we rule out any potential endogeneity problem arising from household self-selection. Thus, the treatment group arguably differ to the control group only in that those receiving the electronic transfer of Oportunidades experienced a reduction in the associated costs of receiving the grant in cash.

Nonetheless, we cannot rule out the presence of endogeneity problems if systematic heterogeneity exists 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 live.

Table 1 indicates that treated households are significantly more likely to be located in urban and peri-urban areas, which 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 exhibit statistical difference from zero. For example, the presence of piped water in the house and whether the house floor is made of concrete. It appears that treated households enjoy better infrastructural quality levels. Similarly, treated household are less likely to have experienced an idiosyncratic shock of the types described above, which may again be associated with differences in environmental settings. Finally, dependency ratios are only marginally lower in treated households.[5] Heads of treated households tend to be marginally older and more educated, although the difference is only statistically significant at the 10 per cent level. They are, 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. Therefore, we adopt a methodology that allows us to control for these sources of bias. The next section presents the methodology in more detail.

5 The dependency ratio estimated here is adjusted to treat household members who did not contribute to household income as dependents. 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 alongside the impact variable, i.e. the treatment measure are regressed on the outcomes of interest. The linear OLS specification is depicted as:

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

where D_i is a dummy variable taking the value one for households receiving *Oportunidades* via electronic payments and zero, 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 clearly arise from model misspecification as well as the risk of overlooking the potential effect of observed and unobserved heterogeneity affecting the outcomes of interest. A partial step towards addressing observables 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, with $D_i = 0$, 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, a number of covariates in X allow 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. Rosenbaum and Rubin (1983) argue that propensity score methods provide the coarsest balancing score, whereas covariate matching provides the finest balancing score. Zhao (2004) explains, 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:

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

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:

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

or

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

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:

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

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 (α) 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 α to be fixed at 0.1.

Before moving to the results, we recall that any threat of violating the Conditional Independence Assumption (CIA) due to endogeneity from household self-selection is ruled out, although local-level heterogeneity remains an issue. To address this shortcoming, we will 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 unreliable due to the fact they do not address the concerns related to the common support and heterogeneity in observables. A first step towards correcting for this constraint 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 to 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 *tanda*s 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.

Table 2: OLS and FILM estimation

	Tanda		HomeSaving		Remittances		ShockCoping	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	FILM	OLS	FILM	OLS	FILM	OLS	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%

Source: Authors.

Locality size only seems to be associated with impact heterogeneity in the case of remittance reception, where more densely populated areas receiving 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 biases in the covariate distribution are 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 with statistical significance in Table 1. Table A1 in the Appendix presents the results from the probit. 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 is presented, where the household sample is disaggregated into rural and urban locality type. The post-estimation routines proposed by Leuven and Sianesi (2003) allow 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 non-treated groups, and the overall mean bias reduction achieved can also be assessed. These details are presented separately in Tables 6-8.

Starting with Table 3, matching on the whole sample indicates that electronic transfer programme 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 implicit risk of losing the funds, may play a role here. Anderson and Baland (2002) and Gugerty (2007) argue that ROSCAs may be used as a saving commitment device. However, Dupas and Robinson (2013) also show that a savings account in a financial institution works in a similar way, by household members, notably women, to keep savings away from sharing pressure from relatives or appropriation risks from spouses. In line with these findings, our results indicate that there is a relatively small substitution effect between saving portfolio choices, with the poor favouring bank accounts over informal saving arrangements.

Interestingly the propensity to save at home, which is the 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 indicates that transaction costs may play an important role. The possibility of saving in a bank account provides an alternative to lower transaction costs in group savings; however, when transaction costs are minimal or inexistent, as in the case of keeping money 'under the mattress', no substitution takes place. Our findings are in line with those of Aportela (1999). The third outcome of interest is the frequency of remittance reception, which does not seem to be influenced by the electronic payment programme. However, when the impact specification is carry out using fixed effects estimates (see Tables 4 and 5), the results turn out to be positive for rural households. We return to this issue in the paragraphs below.

Finally, we find that households who receive the Oportunidades grant 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 loan 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 existing credits. These clearly are undesirable consequences, particularly when considering that for Oportunidades beneficiaries who live near 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 the electronic payment programme.

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. The finding 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, tandas seems to be in Mexico predominantly an urban phenomenon. Klaehn et al. (2006) point out that only 7 per cent of the rural population use tandas as a saving instrument.

The impact of the electronic payment 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) to calculate 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, this frequency nearly doubles. That is, the same household that received an average of one remittance every two months is now able to receive transfers every month. Arestoff and Venet (2011) found similar results in Madagascar, where the availability of a mobile-based transaction system translated to an increase in frequency of remittance reception.

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 at localities with limited or no access to banking services. As a result, transaction and opportunity costs were greatly reduced; which translated into an increase in the frequency of remittance reception among those poor households that were dependent on money transfers.

The impact of the programme on coping strategies is also found to be 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 households receiving Oportunidades in cash. The impact is in the order of 8 to 10 per cent. The absence of significant impact in urban areas may be driven by the higher incidence of idiosyncratic shocks in rural areas and the more pronounced impact on the households that were more disadvantaged to start with.

