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*The liquidity effects of remittances and conditional cash transfers on credit demand:
Evidence from a randomized experiment in Nicaragua*

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Abstract

Understanding the way the poor manage their liquid assets is necessary for the successful implementation of many social and economic development programs. Some examples are Conditional Cash Transfer (CCT) programs that aim to improve child education and health or microfinance programs that aim to provide financial tools for the poor so that they take advantage of existent investment opportunities.

This paper explores how poor agrarian households in Northern Nicaragua make use of different liquidity sources available to them by analyzing data from a randomized field experiment. Specifically, it studies how a rise in income through access to remittances and direct subsidies such as CCTs change these households' use of another liquidity source, namely credit, through income and substitution effects.

Results show that, on average, CCTs do not change the households' decision to request a loan and this is attributed to the restrictions on time and income allocation that these transfers are subject to. Remittances on the other hand are seen to increase the likelihood of requesting a loan as their use is much less restricted allowing households to re-optimize time and income allocations such that they take advantage of the implicitly cheaper credit to get closer to their optimal liquidity level.

When analyzing only the subgroup of households that had access to remittances it is seen that those households that receive CCTs are less likely to request a loan than those that do not. That is households with relatively higher income that receive additional liquidity through CCTs tend to substitute away from credit as they find it optimal to avoid its cost.

1. Introduction

Conditional Cash Transfer (CCT) programs have become an important component of educational and health projects carried out in developing countries. They aim to promote investment in human capital by providing direct cash transfers to poor households conditional on some kind of observable requirement, such as children's attendance to school or the improvement of child health indicators.

CCT programs have been found effective in raising school enrollment and health indicators among the poor, as well as reducing child labor and buffering negative household shocks (Schultz, 2004; Glewwe and Olinto, 2004; Ravallion and Chen, 2005; de Janvry *et al.*, 2004; Bourgignon *et al.*, 2003).

One common characteristic of CCT programs is the randomized formation of control and treatment groups. This implementation method facilitates the program's impact evaluation by making both groups comparable. It overcomes many of the common methodological challenges in impact analysis, which will be mentioned later in this paper, and favors unbiased and robust impact measures. It also helps evaluate the cost-effectiveness of the programs relative to other methods of achieving higher human capital.

Some examples of these programs are Progresa-Oportunidades in Mexico, Bolsa Escola in Brazil, FSSAP in Bangladesh and the program which is the focus of this study, Red de Proteccion Social (RPS), in Nicaragua.

In this empirical study, I explore effects that are exogenous to RPS's main objectives. I explore how poor households make use of different liquidity sources in an effort to reach their optimal liquidity level. Specifically, I analyze how households that receive

remittances and a direct subsidy such as a CCT re-optimize their use of another source of liquidity, namely credit. Thus, this study consists of an evaluation of how access to CCTs and remittances impact the decision to request a loan. Since RPS was implemented in rural communities of Northern Nicaragua, the context of the analysis is that of poor agrarian households.

This study provides evidence that not all sources of liquidity are used in the same way by the participating households. An increase in liquidity due to access to a CCT did not have, on average, any significant impact on households' decision to request a loan. On the other hand an increase in liquidity caused by access to remittances had a significant and positive impact on the likelihood of requesting a loan. This supports the argument that the conditions that are attached to each liquidity source affect households' decision-making in order to reach an optimal liquidity level.

However, I am able to find that RPS did have a significant impact on the *subgroup* of participants that receive remittances. In this case, the positive impact of remittances on the likelihood of requesting a loan is partially offset by the extra liquidity provided by a CCT. That is, RPS had a negative impact on the decision to request a loan for those households that already enjoyed relatively higher liquidity levels. An interpretation of this result is provided.

The paper is constructed in the following way. In section 2 the basic characteristics of the data are presented. Section 3 presents some of the main methodological challenges and the econometric models that I use. Section 4 presents the results and discussion. Section 5 concludes and points out areas of future research.

2. The RPS data

RPS began with a pilot phase carried out between 2000 and 2002, and then continued in a second phase from 2003 to 2005. It was funded by the Inter-American Development bank and the Nicaraguan government. The International Food Policy Research Institute (IFPRI) was in charge of the impact evaluation of the pilot phase.

