LOCAL ECONOMY EFFECTS OF SOCIAL TRANSFERS

FINAL REPORT

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Abstract

This paper reports on the main findings from a study examining the incidence and significance of local economy effects of social cash transfers in developing countries. The study hypothesises that the presence of local economy effects from social transfers is consistent with improvements in household consumption and asset holdings by non-eligible households in affected areas. The quasi-experimental nature of the data collected for the purposes of evaluating the impact of PROGRESA enables the comparison of household consumption and asset holdings among similar non-eligible households in treatment and control areas. The analysis finds that non-eligible households in treatment areas show significantly higher levels of household consumption and asset holdings following the introduction of PROGRESA, compared to non-eligible households in control areas. These results are interpreted to suggest that transfers in poor rural areas in Mexico have measurable local economy effects.

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The main objective of this study is to generate information on the incidence and significance of local economy effects of social cash transfers in developing countries. Economic theory and casual observation suggest that cash injections brought about by the introduction of social cash transfer programmes into rural communities could have effects beyond improvement in the welfare of direct beneficiaries. There is scarce evidence on the local economy effects of social cash transfers, in part due to the paucity of programme data appropriate to identifying these effects. The empirical work focuses on household survey data from PROGRESA covering the period 1998-2000. The data enable observation of consumption and assets of rural households, including those from households eligible to participate in the programme, and those not eligible. The structure of the data enable a comparison of similar households in areas where the programme was initially rolled in 1998, and households in areas incorporated into the programme in late 1999 and 2000. The main findings from the study to date indicate there are positive and significant differences in the consumption and assets of non-eligible households in treatment areas relative to non-eligible households in control areas. This is interpreted to imply the presence of local economy effects of PROGRESA transfers.

There is growing interest in the effectiveness of cash transfers in reducing poverty and vulnerability in developing countries. The knowledge and evidence base around cash transfers is growing rapidly, facilitated by the strong impact evaluation processes of some of the large cash transfer programmes in Latin America. Studies on PROGRESA/ OPORTUNIDADES have naturally focused on identifying the impact of the programme on beneficiaries. The literature points to the programme having large and positive effects on the consumption, schooling, health, and nutrition of beneficiaries (Schultz 2000; Skoufias 2001; Skoufias, Davis *et al*. 2001; Albarran and Attanasio 2002; Attanasio and Lechene 2002; Rubalcava, Teruel *et al*. 2002; Coady 2003; Martinelli and Parker 2003; Rubalcava and Teruel 2005). Overall, cash transfers account on average for around 20 percent of the consumption of around 4 million beneficiary households in rural Mexico, representing a large injection of liquidity (Coady 2003). Less is known about the effects of cash transfer programmes on the local economy. The presence of local economy effects of social transfers has important policy implications. To the extent that social cash transfers spent in the local economy have significant spillover effects, measurement of the impact of the programme on direct beneficiaries will underestimate the full impact of the programme on the communities affected.

Studies on transfer programmes in developing countries have noted that these are likely to have spillover effects on the local economy. Early studies of the social pension in South Africa, for example, remarked upon the fact that transfers stimulated local production and trade (Ardington and Lund 1995). In rural locations in South Africa, transfers are delivered by armoured transport on a particular day and time, usually pre-announced on the local radio. On that day, traders bring their wares to that location while loan sharks come to lend or collect money. Observers would find it hard not to conclude that social pensions have effects beyond the direct beneficiaries. In Brazil, where beneficiaries of cash transfers are in many areas provided with a magnetic card to access their benefits from banks or post offices, researchers noted that possession of these cards facilitates access to credit from financial institutions (Schwarzer 2000). Entitlement to

regular and reliable transfers makes beneficiaries credit-worthy. Cash transfers may also have effects upon the demand and composition of employment. In the Kalomo District Pilot Social Transfer Scheme, which pays only US$7 a month to the poorest ten percent of households, it has been noted that most beneficiary households are headed by older and disabled people. Some beneficiaries have access to land but lack the capacity to farm themselves, and the cash transfer enables them to employ another villager to tend the fields (Schubert 2005). These examples suggest the strong likelihood of local economy effects from cash transfer programmes.

However, few studies have focused on local economy effects. Some studies have focused on multiplier effects of transfers on the immediate beneficiaries Sadoulet, de Janvry and Davis examine the income multipliers of PROCAMPO, a cash subsidy programme introduced in Mexico to stimulate agricultural production in the *ejido* sector. The programme provides a subsidy to small farmers based on land cultivated. Their hypothesis is that cash transfers are likely to have large positive effects in rural Mexico due to the presence of liquidity and credit constraints.[1] Small farmers have both land and labour but lack cash to purchase seeds and equipment and to sustain themselves through to the sale of the produce. Cash transfers can lift these constraints and ensure a better allocation of labour time. They use two waves of programme data, and regress variables capturing change over time in a range of assets, plus a cash transfer receipt measure, on changes in income, and find that "a one peso transfer inducing a direct increase of 1.97 pesos" (Sadoulet, de Janvry *et al*. 2001)(p.1049).

Gertler *et al* have looked at whether PROGRESA beneficiaries invest some of the transfers in income generating activities (Gertler, Martinez *et al*. 2005). They find that eligible beneficiaries in treatment areas show significant increases in productive investment relative to eligible beneficiaries in control areas. They are more likely to invest in land and livestock, effects that are more marked for households without agricultural assets. Beneficiary households are also more likely to engage in income generating activities. The study finds that a quarter of transfers are invested in this way, generating an income multiplier of 1.2 for these households. This level of investment is calculated to support a 24 percent increase in consumption after six years, even after a termination of the programme. The study concludes that "increased entrepreneurial activity brought on by cash transfers have increased the potential for self-sufficiency" and sustain long term improvements in welfare (Gertler *et al*. 2005)(p.36).

These studies consider multiplier effects of cash transfers on eligible or beneficiary households, but less attention has been paid on non-eligible households. This is an interesting route to take to examine potential local economy effects of PROGRESA transfers. Gertler *et al*. test whether investment in assets can be observed among this group, but find no significant effect (Gertler *et al*. 2005). Angelucci and De Giorgio consider possible effects of PROGRESA on the consumption of non-eligible households (Angelucci and De Giorgi 2006). They compare food and non-food consumption by non-

[1] Increasing returns due to non-linear production techniques can reinforce the effects of credit or liquidity constraints (Barrett 2005; Carter and Barrett 2005).

eligible households and find that, after the introduction of PROGRESA, consumption is higher in treatment areas than in control areas.