6.3 Matching quality

To assess the quality 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. Table 6 indicates that over 98 per cent of mean bias reduction was achieved in the whole sample estimation. For the last outcome, while a smaller average bias exists in the unmatched sample to start with, a lower bias reduction of around 85 per cent is achieved after matching. The comparison of the likelihood-ratio test statistics and their corresponding p-values for the unmatched and matched sample confirms that in the latter no explanatory power is left to the covariates. This, in turn, allows us to attribute the differences in outcomes between the treatment and control groups to the intervention. Tables 7 and 8 report similar findings with regard to the split sample. 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

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

	Tanda			HomeSaving			Remittances			ShockCoping		
	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			HomeSaving			Remittances			ShockCoping		
	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			HomeSaving			Remittances			ShockCoping		
	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 supp	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%

Source: Authors.

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.

Nearest neighbour matching, consisting in matching control households only once to the closest treated household, incurs in problems with the overlap region, where probability density is very low. To avoid such bias, it is possible to allow 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 concentration ratio percentage figures for all nearest neighbour estimations presented in Tables 3-5. In this case, 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.

Table 6: Matching quality – % mean bias reduction and pseudo R^2 (whole sample)

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

Source: Authors.

Table 7: Matching quality – % mean bias reduction and pseudo R^2 (urban sample)

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

Source: Authors.

Table 8: Matching quality – % mean bias reduction and pseudo R^2 (rural sample)

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

Source: Authors.

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

	Tanda	HomeSaving	Remittances	ShockCoping
whole sample	69	68.5	69.5	52.6
urban sample	56	56	56	22
rural sample	71.3	73	72	57.5

Source: Authors.

6.4 Sensitivity analysis

Although self-selection into treatment is not a source of concern in our study, we do not have an experimental design. Therefore, 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 rely, 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.[6] 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 are sensitive to a violation of the CIA. The confounder 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 $Y_0, Y_1 \in \{0,1\}$.[7] 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

6 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.

7 The discussion extends to continuous treatment cases.

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.

Table 10 reports the ATTs simulated when a confounding factor U, defined by each of the selected configuration sets, is included in the model. Following Nannicini (2007), the configuration sets are specified so as to represent an increasingly dangerous confounder. The first 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. In all instances, outcomes are remarkably stable. We conclude, therefore, that unobservable factors do not pose a threat to our results.

Table 10: Sensitivity analysis

	Tanda			HomeSaving			Remittances			ShockCoping		
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%

Source: Authors.

7 Conclusions

This study analyses a case of financial innovation in social service delivery, which was implemented in Mexico in the context of the cash transfer programme *Oportunidades*. Currently, nearly all *Oportunidades* beneficiaries receive the transfer electronically. The possibility of using a rich household-level dataset—representative at the three main regions of Mexico—that was collected during the phase-in of the programme gave us the opportunity to construct a quasi-experimental design that in turn, allowed us to evaluate the impact of the electronic payment system.

Our findings clearly indicate important distinctions in policy impact between rural and urban areas. The decreased participation in informal saving arrangements was limited to urban settings, where *tandas* most often take place. In the rural sector; however, *Oportunidades* beneficiaries who receive their income supplement in a bank account received remittances more frequently. They also appeared more likely to cope with idiosyncratic shocks using their own savings, as opposed to contracting loans or reducing consumption levels.

Our results can be associated with two strands of literature. First, the role of transaction costs in interpreting the substitution effect between formal and informal savings particularly *tandas*, which involve costs in terms of peer-monitoring, organization effort, and risks of insolvency. This may explain why, when provided with a bank account, households' participation in tandas decreases.

In rural areas, lack of access to financial services represents a supply constraint and translates into high transaction costs for households wishing to access such services. This seems to explain why the provision of a bank account facilitated the access to remittance reception.

Second, the intervention also achieved a shift in households' choices to cope with idiosyncratic risks. The role of savings in deprived communities is often that of insurance against shocks. Debt contraction often leads to default and its subsequent negative repercussions; while consumption reduction can impair the working potential of the household and lead to future welfare loss. The fact that treatment households resorted to their own savings as a shock coping strategy represents an outcome of the intervention. The shift away from debt accumulation and consumption reduction to usage of savings is likely to reflect improvements in financial planning and consumption smoothing strategies. While the availability of quasi-experimental data gave us the opportunity to assess the impact of the electronic transfer programme on various wellbeing dimensions, further research is still needed to examine longer-term impacts that may not have been materialized within the time window of analysis covered by our quasi-experiment. Additional research could go beyond savings portfolio choices, and explore changes in saving quantities, and the use of other financial services.

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Appendix

Table A1. Probit regressions, marginal effects

	Tanda	HomeSaving	Remittances	Shock Coping	Tanda	HomeSaving	Remittances	Shock Coping	Tanda	HomeSaving	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 χ^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%.

Source: Authors.

