ONLINE SUPPLEMENTARY MATERIAL

1. Data description

The survey questionnaire provided data on a number of household- and location-covariates. For all of them, Table S1 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. More precisely, Table 2 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. Finally, treated households are more likely to be members of slightly larger financial institutions than controls.

Table S1. 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
FInst	0.103	0.304	0.06	0.238	0.000***	2637
Econ_Scale	61359	5049	65825	4586	0.513	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%.

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.[1] 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.

2. OLS and FILM

The OLS estimates are likely to be biased 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 S2, 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

1 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.

reception. The same holds for households in urban areas, those belonging to indigenous groups, and those who suffer idiosyncratic shocks.

Table S2: OLS and FILM estimation

	Tanda		Home Saving		Remittances		Shock Coping	
	(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)
FInst	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
FInst*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%

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.

3. Matching quality

To assess the quality of the ATT matching estimators and the sensitivity of the results, Tables S3-S6 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 S3 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 S4 and S5 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.

Table S3: 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 Mean |bias|	17.35 (9.92)	17.35 (9.92)	17.35 (9.94)	17.35 (9.94)	17.35 (9.92)	17.35 (9.92)	12.94 (10.07)	12.94 (10.07)
Matched Mean |bias|	0.5 (1.55)	0.226 (0.506)	0.47 (1.46)	0.23 (0.515)	0.5 (1.55)	0.668 (1.51)	2.14 (4.27)	1.65 (4.29)
Unmatched Pseudo R^2	0.101 (0.000)	0.108 (0.000)	0.102 (0.000)	0.108 (0.000)	0.101 (0.000)	0.108 (0.000)	0.078 (0.000)	0.069 (0.000)
Matched Pseudo R^2	0.000 (0.996)	0.000 (1.000)	0.000 (0.998)	0.000 (1.000)	0.000 (0.996)	0.001 (0.999)	0.005 (0.978)	0.004 (0.993)

Table S4: 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 Mean |bias|	24.01 (13.51)	24.01 (13.51)	24.01 (13.52)	24.01 (13.52)	24.01 (13.51)	24.01 (13.51)	21.76 (13.32)	21.76 (13.32)
Matched Mean |bias|	0.178 (0.000)	0.178 (0.000)	0.178 (0.000)	0.178 (0.000)	0.178 (0.000)	0.178 (0.000)	0.178 (0.000)	0.178 (0.000)
Unmatched Pseudo R^2	1.22 (2.96)	1.52 (3.32)	1.22 (2.96)	1.52 (3.32)	1.04 (2.35)	1.69 (3.63)	2.18 (5.94)	4.95 (9.11)
Matched Pseudo R^2	0.002 (0.984)	0.003 (0.977)	0.002 (0.984)	0.003 (1.000)	0.001 (0.998)	0.003 (0.955)	0.011 (0.956)	0.019 (0.857)

Table S5: 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 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)

In nearest neighbour matching control households are matched to the closest treated household. However, this incurs problems in those 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 S6 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 S6 ABOUT HERE]

4. Impact Heterogeneity and Robustness Analysis

Our analysis also reveals important dimensions of impact heterogeneity (see tables S7 to S13). Older POP beneficiaries, with a lower educational background, displayed a higher propensity to substitute savings in tandas for savings in programme accounts. These households were also less likely to have saved at home in the previous year (Table S1). 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, Martinez 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 programme saving accounts may pinpoint the benefits of anonymity from formal savings arrangements.

Interestingly, we find that the impact of electronic payments is more pronounced among those with higher dependency ratios (see Table S2). 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. 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. 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 savings account 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 unsurprisingly more prone to resort to their savings to cope shocks as a result of treatment –as opposed to contracting debt or reducing consumption.

[INSERT TABLE S7 – S9 ABOUT HERE]

In Table S10, we included a covariate to capture the scale of localities' economy as proxied by population size. Table S1 demonstrated that there does not appear to be any imbalance with respect to the scale of the economy at baseline. However, one could hypothesise that bank branches locate themselves in areas where the scale of economic activity is larger. If this was the case, we would expect outcomes to be affected by higher levels of economic activity, thus violating the CIA.[2] This does not appear to be the case in our sample, where we can see that results carry over from the benchmark, albeit with slightly decreased coefficients. In addition to this, the pre/post balancing comparison appearing in Figure S1 below shows the imbalance of covariates before and after matching the treated and covariate samples in a sample graph for one of the estimation regressions. It can be seen in the graph that the *Econ_Scale* variable is actually among the ones that has the least bias imbalance to correct. This is the case in all of the twelve estimation graphs, except one where there is more of an imbalance that gets corrected, because the sample for *ShockCoping* is different to start with.