The program consisted in the provision of a food security transfer of \$224 per year to those households in the treatment group. If these households had children between the ages of 7 and 13 they received an additional school attendance transfer of \$133 per year.

This study uses the data collected for the pilot phase, which is publicly available through IFPRI's website. Year 2000 is the baseline year (before treatment) and years 2001 and 2002 are the after treatment periods.

The targeted population consists of poor rural households living in the regions or *departmentos* of Madriz and Matagalpa, in Northern Nicaragua. This area was chosen based on the program's implementation capacity and the poverty levels shown. The region showed worsening poverty rates between 1998 and 2001; a period in which poverty rates declined nationally (Maluccio and Flores, 2004).

The randomization process implemented had a block design. There were 42 communities or *comarcas* selected to participate in the experiment. These were stratified in seven groups of six *comarcas*, each according to a poverty index. Within each stratum of six, randomization was achieved by blindly drawing a colored ball (blue for control and red for treatment) without replacement, thus obtaining three *comarcas* in the control group and three in the treatment group. This ensures that the probability of being selected for the control or treatment group is the same for all strata, making treatment status

random over the whole population of 42 comarcas. It also ensures that the comparison and treatment group are balanced according to poverty indices, reducing the variance of estimated treatment effects (Duflo *et al*, 2007).

The baseline survey was intended for 1,764 households and was a stratified random sample of 42 households in each comarca. The total attrition in the baseline survey was 10.4%, yielding a sample of 1,581 household. Table 1 shows attrition levels for the control and treatment groups for the baseline and follow-up surveys.

Table 1. Households participating in RPS’s baseline and follow-up surveys (percent of attrition is given in parenthesis).

	Baseline 2000	Follow-up 2001	Follow-up 2002
Completed interview	1,581 (10.4)	1,453 (8.1)	1,397 (11.6)
Treatment group	810 (8.2)	766 (5.4)	722 (10.9)
Control group	771 (12.6)	687 (10.9)	675 (12.4)
Completed interviews in 3 rounds	1,359 (23.0)	1,359 (14.0)	1,359 (14.0)
Treatment group	706 (20.0)	706 (12.8)	706 (12.8)
Control group	653 (26.0)	653 (10.5)	653 (10.5)

Source: Maluccio and Flores, 2004. Both follow-up surveys were targeted for baseline respondents thus there are some households that participated in 2001 but not in 2002 and vice-versa, causing the balanced panel to consist of 1,359 households instead of 1,397.

Total attrition among treatment and control groups in the balanced panel seems very similar, which may favor comparability. However the randomization of the sample is valid as long as this attrition is itself random. This is an issue analyzed in section 3.

During the implementation of the program there were some cases of non-compliance. Although nobody assigned to the control group was treated, some assigned to the treatment group were not treated as they refused to participate. Still they participated in

the surveys. In the targeted group of 1,764 households, 4.8% of households assigned to the treatment group did not receive it. In the balanced panel, only 2.6% of households in the treatment group were not treated. This issue is analyzed in section 3.

3. Data analysis and econometric model

3.1 Verification of randomization and general characteristics of the sample

The first step in this evaluation is to verify if attrition was random. Given that there is baseline data available, we can compare attriters with non-attriters to see if there are systematic differences among them. If there are, this would suggest that treatment is somehow correlated with households not participating in the follow-up surveys, therefore yielding biased impact estimates. For example, it is possible that those not so poor households benefited by the program were able to migrate locally to other communities with better job opportunities, which would underestimate impact measures. Alternatively, those households benefiting the least from RPS may drop out which would imply an overestimation of impact measures.

In order to test whether attrition was random I use a linear probability model with a bivariate dependant variable equal to one if the household failed to participate in any of the follow-up surveys, and zero otherwise. The independent variables will be key observable variables shown in table 2. The null that all parameter estimates are *jointly* zero will be tested with an F statistic. If attrition is random, then I should fail to reject the null.

I repeat this analysis for the treatment and control group in the balanced panel to verify that randomization was effective in the final sample used for the evaluation even after attrition. Results of the F-test for attrition and treatment are shown in table 2.