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

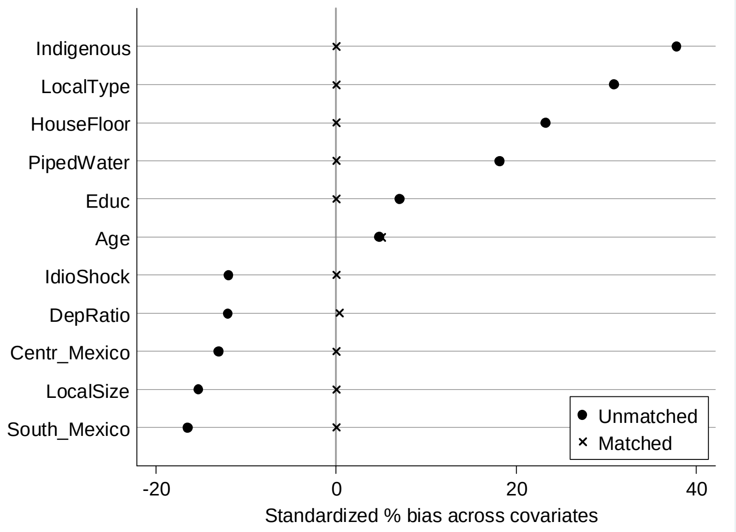


Figure A2: Tanda participation: kernel matching – bias reduction

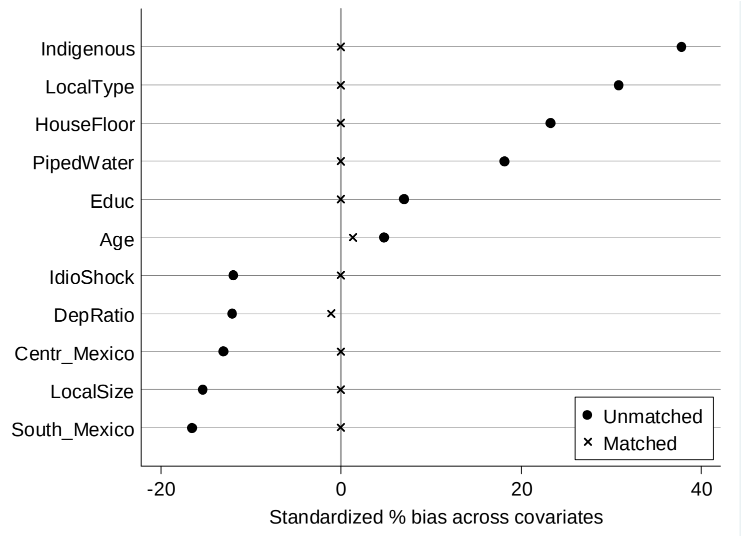


Figure A3: Tanda participation (urban): nearest neighbour matching – bias reduction

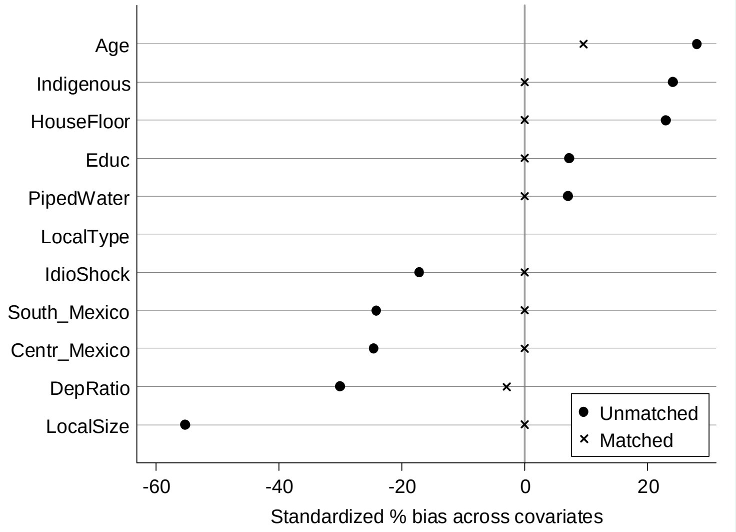


Figure A4: Tanda participation (urban): kernel matching – bias reduction

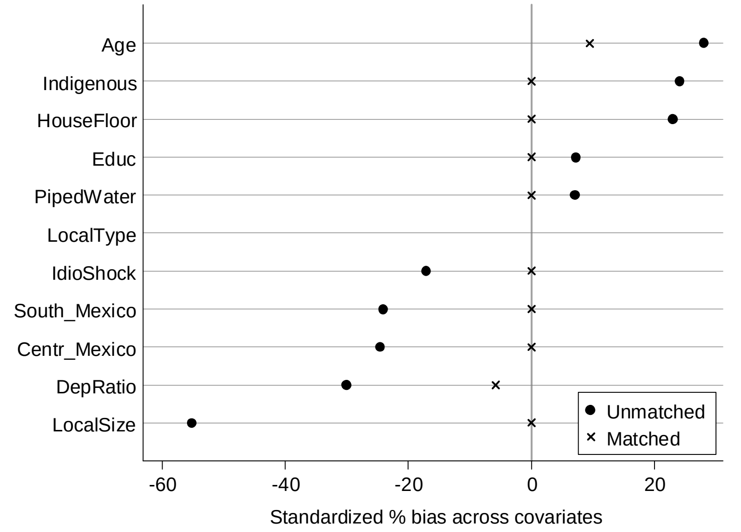


Figure A5: Tanda participation (rural): nearest neighbour matching – bias reduction