In fact, it is indeed the case that in our study, in all rural and urban samples, treated tend to reside in less populous locations than controls. Hence, if anything, economic activity may be more intense where non-treated households reside, thus casting doubt on the concern that outcome estimates may be biased by the fact that treatment-associated financial institutions are located in areas with more economic activity. It is important to note that in the "whole sample", matching does correct an actual bias by reducing the difference between treated households in more populous localities and control households in less populous ones. But this bias is definitely among the smallest in our sample, as shown in Figure S1.

[2] We thank an anonymous referee for this suggestion.

Finally, Tables S15-S17 show that locality types do not univocally identify treatment status of household. This means that there are three types of localities in our sample: (i) those where households included in the pilot only received the income transfer in cash, (ii) those where they only received it electronically, and (iii) those localities where households under both transfer modalities existed, which constitute just a third of the sample. This is an interesting heterogeneity dimension to explore, thus, in Tables S11-S13, we split the samples into those where both treated and control households are present in the same localities and those where households who receive the transfer electronically or in cash reside in separate localities.[3] Our results do not change (albeit with a slightly increased magnitude of the tanda and shockcoping coefficients). They seem to be driven by the non-mixed sample, certainly in the significant cases. But in the "whole sample", there is no clear indication as to which is driving, at least for remittances and shock coping. However, due to the rather small sample size of the mixed group, it is impossible to judge whether the loss of degrees of freedom is responsible for the standard errors size.

[INSERT TABLE S10 – S13 ABOUT HERE]

5. Sensitivity analysis

Finally, we test the sensitivity of our results to possible deviations from the Conditional Independence Assumption (CIA) by applying the test developed by Ichino et al. (2008).[4] The test runs repeated simulations with the inclusion of a confounder variable which mimics a violation of the CIA. Two types of confounders are considered: one with a positive impact on the untreated outcome Y_0, which would then reduce the observed difference between treated and controls, and one with a positive effect on treatment assignment, that is selection bias. A 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.

---

[3] We thank an anonymous referee for this suggestion.

[4] 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.

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 $Y_0, Y_1 \in \{0,1\}$.[5] 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 estimation adopts a grid-search approach and the various sets of combinations are specified so as to represent an increasingly dangerous confounder. So that the first rows of table S14 in the study adopt a set 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.

[5] The discussion extends to continuous treatment cases.

Bibiliography

Lechner, M. (2002). ‘Program Heterogeneity and Propensity Score Matching: An Application to the Evaluation of Active Labor Market Policies’. *The Review of Economics and Statistics*, 84(2): 205-20.

Seira, E. (2010). ‘Electronic Payments of Cash Transfer Programs and Financial Inclusion’. Mimeo Instituto Tecnologico Autonomo de Mexico, SSRN 1628874.

Gertler, P. J., S. W. Martinez and M. Rubio-Codina (2012). "Investing Cash Transfers to Raise Long-Term Living Standards." American Economic Journal: Applied Economics **4**(1): 164-192.

Chiapa, C. and S. Prina (2014). Encuesta de las Caracterısticas de los Hogares e Inclusion Financiera 2014. Mexico City, Instituto Nacional de Salud Pública, El Colegio de México.

Ichino, A., F. Mealli, and T. Nannicini (2008). ‘From Temporary Help Jobs to Permanent Employment: What Can We Learn from Matching Estimators and their Sensitivity?’. *Journal of Applied Econometrics*, 23(3): 305-27.

Lipton, M. (1980). "Migration from rural areas of poor countries: The impact on rural productivity and income distribution." World Development 8(1): 1-24.

López-Córdova, E., A. Tokman R and E. A. Verhoogen (2005). "Globalization, Migration, and Development: The Role of Mexican Migrant Remittances [with comments]." Economía 6(1): 217-256.

Massey, D. S. (1987). "Understanding Mexican Migration to the United States." American Journal of Sociology 92(6): 1372-1403.

McKenzie, D. and H. Rapoport (2007). "Network effects and the dynamics of migration and inequality: Theory and evidence from Mexico." Journal of Development Economics 84(1): 1-24.

Moulaert, F. and P. Deryckere (1984). "A Comparative Illustration of the Life-cycle of Economic Migration (1960 – 1980)." International Migration 22(3): 178-198.

