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Intra-Village Expansion of Welfare Programs[†]

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This paper studies, both theoretically and empirically, the intra-village expansion of the *Oportunidades* welfare program, to understand the relationship between the number of beneficiaries and the size of average treatment effects. At some point, the Mexican government relaxed the program's eligibility requirements. As a result, rural villages experienced a large increase in the share of residents receiving *Oportunidades* cash benefits. We compare, shortly before and after the inclusion of additional beneficiaries, the size of the average treatment effect (ATE) on households who were beneficiaries already prior to the program's intra-village expansion. Our findings suggests an increase in the ATE for nutrition and health, yet a decrease in the ATE for school enrollment and attendance. Disregarding possibly adverse affects on schooling, our analysis implies that geographic targeting would increase the program's global impact.

JEL Classification: E21, H43, I38, O12, O17

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1 Introduction

Changes in the number of beneficiaries are inherent to a welfare program's life cycle. Many programs start as small pilots, to then grow to state or national level. For example, the largest conditional cash transfer program in the world, Brazil's *Bolsa Familia* program, which today benefits over fifty million people, started as a small pilot program in only a few municipalities of the country. In the process of growing mature, welfare programs then often adjust benefits and eligibility criteria in order to accommodate 'lessons learned', changing societal needs, and the government's current budget constraint. *Bolsa Familia*, for example, revised nine times[1] its benefits and eligibility criteria since its conception in 2003, always leading to a change in the number of beneficiaries.

Furthermore, economic shocks frequently lead to automatic inclusion (or exclusion) of beneficiaries. During a recession, for example, the number of unemployment-benefit recipients increases. Agricultural insurance programs have few beneficiaries during periods of 'good' weather, while many during periods of 'bad' weather.

Other social policies diminish in size because beneficiaries graduate from the program. Conditional cash transfer programs, for example, with their health and schooling components, can be expected to gradually promote beneficiaries out of extreme poverty.

Very little, however, is known about the relationship between number of beneficiaries and size of average program effects. This is a particularly relevant question when a large share of benefit recipients resides in isolated and small rural communities. Economic intuition tells us that liquidity injections, in such a context, possibly affect local market equilibria (e.g. prices and wages). Increasing or decreasing the number of beneficiaries,

[1] http://www.mds.gov.br/saladeimprensa/noticias-1/2013/marco/NotaTecnicaIPEA.pdf/view (accessed 08/22/2013)

consequently, may have implications for the average effectiveness of the program.

This paper studies the intra-village expansion of *Oportunidades*, Mexico's flagship welfare program. *Oportunidades*, which today benefits over thirty million people, makes regular cash payments to poor families. These payments are of substantial size, corresponding to roughly twenty percent of beneficiaries' pre-transfer income. When the program was launched in early 1998, *Oportunidades* eligibility was based on the poverty level of a household, which was calculated in September 1997 as a discriminant score of household demographics and dwelling characteristics. A household was considered eligible for *Oportunidades* if its score did not exceed a threshold that was determined by the Mexican government. Throughout the year 1998, the cutoff point was such that roughly half of rural households qualified. At the end of 1998, however, the Mexican administration relaxed the threshold. Rural villages, consequently, experienced a large increase in the share of residents receiving *Oportunidades* cash benefits.

Households that joined the program due to the new cut-off point, were called *densificados* by the Mexican government. We'll henceforth adopt this terminology. Households who were beneficiaries already prior to inclusion of *densificados*, on the other hand, will be referred to as 'ultra-poor'. The question is whether and how the intra-village expansion may have affected the program's average impact on ultra-poor households. We use data from an experiment conducted in 506 of the roughly 50,000 program villages. With the aim of assessing the effectiveness of the program, these 506 villages were randomized into treatment and control villages. Only eligible households in treatment villages would receive *Oportunidades* payments.[2] Every six months the Mexican government collected detailed

[2] See Hoddinott and Skoufias (2004) for a detailed description of the experiment.

household data on all village residents, for four consecutive periods.

Within this experimental setting, the intra-village expansion is sort of an 'experiment within the experiment'. In the 506 experimental villages, the share of eligible households increased from on average 54 percent to about 80 percent. We compare, shortly before and after the inclusion of *densificado* households, the average treatment effect (ATE) on ultra-poor households, and find significant differences. The ATE for ultra-poors' food and non-food expenditure, for example, is much higher after the inclusion of *densificado* households.

The before-after comparison, however, is not likely to identify the causal effect of the intra-village expansion. There are potentially many confounding factors. For example, by the time *densificado* households were incorporated, the program administration also raised the average cash benefit of ultra-poor households. It is difficult to tell how much of the change in ultra-poors' treatment effects is due to the inclusion of *densificado* households, and how much due to higher benefits. Moreover, because neither non-food prices nor quantities are observed, we do not know whether higher consumption expenditure reflects or not an increase in real consumption.

We attempt to address these issues using a computable model, which is calibrated to the data of the *Oportunidades* experiment. This allows us to study the inclusion of the *densificado* households, holding everything else constant. The model is simple in the sense that, unlike Todd and Wolpin (2006) and Attanasio et al. (2012), it is not dynamic and ignores fertility and educational choices. Instead, the model puts emphasis on local general equilibrium effects in factor and commodity markets.