Table 2. F-statistic for the regression of observables on attrition and treatment using baseline survey (year 2000)

Independent variables	Dependant variable	
	Attrition	Treatment
Intercept	0.175** (0.029)	0.450** (0.058)
Average age of household members	0.000 (0.001)	0.003* (0.001)
Requested a loan ^a	-0.015 (0.023)	-0.073* (0.036)
Existence of a microenterprise ^b	0.023 (0.027)	0.032 (0.043)
Value of durable and non-durable goods ^c	-0.011 (0.009)	0.007 (0.013)
Household's annual per capita consumption ^d	-0.010 (0.033)	0.005 (0.006)
Access to remittances ^e	0.052 (0.035)	-0.059 (0.056)
Value of annual remittances ^f	0.000 (0.006)	0.006 (0.01)
Participation in community organizations ^g	-0.036 (0.022)	0.012 (0.034)
Number of members in the household	-0.004 (0.003)	-0.001 (0.005)
F-statistic	1.29	1.62
Number of observations	1,581	1,359

**Significant at the 99% confidence level. * Significant at the 95% confidence level.
Standard errors are in parenthesis

^a Dummy for whether the household requested a loan in the previous 12 months of the survey

^b Dummy for whether there existed a microenterprise in the household or not

^c Value of goods such as agricultural tools, furniture, sowing machines, vehicles, etc. in thousands of Cordobas

^d In thousands of Cordobas

^e Dummy for whether the household receives a periodic help in kind or in cash from families or friends living outside the community

^f Average annual remittances received in thousands of Cordobas

^g Dummy for whether household members participated in community organizations related to technical training, religion, sports, women empowerment, among others.

The F-statistics obtained for the regressions of attrition and treatment are low enough so that the null cannot be rejected. This is showing there are no systematic differences between attritors and non-attritors or between households in the control and treatment group, providing evidence that attrition was indeed random.

Descriptive statistics of some of the observable variables are shown in Table 3 for attritors and non-attritors as well as for those households in the control and treatment group. These statistics are useful to recognize the type of households that participated in RPS, as well as for getting a feel of how similar each group is.

The household members participating in RPS are characterized by being young. The average age in each household is 23 years old. Their request for loans was low. On average, only 17% of households requested a loan during the 12 months before the 2000 baseline survey. This does not mean all households received the loan they requested. As a matter of fact, none of the households that requested a loan received it in the 2000 survey (not shown). In the 2001 follow up survey, 14% of households had requested a loan and of those 94% received it. In the 2002 follow-up survey, 11% of households requested a loan and 93% of them received it.

The proxy I use for credit demand is whether the household requested a loan or not. This proxy is not perfect as it may not capture the fact that households with demand for a loan do not request it because they believe they will not get it or because the implicit transaction costs are too high. But this proxy has the advantage of reflecting a decision that is exclusively of household members, something that would not be captured in a proxy such as whether the household received a loan or not. The latter case includes decisions of the lender. In addition, the survey asks whether the loan was requested from

formal sources (banks, NGOs or credit unions) or informal sources (such as neighbors, friends, money lenders, etc.) which cover most of the means through which the household may be seeking credit.

Table 3. Main statistics for non-attritors and attritors and for control and treatment groups in the baseline survey (year 2000).

	Non-attritors n=1,359	Attritors n=222	Control n=653	Treatment n=706
Average age of household members				
Mean	23.035	23.516	22.199	23.808
Std Error	0.312	0.941	0.3949	0.476
Sample Var	132.603	196.558	101.818	160.015
Skewness	2.101	2.172	1.862	2.117
Percentage of households that requested a loan				
Mean	0.179	0.153	0.199	0.160
Std Error	0.010	0.024	0.016	0.014
Sample Var	0.144	0.130	0.160	0.135
Skewness	1.678	1.939	1.511	1.858
Percentage of households that report having a microenterprise				
Mean	0.123	0.131	0.115	0.130
Std Error	0.009	0.023	0.012	0.013
Sample Var	0.108	0.114	0.102	0.113
Skewness	2.300	2.207	2.421	2.201
Value of durable and non-durables (Cordobas)				
Mean	399.163	297.545	375.110	421.411
Std Error	29.249	51.948	34.754	46.231
Sample Var	1162590.318	599098.358	788,741.212	1,508,952.302
Skewness	10.114	7.818	6.114	11.030
Per capita annual consumption (Cordobas)				
Mean	3,885.081	3,854.267	3,738.242	4,020.897
Std Error	77.283	210.808	111.181	107.321
Sample Var	8,116,921.294	9,865,714.247	8,071,903.103	8,131,625.206
Skewness	2.484	2.655	2.779	2.236
Percentage of households with access to remittances				
Mean	0.064	0.086	0.070	0.058
Std Error	0.007	0.019	0.010	0.009
Sample Var	0.060	0.079	0.065	0.055
Skewness	3.566	2.983	3.365	3.787
Value of remittances per year (Cordobas)				
Mean	131.077	166.464	121.770	139.686
Std Error	39.251	86.685	28.996	70.663
Sample Var	2,093,790.562	1,668,176	549,040.712	3,525,226.032
Skewness	27.442	9.762	8.328	23.744