This paper will follow this route to identifying local economy effects from transfers. It will focus on household consumption and asset holdings among non-eligible households in rural Mexico following the introduction of PROGRESA in 1997. The paper is organised as follows: the next section describes the data used in the study. The section which follows considers appropriate methodologies and sets out the estimation strategies to be implemented. The following two sections report on the main findings, firstly those relating to household consumption and then those relating to asset holdings. A further section considers the possible channels and processes through which local economy effects operate. A final section summarises the main conclusions and identifies areas where further research is needed.

DATA SOURCES

The analysis in the paper uses household data generated from the implementation and evaluation of Mexico's PROGRESA programme. A brief description of the programme below is followed by information on the dataset used in the paper.

The Government of Mexico introduced in 1997 the *Programa de Educación, Salud y Alimentación* (PROGRESA) providing conditional cash transfers to poor households in rural Mexico (Skoufias 2001). The introduction of the programme reflected concerns among policy makers relating to the persistence of poverty and vulnerability in rural areas in Mexico, especially in the context of the liberalisation of the agricultural sector. The design of PROGRESA responded to the lessons from the failure of previous anti-poverty programmes focused on rural areas. PROGRESA provides regular income transfers to poor households with children of school age, conditional on these children attending school, and on household members accessing primary health care. It therefore combines income transfers with basic service provision. Beneficiary households are identified through a process involving three levels: first geographic targeting which identifies marginalised communities with service infrastructure, a second level in which poor households in these communities are selected through a proxy means test, and a third level of community validation. The monthly transfers include a household consumption subsidy, and supplements for each child of school age up to a maximum amount. There is also an annual subsidy for each child of school age to cover the costs of school uniforms and texts. The schooling subsidies increase with the school grade attended, and are higher for girls in secondary school. Transfers are paid to the mother. There is also a subsidy to health and education providers involved in the programme. PROGRESA has multiple objectives, to improve nutrition and to ensure schooling and use of primary health care. It seeks to break the intergenerational persistence of poverty through facilitating investment in human capital by poor households.

The programme was rolled on gradually, according to a planned strategy. At the start in August 1997, 140,544 households in 3,269 locations were incorporated into the

programmes, with a further 160,161 households joining in November of that year. During 1998, a further 1.63 million households were incorporated into the programmes from 43,485 locations. By 2000, PROGRESA covered 2.6 million households, or 40 percent of all rural households in Mexico. In 1999, PROGRESA absorbed 0.2 percent of GDP, just under 20 percent of the federal poverty alleviation budget (Skoufias 2001). In 2002, PROGRESA was renamed OPORTUNIDADES by the incoming administration and extended to all other areas of rural Mexico and to marginalised urban areas too. In 2006, OPORTUNIDADES reaches 5 million households.

The designers of PROGRESA paid close attention to the evaluation of the programme, and to collection of the data needed to support this evaluation. Census data was employed to rank communities in terms of their socio-economic conditions and to select the most marginalized communities to be incorporated into the programme. A survey of rural households in Mexico ENCASEH (*Encuesta de Características Socio-Económicas de los Hogares*) was collected in 1997 and was used to identify eligible households within the selected communities with the scores from a proxy means test. Follow up surveys of a sample of households for the purposes of evaluating the programme (denominated ENCEL or *Encuesta de Evaluación de los Hogares Rurales*) were conducted every six months between March 1998 and November 2000.[2] In the empirical work in this paper, we shall be using the datasets for October 1998, May 1999, November 1999, May 2000 and November 2000 (the dataset for March 1998 included questions on expenditures, but these were formulated in a different way to the other follow up surveys and will not be used in the empirical work below).

The evaluation datasets sample 24,000 households in 506 villages or states. This sample includes 320 villages randomly selected from those in which the programme was implemented in 1998, which will be referred to as treatment villages/areas; and 186 villages randomly selected from among those in which the programme was delayed until the end of 2000.[3] The advantage of this sample design is that it enables difference in difference estimates of the impact of the programme. It is possible to factor in the changes in the variables of interest for the control group from those changes observable among the treatment group to arrive at an estimate of the net impact of the programme.

METHODOLOGY

Studies of impact evaluation of social cash transfer programmes require the identification of an appropriate counterfactual. The actual implementation of the programme makes it impossible to observe directly an outcome variable for a particular household with and without the programme, as the household will be in one or the other state. The quasi-

[2] A further evaluation survey was collected in November 2003, which included a further 151 communities to act as the new control group. These locations were selected using propensity score matching techniques. This dataset was not used in this report.

[3] The localities were randomly selected using proportionate to size probabilities from the full sample of localities in seven states in which the programme was implemented by November 1997 (treatment group), and with the same methodology from the full sample of localities in which the programme was planned to be rolled in by December 2000 (control group) (Skoufias 2001).

experimental nature of the PROGRESA data, in which marginalised villages are randomly selected into a control or treatment group, comes closest to providing an optimal counterfactual. As households were classified into eligible and non-eligible at an earlier stage, and the assignment of locations into treatment and control areas was randomised, eligible (non-eligible) households in control areas are an appropriate counterfactual for eligible (non-eligible) households in treatment areas. In the analysis below we focus on non-eligible households.

For the purposes of identifying an appropriate strategy guiding the empirical work, the study has followed closely the approach suggested in Angelucci and de Giorgi [2006] and Gertler *et al*. [2005]. Angelucci and de Giorgi specify an Indirect Treatment Effect [ITE] estimator for the indirect effects of a social cash transfer on non-eligible households as:

$$ITE = E[Y_i \mid T_i=1, NE_i=1] - [Y_i \mid T_i=0, NE_i=1] \quad (1)$$

Where Y is the outcome variable and 'i' indexes households. T is the area indicator, with a value of 1 if it is a treatment area, 0 otherwise. NE is an eligibility indicator, with a value of 1 if non-eligible and 0 if eligible. ITE compares the outcome variable for non-eligible households in treatment areas and control areas.[4]

Using the PROGRESA datasets, ITE will be estimated by running regressions of the type:

$$I_i = a + bT_i + c(X_i) + e_i \quad (2)$$

For a sample of non-eligible households, I_i is a variable of interest for household i. T_i is the area indicator, X_i is a vector of control variables, and a, b, and c are parameters to be estimated. The parameter b identifies the ITE, the indirect effects of the transfers on non-eligible (non-poor) households.

