Long-run Effects of Conditional Cash Transfers [†]

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Abstract: *There is a growing literature on Conditional Cash Transfer (CCT) programs, like Bolsa Familia in Brazil or Progresa in Mexico. Very little, however, is known regarding the long-term effects of these programs. Using a structural model that is calibrated to the well known Progresa randomized control trial, this paper studies how treatment effects could evolve over time. The increase in cash recipients' utility is found to be smaller in the long-run.*
Keywords: General Equilibrium Effects, Impact Evaluation, Structural Modelling

Resumo: *Há muitos estudos sobre programas de transferência de renda, como por exemplo o programa Bolsa Família no Brasil ou o Progresa no México. Muito pouco, no entanto, é conhecido acerca dos efeitos de longo prazo destes programas. Este artigo estuda como os efeitos do programa poderiam evoluir ao longo do tempo, usando um modelo estrutural que é calibrado para dados de Progresa. A análise sugere que o aumento na utilidade dos beneficiários é menor no longo prazo.*
Palavras-chave: Efeitos de equilíbrio geral, avaliação de impacto, modelagem estrutural

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

1 Introduction

By addressing problems of selection effects, simultaneity bias, and omitted variables the recent wave of randomized control trials (RCTs) has generated much knowledge on the causal impact of an aid intervention on consumption of recipients (Banerjee and Duflo, 2009). Taking the example of the well-known *Progresa* conditional cash transfer randomized experiment, I explore what more can be learned about the intervention's consumption effect if the experimental data is analyzed within a general equilibrium framework. I develop a simple general equilibrium model which elicits how cash transfers change the commodity and factor market equilibrium of the village economy, and how these changes in turn shape the treatment effects of the intervention. I find that the general equilibrium framework helps to provide a structural interpretation for the signs and magnitudes of the (reduced-form) treatment effects observed from the experimental data.

Using an empirical version of the model that is calibrated to the experimental data, I study how treatment effects could evolve over time. My simulated long-run consumption effects of *Progresa* differ quite a bit from the shorter run effects observed in the experimental data.

Between October 1999 and March 1999 the Mexican adminstration relaxed *Progresa*'s eligibility requirements. Consequently, the share of village residents receiving cash transfers increased from about 50 percent to roughly 80 percent. My general equilibrium analysis suggest, quite interestingly, that this within-village expansion (scale-up) may have changed the magnitudes of the program's consumption effects. For example, the average treatment effect on non-food consumption seems to be lower after the scale-up, while that on food consumption seems to be higher.

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I also explore how sensitive treatment effects are to the local context, and hence how far one can extrapolate from the observed treatment effects to different environments.

The structural model helps to better understand how the effects of cash transfers extend beyond those households receiving them. I find, perhaps surprisingly, that *Progresa* may have *adversely* affected the welfare of some program-ineligible households residing in the same villages as cash transfer recipients. At the same time, however, my model provides some guidance how the program may be designed to mitigate these adverse affects.

Theoretically pinning down how program effects 'spill-over' to non-targeted populations (including the control group) further allows to better understand how well identified treatment effects are in which environment.