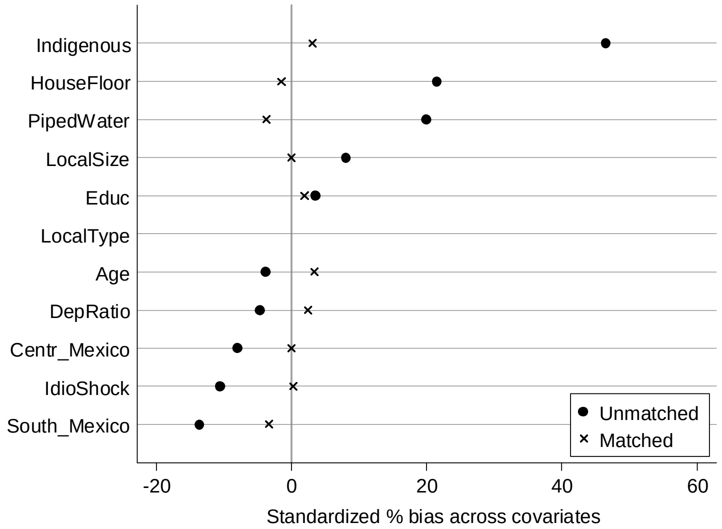


Figure A6: Tanda participation (rural): kernel matching – bias reduction

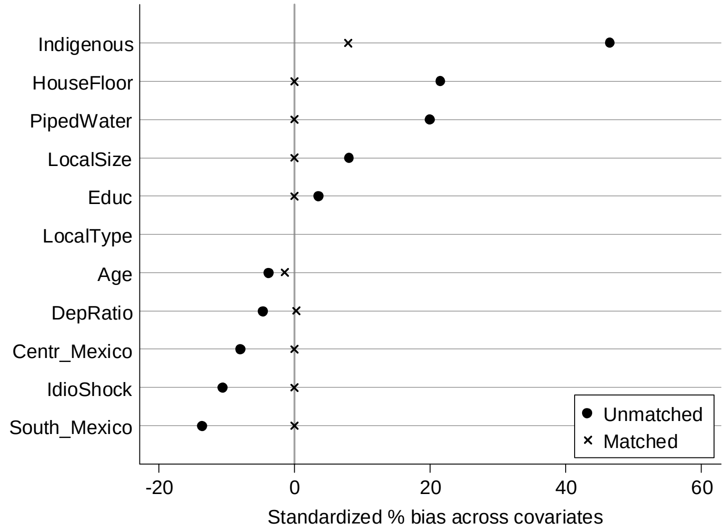


Figure A7: Home saving: nearest neighbour matching – bias reduction

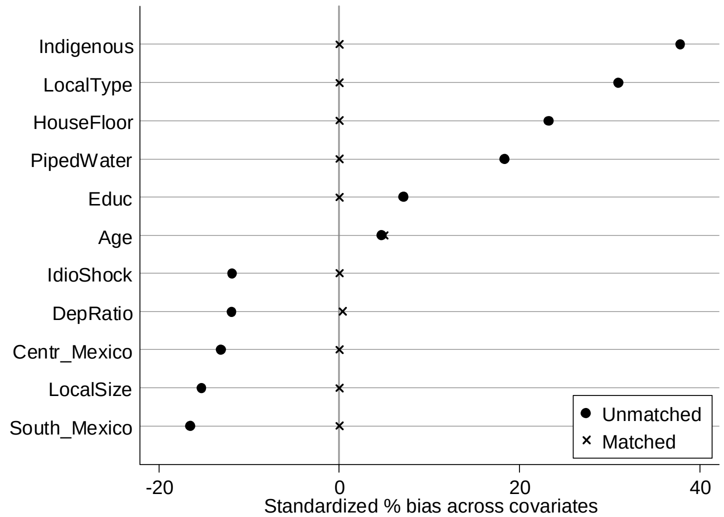


Figure A8: Home saving: kernel matching – bias reduction

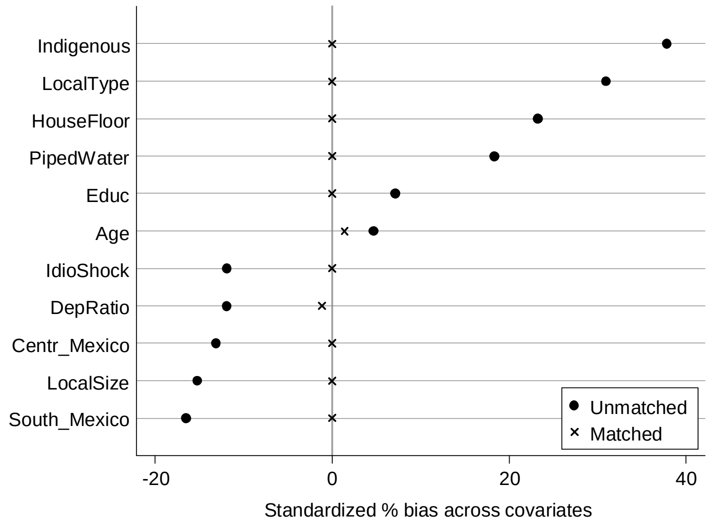


Figure A9: Home saving (urban): nearest neighbour matching – bias reduction