In terms of variables of interest, there are many options but the empirical work below will focus on food consumption and assets. Household consumption is a good proxy for current welfare, especially in the marginalised communities in our data, and assets provide some indication of longer term welfare and vulnerability. Focusing on these two sets of variables will provide information on whether PROGRESA has also generated some positive welfare effects among non-eligible households. This is turn will provide some indication of the extent to which PROGRESA has welfare effects which go beyond those observed for the direct beneficiaries, and therefore an indication of impact on the local economy.

[4] The ITE as defined approximates the real value of ITE defined as the comparison of the same household 'living' in the two possible situations or $ITE = E[Y_{1i} \mid T_i=1, NE_i=1] - [Y_{0i} \mid T_i=0, NE_i=1]$ where Y is also indexed by whether households in the village have received treatment. The second argument of this equation is not possible, and is replaced by the one in the text.

HOUSEHOLD CONSUMPTION

In this section we report on our estimates of the changes in household consumption for non-eligible households across treatment and control groups. This will provide information on whether the cash injection into a community, following the implementation of PROGRESA, has any effects on the consumption of non-eligible households living in that community. We test this by comparing measures of household consumption for non-eligible households living in communities where the cash transfer has been implemented, and for households with similar characteristics living in a community where no external cash injection was made. We hypothesise that finding no difference in the change in household consumption across treatments and control areas would indicate that the impact of PROGRESA was restricted only to direct beneficiaries. If PROGRESA had no effect on non-eligible households, then it can be safely concluded that the programme had no spillover effects on the local economy. If, on other hand, it can be shown that changes in household consumption among non-eligible households in treatment areas show an improvement compared to changes in household consumption of non-eligible households in control areas, then the presence of spillover effects cannot be ruled out.

To test this hypothesis we use four rounds of data from the PROGRESA programme: October 1998, May 1999, November 1999 and November 2000. In Table 1 below we provide some descriptive statistics for household consumption and other indicators across non-eligible households. The Table shows household consumption levels and trends over four periods for non-eligible households in treatment and control villages. Household consumption is higher for non-eligible living in treatment areas across all four periods. Average household consumption for non-eligible households is about 200 pesos a month in control areas, and around 220 pesos a month in treatment areas.[5]

Table 1: Summary statistics for household consumption in non-eligible households

Household Consumption	Oct 1998	May 1999	Nov 1999	Nov 2000
Non-eligible: control area				
Valid observations	4214	1817	1944	1706
Mean	208.40	196.71	199.54	207.2
Standard error	(254.77)	(154.72)	(251.22)	(144.4)
Non-eligible: treatment area				
Valid observations	5016	2908	2923	2656
Mean	229.64	221.52	203.0	237.6
Standard error	(924.3)	(583.85)	(147.21)	(947.8)
Zero's in household consumption	40	18	3	11
% non-eligible in control areas	49	21	20	19
% of non-eligible in treatment areas	36	21	21	19

[5] There are some outliers on the consumption values. If we restrict consumption to <= 10000 pesos the standard errors reduce quite dramatically. However, excluding outliers does not change the significance of the regression parameters so we decide to keep all the observations for the estimations below.

Table 2 below displays the results from OLS regression of a set of variables on the constructed measure of food consumption (Appendix 1 describes the construction of the variables). The results are from the estimation of equation (2) above, with the log of equivalised household consumption as the dependent variable. The estimation is done separately for the four rounds of data. The regression includes a number of control variables measured at the household, village, and state levels. The household level variables include controls for the age, gender, language, and age of the head of households; there are also controls for shocks affecting the household in the last year. The inclusion of household income and land variables measured for 1997 by the ENCASEH dataset control for initial conditions, i.e. prior to the introduction of PROGRESA. Village level variables include the number of households and the number of treated households. Finally state dummies are also included.

The key variable of interest is the *treatment* variable, which parameter provides an estimate of the indirect treatment effects of PROGRESA for household consumption on non-eligible households across four periods of data. The results indicate that non-eligible households' household consumption in treatment areas is not significantly different than non-eligible households' household consumption in control areas for October 1998. The lack of significance is expected and is likely to reflect the fact that at this point PROGRESA was in its initial stages of implementation and that spillover effects require some time to play themselves out. For May 1999 and November 1999 household consumption is significantly higher for non-eligible household living in treatment areas, compared to non-eligible households in control areas. By November 2000 all households in the sample are now in treatment areas. However, in order to explore whether the treatment effects somehow persist over time we use the control/treatment areas identification from 1999 with the November 2000 dataset. The results show that there is no significant difference in 2000, between non-eligible households living in the 1999 treatment areas or control areas.

Overall we interpret these results to indicate that we cannot rule out significant spillover effects on non-eligible households living in treatment areas. The effects of PROGRESA on non-eligible households are not apparent initially, in the October 1998 round, but they are significant in the next two rounds of data. The estimates show that household consumption among non eligible households in treatment areas was about 12 percent higher in May 1999 than among those in control areas, and 4.5 percent higher in November 1999 (the parameter for the May round is significant at the 1% level whereas the parameter for the November round is significant at the 10% level). The effect appears to diminish once all areas become part of the programme.