The paper contributes to several different strings of literatures. First, the paper adds to a small literature combing both structural and reduced-form techniques to generate a richer policy analysis. The 'reduced-form approach' adopted by the majority of program evaluations has been criticized by some for the narrow set of questions that can be answered with it (Heckman and Vytlacil (2005), Rodrik (Rodrik), Rosenzweig and Wolpin (2000), Deaton (2010)). These authors argue that highly policy relevant questions (e.g. what would be the effect of a nation-wide program?', 'in how far can one generalize from results to different contexts?', or 'what would happen if we change some parameters of the program?) can only be adequately addressed by using structural models. Proponents of the reduced-form approach, on the other hand, argue that the latter needs fewer and less severe assumptions than the structural approach (Banerjee and Duflo (2009); Imbens and Wooldridge (2009); Duflo et al. (2007)). Despite the obvious advantages of both the structural and the reduced-form approach, few attempts have been made to combine the two approaches (Todd and Wolpin (2006), Attanasio et al. (2012), Lise et al. (2004), Kaboski and Townsend (2012)). The studies most relevant to my analysis are (Todd and Wolpin (2006) and Attanasio et al. (2012), who combine a structural model with data from the *Progresa* randomized control trial. While their models are designed to study the *schooling* impacts of *Progresa*, my model is designed to study the *consumption* effects of *Progresa*. My model, for example, explicitly incorporates the general equilibrium effect of higher consumption demand on local prices and wages. Different from their analysis, I attempt (1) to provide a structural interpretation for the consumption effects of the cash transfer program, (2) to distinguish between short, medium, and long-run consumption effects, (3) to look at scaling-up effects, and (4) to study the spillover effects on consumption of the rest of the population not targeted by the cash transfer program.

Second, I extend the literature on general equilibrium effects of social policies. Heckman et al. (1998) argue that program effects estimated for small-scale pilot interventions (as is the standard practice in the program evaluation literature) may change substantially when the program reaches a larger scale (e.g. nation-wide implementation). This is because a large-scale program is likely to affect the general equilibrium of the economy. And because program effects are functions of general equilibrium prices and wages, program effects do change as well. Yet, despite the importance of the topic (because in the end, governments want to implement large-scale interventions and not small scale pilot programs), general equilibrium effects of social policies are largely understudied in the literature.[1] Taylor et al. (2005) and Coady and Harris (2004) use computable general equilibrium models to study general equilibrium effects of cash transfers. My analysis differs from theirs in at least three ways. First, while their models are designed to study the effects of cash transfers on economic efficiency, agricultural production and related variables (land rents, agricultural wages, production), my model instead is designed to provide a detailed analysis of non-agricultural production and related variables (e.g. prices of non-food items, non-agricultural wages). Second, my model is calibrated to the *control group* of the *Progresa* randomized experiment rather than social accounting matrices (SAMs) from conventional household survey data. This allows me, in the vein of Todd and Wolpin (2006) and Lise et al. (2004), to test the model by comparing simulated treatment effects with actual treatment effects. Third, my analysis looks at potential scaling-up effects. Quite interestingly, my analysis suggest that a scale-up of

[1] Results of Heckman et al. (1998) and Duflo (2004) suggest that policies which encourage school attendance may reduce future wages. Buera et al. (2012) predict that microfinance will lead to a more efficient distribution of capital and entrepreneurs in the economy, which will increase demand for labor hence wages. Empirically, this prediction is supported by Kaboski and Townsend (2011) and Kaboski and Townsend (2012), who find increasing wages following the introduction of a large-scale microfinance initiative in Thailand. See also Townsend (2010) for a discussion of general equilibrium effects in credit markets in developing countries.

the program (i.e. increasing the number of beneficiaries) may change quite substantially the magnitudes of the effects of cash transfers on consumption effects. The average treatment effect on non-food consumption seems to be lower after the scale-up, while that on food consumption seems to be higher.

Third, I contribute to the program evaluation literature on consumption effects of cash transfer programs. Angelucci and De Giorgi (2009), Hoddinott and Skoufias (2004), Soares et al. (2010), Maluccio and Flores (2005), Attanasio and Mesnard (2006), Fizbein and Schady (2009) report consumption effects of cash transfers for the major cash transfer programs in Mexico, Brazil, Nicaragua, and Ecuador, respectively. Yet, it is unclear from these exclusively empirical studies why the reported magnitudes are what they are, and not higher or lower. Also across studies, there is significant variation in the consumption impacts (lower impacts are found in Brazil and Ecuador, higher impacts in Mexico and Nicaragua). These differences can partly be explained by differences in timing of surveys and the method of measurement, as well as differences in the size of transfers. My analysis unveils some of the structure underlying the reduced-form treatment effect reported in these studies. I analytically derive the consumption treatment effect as a function of household, village, and program characteristics. This helps to interpret the magnitudes reported in the above mentioned empirical studies. Furthermore, King and Behrman (2009) point out an existing knowledge gap regarding the relationship between timing of an evaluation and estimated impacts. By studying how treatment effects evolve over time, my analysis may eventually contribute to narrowing this gap. In the case of Progresa, the Mexican government decided to conduct three follow-up surveys: 6 months, 12 months, and 18 months after Progresa was launched. Suppose the Mexican government would have decided, instead of conducting these follow-ups, to have only one follow-up some 24 months after Progresa was launched. My results, for example, suggest that the latter follow-up survey would have measured a higher treatment effect on food consumption (and a lower effect for non-food consumption) than the previous follow-ups.