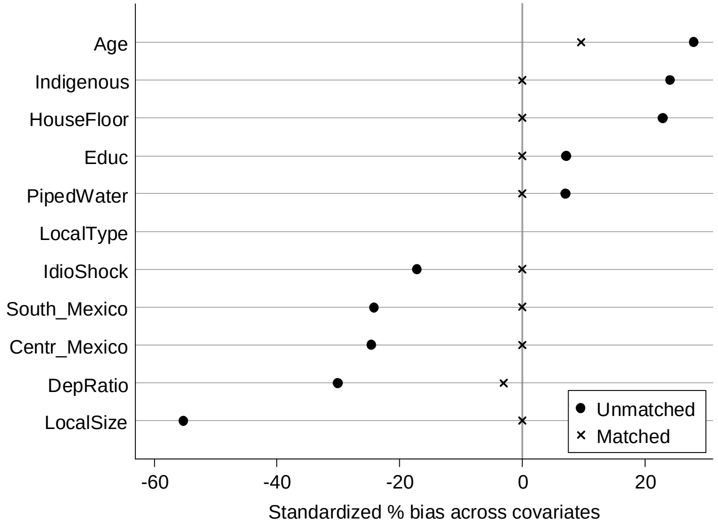


Figure A10: Home saving (urban): kernel matching – bias reduction

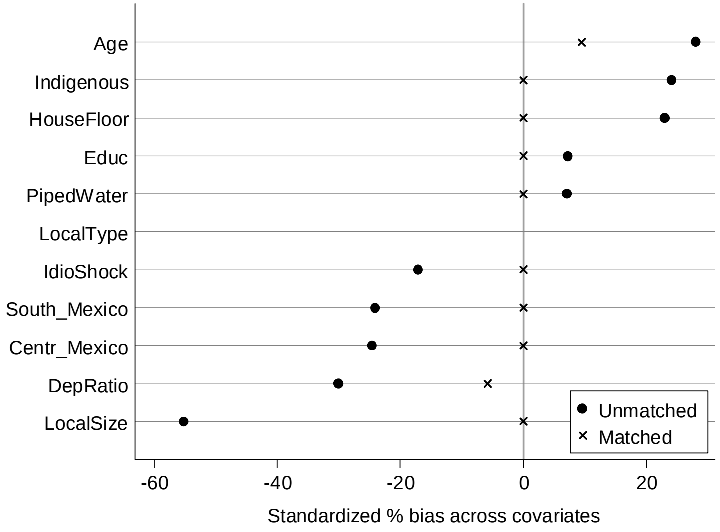


Figure A11: Home saving (rural): nearest neighbour matching – bias reduction

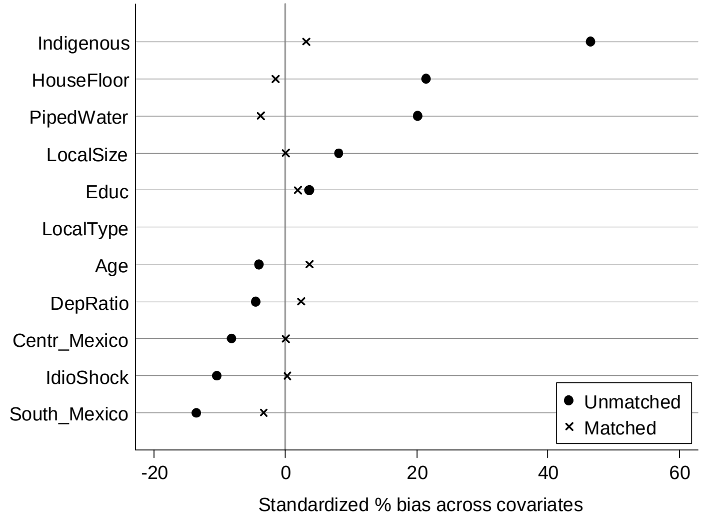


Figure A12: Home saving (rural): kernel matching – bias reduction

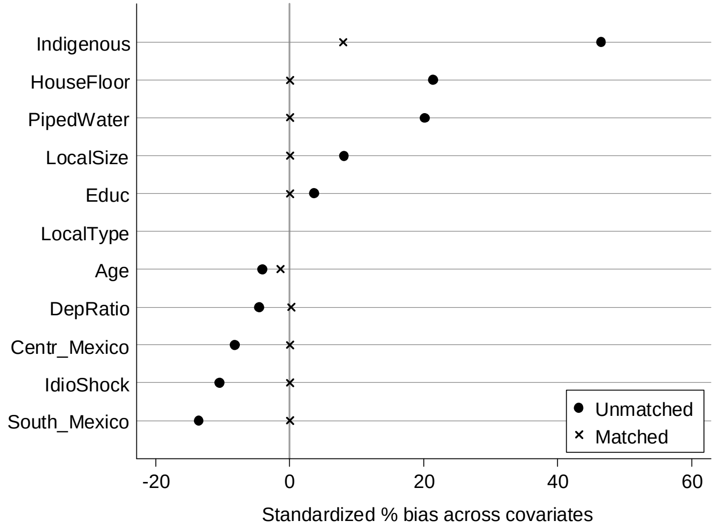


Figure A13: Remittances: nearest neighbour matching – bias reduction