The basic setup consists of a village populated by three types of households: ultra-poor, poor (*densificado*), and non-poor households. Each household is endowed with staple (food) as well as labor, and chooses a consumption bundle (food, non-food, leisure) which maximizes utility subject to a budget constraint. The staple (food) is produced inside the village, while the non-food commodity is produced outside. The village exports a fraction of its staple and imports non-food commodities. These assumptions reflect that rural areas specialize in staple production, while importing industrialized products from urban areas. Importing implies transactions costs, in particular labor. The village price of non-food consists of an exogenous component ('market' price) plus an endogenous component (labor costs linked to importing non-food to the village). Labor costs are endogenous because the procurement of non-food requires village residents' labor. In equilibrium the village's labor market must clear, which determines the village's wage rate hence the village price of non-food. Food prices are determined in a similar vein but, because food is exported, its village price consists of an exogenous component minus labor costs linked to export food. If, as is common in practice, food is sold at the farmgate (hence no village residents' labor is required for export), the endogenous component is nil. Initially, the *Oportunidades* payment is only granted to ultra-poor households.

In this framework, if the program is expanded to include additional households (the *densificados*), demand for non-food items increases. The additional labor requirements for procurement cause growth of village wages. Because food prices are exogenous, higher wages imply an increase in food consumption of ultra-poor households. The effect on non-food consumption is ambiguous, because higher wages raise the costs of importing non-food items. Non-food consumption decreases if the price effect outweighs the income effect. This will depend on parameters that describe the program (size of cash payments), household endowments, preferences, and the local context (village size and agricultural productivity).

The parameters of the model are calibrated to the control group of the *Oportunidades* experiment. Before simulating the inclusion of *densificado* households, however, we test the predictive ability of the model. As in Todd and Wolpin (2006), we simulate *Oportunidades* on ultra-poor households in the control group (prior to the inclusion of *densificado* households), in order to compare the resulting simulated impacts to the actual from the experimental data observed impacts. The model is able to match closely the actual treatment effects, which strengthens our confidence in the model's ability to predict the impacts of *Oportunidades*' expansion.

According to our simulations, the relationship between beneficiary number and size of treatment effects is positive for food consumption but negative for non-food consumption: The inclusion of *densificado* households increases the ATE on ultra-poors' food consumption by about two percentage points, and decreases the ATE on non-food consumption by 1.4 percentage points.

Our results contribute primarily to a literature concerned with measuring the causal impact of welfare programs on health, consumption, and other outcomes of benefit recipients. The literature uses mainly evaluations based on experimental and quasi-experimental techniques. There is now a substantial body of research documenting the average treatment effect (ATE) of welfare programs, including *Oportunidades.* An open question in this literature is the relationship between size of treatment effects and the number of treated subjects. This is particularly relevant because the literature evaluates mostly small-scale pilot programs. Yet, very little is known about how treatment effects would evolve if these programs would be taken to a larger scale.

The papers most relevant to our work are Hoddinott and Skoufias (2004), Angelucci and De Giorgi (2009), and Gertler et al. (2012), who estimate the average treatment effect of *Oportunidades* on food consumption. Our results suggest that these treatment effects are increasing in the beneficiary number.
Gertler (2004) documents the average treatment effect on child health. Because higher food consumption increases disease resilience of pregnant mothers and their children, the inclusion of additional beneficiaries potentially increases the ATE on child health.
One of the main features of *Oportunidades* is that the size of benefits does partially depend on regular school attendance of a household's school-aged children. Behrman et al. (2005) and Behrman et al. (2009) find a significant increase in school enrollment. Our results suggest a positive relationship between number of beneficiaries and local wages hence the opportunity costs of schooling. The treatment effect on schooling reported by the literature, consequently, may be inversely related to the number of beneficiaries.

The ATE for non-food consumption decreases in the beneficiary number. However, because non-food items are generally no necessities, we believe the benefits of higher food consumption to outweigh potentially adverse effects of lower non-food consumption. In fact, our simulations show higher utility for ultra-poor households after the inclusion of *densificado* households.

Disregarding the possibly adverse affects on schooling, our results suggest that *Oportunidades*' global impact could be increased (and costs reduced) by adopting a geographic targeting scheme, which concentrates benefits in few markets (e.g. the poorest Mexican states) rather than the entire country.

2 Preliminary Empirical Evidence

We start by providing some suggestive empirical evidence on the relationship between number of beneficiaries and size of treatment effects. In the 506 experimental villages, the Mexican administration collected household data in October 1998 and March 1999 (i.e. shortly before and after inclusion of *densificado* households). A third data collection was conducted in November 1999. The following regression model is estimated for each of the three follow-up data waves:

$$\text{consume}_i = \text{const.} + \theta \,\text{treat village}_i + \gamma X_i^{'} + \epsilon_i \tag{1}$$
$$\text{if} \;\; i \in \text{eligible in September 1997}$$

where consume_i is a measure of consumption of household i. The variable treat village_i indicates whether household i lives in a treatment village. Regression (1) includes only households classified as 'eligible' in September 1997, i.e. before *densificado* households became eligible. The vector $X_i^{'}$ denotes a set of controls, including state dummies and household demographics. Standard errors are clustered at the village level, in order to take into account the intra-village correlation of the error term ϵ_i. The top percentile of the dependent variable is trimmed.