The 2000 baseline survey reports that household members work mostly in agricultural activities, but 12% of them have some sort of microenterprise: manufacturing and selling goods, just trading goods or providing some sort of service.

The value of durable and non-durable goods averaged 385 Cordobas (about \$30 at the year's official average exchange rate), although the variance is high. This value includes furniture, electronics, agricultural tools, sowing machines and vehicles.

Household annual per capita consumption averaged 3,900 Cordobas (about \$307), which is less than a dollar per day. This consumption captures basic needs such as food, housing, transportation, education and utilities.

About 7% of households reported access to remittances in 2000. The concept of remittances used in the survey was that of access to some periodic help, either in cash or in kind, from family or friends that emigrated locally or internationally. For those households that report having access to remittances, the annual average value was of 2,047 Cordobas, or about \$162. Table 3, shows a lower average value for remittances because it also includes those households that do not have access to remittances, which was necessary to compute the F statistic in table 2.

Therefore these rural households are characterized by having young members. They make little use of credit and have low access to remittances. They would be categorized as extreme poor using as a threshold an average consumption per capita of less than a dollar a day. These characteristics coincide well with the livelihood of the poor in many parts of the world as described by Banerjee and Duflo, 2006.

3.2 Non-compliance and the effect on the treated

During the implementation of RPS, not all households assigned to the treatment group were actually treated, but it would be incorrect to eliminate them from the analysis as it may cause selection bias. Therefore, in the balanced panel I use all those households initially randomized to the treatment group, even if not all of them were treated.

By doing so, the parameter estimate obtained is actually the intention-to-treat estimate, which should be *lower* than the real impact on the treated. If the intention-to-treat estimate is statistically significant, one can still estimate the treatment effect on the treated. Dividing the intention-to-treat estimate by the percentage of households in the treatment group that were actually treated yields the true impact estimate. This is true as long as treatment does not affect non-compliers, i.e. CCTs do not cause externalities that affect those that decided not to participate (Duflo, *et al* 2007).

Since only 2.6% of households in the treatment group were not treated, the difference between the intention-to-treat and the true impact estimates is expected to be negligible.

3.3 The impact evaluation

The main problem of impact analysis is that it tries to answer a basic counterfactual question: how would the conditions of households participating in RPS would have differed if they had not participated in this program. Of course, it is not possible to observe a household participating and not participating in the program at the same time in order to see the difference.

One would then be tempted to compare those households participating in the treatment group to those participating in the control group. The difference between them would be given by D:

$$D = E(Y_{1i} | P_i = 1) - E(Y_{0i} | P_i = 0) \quad (1)$$

where Y_{1i} is household i credit demand in the presence of the program, Y_{0i} is household i credit demand in the absence of the program and P_i is a dummy equal to 1 if the household is in the treatment group and 0 otherwise.

But equation (1) is not our true interest. We are interested in the counterfactual or the mean impact which is given by G :

$$G = E(Y_{1i} - Y_{0i} | P_i = 1) \quad (2)$$

However there is a relationship between (1) and (2) as shown by Ravallion (2001) and Duflo (2005). Adding and subtracting $E(Y_{0i}|P_i=1)$ to (1) we get:

$$D = G + E(Y_{0i} | P_i = 1) - E(Y_{0i} | P_i = 0) \quad (3)$$

The last two terms in equation (3) constitute the selection bias which represents the systematic differences that may exist between the treatment and control group.

Although the counterfactual is not observable, randomization allows the selection bias to disappear because, *on average*, credit demand in the absence of the program would have been the same for the control and treatment group. Given randomization, D will be equal to the mean impact, G .