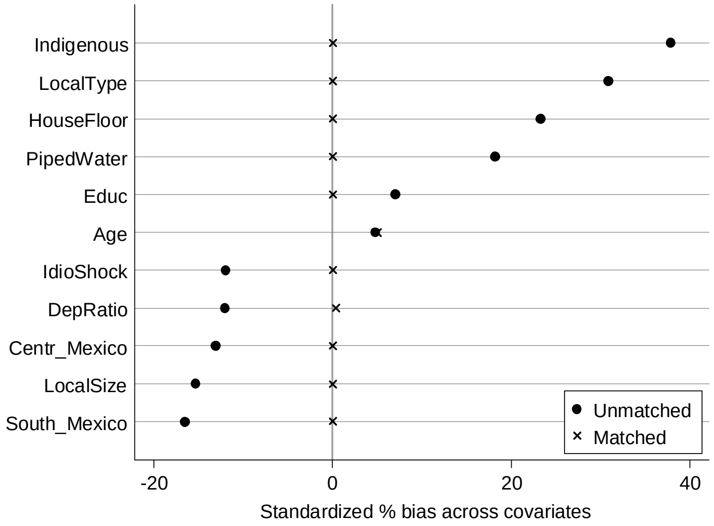


Figure A14: Remittances: kernel matching – bias reduction

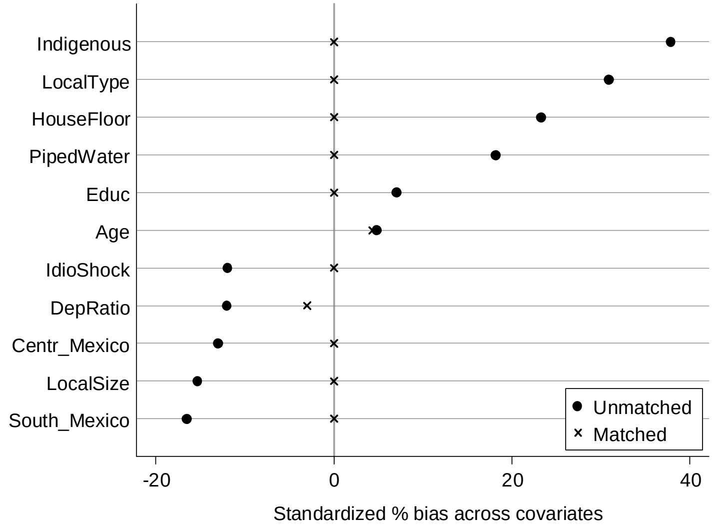


Figure A15: Remittances (urban): nearest neighbour matching – bias reduction

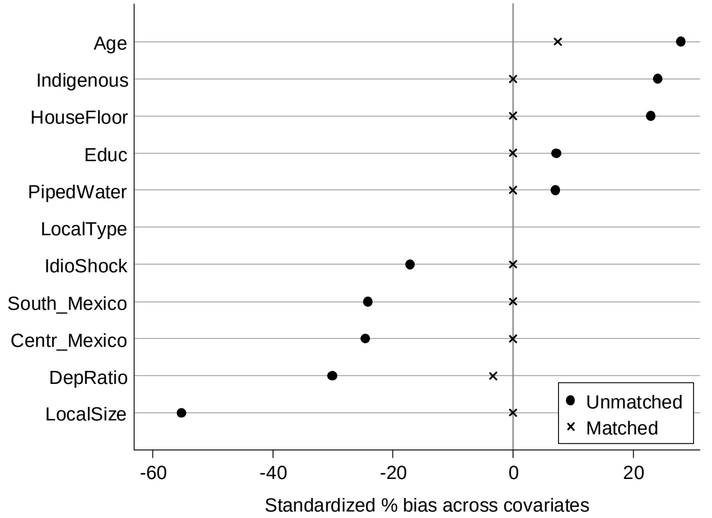


Figure A16: Remittances (urban): kernel matching – bias reduction

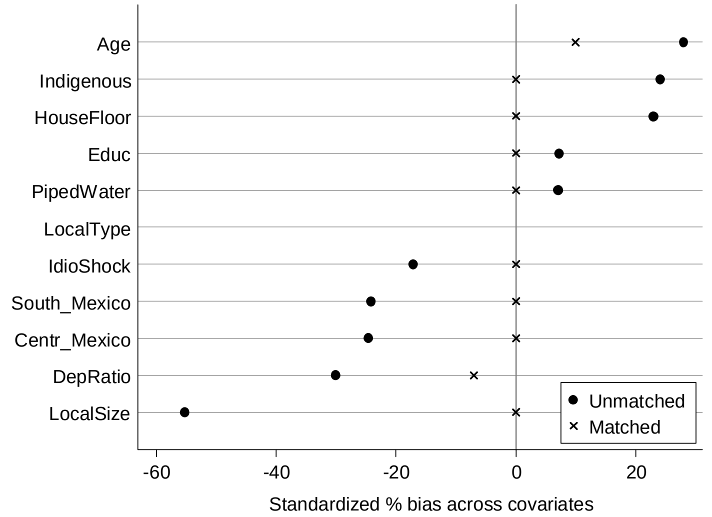


Figure A17: Remittances (rural): nearest neighbour matching – bias reduction