Table 2: Determinants of the household consumption of non-eligible households

	October 1998	*May 1999*	*November 1999*	*November 2000*
Treatment	0.011	0.123	0.048	0.022
	(0.51)	(4.05)***	(1.75)*	(1.02)
Poverty Index	0.053	0.073	0.028	0.076
	(2.32)**	(2.40)**	(1.02)	(2.43)**
Shock	-0.010	0.001	-0.052	-0.027
	(0.39)	(0.01)	(1.51)	(0.57)
Number of shocks	0.016	0.002	-0.008	-0.032
	(0.92)	(0.08)	(0.39)	(1.38)
Household income	1.754	0.726	0.327	-0.335
	(7.18)***	(3.56)***	(1.54)	(1.48)
Works status of head	0.124	0.131	0.128	0.087
	(6.03)***	(5.04)***	(4.91)***	(2.88)***
Gender of head	-0.198	-0.154	-0.174	-0.170
	(8.00)***	(4.11)***	(4.90)***	(4.53)***
Age of head	0.008	0.004	0.005	0.004
	(16.59)***	(6.25)***	(7.67)***	(4.86)***
Land	-0.000	-0.001	0.000	-0.000
	(0.26)	(1.68)*	(1.20)	(0.26)
Spanish and native dialect	-0.116	-0.054	-0.112	-0.067
	(5.77)***	(1.83)*	(4.30)***	(2.22)**
Native dialect only	-0.165	-0.163	-0.007	-0.080
	(3.00)***	(1.88)*	(0.08)	(0.91)
Village marginalisation	-0.073	0.031	0.032	0.030
	(3.04)***	(1.00)	(1.12)	(0.92)
Number of household	-0.000	0.000	-0.000	-0.001
	(0.65)	(0.59)	(0.87)	(1.01)
Number of treated household	-0.001	-0.002	0.001	-0.000
	(1.93)*	(2.05)**	(1.20)	(0.07)
State 13 - Hidalgo	0.219	0.220	0.254	0.281
	(4.22)***	(3.99)***	(4.97)***	(4.64)***
State 16 - Michoacán	0.193	0.280	0.319	0.210
	(3.73)***	(4.84)***	(6.24)***	(3.25)***
State 21 - Puebla	0.063	0.152	0.206	0.094
	(1.22)	(2.91)***	(4.26)***	(1.53)
State 22 - Queretaro	-0.009	0.069	0.052	-0.036
	(0.16)	(1.09)	(0.95)	(0.50)
State 24 – San Luis Potosí	0.114	0.039	0.203	0.443
	(2.26)**	(0.74)	(4.19)***	(7.23)***
State 30 - Veracruz	0.068	0.254	0.211	0.257
	(1.35)	(4.69)***	(4.35)***	(4.21)***
Constant	3.458	2.904	2.927	3.063
	(28.58)***	(18.84)***	(19.89)***	(17.73)***
Observations	9107	4367	4459	3715
F test: Bi=0	34.00	11.88	11.39	13.85

Robust t statistics in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Other results from the estimations display consistency over all four rounds. The significant and positive results on the poverty index parameter indicate that, on average, wealthier households show higher household consumption. Higher per capita 1997 household income is associated with higher levels of household consumption in the first two rounds of date. However this effect is not apparent in the later rounds. Households with employed heads of household as well as older heads of household have higher consumption, on average, than households with unemployed heads and younger heads. Households with male heads consume less than households with female heads. The Language variable groups households in ways that proxy for ethnicity and, to some extent, marginalisation. The results indicate that communities using native dialects have lower levels of household consumption, on average, that the dominant group.

We tested for the robustness of these findings. We ran the same models eliminating consumption outliers, and we also re-estimated the equations using consumption values rather than logs. The sign and significance of the parameters were unchanged.

To further verify the above findings we estimated a model that allows us to take advantage of the panel structure. We estimated a fixed-effects regression model to control for time invariant unobservables. This enables us, for instance, to control for the effects of initial differences in treated versus control areas due to time invariance. In this model, we can only use control variables that vary over time, as 'fixed' variables, such as gender and ethnicity, are netted out. The model is of the type:

$$I_{it} = a + bWave + c(Wave*T_i) + d(X_{it}) + e_{it} \quad (3)$$

The results presented in Table 3 below show that living in a treatment area has a positive and significant impact on the household consumption of non-eligible households for the May 1999 and November 1999 data rounds. The results also indicate that this effect is extinguished by the November 2000 data round. These results confirm those from the cross-section estimates presented above. The advantage of estimating the fixed effects model is that it takes account of the whole panel (the waves are included as well as the waves plus an interaction with the treatment effects), and it is helpful that the results emerging from this estimation shows a similar story.

Table 3: Effects of PROGRESA on the households consumption of non-eligible households estimated using a fixed-effects regression model	
Variable	*Coef/ (s.e).*
Number of shocks	-.0084** (.0035)
Shock	.01423 (.0100)
Work status of head	.03089** (.0120)
WAVE	
May 1999	-.1138** (.0139)
November 1999	-.0668** (.0136)
November 2000	.0238 (.0268)
October 1998	(dropped)
*WAVE*T*	
May 1999*Treatment	.0758** (.0177)
November 1999*Treatment	.0825** (.0174)
November 2000*Treatment	.0214 (.0186)
Constant	5.099 (.0114)
N = 23918; R-sq: within = 0.0089; between = 0.0007 F (9,12678) = 12.60. Prob > F= 0.0000	

Assets

As well as affecting consumption, we hypothesise that the presence of local economy effects of social cash transfers would be consistent with observed asset accumulation within affected areas. It should therefore be possible to observe, as a consequence of the implementation of PROGRESA, positive and significant asset growth among non-eligible households in treatment areas compared to those in control areas.

The empirical work focuses on measures of land and livestock assets which seem especially appropriate to communities in rural Mexico. We constructed two measures of land tenure: a binary measure of whether households own or used land for agricultural purposes [6] and a continuous measure of the hectares owned/used in the largest five plots.

[6] The actual question in the survey instrument asks: how many plots of agricultural, livestock, or forestry land are own or used by members of the household in the last 12 months? This is followed by questions on the hectares and use of each of these plots, up to 5 in the baseline 97 Survey.

Four measures of livestock were constructed, focusing on two separate types of livestock. Production livestock includes poultry, pigs, goats, and cows, which as their label indicates produce milk, cheese, meat, etc. Draft livestock includes horses, mules, oxen, etc, which use is primarily to assist in farming or transport. Two livestock variables constructed are binary measures of whether households have production or draft livestock. A further two variables provide a continuous measure of the number of production or draft livestock measured in cow equivalents using a conversion table constructed from information on livestock values in rural Mexico (Gertler *et al*. 2005).

It is important to keep in mind that the period covered by the data is characterized by structural changes in the rural economy in Mexico, showing a decline in farm or agricultural production. In a sense, PROGRESA came about as a means to mitigate the impact of structural change on the poorest rural households.