If randomization is well implemented then not only the mean value of Y_i will be the same for the control and treatment group in the absence of the program, but their whole distribution will be the same. It is safe to say that any statistically significant difference between these two distributions after the implementation of the program was caused by the program itself (Ravallion, 2001).

Duflo (2005) shows that D can be estimated using OLS for the regression $Y_i = \alpha + D P_i + \varepsilon_i$, where ε_i is an error term.

3.4 The use of double difference technique to eliminate selection on unobservables

In section 3.1 I presented several tests to verify that treatment was properly randomized relative to observables. But there is a possibility of treatment being correlated to unobservable characteristics, which would make treatment P_i correlated to the error term ε_i in the OLS regression above. For example, it is possible that treatment is correlated to unobservables such as household members' entrepreneurship level or knowledge of local markets, which may be determinants of credit demand. To solve this issue one can use the double difference (DD) technique.

The main idea of the DD technique is to take the difference in the variable of interest between the control and treatment group after the program was implemented, and subtract from it the difference that existed before the program was implemented. This double difference is the true impact of the program.

This method allows the elimination of those time invariant error terms that may be correlated with treatment. To illustrate this I follow Ravallion (2001). Assume randomization on observables was successfully made. Then credit demand for household i after the program is implemented is given by

$$Y_{1i} = a + bP_i + cX_{1i} + \eta_i + \mu_{1i} \quad (4)$$

Here X represents the observed explanatory variables and ε_i is decomposed into η_i , a time invariant error which is allowed to be correlated with P_i , and μ_{1i} a time variant error which is assumed uncorrelated with P_i .

Before the implementation of the program, credit demand was given by

$$Y_{0i} = a + cX_{0i} + \eta_i + \mu_{0i} \quad (5)$$

By subtracting equation (5) from (4) we get:

$$Y_{1i} - Y_{0i} = bP_i + c(X_{1i} - X_{0i}) + \mu_{1i} - \mu_{0i} \quad (6)$$

The problematic time invariant error has been eliminated and an unbiased estimate of the program impact, b , may be obtained.

The main assumption of the DD technique is that of a parallel trend between the treatment and control group. That is DD estimates are unbiased as long as the average change in $Y_{1i} - Y_{0i}$ for household i would have been the same for the treatment and control group in the absence of the program, i.e. if treatment is as good as random (Bertrand *et al*, 2003).

To avoid the risk of unobservable errors being correlated to treatment, I use the DD technique to estimate the mean impact of RPS on credit demand. This approach is valid due to the fact that treatment was randomized.

Following Duflo (2005), the specification that I use to obtain the DD impact estimate is the following:

$$Y_{it} = \alpha + \beta t_1 + \phi t_2 + \gamma P_i + \delta_a t_1 P_i + \delta_b t_2 P_i + \varepsilon_{it} \quad (7)$$

where t_1 and t_2 are dummies for the years 2001 and 2002, once the program was implemented.

The parameters of interest are δ_a and δ_b which represent the mean program effect for 2001 and 2002, respectively, relative to 2000. I evaluate RPS' impact on four different dependant variables: request of credit, actual receipt of credit, access to remittances and value of remittances.

An important consideration in this case, is the fact that RPS randomized treatment at the community level, not the household level. This creates a problem of grouped error terms as the unit of observations (the household) is more detailed than the level of

variation (the community). Therefore the variance covariance matrix of the OLS estimation of (7) may be block-diagonal with correlation among error terms within each community cell. The grouped error term problem causes an *over rejection* of the null hypothesis of no effect (Bertrand *et al*, 2003). Since OLS assumes the existence of a diagonal variance-covariance matrix, OLS estimates would be inefficient. I therefore control for the clustering at the community level, which is analogous to the White adjustment for heteroscedasticity (Schultz, 2004).

To estimate equation (7) I use a linear probability model because effects are more tractable and easy to interpret in the presence of heterogeneity (Hyslop, 1999).

3.5 Estimating the effect of remittances and conditional cash transfer on the demand for credit

The DD technique will allow me to estimate the mean impact of RPS on the decision to request a loan. However, I would like to analyze how this decision changes (if at all) when households have access to remittances.