Fourth, I contribute to a small literature on spillover effects of social policies on other households not targeted by the policy. The majority of impact evaluations look exclusively at the effect of the program on those households receiving the aid intervention. Yet, general equilibrium effects, kinship networks, and other linkages between aid recipients and non-recipients are likely to extend the effects of program way beyond targeted households. Knowledge on how these non-targeted households are affected is important for calculating the overall welfare effect of a social policy. For example, positive effects for aid recipients may appear in a different light when weighed against potentially adverse spillover effects on the non-targeted population. Despite their importance, such spillover effects have received little attention. Miguel and Kremer (2004) find that anti-worm treatment to some individuals, because of reducing disease transmission, generate large benefits for other individuals not receiving treatment. In terms of cash transfer programs, Bobonis and Finan (2009) and Lalive and Cattaneo (2009) find that school enrollment of Progresa-ineligible household is higher in treatment villages vis-a-vis control villages. In another seminal paper, Angelucci and De Giorgi (2009) find higher food consumption of Progresa-*ineligible* households in treatment vis-a-vis control villages.[2] My analysis helps to further pin down the mechanisms underlying the latter effect. Angelucci and De Giorgi (2009) hypothesize that increase in food consumption of ineligible households is a consequence of lower pre-cautionary grain savings. My structural analysis suggests that general equilibrium effects in factor and commodity markets may play an important role as well. While Angelucci and De Giorgi (2009) find no effect on average non-food consumption of ineligible households, my model reveals that this average effect masks a significant degree of heterogeneity. For example, model and data suggest heterogenous impacts depending on a household's labor endowment. Lastly, using the parameter values obtained from the calibration of the model, I am able to calculate the program effects on utility. Astonishingly perhaps, my analysis suggests that despite the increase in food consumption documented by Angelucci and De Giorgi (2009) (and depending on the form of the utility function), welfare of some program-ineligible households may potentially decrease.

[2] In a more recent paper, Bandiera et al. (2009) look at the effects of a large-scale asset transfer and training programme which is targeted at the poorest women in rural Bangladesh. They find evidence for wealth spillover on households not eligible for the intervention but living in the same community.

2 Theory

2.1 The Model

Consider a village populated by $g = 2$ groups of households: poorer households (**P**s) and somewhat richer households (**R**s)

$$g \in \{\mathbf{P}, \mathbf{R}\} \tag{1}$$

Household i in group g has two sources of initial endowment. First, a share $\bar{\lambda}_{\{i,g\}}$ of the village's staple endowment $\bar{Q}$. Second, its stock of labor $\bar{L}_{\{i,g\}}$, net of labor needed to produce the staple household's staple endowment. The simplification of an exogenous staple endowment allows to keep the analysis analytically tractable. It further helps to maintain the analytical focus of the analysis on the non-food sector. This sector, so the analysis suggests, is quite important to better understand the structure of the consumption effects of cash transfers in (local) general equilibrium. The household consumes part of its endowment (I denote consumption of the staple henceforth $q_{\{i,g\}}$), and liquidates the remainder to purchase a non-food commodity $x_{\{i,g\}}$ (e.g. batteries).