Nannicini, T. (2007). ‘Simulation-based Sensitivity Analysis for Matching Estimators’. *The Stata Journal*, 7(3): 334-50.

Figures

Figure S1

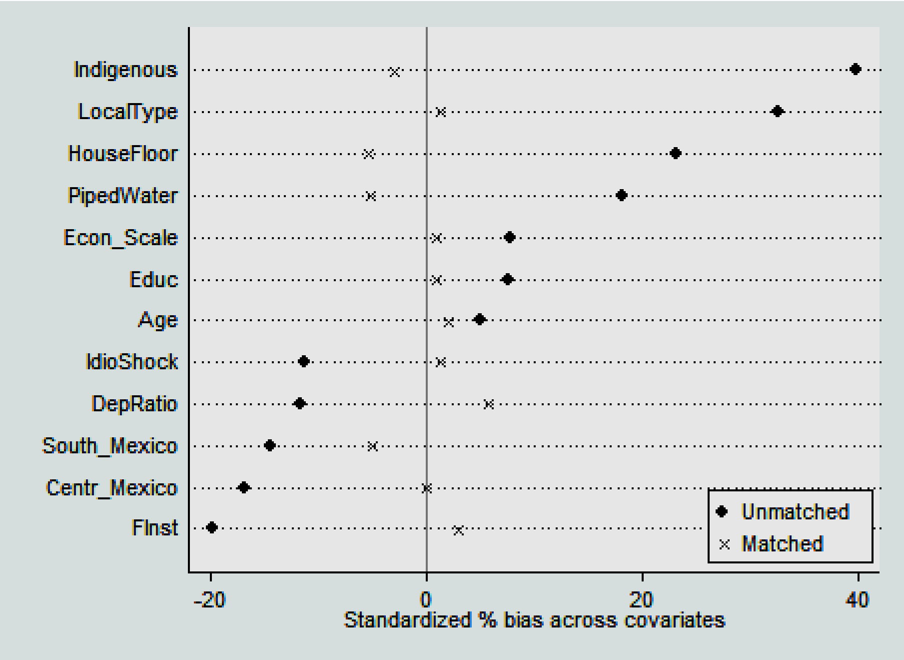

Table S6: Matching quality – % concentration ratio (nearest neighbour estimation)

	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 S7: 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**	-0.037	-0.067	-0.048*	-0.041	-0.04	-0.047	-0.068*	-0.07*	-0.09*	0.009	-0.052*	0.008	-0.054*
	(0.024)	(0.028)	(0.051)	(0.026)	(0.054)	(0.026)	(0.029)	(0.035)	(0.04)	(0.055)	(0.051)	(0.027)	(0.038)	(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 S8 : Remittances (rural sector sample, as no ATT effect is detected in the whole sample 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***	0.52*	0.1	0.62***	0.742*	0.64***	0.7*	0.9**	0.863*	0.415	-0.65	0.991***	-0.113	0.735**
	(0.239)	(0.27)	(0.57)	(0.266)	(0.386)	(0.34)	(0.39)	(0.39)	(0.45)	(0.5)	(0.6)	(0.36)	(0.66)	(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	1207	810	446	255	317	1243	455	1068
Off sup	0	0	0	0	0	0	0	0	0	0	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 S9: ShockCoping (whole sample)

		DepRatio		Education		Home Saving	
	Benchmark	High (>0.55)	Low (<0.55)	Low	high	yes	No
ATT	0.08**	0.077*	0.104*	0.028	0.181*	0.09	0.072*
	(0.038)	(0.045)	(0.06)	(0.04)	(0.09)	(0.06)	(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 S10. Estimation results when *Econ_Scale* is included

	Tanda			**HomeSaing**			**Remittances**			**ShockCoping**		
	Whole sample	**Urban**	**rural**	**Whole sample**	**urban**	**rural**	**Whole sample**	**urban**	**rural**	**Whole sample**	**urban**	**rural**
ATT	-0.015 (0.022)	-0.087* (0.047)	0.01 (0.16)	-0.039 (0.031)	0.002 (0.051)	-0.051 (0.036)	0.138 (0.154)	-0.279 (0.174)	0.455** (0.207)	0.073** (0.035)	0.089 (0.055)	0.074* (0.043)
Obs.	2431	894	1537	2429	894	1535	2431	894	1537	507	195	312
Treated	1390	603	787	1388	603	785	1390	603	787	262	118	144
Controls	1041	291	750	1041	291	750	1041	291	750	245	77	168
Comm Supp	2100	695	1537	2098	894	1535	2188	755	1537	440	159	312
Off sup	331	199	0	331	0	0	243	0	139	0	36	67