In order to pool two additional groups (those households with and without access to remittances) in a single regression that already accounts for control and treatment groups I can use fixed effects. In fact, fixed effects are a generalization of the DD technique to account for more than two time periods and two groups (Duflo, 2005).

The main concern when incorporating access to remittances into the regression is that these, unlike treatment, may not be exogenous to household's credit demand. That is, it is unlikely that remittances are determined independently of credit demand so that they are uncorrelated with the error term.

Household fixed effects control for time invariant heterogeneity and time fixed effects control for all context variables common to all households (de Janvry *et al*, 2006). By using both, endogeneity issues are solved since identification is obtained out of group specific changes over time (Duflo, 2005).

The specification I use to obtain the joint effect of treatment and remittances on credit demand is given by

$$Y_{it} = \alpha_1 + \alpha_2 + \theta_i^s + \delta_a t_1 P_i + \delta_b t_2 P_i + \beta R_{it} + \gamma R_{it} P_i + \varepsilon_{it} \quad (8)$$

where α_1 and α_2 are time fixed effects for year 2001 and 2002 respectively. Household fixed effects are given by θ_i^s , δ_a and δ_b represent the program's mean impact for 2001 and 2002 respectively and R_{it} is a dummy for household's i access to remittances during period t .

It is important to note that equation (8) is equivalent to equation (7) in the sense that both show the overall impact of the RPS on credit demand allowing *all other* characteristics to change in response to the program. That is, they both represent the total derivative of credit demand with respect to the program (Duflo, *et al*, 2007). However, equation (8) allows me to see the effect of remittances on credit demand, represented by β , and how conditional cash transfers modify this effect, represented by γ . RPS mean impact as shown by δ_a and δ_b should be equivalent to the one estimated with the DD technique.

An assumption made by fixed effects is that treatment and remittances have an immediate effect. That is, the total effect of the remittances and cash transfers received by a household during the year are executed during that year. This assumption seems

reasonable for rural households characterized by being highly credit constrained and having low access to remittances.

As in the case of the DD specification, I use a linear probability model solved using Generalized Least Squares to determine equation (8) given the ease of interpretation it provides under unobserved heterogeneity.

4. Results and discussion

Table 4 reports the DD estimation of RPS' mean impact, on credit demand (decision to request a loan), credit receipt (actual receipt of credit requested), access to remittances, and the value of remittances in local currency.

The parameters of interest δ_a and δ_b are statistically insignificant suggesting that, on average, participation in RPS did not modify households' decision to request a loan.

RPS seems not to have modified the amount of credit actually received by households that requested a loan, suggesting that participation in RPS was not a signal that may have improved the household's creditworthiness as perceived by lenders.

In addition, RPS does not seem to affect household's access to remittances, nor the value of remittances received. This suggests that family and friends sending remittances did not stop doing so nor did they reduce the amount sent after the household began participating in RPS.

The White adjustment for heteroscedasticity was made following Schultz, 2004. This procedure may not correct for the grouped error term completely, because empirical work by Bertrand *et al.*, 2003, shows that this correction is effective for large number of strata (above 15 strata). RPS had only 7 strata, but since our impact estimates are insignificant,

this means that complete correction of the grouped error term problem would only make our estimates even *less* significant. This provides confidence in my results.

Table 4. Estimated parameters for equation (7) using credit request, credit receipt, access to remittances and value of remittances as dependant variables.

Parameter	Credit demand	Credit receipt	Access to remittances	Value of remittances
Intercept	0.199* (0.016)	0.000 (0.016)	0.070* (0.010)	1.729* (0.826)
t_1	-0.047* (0.021)	0.960** (0.024)	0.006 (0.015)	1.330 (1.145)
t_2	-0.083* (0.020)	0.934** (0.027)	-0.011 (0.015)	0.458 (1.220)
P_i	-0.039 (0.021)	0.000 (0.024)	-0.012 (0.014)	0.677 (1.204)
δ_a	0.016 (0.027)	-0.047 (0.036)	-0.017 (0.020)	-0.817 (1.741)
δ_b	0.027 (0.027)	0.012 (0.038)	0.022 (0.020)	-0.207 (1.701)
Number of observations	4,077	583	4,077	258

Significant at the * 95% and ** 99% confidence level. White's standard errors are given in parenthesis. Credit receipt and value of remittances are regressed over the subgroup that requested a loan and that has access to remittances, respectively.