Table 1 displays the estimated regression coefficients of equation (1). Panel I shows the average treatment effect (ATE) shortly before the inclusion of *densificados*. The ATE on household monthly food and non-food expenditure is 26 Peso and 40 Peso, respectively. For the same set of households, the ATE increases to 68 Peso and 94 Peso shortly after the inclusion of *densificado* households.[3]

[3]A note on the identification of reported treatment effects: Behrman and Todd (1999) show that treatment and control group samples are balanced at baseline. Furthermore, Gertler (2004) notes that over the two-year experimental period, 5.5% of the households and 5.1% of the individuals dropped from

The question is how much of the observed change in the average treatment effect can be attributed to the inclusion of *densificado* households. There are potentially many confounding factors. Most importantly, by the time *densificado* households were included, the Mexican administration also raised per capita payments of ultra-poor households. Furthermore, results of Gertler et al. (2012) and Bianchi and Bobba (2013) suggest that ultra-poor households invest part of the *Oportunidades* payment into agriculture. Investments made prior to the inclusion of *densificado* households possibly pay off only months later. How much of the change in treatment effects is due to the inclusion of *densificados*, and how much caused by higher payments, lagged returns to investment, and possibly other confounding factors, remains to be determined. Moreover, because neither non-food prices nor quantities are observed, we do not know whether higher consumption expenditure reflects or not an increase in real consumption. In the next section, we attempt to address these issues using a computable general equilibrium model, whose parameters are calibrated to the *Oportunidades* data. This allows us to simulate the impact of the expansion, holding everything else constant.

the sample, but that there were no differences in attrition between the control and treatment areas, suggesting no systematic attrition bias in the analysis

Table 1: Consumption treatment effects for households classified as 'eligible' prior to the inclusion of *densificados*

	Non-Food						Food
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	hygiene	hh utensils	toys	clothing	shoes	total	total
Panel I: October 1998							
(before inclusion of densificados)							
treat village	3.119***	1.825***	0.000	19.747***	13.174***	40.135***	26.092**
	(0.965)	(0.465)	(.)	(3.634)	(2.803)	(12.102)	(11.143)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	10988	10959	11078	10874	10951	11020	10893
R-squared	0.061	0.015	.	0.065	0.087	0.113	0.139
Panel II: March 1999							
(after inclusion of densificados)							
treat village	2.745**	3.153***	0.178***	41.314***	30.876***	94.152***	67.945***
	(1.391)	(0.714)	(0.056)	(6.470)	(5.944)	(19.178)	(10.056)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	10211	10149	10120	10154	10264	10220	10053
R-squared	0.048	0.012	0.008	0.080	0.108	0.123	0.126
Panel III: November 1999							
(after inclusion of densificados)							
treat village	4.012***	4.329***	0.000	42.280***	32.347***	102.210***	71.007***
	(1.195)	(1.171)	(.)	(5.741)	(4.884)	(16.862)	(13.123)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	10242	10105	10300	10157	10110	10133	9638
R-squared	0.092	0.024	.	0.100	0.143	0.192	0.160

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Displayed are OLS estimates of θ from the regression model $\text{consume}_i = \text{const.} + \theta\,\text{treat village}_i + \gamma X_i^{'} + \epsilon_i$ if $i \in$ eligible in September 1997. The dependent variable is monthly expenditure of household i for *item*, measured in Mexican *Peso*. The exchange rate in 1999 was roughly 1 US Dollar=10 Mexican *Peso*. The dummy treat village_i indicates whether household i lives in a treatment village, and $X_i^{'}$ is a vector of controls, including state dummies and household demographics. The regression includes only households who were classified as 'eligible' in September 1997. Standard errors are clustered at the village level. The top percentile of the dependent variable is trimmed.

3 Simulation

3.1 Theoretical Framework

Consider a village populated by $g = 3$ groups of households: ultra-poor (**U**s), poor/*densificado* (**D**s), and somewhat richer households (**R**s). Household i in group g is endowed with staple $\bar{Q}_{\{i,g\}}$, and labor $\bar{L}_{\{i,g\}}$ (net of labor needed to produce its staple endowment). A household consumes leisure ($l_{\{i,g\}}$), staple ($q_{\{i,g\}}$), and a non-food commodity ($x_{\{i,g\}}$). The village exports part of its staple in exchange for non-food commodities.[4] Importing these items implies transactions costs. For ease of exposition assume that, for each unit of consumption of non-food, it is required one unit of labor. The village price of non-food is consequently $p_x = \bar{p}_\chi + p_L$, where $\bar{p}_\chi$ is the (exogenous) market price of x, and p_L is the village price of labor. Household i chooses a consumption bundle $\{q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}\}$ which maximizes its utility function subject to the household's budget constraint:

$$\max_{q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}} u_{\{i,g\}}(q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}) \qquad s.t.$$
$$p_x \times x_{\{i,g\}} + \bar{p}_q \times q_{\{i,g\}} \equiv [\bar{L}_{\{i,g\}} - l_{\{i,g\}}]p_L + \bar{Q}_{\{i,g\}}\bar{p}_q + \bar{T}_{\{i,g\}}$$

where $\bar{T}_{\{i,g\}}$ is an *Oportunidades* payment. Initially, only ultra-poor households receive the latter, i.e. $\bar{T}_{\{i,U\}} > 0$ and $\bar{T}_{\{i,D\}} = \bar{T}_{\{i,R\}} = 0$. The resulting labor supply and food/non-food demand are functions of the village's wage rate (p_L), the parameter vector $\mathbf{\Omega}$=$\{\bar{p}_\chi,\ \bar{p}_q,\ \bar{L}_{\{i,g\}},\ \bar{Q}_{\{i,g\}}\}$, and the cash payment $\bar{T}_{\{i,g\}}$.

$$\{q_{\{i,g\}}, x_{\{i,g\}}, L_{\{i,g\}}\} : \quad (p_L, \bar{T}_{\{i,g\}}, \mathbf{\Omega}) \rightarrow \Re$$

[4] Descriptive statistics provided in the online appendix show that at least 80 percent of the adult village population report agriculture as their main occupation. At the same time, about half of the monthly value of consumption are non-food items such as hygiene products, household utensils, industrialized clothing, school supplies, and energy.