Panel estimates of changes in asset holdings

We estimate random effects models to control for time invariant unobservables. Within treatment or control areas individual heterogeneity could be large with the implication that what is estimated to be the effect of the programme might be attributable to other factors. In random effects models, individuals' time invariant heterogeneity is explicitly introduced as a random effect. The model estimated is a version of the one described in equation (3) above. Where the dependent variable is continuous but censored at zero, such as hectare use, the number of draft animals in cow equivalent, and the number of production animals in cow equivalent, we employ Tobit models. Where the dependent variable is binary, as in land and livestock ownership, we use probit models. The results are summarised in Table 4 below. The variables of interest are the wave variables interacted with the treatment area identifier.

In terms of hectare use, we find that non-eligible households in treatment areas have significantly higher hectare use in May 99, but thereafter there appears to be no statistically significant difference between non-eligible households in treatment and control areas. As regards land ownership, non-eligible households in treatment areas have a significantly higher probability of owning land compared to non-eligible households in control areas. The probability of owning land is, for non-eligible households in treatment areas, 5.1 percentage point higher in May 1999, 8.2 percent higher in November 1999, and 9.7 percent higher in November 2000.

The results indicate that in May 1999, one year after the programme was introduced, non-eligible households in treatment areas have a larger number of production animals than non-eligible households in non-treatment areas (on average about 10 percent of an animal measured in cow equivalent). This effect appears to increase in the next round in November 1999 to just over 20 percent of an animal measured in cow equivalent. We also find some evidence of an effect on livestock when analysing draft animal ownership measured in cow equivalent.

Table 4: Determinants of assets holdings among non-eligible households					
	Hectare Use	*Land Ownership*	*Production Animals*	*Draft Animals*	*Livestock Ownership*
Poverty Index	0.873	0.211	-0.126	-0.273	-0.015
	(6.08)***	(11.13)***	(3.27)***	(12.62)***	(0.80)
Number of shocks	-0.055	-0.037	0.129	0.102	0.050
	(0.32)	(2.14)**	(3.24)***	(4.14)***	(2.26)**
Shock	5.933	0.867	0.135	0.154	0.124
	(21.26)***	(30.93)***	(2.24)**	(4.12)***	(3.79)***
Work status of head	0.763	0.190	0.114	0.130	0.124
	(3.42)***	(8.55)***	(2.35)**	(4.38)***	(5.03)***
Gender of head	2.141	0.291	0.335	0.362	0.095
	(7.79)***	(9.43)***	(4.90)***	(8.93)***	(2.90)***
Village marginalisation	0.664	0.080	-0.008	0.161	0.091
	(2.83)***	(2.68)***	(0.13)	(4.82)***	(3.05)***
May 99 * Treatment	0.501	0.051	0.094	0.084	0.107
	(1.76)*	(1.85)*	(1.83)*	(2.61)***	(4.04)***
Nov 99 * Treatment	0.353	0.082	0.205	0.033	0.167
	(1.29)	(2.96)***	(4.01)***	(1.06)	(6.23)***
Nov 00 * Treatment	0.242	0.097			
	(0.85)	(3.44)***			
May 99 wave	-1.735	-0.365	-0.230	-0.209	-0.220
	(6.24)***	(14.12)***	(4.78)***	(6.75)***	(8.63)***
November 99 wave	1.765	0.142	-0.142	-0.110	-0.103
	(6.58)***	(5.55)***	(3.01)***	(3.66)***	(4.08)***
November 00 wave	4.694	0.280			
	(16.64)***	(10.59)***			
State 13 – Hidalgo	-2.040	-0.316	0.027	-0.756	0.104
	(6.61)***	(7.83)***	(0.32)	(17.00)***	(2.68)***
State 16 - Michoacán	0.162	-0.138	0.934	0.146	0.698
	(0.47)	(3.13)***	(10.29)***	(3.13)***	(15.33)***
State 21 - Puebla	-2.614	-0.346	-0.512	-0.609	0.149
	(8.45)***	(8.56)***	(6.18)***	(13.85)***	(3.82)***
State 22 - Queretaro	-4.065	-0.549	0.096	-0.308	0.354
	(9.27)***	(10.16)***	(0.86)	(5.25)***	(6.49)***
State 24 – San Luis Potosí	-1.523	-0.500	0.627	-0.363	0.073
	(4.74)***	(11.99)***	(7.39)***	(8.13)***	(1.83)*
State 30 - Veracruz	-1.104	-0.143	-0.208	-0.925	0.254
	(3.95)***	(3.83)***	(2.76)***	(22.68)***	(7.09)***
Constant	-11.190	-0.553	0.194	-1.605	0.010
	(9.99)***	(3.93)***	(0.67)	(10.14)***	(0.07)
Observations	59623	58911	43834	43835	43859
Individuals	18270	18239	18065	18065	18075
Chi 2 Test: Bi=0	2002.06	3886.11	796.82	1730.71	633.72
Sigma_u	1.96	0.87	1.79	0.69	0.70
Sigma_e	15.52		2.58	1.36	
Rho	0.02	0.43	0.33	0.20	0.33
Robust t statistics in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Estimation for hectare use, number of draft animals in cow equivalent, and number of production animals in cow equivalents are based on random effects Tobit models. Land ownership and animal ownership are random effects Probit models.					

The probability of livestock ownership is higher for non-eligible households in treatment areas compared to those in control areas. The former have 10.7 percent higher probability to own livestock in May 1999. This is a 2.2 percentage point increase in average livestock holding of 74 percent among non-eligible households. In November 1999, the probability of livestock ownership among non-eligible households in treatment areas is also significantly higher compared to non-eligible households in control areas, by 16.7 percent.

These results above suggest that the implementation of PROGRESA is associated with higher asset levels among non eligible households compared to non-eligible households in control areas. The results confirm that PROGRESA has measurable effects beyond those on the immediate beneficiaries, and suggest that it is not possible to rule out spillover effects of a social transfer programme like PROGRESA.

Cross-section estimates and the distribution of asset effects

We are able to get some interesting insights into the distribution of the asset effects of PROGRESA, by conditioning the treatment effects on initial asset holdings. The treatment effects on assets of non-eligible households are estimated through a version of equation (2) above, the main difference being that the control variables are taken from the 1997 ENCASEH survey which predates the roll on of the programme. These variables control household and community characteristics at the baseline, and include: the number of households in the community/village, whether the community has mains water. Household controls include the age and sex of the head of household, whether s/he is literate, speaks indigenous dialect, the highest grade attended at school by the head, and her/his employment status. The number of rooms in the household is also included as a control variable. Table 5 below reports on the results.