Assumption A.1 The staple is produced inside *the village, while the non-food commodity is produced* outside *the village.*

Thus, the non-food commodity needs to be imported into village. Assumption A.1 reflects the fact that rural areas usually specialize in agricultural production, while importing manufactured and services from urban areas items (e.g. batteries). For example, for the Mexican data that I'll be using at a later point, 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 (e.g. soap, combs, tooth and hair brushes, detergents, whiteners), household utensils (ollas, platos, cazuelas, sartenes, sabanas, toallas y cobijas), industrialized clothing, tennis shoes and boots, school supplies (pens and paper), and energy (batteries, gas, carbon, petrol). Given the latters' industrialized nature and the aforementioned high share of labor force employed in agriculture, these non-food items are unlikely to be produced by the village. Importing these items implies transactions costs:

Assumption A.2 For each unit of consumption of x it is required one unit of labor.

The village price of x is consequently $p_x = \bar{p}_\chi + p_L$, where $\bar{p}_\chi$ is the factory price of x and p_L being the price of labor. The interpretation of x is not necessarily limited to consumption of imported non-food commodities. It can also be thought of as a consumed *service*. Think of, for example, a carpenter service: In this case $\bar{p}_\chi$ may be the remuneration of the wood and tools that the carpenter is using, and p_L the remuneration of the carpenter's labor.

Assumption A.3 Utilities are comparable between households, and each household maximizes a utility function that represents its reflexive, transitive, complete, continuous, and convex preferences.

The utility function of i writes $u_{\{i,g\}}(q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}})$ where $l_{\{i,g\}}$ is consumption of leisure.

Assumption A.4 All agents treat prices as parametric, and no trade is permitted to take place except at equilibrium prices.

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:

$$\max_{q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}} u_{\{i,g\}}(q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}) \qquad s.t. \tag{2}$$

$$p_x \times x_{\{i,g\}} + \bar{p}_q \times q_{\{i,g\}} \equiv [\bar{L}_{\{i,g\}} - l_{\{i,g\}}]p_L + [\lambda_{\{i,g\}} \times \bar{Q}]\bar{p}_q + T_{\{i,g\}} \tag{3}$$

where $T_{\{i,g\}}$ is a cash transfer granted by the government *exclusively* to $g \in \mathbf{P}$, i.e. $T_{\{i,P\}} > 0$ and $T_{\{i,R\}} = 0$. Under the maintained assumption of strict quasi concavity of the utility function, the solution of the household's maximization problem will result in a demand function for the food item

$$q_{\{i,g\}} : (p_L, T_{\{i,g\}}, \mathbf{\Omega}) \to \Re \tag{4}$$

the non-food item

$$x_{\{i,g\}} : (p_L, T_{\{i,g\}}, \mathbf{\Omega}) \rightarrow \Re \tag{5}$$

and a labor supply function ($L_{\{i,g\}} = \bar{L} - l_{\{i,g\}}$)

$$L_{\{i,g\}} : (p_L, T_{\{i,g\}}, \mathbf{\Omega}) \rightarrow \Re \tag{6}$$

as functions of the village's wage rate (p_L), the parameter vector $\mathbf{\Omega}$=$\{\bar{p}_\chi, \bar{p}_q, \bar{L}_{\{i,g\}}, \bar{\lambda}_{\{i,g\}}, \bar{Q}\}$, and the cash transfer $T_{\{i,g\}}$.

2.2 Program Effects in Partial Equilibrium

In partial equilibrium the village wage rate p_L is exogenous.