The village's labor market equilibrium writes

$$\sum_g \sum_i L_{\{i,g\}} \equiv \sum_g \sum_i x_{\{i,g\}} \tag{2}$$

where left and right hand side represent the village's aggregate labor supply and demand, respectively.[5] Equation (2) determines the village's wage rate hence the village price of non-food. Food prices are determined in a similar vein but, because food is exported, its village price consists of an exogenous component *minus* labor costs linked to export food. If, as is common in practice, farmers sell produce at their farmgate, then the endogenous component is nil. Exogenous food prices are corroborated by table 5 in the appendix, which shows no statistically significant differences in food prices between *Oportunidades* treatment and control villages.

In this simple-minded framework, if the cash benefit is extended to the *densificado* households ($\bar{T}_{\{i,D\}} > 0$), then demand for non-food items increases. The additional labor requirements for procurement raise village wages.

We take this first prediction of the model to the data. As wage proxy, we calculate daily profits made from within-village commercial activities (e.g. petty sales, tailoring, washing and ironing, etc.). It is then checked

[5] Equation (2) assumes that the village labor market is local (i.e. limited to the village's population). This assumption is corroborated by, first, data from the 2002 *Encuesta Nacional de Hogares Rurales* (a representative household survey of rural Mexico), where only six percent of adult village residents report to do non-agricultural work in a different village. I am not aware of any studies looking at *cross-village* migration in Mexico. Existing studies exploit the Mexican census, where respondents are asked the state in which they were born (Bush, 1993). It is however difficult to conclude from cross-state migration about cross-village migration, because it is unclear to which extent cross-state migration simply reflects rural-to-urban migration. But several factors suggest the magnitude of cross-village migration to be rather low. First, land markets are often imperfect, which may constrain the acquisition of land of emigrants (Finan et al., 2005). Second, formal credit and insurance markets are imperfect and informal insurance networks within the village a dominant source of insurance (Fafchamps and Lund, 2003). Thus, emigration is costly, because it may disconnect emigrants from these networks. Furthermore, there are usually no large supermarket/retail chains (which receive products from its urban area headquarters) in rural villages. This implies that village residents' labor is needed to import non-food (industrialized) products from urban areas.

whether this wage proxy, ω_i, is different between treatment and control villages. Specifically, we estimate the following model, both before and after the inclusion of *densificado* households:

$$\omega_i = \text{const.} + \theta \, \text{treat village}_i + \gamma X_i^{'} + \epsilon_i \qquad \text{if} \quad i \in \text{always ineligible} \tag{3}$$

Note that regression (3) only includes households who were considered eligible neither before, nor after the inclusion of *densificados*. If daily profits increase for households who never received an *Oportunidades* payment, then we think this is quite indicative for growth in village wages.

Table 2 reports the results of equation (3), and reveals that wage treatment effects indeed follow the same growth pattern as consumption treatment effects seen in table 1. The ATE on ineligible households' daily profits significantly increases between October 1998 and March 1999 follow-up survey. Shortly before the inclusion of *densificado* households, the ATE is not statistically different from zero. After the inclusion, the ATE is about 2.8 Peso per day according to the Tobit estimates. The OLS estimates are smaller, yet still statistically significant.

In the model, because food prices are exogenous, higher wages have a positive effect on food demand of ultra-poor households. The effect on non-food consumption is ambiguous, because higher wages raise the village price of non-food items. Non-food consumption decreases if the price effect outweighs the income effect. This depends on parameters that describe household endowments and preferences, village size, etc. Thus, the effect on non-food consumption is, in the end, an empirical question.

Table 2: Treatment effect on ineligible households' daily within-village profits

	OLS	OLS	Tobit	Tobit
Panel I: October 1998				
(before inclusion of densificados)				
treat village	-0.001	0.081	0.286	0.864
	(0.173)	(0.168)	(1.130)	(1.133)
constant	1.052***	0.811***	-28.330***	-29.628***
	(0.128)	(0.120)	(1.553)	(1.626)
	(0.092)	(0.084)	(1.094)	(1.124)
Controls	No	Yes	No	Yes
Number of Obs	4476	4457	4476	4457
R-squared	0.000	0.009		
Panel II: March 1999				
(after inclusion of densificados)				
treat village	0.113	0.116*	2.877***	2.810***
	(0.069)	(0.070)	(0.862)	(0.869)
constant	0.254***	0.134*	-23.226***	-23.278***
	(0.047)	(0.080)	(1.488)	(1.514)
Controls	No	Yes	No	Yes
Number of Obs	4662	4588	4662	4588
R-squared	0.001	0.009		
Panel III: November 1999				
(after inclusion of densificados)				
treat village	0.044*	0.046*	2.416*	2.601**
	(0.025)	(0.026)	(1.286)	(1.315)
constant	0.063***	0.072***	-30.225***	-29.651***
	(0.014)	(0.019)	(3.299)	(3.432)
Controls	No	Yes	No	Yes
Number of Obs	4662	4588	4662	4588
R-squared	0.001	0.009		

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Displayed are results of the regression model ω_i=const.$+\theta$ treat village$_i + \gamma X_i^{'} + \epsilon_i$ if $i \in$ always ineligible. The dependent variable is daily profits from within-village commercial activities, measured in Mexican *Peso*. The exchange rate in 1999 was roughly 1 US Dollar=10 Mexican *Peso*. The dummy treat village$_i$ indicates whether household i lives in a treatment village, and $X_i^{'}$ a set of controls, including state dummies and household demographics. Standard errors are clustered at the village level. The top percentile of the dependent variable is trimmed.