The parameters reported in the Table are mixed, but overall provide a measure of support for the hypothesis that cash transfers have spillover effects in rural communities. The probits exploring whether households own/use land for agricultural purposes generally identify significant differences between non-eligible households in treatment areas and control areas. Non-eligible households in treatment areas are more likely to own/use land than those in control areas only for the November 1999 and May 2000 rounds. When the probits are run on the full sample, non-eligible households in treatment areas show 3.6 percent higher probability of having land in May 1999, and a 4.2 higher probability in May 2000. Conditioning on not having land in 1997, the baseline year, non-eligible households in treatment areas have a significantly higher likelihood of having land in the follow up surveys, with the marginal effects rising from the October 1998 survey to the May 2000 survey, and declining afterwards. The effects are reversed in direction when the sample is restricted to those having land in 1997, with non-eligible households in treatment areas showing a lower likelihood of having land compared to non-eligible households in control areas. Overall, we interpret these results as confirming that social transfers are associated with higher asset holdings among non-eligible households in treatment areas, and suggesting potential spillover effects of transfers on the local economy. It is particularly noteworthy that the effects are stronger among non-eligible

households without land prior to the programme, which would seem to indicate that the spillover effects re concentrated among this group.

Table 5. Treatment parameters in asset equations for different waves in PROGRESA data (estimated using probit models and marginal effects are reported)[a]

Asset variable	*October1998*	*May1999*	*November1999*	*May2000*	*November2000*
Land (yes=1; no=0)	-0.003	0.006	0.036*	0.042*	0.009
Land (yes=1; no=0); conditioned on no land in 1997	0.052	0.031**	0.048*	0.081*	0.036**
Land (yes=1; no=0); conditioned on land owned/used in 1997	-0.020***	-0.022***	0.007	-0.002	-0.028*
Livestock (yes=1; no=0)	0.006	0.027*	0.037*	0.019*	
Livestock (yes=1; no=0); conditioned on having no livestock in 1997	0.022**	0.025**	0.055*	0.024**	
Livestock (yes=1; no=0); conditioned on having livestock in 1997	-0.019**	0.014	0.004	0.005	

* significant at 1%; ** significant at 5%; *** significant at 10%

[a] All models include household and community controls measured at their 1997 baseline values: number of households in village, whether village has mains water; head of household age, sex, literacy, top grade attended at school, whether speaks indigenous dialect, and employment status; number of rooms in household.

Similar results apply to probits exploring whether non-eligible households in treatment areas are more likely to have any form of livestock than those in control areas. The marginal effects associated with the treatment identifier are, with one exception, significant, rising to November 1999 and then falling. The marginal effects are again stronger when the sample is restricted to those with no livestock in 1997. Conditioning on households which had livestock in 1997, the effects lose significance. These results are again consistent with the presence of spillover effects from the cash transfers, and these are stronger for households lacking agricultural assets prior to the roll on of the programme.

EXPLAINING LOCAL ECONOMY EFFECTS OF CASH TRANSFERS

The findings presented in the paper suggest that social transfers have effects beyond the direct improvement of the income and consumption of beneficiary households. We find that household consumption and asset holdings of non-eligible households in treatment areas are significantly higher than those observed among non-eligible households in control areas. We also find that this effect is stronger immediately after the implementation of PROGRESA. We interpret our results as consistent with the presence of local economy effects of social transfers. The analysis in the paper does not illuminate on the potential channels and processes through which the spillover effects of social transfers work. However, these are extremely important not only in terms of the plausibility of our interpretation of the findings, but also in a policy context. This section pieces together findings from the emerging literature on PROGRESA to map out what is known about the potential processes supporting local economic effects, to contextualise the findings in this paper, and to indicate the limitations of existing studies and suggest avenues for future work.

Economics suggests that local economy effects of cash transfers would result from social transfers leading to an increase in demand for goods and services, which in turn generate increases in employment and productive capacity. This reflects canonical views about the working of multipliers, say from Keynesian models. The extent to which transfers have effects on prices depends on market effectiveness and on supply conditions. Assuming that cash transfers yield an increase in effective demand and that markets operate with a degree of efficiency, the rise in demand brings forth a proportionate rise in supply, leaving prices unchanged. If, on the other hand, supply is unresponsive, the effects will be felt partly on output and partly on prices. The sense we get from threading together findings from this and other studies examining the impact of PROGRESA is that the processes at work in the affected areas are more complex than those predicted by the canonical approach.

It may be useful to track the different stages going from the introduction of social cash transfers to the likely effects in the local economy, and to insert the available evidence where appropriate.

The impact of transfers on effective demand is confirmed by several studies. There is strong evidence to the effect that households in rural Mexico spend the larger part of their transfers. Hoddinott and Skoufias find that transfers are associated with a significant rise in household consumption, which they estimate to be around 14.5 percent on average among beneficiary households, while transfers have been estimated to represent on average 20 percent of pre-programme household consumption (Hoddinott, Skoufias *et al*. 2000; Hoddinott and Skoufias 2004). A study by Gertler *et al*. extracts measures of the marginal propensity to consume among eligible households (Gertler *et al*. 2005). They conclude that, on average, eligible households consume three quarters of their transfers, and save or invest the remaining quarter. Note the consistence between these two studies. Among poorer households, those without agricultural assets in the pre-programme stage,

they estimate a marginal propensity to consume at 0.961. Poorer households therefore consume almost the entire transfer amounts. There is therefore strong evidence that the introduction of PROGRESA resulted in a rise in effective demand and consumption.

In the context of rural Mexico, the supply response to this rise in effective demand will involve a mix of a rise in beneficiary households' production for own consumption and a rise in production for sale by eligible and non-eligible households. We could speculate that if the bulk of the rise in demand is met by production for own consumption, prices of consumption goods will be unaffected. Similarly, wages will be unaffected. If on the other hand the bulk of the supply response comes from production for sale by eligible and non-eligible households, we could expect observable changes in prices, labour supply and wages.