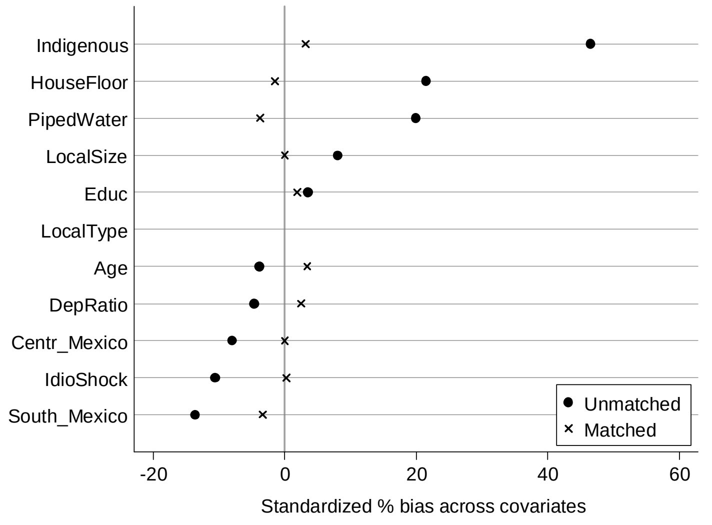


Figure A18: Remittances (rural): kernel matching – bias reduction

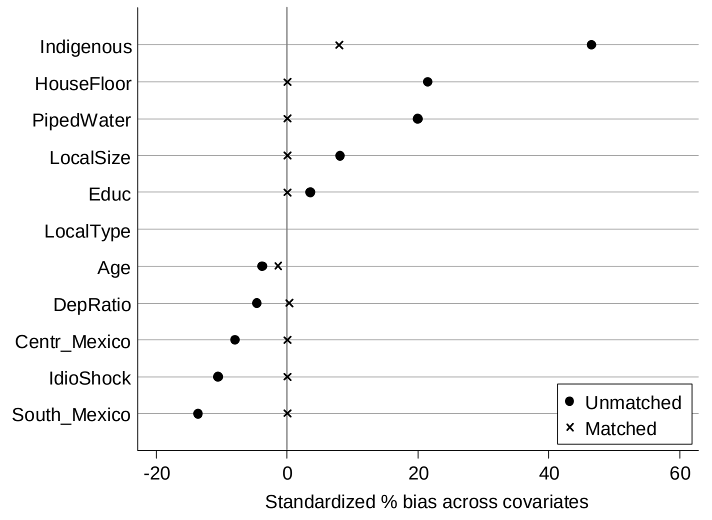


Figure A19: ShockCoping: nearest neighbour matching – bias reduction

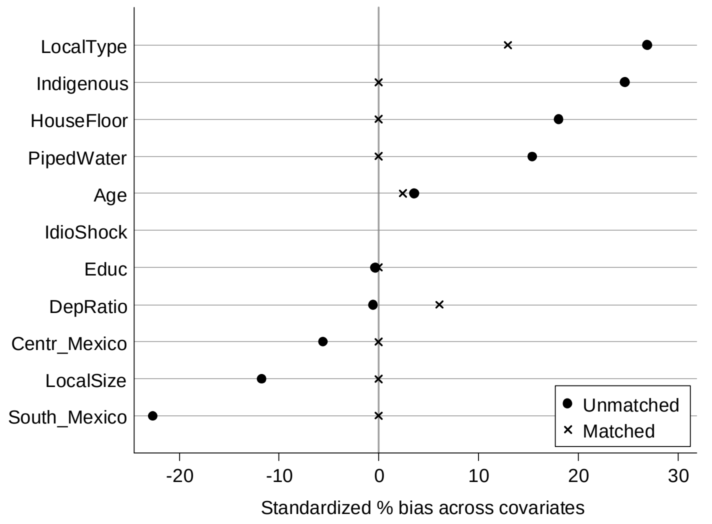


Figure A20: ShockCoping: kernel matching – bias reduction

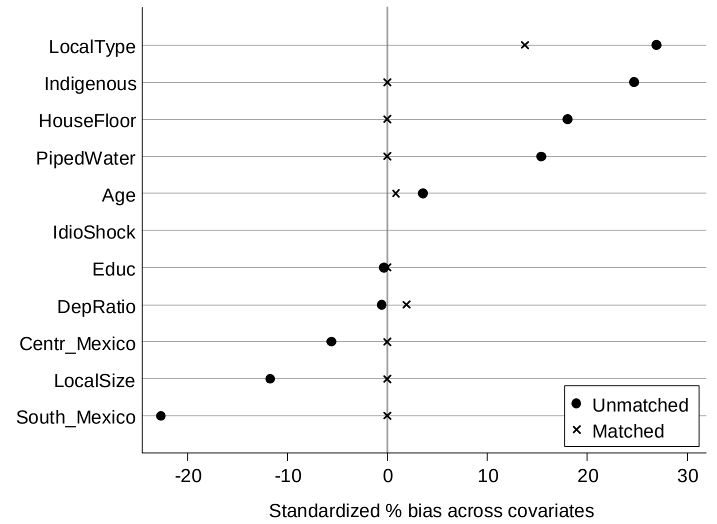