3.2 The Computable Model

In an attempt to quantify the effect of the expansion, we now turn to numerical simulations. The theoretical framework described above can be captured as a set of thirteen equations. Household full income is given by

$$I_i = \bar{L}_i \times p_L + \bar{Q}_i \times \bar{p}_q + \bar{T}_i \qquad i \in \{U, D, R\}$$

Where I_i denotes the full income, $\bar{L}_i$ labor endowments, $\bar{Q}_i$ staple endowments, p_L and $\bar{p}_q$ are the village wage rate and staple price, respectively. $\bar{T}_i$ are *Oportunidades* payments, whereby $\bar{T}_U > 0$, $\bar{T}_D > 0$ after the expansion, and $\bar{T}_R = 0$.
Assuming Cobb-Douglas utility, $u_i = l_i^{\bar{\alpha}_l} q_i^{\bar{\alpha}_q} x_i^{\bar{\alpha}_x}$, labor supply is given by

$$L_i = \bar{L}_i - \bar{\alpha}_l \times I_i / p_L$$

Demand for non-food consumption is given by

$$x_i = \bar{\alpha}_x \times I_i / p_x$$

where $p_x = \bar{p}_\chi + \bar{m} \times p_L$, with $\bar{p}_\chi$ being the market price of the non-food commodity. The parameter $\bar{m}$ describes how many units of labor are needed to import one unit of the non-food item.
Demand for food consumption is given by

$$q_i = \bar{\alpha}_q \times I_i / \bar{p}_q$$

A village's labor market equilibrium is given by

$$\left(\sum_i \bar{n}_i \times x_i \right) \bar{m} = \sum_i n_i \times L_i$$

where $\bar{n}_U$, $\bar{n}_D$, and $\bar{n}_R$ denote the number of ultra-poor, *densificado*, and non-poor resident households, respectively.

The model is simple in the sense that it ignores potential effects on agricultural production, and assumes away transaction costs for staple exports. There is no credit and insurance market. Furthermore, the model does not account for the schooling conditionalities that come with *Oportunidades*. In section 4, we will discuss the implications of relaxing these assumptions.

3.3 Calibration

The model is calibrated to the *Oportunidades control group* sample. The vector of model parameters is:

$$\mathbf{\Omega} = \{\bar{p}_\chi, \bar{p}_q, \bar{L}_i, \bar{Q}_i, \bar{\alpha}_{\{q\}}, \bar{\alpha}_{\{x\}}, \bar{m}, \bar{n}_i\} \quad i \in \{U, D, R\} \tag{4}$$

The Mexican staple is corn. Some 78 percent of households in the control group cite corn as their main cultivated crop. Corn is also the dominant ingredient in the food consumption basket of Mexicans. A value for the market price of corn ($\bar{p}_q$) can be observed directly from administrative records (Ministry of Agriculture). Monthly corn production of the average ultra-poor household in the control group (which amounts to roughly 307kg) is taken for $\bar{Q}_U$. In a similar vein, monthly corn production of the average *densificado* and non-poor household in the control group is taken for $\bar{Q}_D$ and $\bar{Q}_R$, respectively. The number of ultra-poor, *densificado*, and non-poor households in the average control village, $\bar{n}_U$, $\bar{n}_D$, $\bar{n}_R$, is twenty-four, ten, and eight, respectively.

Parameter values for labor endowments, preferences, market price of the non-food commodity, and import technology can neither be obtained from administrative records nor from the *Oportunidades* experiment:

$$\mathbf{\Lambda} = \{\bar{L}_i, \bar{\alpha}_q, \bar{\alpha}_x, \bar{p}_\chi, \bar{m}\} \quad i \in \{U, D, R\} \tag{5}$$

In order to obtain values for these parameters, we exploit that some of the model's endogenous *variables*, such as food consumption, non-food expenditure, and labor supply, are observed from the control group data. Denote this vector

$$\mathbf{Y}_{\mathbb{C}}^{RCT} = \{q_i^{RCT}, (p_x \times x_i)^{RCT}, L_i^{RCT}\}, \tag{6}$$

where the *RCT* superscript (randomized control trial) and $\mathbb{C}$ subscript is used to indicate sample averages of the *Oportunidades* control group. Denote $\mathbf{Y}^{sim}(\mathbf{\Lambda})$ by the model simulated averages. Values for $\mathbf{\Lambda}$ are then obtained by minimizing the standardized squared distance between $\mathbf{Y}_{\mathbb{C}}^{RCT}$ and $\mathbf{Y}^{sim}(\mathbf{\Lambda})$

$$\min_{\mathbf{\Lambda}} E = \left(\frac{\mathbf{Y}_{\mathbb{C}}^{RCT} - \mathbf{Y}^{sim}(\mathbf{\Lambda})}{\mathbf{Y}_{\mathbb{C}}^{RCT}}\right)^2 \tag{7}$$

The full calibration writes

$$\begin{aligned} \min_{\Lambda} E &= \sum_i \left(\frac{q_i^{RCT} - \bar{\alpha}_q \times I_i/\bar{p}_q}{q_i^{RCT}}\right)^2 \\ &+ \sum_i \left(\frac{(p_x \times x_i)^{RCT} - p_x \times x_i}{(p_x \times x_i)^{RCT}}\right)^2 \\ &+ \sum_i \left(\frac{L_i^{RCT} - [\bar{L}_i - \bar{\alpha}_l \times I_i/\bar{p}_L]}{L_i^{RCT}}\right)^2 \end{aligned}$$

$$s.t.$$

$$\begin{aligned} p_x &= \bar{p}_\chi + \bar{m} \times p_L \\ I_i &= \bar{L} \times p_L + \bar{Q}_i \times \bar{p}_q + \bar{T}_i \\ x_i &= \bar{\alpha}_x \times I_i/p_x \\ \left(\sum_i n_i^{RCT} \times x_i^{RCT}\right) \bar{m} &= \sum_i n_i^{RCT} \times L_i^{RCT} \\ 1 &= \bar{\alpha}_x + \bar{\alpha}_q + \bar{\alpha}_l \end{aligned}$$

where $i \in \{U, D, R\}$. Table 3 shows all the parameter values of the model.