As mentioned in the previous section, the DD estimation does not allow for the inclusion of more than two groups, which prevents us from exploring the partial effects of access to remittances and treatment on the decision to request a loan.

Table 5 reports the results from fixed effect estimation. Time fixed effects were significant. There were contextual settings during 2001 that increased the household's likelihood of requesting a loan by 7% relative to 2000. This likelihood decreased to 3% in 2002, relative to 2000. This may have been due to the historic drop of international coffee prices during 2001, which had tremendous impact on local economies in Northern Nicaragua, the most important coffee producing region of the country. During 2002,

coffee prices rose slightly, which allowed for a relative improvement in the region's economy (Maluccio, 2005).

Table 5. Estimated parameters for equation (8) using credit demand as dependant variable¹.

Parameter	Credit demand
α_1	0.068** (0.013)
α_2	0.028** (0.013)
δ_a	0.018 (0.025)
δ_b	0.025 (0.025)
β	0.167** (0.037)
γ	-0.136* (0.053)
Number of observations	4,077

1. Parameters for households fixed effects are not shown.

** Statistically significant at the 99% confidence level. * Statistically significant at the 99% confidence level. Standard errors in parenthesis

Using fixed effects provides additional confirmation that RPS had, on average, no impact on credit demand. This is consistent with the results found using the DD technique and can be considered a robustness check.

However, fixed effects show that those households with access to remittances are, on average, 17% more likely to request a loan. If in addition to remittances, those households receive a CCT from the program the effect of remittances on credit demand is partially offset. That is, additional liquidity provided by RPS to those households that receive remittances reduces their likelihood of requesting a loan by 14%.

These results raise some interesting questions. First of all, they suggest that although both CCT and remittances represent an increase in household liquidity they do not have the same effect on the proxy of credit demand. This empirical analysis suggests that

CCTs have no significant impact, while remittances have a significant and positive impact on the likelihood of requesting a loan.

From a theoretical perspective, the net effect of a rise in liquidity on credit demand is ambiguous. A rise in liquidity is really an increase in income that expands the set of investment opportunities for households by relaxing the budget constraint. This relaxation of the budget constraint changes the implicit price of many goods and services including credit and causes income and substitutions effects, with a net effect that depends on households' initial conditions. In the particular case of credit, we would expect its implicit price to decrease with higher liquidity as household members are able to cover the transactions costs implied in the process of requesting a loan and also because it signals creditworthiness.

Therefore, a household that is benefited by a rise in liquidity will re-optimize its time and income allocations to take advantage of new investment opportunities, which may or may not translate into a higher demand of credit, depending on the net result of income and substitution effects.

The main difference between CCTs and remittances lie on the conditions attached to receiving each type of transfer. In order to keep receiving CCTs, households benefited by RPS were required to attend health education workshops, qualified children were required to show adequate weight gain and regular attendance to school, payment to the children's teachers was also required as well as keeping children's vaccination up to date. This represents a strong restriction on the time and income allocations that households are able to make once the CCT is received, and the empirical results suggest these are enough to leave the demand for credit unchanged. That is, CCT restrictions make the

substitution effect strong enough to offset the income effect, making the net effect statistically insignificant.

Remittances, on the other hand, are generally not subject to conditions as strict as those set by RPS. Even if remittances were conditioned by migrants to be used in certain activities it is highly unlikely that their monitoring capacity on relatives back home is as effective as the one shown by RPS. This allows households with access to remittances greater flexibility in income and time allocations in order to take full advantage of new investment opportunities provided by the rise in liquidity. The empirical results presented here suggest the income effect of remittances is dominant, therefore increasing our proxy of credit demand, as represented by parameter β . This suggests that remittances allow households to re-optimize time and income allocations such that they take advantage of the implicitly cheaper credit to get closer to their optimal liquidity level.

The second empirical result of interest is that RPS *does* seem to have a significant and negative effect on our proxy of credit demand on the *subgroup* of households that receive remittances. This sheds further light on how the net result of income and substitution effects depends on the initial liquidity level households are subject to. As mentioned above, the liquidity provided by remittances seems to increase the likelihood of requesting a loan. But if *additional* liquidity is provided through CCTs, this positive impact of remittances on credit demand is partially offset.