PROPOSITION 2.1 ***(Partial equilibrium) Under assumptions A.1-A.4 and in partial equilibrium, we have that*** $\partial x_{\{i,P\}}/\partial T_{\{i,P\}} > 0$ ***and*** $\partial q_{\{i,P\}}/\partial T_{\{i,P\}} > 0$***. For*** $g \in \mathbf{R}$***, however,*** $\partial q_{\{i,R\}}/\partial T_{\{i,P\}} = x_{\{i,R\}}/\partial T_{\{i,P\}} = 0$***.***

Proof. A utility function which fulfills the preference requirements of assumption A.2 is the Cobb-Douglas utility function[3]

$$u_{\{i,g\}}(q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}) = q_i^{\{\alpha_q\}}\, x_i^{\{\alpha_x\}}\, l_i^{\{1-\alpha_q-\alpha_x\}} \quad \text{with} \;\; 0 < \alpha_q + \alpha_x < 1. \tag{7}$$

Utility maximization then yields demand and labor supply functions of the form

$$\begin{aligned}
q_{\{i,g\}} &= \bar{\alpha}_q[\bar{L} \times p_L + \lambda_{\{i,g\}} \times \bar{Q} \times \bar{p}_q + T_{\{i,g\}}]/\bar{p}_q && (8)\\
x_{\{i,g\}} &= \bar{\alpha}_x[\bar{L} \times p_L + \lambda_{\{i,g\}} \times \bar{Q} \times \bar{p}_q + T_{\{i,g\}}]/[\bar{p}_\chi + p_L] && (9)\\
L_{\{i,g\}} &= \bar{L}_{\{i,g\}} - [1 - \bar{\alpha}_q - \bar{\alpha}_x][\bar{L} \times p_L + \lambda_{\{i,g\}} \times \bar{Q} \times \bar{p}_q + T_{\{i,g\}}]/p_L && (10)
\end{aligned}$$

Deriving with respect to $T_{\{i,P\}}$ yields $\partial q_{\{i,P\}}/\partial T_{\{i,P\}} = \alpha_q/p_q > 0$, and $\partial x_{\{i,P\}}/\partial T_{\{i,P\}} = \alpha_x/[p_\chi + p_L] > 0$, and $\partial L_{\{i,P\}}/\partial T_{\{i,P\}} = -[1 - \alpha_q - \alpha_x]/p_L < 0$. For $g \in \mathbf{R}$ we have $\partial x_{\{i,R\}}/\partial T_{\{i,P\}} = \partial q_{\{i,R\}}/\partial T_{\{i,P\}} = 0$ and $\partial L_{\{i,R\}}/\partial T_{\{i,P\}} = 0$. ■

In partial equilibrium, the cash transfer generates a positive income effect for $g \in \mathbf{P}$. The latter shifts the budget constraint outwards. Therefore, demand for $q_{\{i,P\}}$, $x_{\{i,P\}}$ and $l_{\{i,P\}}$ increases. The income effect for $g \in \mathbf{R}$ is zero and, consequently, demand remains unchanged.

Empirical evidence rejects the predictions of the partial equilibrium model. In particular, the negative relationship between the cash transfer and recipient's labor supply is not supported by existing empirical evidence (Parker and Skoufias, 2000). Furthermore, Angelucci and De Giorgi (2009) show that food consumption of program-ineligible households increases, whereas the partial equilibrium model predicts that $g \in \mathbf{R}$ is not affected.

2.3 Local General Equilibrium

In general equilibrium, the village's labor market equilibrium writes

$$\sum_g \sum_i L_{\{i,g\}} \equiv \sum_g \sum_i x_{\{i,g\}} \tag{11}$$

where the left hand side is the village's aggregate labor supply, and by assumption A.2 the right hand side being the village's aggregate labor demand. Equation (11) assumes that the village labor market is local (i.e. limited to the village's population). This assumption is corroborated by data from the 2002 *Encuesta*

[3]Proposition 2.1, however, holds for every other utility function which fulfils the preference requirements of A.2.

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 that the magnitude of cross-village migration to be rather low. First, land markets are often imperfect, which may constraints the acquisition of land of emigrants (Finan et al., 2005). Second, formal credit and insurance markets are imperfect hence informal insurance networks with other village members a dominant source of insurance (Fafchamps and Lund, 2003). Thus, emigration is costly, because it may disconnect emigrants from these networks. Furthermore, there are no large supermarket/retail chains (which receive products from its urban area headquarters) in these villages.