Figure A21: ShockCoping (urban): nearest neighbour matching – bias reduction

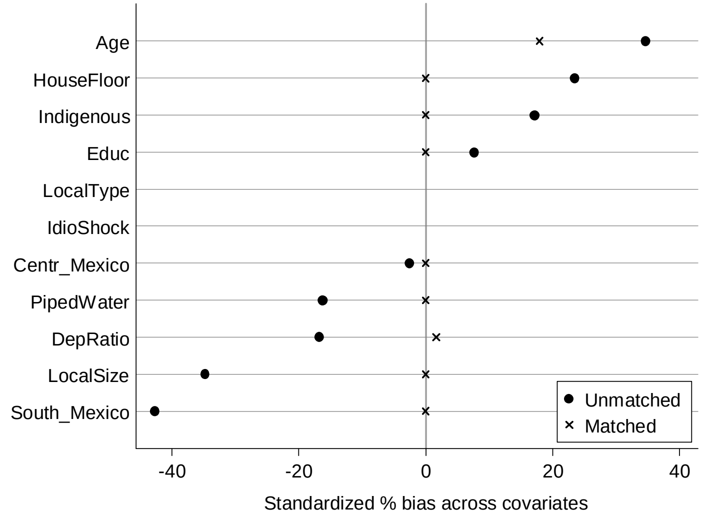


Figure A22: ShockCoping (urban): kernel matching – bias reduction

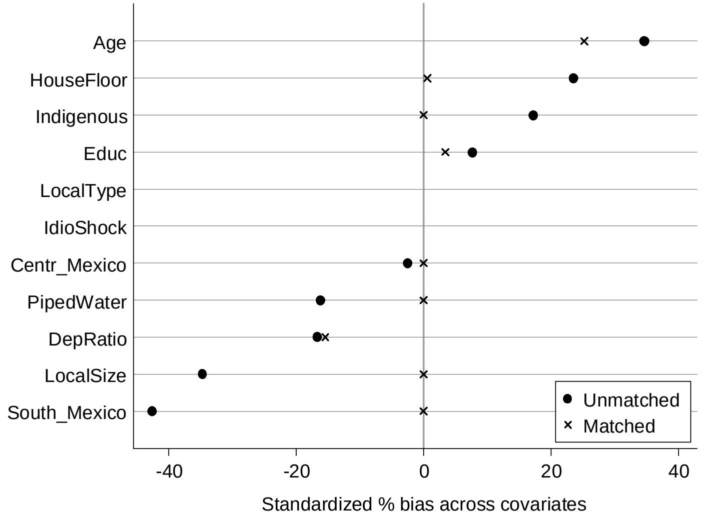


Figure A23: ShockCoping (rural): nearest neighbour matching – bias reduction

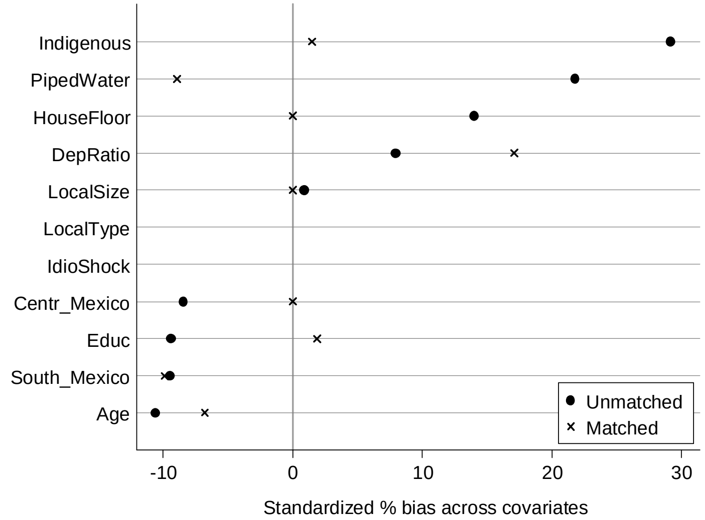
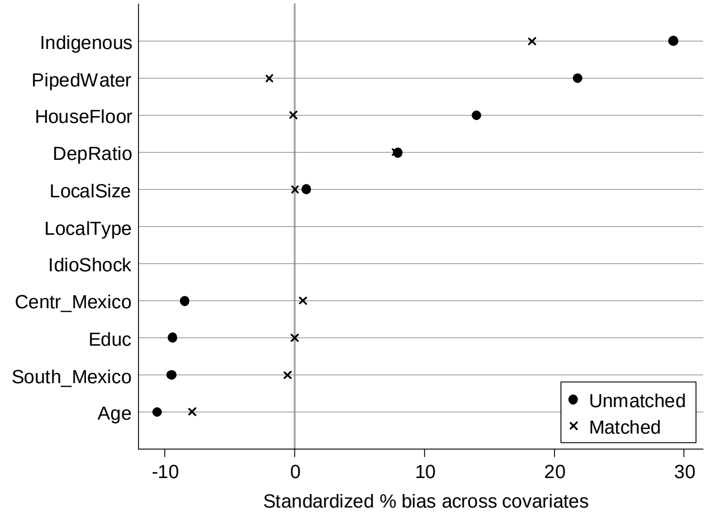


Figure A24: ShockCoping (rural): kernel matching – bias reduction



Source: Authors.