Table S11. whole sample

	Tanda		HomeSaving		Remittances		Shockcoping	
	Non-mixed	Mixed	Non-mixed	mixed	Non-mixed	mixed	Non-mixed	Mixed
ATT	-0.031 (0.028)	-0.023 (0.033)	0.016 (0.08)	-0.06 (0.072)	0.234 (0.29)	0.331 (0.255)	0.075 (0.055)	0.061 (0.129)
Obs.	1371	1060	1369	1060	1371	1060	283	224
Treated	837	553	835	553	837	553	142	120
Controls	534	507	534	507	534	507	141	104
Comm Supp	1208	896	1101	972	1103	1060	247	202
Off supp	163	164	268	88	268	88	36	22

Table S12. urban sample

	Tanda		HomeSaving		Remittances		Shockcoping	
	Non-mixed	Mixed	Non-mixed	mixed	Non-mixed	mixed	Non-mixed	Mixed
ATT	-0.127* (0.073)	0.022 (0.068)	0.085 (0.072)	-0.118 (0.135)	-0.24 (2.26)	0.067 (0.66)	0.019 (0.095)	0.045 (0.108)
Obs.	503	317	503	391	503	391	106	89
Treated	391	212	391	212	391	212	68	50
Controls	112	179	112	179	112	179	38	39
Comm Supp	363	317	253	297	253	317	90	61
Off supp	140	74	250	94	250	74	16	28

Table S13. rural sample

	Tanda		HomeSaving		Remittances		Shockcoping	
	Non-mixed	Mixed	Non-mixed	mixed	Non-mixed	mixed	Non-mixed	Mixed
ATT	-0.019 (0.026)	0.041 (0.035)	-0.074 (0.077)	-0.036 (0.067)	0.685* (0.41)	-0.58 (2.26)	0.135** (0.065)	-0.016 (0.095)
Obs.	868	669	866	669	868	503	177	135
Treated	446	341	444	341	446	391	74	70
Controls	422	328	422	328	422	112	103	65
Comm Supp	837	669	841	655	843	253	177	128
Off supp	31	0	25	14	25	250	0	7

Table S14: 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%.

Table S15. Locality Break-up

Localities receiving *POP*	
Total	236
Mixed (pilot plus cash transfers)	83
Cash transfers Only	100
Electronic payments pilot only	54

Table S16. Locality break-up and locality distribution

	Localities receiving *POP*, including rural to urban distribution											
	Mixed (pilot plus cash transfers)			**Cash transfers only**			**Electronic payments pilot only**			**Total**		
	Rural	*Urban*	*Total*	*Rural*	*Urban*	*Total*	*Rural*	*Urban*	*Total*	*Rural*	*Urban*	*Total*
Number	45	37	83	70	30	100	43	11	54	158	78	236
%	55%	45%	100%	70%	30%	100%	80%	20%	100%	67%	33%	100%
% of total localities	19%	16%	35%	44%	38%	42%	27%	14%	23%	67%	33%	100%

Table S17. Locality distribution (share of urban or rural)

Share of localities as a % of total rural and urban categories						
	Mixed (pilot plus cash transfers) as a % of all localities		**Cash transfers only as a % of all localities**		**Electronic Payments Pilot only as a % of all localities**	
	Rural	*Urban*	*Rural*	*Urban*	*Rural*	*Urban*
Number	**45**	**37**	**70**	**30**	**43**	**11**
% of total rural or urban localities	**28%**	**47%**	**44%**	**38%**	**27%**	**14%**

Table S18. Probability of being treated (Probit Regression, marginal effects)

Treated	dF/dx	x-bar
LocalType	.174***	0.365
	(.02)	
LocalSize	-.082**	1.08
	(0.039)	
South_Mexico	-.331***	0.608
	(.026)	
Centr_Mexico	-.25***	0.191
	(.034)	
HouseFloor	.171***	0.779
	(.026)	
PipedWater	.075*	0.825
	(.029)	
DepRatio	-.046***	1.111
	(.012)	
Age	.000	46.8
	(.0008)	
Education	.039	1.19
	(.028)	
IdioSock	-.071***	0.209
	(.026)	
Indigenous	.264***	0.378
	(.022)	
Obs prob	0.575	
Pred prob	0.583	(at x-bar)
Obs.	2456	
LR χ^2	360.18	
p > χ^2	0.000	
Psuedo R^2	0.11	

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