3.4 Impacts Before Expansion

Before simulating the inclusion of *densificado* households we check the predictive capacity of the calibrated model. As in Todd and Wolpin (2006), this is done by comparing simulated treatment effects to actual treatment effects. The simulated treatment effect for some outcome Y_j of household i, $\theta^{sim}_{\{i,j\}}$, is computed as the difference between *simulated* control group, $Y^{sim}_{\{i,j\}}|_{\{\mathbf{\Omega},\bar{T}_U=0\}}$, and *simulated* treatment group, $Y^{sim}_{\{i,j\}}|_{\{\mathbf{\Omega},\bar{T}_U>0\}}$.

$$\theta^{sim}_{\{i,j\}} = Y^{sim}_{\{i,j\}}|_{\{\mathbf{\Omega},\bar{T}_U>0\}} - Y^{sim}_{\{i,j\}}|_{\{\mathbf{\Omega},\bar{T}_U=0\}} \tag{8}$$

The predictions are shown in column (2) of table 4. The model forecasts a treatment effect on ultra-poor households' food consumption of roughly eleven percent. This compares to an actual ATE of nine percent. For non-food consumption expenditure, the predicted treatment effect is eleven percent, compared to fifteen percent observed from the experimental data. Thus, the model approximates to some extent the actual treatment effects which builds some confidence in the model's ability to forecast the inclusion of *densificado* households.

3.5 Impacts after Expansion

We now add the cash payment to the income of *densificado* households. This results in a three percent increase in the village wage rate. The implications for ultra-poor households, can be seen by comparing columns (2) and (3) in table 4. Column (2) shows the simulated treatment effects before, and column (3) after the inclusion of *densificado* households. According to our simulations, the relationship between number of beneficiaries and size of treatment effects is positive for food consumption, but negative for non-food consumption: The inclusion of *densificado* house-

Table 3: Parameter values

		village level parameters	
price of food commodity ($\bar{p}_q$)		2.0	
price of non-food commodity ($\bar{p}_X$)		0.1	
number of eligible households ($\bar{n}_U$)		23.69	
number of *densificado* households ($\bar{n}_D$)		10.24	
number of always ineligible households ($\bar{n}_R$)		8.44	
labor requirement per unit of x ($\bar{m}$)		1.4	
		household level parameters	
	ultra-poor	*densificado*	non-poor
food item endowment ($\bar{Q}_i$)	307.3	377.0	439.3
hh efficient units of labor ($\bar{L}_i$)	38.5	32.5	37.4
preference food item ($\bar{\alpha}_q$)	0.445	0.445	0.445
preference non-food item ($\bar{\alpha}_x$)	0.243	0.243	0.243
preference leisure ($\bar{\alpha}_l$)	0.312	0.312	0.312

holds increases the average treatment effect on ultra-poor households' food consumption by about two percentage points. The ATE for non-food consumption, however, decreases by 1.4 percentage points.

4 Discussion & Conclusion

4.1 Implication of Findings

The main objective of Oportunidades is to improve the education, health and nutrition of poor families. There is a substantial body of research documenting the program's impact along these dimensions. Hoddinott and Skoufias (2004), Angelucci and De Giorgi (2009), and Gertler et al. (2012) find positive average treatment effects on food consumption. Our results suggest the size of these treatment effects to increase in the number of beneficiaries.

Gertler (2004) reports positive treatment effects on child health. Because higher food consumption possibly strengthens the immune system of pregnant mothers and their children, the inclusion of additional beneficiaries

Table 4: Impact of *Oportunidades* on ultra-poor households

	October 1998		
	(1) observed	(2) predicted	(3) expansion predicted
Panel I: *Food consumption*			
control group mean (C)	728.1	680.4	680.4
treatment group mean (T)	790.6	757.4	771.4
ATE ($T-C$)	62.5	76.9	91.0
$(T-C)/C$	0.086	0.113	0.134
Panel II: *Non-food expenditure* [quantity]			
control group mean (C)	382.7 [n/a]	371.6[11.2]	371.6[11.2]
treatment group mean (T)	441.5[n/a]	413.7[11.6]	421.3[11.4]
ATE ($T-C$)	58.7 [n/a]	42.0 [0.34]	49.7 [0.19]
$(T-C)/C$	0.154[n/a]	0.113 [0.031]	0.134 [0.017]

All displayed values are for ultra-poor households. The first row of column (1) in panel I and II show October 1998 sample means of household monthly food consumption (Mexican *Peso* value) and non-food expenditure (Mexican *Peso* value) of ultra-poor households living in control villages. In 1999, the exchange rate was roughly 1 US Dollar=10 Mexican *Peso*. The second row of column (1) in panel I and II show October 1998 sample means of household monthly food consumption (Mexican *Peso* value) and non-food expenditure (Peso value) of ultra-poor households living in treatment villages. The third and fourth row of column (1) show the difference and growth rate between first and second row, respectively. Column (2) shows simulated values for October 1998. Simulations of column (3) include cash payments to *densificado* households. Values in [ ] are consumed quantities (non-food quantities are not observed from the *Oportunidades* data).

potentially increases the ATE on child health. In fact, Gertler (2000) indeed finds higher treatment effects in March 1999 (before inclusion of *densificados*) vis-a-vis October 1998 (after inclusion). Again, though, it is difficult to tell how much of the difference is due to the increase in the number of beneficiaries, and how much due to other confounding factors.