The findings from several sources indicate that beneficiary households stepped up production, with a consequent rise in the use of agricultural assets and investment in associated materials and equipment (Gertler *et al*. 2005; Angelucci and De Giorgi 2006). However, there is no evidence of a significant change in labour earnings or hours of work (Parker and Skoufias 2000). A reduction of child labour in response to the conditional cash transfer appears to have been compensated for by a marginal rise in adult labour, but labour earnings and aggregate hours of work are not significantly different across treatment and control areas, and over time. There is no evidence of a significant change in food prices (Hoddinott *et al*. 2000), suggesting that whatever the fraction of the increase in consumption that was absorbed through market demand, there was sufficient unused capacity to meet this rise in demand without any impact on prices.

Examining the potential contribution of non-eligible households is crucial to closing this loop, but also elusive. Gertler *et al*. check for the presence of community level income effects by comparing differences in assets of non-eligible households across treatment and control areas in a panel including data from October 1998, May 1999 and November 1999, and they find that "there are no significant program impacts for the sub-sample of ineligibles, the exception being an increase in the number of draft animals for big farms" (p.15). Angelucci and De Giorgi, on the other hand, test for differences in sales of agricultural produce and livestock in October 1998 and agricultural sales in May 1999, but find that sales are lower in treatment areas compared to control areas both among eligible and non-eligible households. These findings would suggest a muted market supply response.

The results reported in this paper diverge directly from those reported in Gertler *et al*. We do find some support for the hypothesis that there are local economy effects of the introduction of PROGRESA among non-eligible households. We also find that these effects are stronger for non-eligible households with low asset holdings prior to the programme. Our asset results could be made consistent with those in Angelucci and De Giorgi if we factor in that the supply response among non-eligible households is stronger among those with a low asset base, both in terms of land use and livestock. Our findings are consistent with the possibility that the rise in demand absorbed by the market was met mainly by increases in production among small producers with low asset holdings. It

could be argued that this may be a possible explanation for the fact that the rise in production did not fully register in more formal markets, through changes in prices or labour utilisation.

This also helps with a related question: what are the sources for the increase in consumption among non-eligible households? Angelucci and De Giorgi argue that these are explained in part by reductions in precautionary saving (reductions in saving and in liquid assets, mainly livestock) which followed on from the introduction of PROGRESA. It is not clear how far would this explanation go, as rough measures of precautionary savings do not come close to covering the estimated increase in consumption among non-eligible households. The behavioural processes explaining the reduction in precautionary saving are also cloudy. One possible explanation is a 'demonstration effect', with non-eligible households replicating the consumption behaviour of eligible households (keeping up with 'poor' Joneses?). Perhaps more interestingly, Angelucci and De Giorgi suggest the increase in consumption among non-eligible households reflects a reduction in precautionary saving brought about by improved informal networks of insurance and protection. These underline the potential role of local networks (Angelucci, De Giorgi *et al*. 2006).

An alternative explanation points to the potential role of equivalent (compensating) transactions through private transfers. It is possible that beneficiary households share their transfer income with non-eligible households thus raising effective demand of the latter. Alternatively, non-eligible households may respond to the cash transfer by withdrawing private transfers to eligible households. These may be contributory explanations, but the proposed channels are very small. There is some evidence on crowding out of private transfers, but their incidence is necessarily small as only 7 percent of households in a single data round report receiving private transfers (Albarran and Attanasio 2002). There is also some evidence of private transfers (including from eligible to non-eligible households) responding to shocks in treatment areas (Angelucci and De Giorgi 2006). Again the incidence is very small, only 58 households received transfers when affected by shocks (Angelucci and De Giorgi 2006).[7] Another potential compensating effect could come from public transfers other than PROGRESA. For example, if other public transfers are now redirected to less poor households, this could lead to increased consumption among non-eligible households. However, Angelucci and De Giorgi find there is no significant difference in the value or incidence of other public transfers between treatment and control areas.

Further work is needed to pinpoint with greater precision the channels and processes through which the local economic effects of transfers work. Piecing together findings from the merging literature on PROGRESA suggests that, in rural Mexico, supply responses in the main did not operate through formal markets. There is no supporting evidence from expected changes in the prices of goods, labour supply, and earnings. Our findings suggest that changes in asset holdings were stronger among non-eligible households in treatment areas with a low asset base before the implementation of the

[7] There is very little work on potential effects of remittances in compounding or compensating for PROGRESA transfers.

programme. It is therefore likely that the local economy effects worked through informal economic networks, but we can only speculate on the relevant channels and processes. Further research is needed to assess the strength of this explanation. There are important policy implications to be considered too. The presence of local economic effects following the introduction of PROGRESA in rural Mexico suggest that ex ante assessments of the impact of social transfers which focus only on the impact on direct beneficiaries may well underestimate the overall impact of such programmes. Ex ante concerns with the impact of social transfers on prices and markets in rural areas should also factor in social and economic networks.

CONCLUSIONS AND FURTHER RESEARCH

The paper sought to provide information on the incidence and significance of local economy effects of social transfers, by focusing on changes in household consumption and asset holdings of non-eligible households in rural Mexico. The introduction of PROGRESA in 1998 in rural Mexico provides an example of a large scale cash transfer programme targeted on poor and poorest households. The quasi-experimental nature of the evaluation data collected for the purposes of evaluating the impact of PROGRESA enables a difference in difference impact analysis. This has been applied to beneficiary households suggesting that the programme has had positive effects in terms of its stated objectives, the improvement of nutrition and consumption, as well as investment in education and health care, among poor households in rural Mexico. In the paper we apply a similar methodology to compare household consumption and asset holdings among non-eligible households in treatment areas and similar households in control areas. Our hypothesis is that the presence of local economy effects of cash transfers is consistent with improvements in household consumption and asset holdings by non-eligible households in treatment areas, compared to similar households in control areas.