Results in table 5 suggest that within the subgroup of households that receive remittances, the substitution effect will be greater for those that receive CCTs relative to those that do not, as shown by the negative value of γ . I suggest this is because the new investment opportunities provided by access to remittances are well exploited by

households and additional liquidity provided by CCT reduces the need to request for a loan as there is more cash available making it optimal for the household to reduce the costly use of credit. It is likely that households with access to remittances are *already* investing relatively more in child education and health (Cordoba, 2006; Edwards Cox and Ureta, 2003), and since CCTs granted by RPS have been found to be designed such that households find it optimal to invest them in what they were intended for, this allows households free up cash and substitute away from credit.

Still, notice that those households with access to both remittances and CCTs still seem to be 3% (17% - 14%) more likely to request a loan than those households with access to neither. That is, in the context of the poor rural households participating in RPS, income effects of remittances on credit demand seem to be dominant even after the provision of CCTs.

5. Conclusions and further research

The main purpose of this paper is to empirically explore how access to remittances and conditional cash transfers affect the credit demand of households participating in RPS, a social program conducted in Nicaragua that aims at improving school enrollment and child health indicators among the rural poor.

Evidence is presented to support the statement that RPS successfully randomized treatment even after considering attrition. This favors the robustness and precision of the program's impact estimates.

Using double difference technique I find that CCTs did not seem to have a significant impact on households' credit demand. However, the impact of remittances on credit demand seems to be significant and positive, as shown by results from fixed effects.

Although CCTs and remittances both represent an increase in liquidity, they do not seem to have the same effect on the proxy of credit demand.

The interpretation of these results is based on the theoretical implications of a rise in household liquidity. Access to liquidity is really an increase in income that expands the set of investment opportunities for households and changes the implicit price of credit. Therefore the net effect on credit demand is the result of the interaction of income and substitutions effects.

The main difference between CCTs and remittances lies on the conditions attached to receiving each type of transfer. The conditions attached to CCTs represent a constraint on the time and income allocations that restrict household members from taking advantage of new investment opportunities that would be available otherwise given the rise in liquidity. My analysis suggests the net result of income and substitution effects of CCTs on credit demand is insignificant on average, due to the restrictions imposed on time and income allocations.

Remittances, on the other hand, are generally not subject to conditions as strict as those set by RPS. This allows households with access to remittances greater flexibility in income and time allocations in order to take full advantage of new investment opportunities provided by the rise in liquidity. The empirical results presented here suggest the income effect of remittances is dominant so that, on average, households with remittances are more likely to request a loan.

An additional analysis is made on the effects of CCTs on credit demand for the *subgroup* of households that receive remittances. Liquidity provided by remittances seems to raise the proxy of credit demand, but if additional liquidity is given through

CCTs, the positive effect of remittances on credit demand is partially offset. The empirical results presented suggest that within this subgroup of households substitution effect on credit demand will be higher for those households that receive both remittances and CCTs. This may mean that new investment opportunities provided by access to remittances are well exploited by households and these investments are likely to include education and health. Thus additional liquidity provided by CCT reduces the need to request for a loan as there is more cash available in the household making it optimal to reduce the costly use of credit. Still, those households with access to both remittances and CCTs seem to be more likely to request a loan than those households with access to neither. This suggests that in the context of the poor rural households participating in RPS, income effects of a rise in liquidity through remittances *and* CCTs seem to be dominant.

The results provided in this paper provide insights on how the rural poor make different use of different liquidity sources in their optimization process. This use depends very much on the constraints the liquidity sources impose on their income and time allocations. The fact that CCTs do not affect credit demand seems consistent with other studies that find a positive impact of CCTs on child education and health. The results provided in this paper suggest that the conditions attached to programs like RPS do not make it optimal for households to invest the additional liquidity in activities that may increase income in the short run and that require a loan, but in other types of investments more likely in the education and health of children, which may increase income in the long run.

This paper has the limitation that it does not provide a formal theoretical model that explicitly shows households' optimization process with a well defined utility and production function and a budget constraint. Although the role of CCTs and remittances is analyzed using well known microeconomic concepts of utility maximization, in further research I plan to incorporate a theoretical model that formally explains the effect of these liquidity sources on household credit demand.

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