Behrman et al. (2005) and Behrman et al. (2009) find higher school enrollment in treatment villages vis-a-vis control villages. Because more beneficiaries tend to drive up local wages hence the opportunity costs of schooling, our results indicate that the size of these treatment effects is decreasing in the number of beneficiaries. On the other hand, better nutrition from higher food consumption possibly increases the cogitative capacity hence learning success of children.

One also needs to keep in mind the negative relationship between number of beneficiaries and the ATE on non-food consumption. However, because non-food items are generally no necessities, we believe the benefits of higher food consumption to outweigh potentially adverse effects of lower non-food consumption. In fact, our simulations show higher utility for ultra-poor households after the inclusion of *densificado* households.

Our results give reason to believe that the program's global impact could be increased by concentrating benefits in few rather than many markets. The current practice of *Oportunidades* is proxy-means targeting, which distributes benefits across the entire country. Disregarding the potential adverse effects on schooling outlined above, our results imply that geographic targeting, which concentrates benefits in the poorest regions of Mexico, may increase the programs global impact at similar costs. In fact, given the high costs of proxy-means targeting (e.g. data collection, monitoring) and the logistic costs of implementing the program in all states of the country, a switch to geographic targeting most likely decreases the program's overall costs while increasing its global impact.

4.2 Limitations and Extensions

The computable model is simple in that it does not allow for changes in agricultural production, and assumes away transaction costs for staple exports. There is no credit and insurance market, and schooling conditionalities that come with *Oportunidades* are disregarded. The implication of these omissions are discussed in the following.

(i.) Exogenous vs. endogenous agricultural production

Exogenous staple production may be reasonable in the short, but not credible in the long-run. The implications of endogenizing staple production will depend on assumptions about the agricultural market. First, consider the standard separable agricultural household model setting (Singh et al., 1986), with a perfect village labor market, no transaction costs for selling staple, and a surplus-producing village, i.e. exogenous agricultural prices. The increase in local wages resulting from the inclusion of *densificado* households would cause agricultural production to fall. Second, consider the case where the village is not producing a surplus. The price of the staple becomes endogenous. Higher demand exhibits upward pressure on prices which, ceteris paribus, increases production. Agricultural production falls if higher wages outweigh the price effect. Third, in the case of transaction cost for exporting the staple, higher wages imply a decrease in a farmer's selling price hence production.

Should agricultural production fall, this would dampen the income effect from higher wages. The increase in the food consumption ATE, therefore, would be smaller as reported in table 4. The decrease in the ATE for non-food consumption, on the other hand, would be even stronger.

Another possibility is that ultra-poor households are credit constrained and invest additional wage income into agricultural production. In this case, the increase in the food consumption ATE would be higher as in table 4, and the decrease in the ATE for non-food consumption smaller.

(ii.)Farmgate selling vs. transaction costs for staple exports

The simulation further assumed that there are no transaction costs for exporting staple. Fafchamps and Hill (2005) show that farmgate selling is the most common selling method of farmers in Uganda. For Mexico, we are not aware of any quantitative study that documents the most common selling method of farmers. In field work the author conducted in about twenty *Oportunidades* villages, farmers reported to sell their harvest directly to a crop merchant who visits the village with a truck after harvest. This suggests small transaction costs for farmers to sell their produce. There may, however, be villages or regions were, for some reason, farmgate selling is not common. In this case, farmers need to transport their produce to the next regional market. If labor is the only source of transaction costs - and assuming, for ease of exposition, that the export of one unit of staple requires one unit of the village's labor - then the village price of staple is $p_q = \bar{p}_Q - p_L$, where $\bar{p}_Q$ is the exogenous market price of staple. Transaction costs for food exports further reinforce the increase in the food consumption ATE, because higher wages reduce the price of staple.

(iii.) Risk sharing
Angelucci and De Giorgi (2009) find ultra-poor households to partially share the *Oportunidades* payment with their extended family. If *densificado* households were to share their payments with ultra-poor households, then this would increase ultra-poor households' income, on top of the increase in wages. The data, however, suggests otherwise: For ultra-poor households, Angelucci and De Giorgi (2009) find no increase in received loans, monetary or inkind transfers from family and friends, neither before nor after the the inclusion of *densificado* households.

(iv.) School enrollment, classroom attendance, and social interactions
Our simulations further ignore the schooling conditions of Oportunidades. Higher payments are made to households whose children enroll and regularly attend school. Paul Schultz (2004) finds a significant impact of Oportunidades on school enrollment and school attendance. In our model, school enrollment can be interpreted, at least in the short term, a as reduction in a household's net labor endowment. In this case, the inclusion of *densificado* households would have less of an impact on treatment effects

of ultra-poor households. To illustrate this point, consider the stylized case where the loss of child income corresponds exactly to the value of the Oportunidades payment. In this case, a household's budget constraint does not change. Demand and labor supply stay the same. The effect on wages is nil. Ultra-poor households, consequently, would not be affected by the inclusion of *densificado* households. A similar reasoning applies if the model were to account for social interactions and peer effects in school enrollment decisions.[6]

Overall, given the simplified nature of our computable model, one necessarily needs to be careful interpreting the magnitudes presented in this paper. Further research, using experimental (exogenous) variations in the number of beneficiaries, would be helpful to further support our findings.