We find that non-eligible households in treatment areas have significantly higher household consumption than similar households in control areas in the two years after the introduction of PROGRESA. We also find evidence that asset holdings among non-eligible households in treatment areas are significantly higher than non-eligible households in control areas, but that this effect is stronger for land and livestock ownership. The analysis in the paper suggests that household consumption among non-eligible households in treatment areas was 12.3 percent higher in May 1999 compared to non-eligible households in control areas, and 4.8 percent higher in November 1999. As regards asset holdings, the probability of land ownership was 5.1 percent higher in May 1999 for non-eligible households in treatment areas compared to non-eligible households in control areas, and 8.2 percent higher in November 1999. The probability of livestock ownership increased by 9.1 percent in May 1999 for non-eligible households in treatment areas compared to similar households in control areas, 10.7 percent in November 1999 and 16.7 percent in November 2000. We also find that the asset holdings effects are stronger for non-eligible households with a low asset base before the introduction of PROGRESA. We interpret these results as providing supportive evidence for the

presence of local economy effects of social transfers in Mexico. At the very least, they confirm we cannot rule out local economy spillover effects of social transfers.

The paper goes some way towards providing evidence on the incidence and significance of the effects of cash transfers on the consumption and assets of non-eligible households. We show that a focus on non-eligible households is capable of yielding evidence in support of the presence of local economy spillovers. This has important implications for our understanding of the impact of social transfers in poor communities. The results imply that cash transfer programmes are able to achieve much more than to raise welfare among direct beneficiaries. They are also capable of promoting and encouraging wider growth effects among non-eligible groups. Further research could productively focus on measuring the size of these effects and identify the possible channels and timing of diffusion processes.

Obviously some caveats need to be made in relation to our findings. First, our study could not account for the range of factors outside the PROGRESA programme that could have influenced the results presented above. For instance, the presence of fluid credit and labour markets, dynamic land markets, and thick food markets that enable fast supply response, could be important to ensuring transfer programmes have positive and significant effects upon the local economy. Furthermore, as mentioned above, other policies enacted within the time period of our study may have reinforced, or dampened, the results described in the paper. To the extent that these factors are important, great care must be taken when seeking to replicate similar programmes in different market and policy environments. We have hopefully shown that transfers *can* have beneficial local economy effects, not that they *will* always have positive effects.

Replicating this analysis in different contexts would greatly increase our confidence in the extent to which our results could be generalized. An interesting result from the analysis above is that the qualitative findings using panel data are not that different from those using cross-section data. To the extent that we could confirm this to be the case elsewhere, it would open the way for exploring this issue using data from different countries, such as Africa, where panel data is scarce. Even if similar results to those presented here are not achievable elsewhere, replicating the analysis would make it possible to investigate the nature of country - and context-specific determinants of positive local economy effects from transfer programmes.

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Appendix 1. Variable description

The analysis in the paper focuses on equivalised food consumption. This brief note provides information on the construction of this variable. The datasets contain detailed and consistent information on consumption of a range of food items during the week before the interview with respondents, and non-food items over different periods of time. We constructed a household monthly consumption variable with this information.

The survey instrument asked for information on the consumption, expenditure, and production for own consumption, of food. Where there was full information on these, it was straightforward to compute a monetary value for each item of food consumed (including both food purchased and produced for own consumption), and aggregate these across all the food items to get a measure of food consumption. For many households, the information was not provided in full, and it was necessary to complete it. If the expenditure on a particular food item was missing, but we had information on the quantity bought, it was possible to reconstruct the amounts spent by calculating the median price of each food item using the expenditure information and subsequently imputing these values. However, we could only impute monetary values as indicated for food items that are reported having been purchased or produced for own consumption in the same units (kilograms, litres, etc.). It was also not possible to impute values where information on the quantities consumed was missing. Including monetary values of production for own consumption is important for rural households, as considering only food expenditure would significantly underestimate the amounts actually consumed. We also included household non-food expenditures translated into a monthly equivalent.

The survey instruments asked respondents to estimate their weekly food expenditure and weekly total expenditure. We decided not to use this variable because in many cases the reported weekly food expenditure was greater than the reported total expenditure. Using reported food expenditure and total expenditure it was possible to compute non-food expenditure as a residual. However, because this variable would have been affected by same problem we decide not to use it.

We compute adult equivalents for the consumption data, where children aged 14 or below are weighted as 0.73 of an adult. This is in line with equivalence scales used for Mexico (Teruel, Rubalcava *et al*. 2005).

We have not standardized prices across the datasets, and therefore use the price information from each round of data to impute prices, where this is required. Alternative strategies could be used such as deflating the values of consumption across all the datasets using a single set of prices, for example by using October 1998 prices or estimates of the Consumer Price Index for Mexico. Both strategies present some disadvantages, the former assumes relative prices remain stable across the different waves, and the latter may not match the exact geographic spread of the sample. We do not anticipate that a different approach would significantly affect our estimates, especially as our results corroborate findings in Angelucci and De Giorgi (2006) who apply prices from a single round of data to all the other rounds.

Table A1.1 below describes the variables used in the estimation.

Table A1.1: Variable description		
Variable Name	Description of Variable	Type
Household consumption	Log of value of total consumption	Continuous
Treatment	Indicates whether the household resides in a village that receives the PROGRESA programme	1=Treatment; 0=Control
Poverty Index	Poverty index	Continuous
Shock	Indicates whether the household has experienced a shock in the last year	1=shock; 0=no shock
Number of Shocks	Indicates the number of shocks experienced by the household in the last year	Continuous
Household income	Household monthly income/10000 (from 1997 dataset)	Continuous
Work status of head	Indicates whether the head of household is employed	1=employed; 0=otherwise
Gender of head	Indicated the sex of the household head	1=male; 0=female
Age of head	Indicates the age of the household head	Years - Continuous
land	The amount of land owned by the household in 1997	hectares
Spanish	Only Spanish is spoken in the household	1= yes; 0 = no
Spanish and Native dialect	A native dialect and Spanish are spoken in the household	1= yes; 0 = no
Nativ dialect only	Only a native dialect is spoken in the household	1= yes; 0 = no
Marginalisation	Village marginalisation index	Continuous
Number of households	Indicates number of households in locality	Continuous
Number of treated households	Indicates number of treated households in locality	Continuous
STATE	State in which the household is located 12 = Guerrero 13 = Hidalgo 16 = Michoacán 21 = Puebla 22 = Queretaro 24 = San Luis de Potosí 30 = Veracruz	13,16,21,22,24,30
Land	Whether household owns land in 1997	1=yes; 0=no
Livestock	Whether household had livestock in 1997	1=yes; 0=no