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[6]Bobonis and Finan (2009) and Lalive and Cattaneo (2009) find that, due to peer effects and social interactions inside the village, Oportunidades also increase school enrollment of non-beneficiaries residing in the same neighborhood.

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5 Tables for Online Appendix

Table 5: T-tests for equality in food prices (in peso) between ineligible households in treatment and control villages for quartiles of the household poverty score

	Median Village Price	T-statistic
Tomato	0.058	0.346
Onion	−0.127	−0.747
Potato	0.044	0.249
Carrot	0.360	1.192
Lettuce, spinach, beet	2.262	1.325
Oranges	0.014	0.089
Banana	−0.086	−0.578
Apple	−0.122	−0.395
Lemon	−0.063	−0.228
Nopales	0.632	1.257
Corn tortillas	0.134	0.951
Corn(grain)	−1.969	−0.986
Wheat bread	−2.675	−1.041
Sweet bread	1.183	0.661
Flour	0.770	1.446
Pasta soup	2.299	0.877
Rice	−0.177	−1.334
Biscuit	−0.462	−0.714
Beans	−0.033	−0.243
Chicken	−0.519	−1.397
Pork	1.541	1.705
Sardine and Tuna	−0.686	−0.314
Eggs	0.042	0.333
Milk	0.079	0.145
Cheese	0.476	0.190
Butter	−0.740	−0.807
Soft drinks	−0.204	−1.041
Coffee	0.732	0.349
Sugar	0.004	0.127
Oil	0.000	−0.004

Table 6: Descriptive statistics: monthly household consumption

	(1) October 1998	(2) March 1999	(3) November 1999
hygiene products (soap, combs, tooth and hair brushes detergents, whiteners)	40.1	50.3	59.2
household utensils (ollas, platos, cazuelas sartenes, sabanas, toallas y cobijas)	4.6	6.2	9.8
fuels (gas, carbon, petrol)	14.3	10.4	16.7
electricity (batteries, light, etc.)	21.6	24.0	25.3
industrialized clothes	55.4	116.5	105.2
shoes (tennis shoes, boots, etc.)	59.3	114.7	104.6
school supplies (pens, paper, etc.)	15.6	10.0	28.3
total non-food expenditure	344.9	468.9	495.1
total value of consumed food items	513.1	471.5	520.0

Displayed values are sample means of the control group sample. All values are in Mexican *Peso*. In 1999, the exchange rate was roughly 1 US Dollar=10 Mexican *Peso*.

Table 7: Descriptive statistics: Main occupational choice

	(1) October 1998 (in %)	(2) March 1999 (in %)	(3) November 1999 (in %)
agricultural day laborer (*jornalero*)	60.35	60.13	64.42
other employment in agricultural sector	14.93	12.72	14.87
self-employed	10.86	13.4	8.89
family business	5.06	4.72	3.77
ejidatario	6.87	5.87	6.42

Displayed values are sample means of the control group sample.

Table 8: Descriptive statistics: Non-agricultural labor supply

	(1) yes/no (in %)	(2) hours per day	(3) days per week	(4) revenue per month
October 1998				
tailoring	1.14	4.3	4.3	220.2
preparing food for sale	0.72	5.6	3.7	284.9
construction/carpenter	0.79	8.7	5.5	560.1
buying and reselling	2.17	7.0	5.7	478.6
transport	0.15	5.6	2.9	1127.5
fixing items	0.01	8.0	6.0	400.0
wash, iron, cooking for pay	1.19	5.3	3.0	102.0
other	3.84	7.0	5.3	1052.5
March 1999				
tailoring	0.77	4.2	4.1	140.8
preparing food for sale	0.35	5.3	4.2	281.1
construction/carpenter	0.87	8.5	5.2	478.1
buying and reselling	2.42	7.3	6.4	394.4
transport	0.09	5.1	3.5	1162.5
fixing items	0.02	8.0	3.5	1030.0
wash, iron, cooking for pay	1.04	5.5	3.2	164.1
other	2.00	6.5	5.1	442.1
November 1999				
tailoring	0.48	4.0	4.2	242.7
preparing food for sale	0.24	4.7	3.4	275.9
construction/carpenter	0.24	8.1	5.1	900.0
buying and reselling	0.51	7.1	5.0	338.7
transport	0.05	8.2	3.4	466.0
fixing items	0.00	.	.	.
wash, iron, cooking for pay	0.69	5.4	3.0	112.6
other	0.59	6.2	5.0	582.4

Values shows are sample means of the control group. Values in columns (2) to (4) are conditional on 'yes' in column (1).

Table 9: PROGRESA Bi-monthly Monetary Benefits (in Peso)

	PROGRESA bi-monthly monetary benefits			
Type of benefit	1st Semester 1998	2nd Semester 1998	1st Semester 1999	2nd Semester 1999
Nutrition support	190	200	230	250
Primary school				
3rd grade	130	140	150	160
4th grade	150	160	180	190
5th grade	190	200	230	250
6th grade	260	270	300	330
secondary school				
1st year				
boys	380	400	440	480
girls	400	410	470	500
2nd year				
boys	400	400	470	500
girls	440	470	520	560
3rd year				
boys	420	440	490	530
girls	480	510	570	610
Maximum support	1.170	1.250	1.390	1.500

Note: All figures are Mexican peso (in 1999, one US dollars was around 10 pesos. Source: Attanasio et al. (2012).

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