PROPOSITION 2.2 ***(General equilibrium) Under assumptions A.1-A.4,*** $\partial q_{\{i,R\}}/\partial T_{\{i,P\}} > 0$***. However,*** $\partial x_{\{i,R\}}/\partial T_{\{i,P\}} > 0$ ***only if (1) labor endowment of*** $i \in \mathbf{R}$ ***is sufficiently large, or (2) staple endowment of*** $i \in \mathbf{R}$ ***is sufficiently low.***

Proof. Substituting (9) and (10) into (11) and solving for p_L yields

$$p_L^* = \frac{\bar{p}_\chi[(\bar{\alpha}_x - 1)(\bar{Q} \times \bar{p}_q + \sum_i T_{\{i,P\}})]}{\sum_i T_{\{i,P\}} + \bar{Q} \times \bar{p}_q - \bar{\alpha}_x \bar{p}_\chi \sum_g \sum_i \bar{L}_{\{i,g\}}}. \tag{12}$$

Note that the equilibrium wage is positive only if

Assumption A.5: $\bar{\alpha}_x \sum_g \sum_i \bar{L}_{\{i,g\}} > [\sum_i T_{\{i,P\}} + \bar{Q} \times \bar{p}_q]/\bar{p}_\chi$,

i.e. if the village's aggregate labor endowment is large enough to allow the village's aggregate consumption demand for x to be satisfied. Substituting (12) into (8) and deriving with respect to $T_{\{i,P\}}$ yields $\partial q_{\{i,R\}}/\partial T_{\{i,P\}} > 0$.
Substituting (12) into (9) and deriving with respect to T_P yields

$$\frac{\partial x_{\{i,R\}}}{\partial T_{\{i,P\}}} = \frac{[\bar{L}_{\{i,R\}}\bar{p}_\chi - \bar{\lambda}_{\{i,R\}}\bar{Q}\bar{p}_q][\bar{\alpha}_x - 1]\sum_g \sum_i \bar{L}_{\{i,g\}}}{-(\sum_g \sum_i \bar{L}_{\{i,g\}}\bar{p}_\chi - \bar{Q}\bar{p}_q - \sum_i T_{\{i,P\}})^2} \tag{13}$$

By assumption A.5 the denominator in equation (13) is always negative. Since $\bar{\alpha}_x < 1$, the term $[\bar{\alpha}_x - 1]$ in the numerator is always negative. Consequently, the sign of $\partial x_{\{i,R\}}/\partial T_{\{i,P\}}$ will depend on the sign of the first term in brackets in the numerator. We have that

$$\frac{\partial x_{\{i,R\}}}{\partial T_{\{i,P\}}} > 0 \text{ if } \bar{L}_{\{i,R\}} > \bar{\lambda}_{\{i,R\}}\bar{Q}\bar{p}_q/\bar{p}_\chi. \quad \blacksquare \tag{14}$$

The intuition behind proposition 2.2 is the following: The cash grant increases cash recipients' demand for non-food items. Importation of these items requires labor. The village's labor demand increases, raising the village's equilibrium wage.[4] A higher wage, however, has an a priori ambiguous effect on non-food consumption of the remainder of the village population not receiving cash transfers. On the one hand, higher wages imply a positive income effect which ceteris paribus increases consumption of non-food items. On the other hand, because $p_x = p_\chi + p_L$, higher wages raise the village price of these non-food items, making their consumption more expensive. Consumption of program-ineligible households decreases if the price effect outweighs the income effect. This is the case, according to condition (14), when the program-ineligible household's labor demand (pertaining to the household's consumption of the imported good) is larger than the labor that the household is able to supply. In this case, the household relies on labor from other households. When labor becomes more expensive, the household is then forced to reduce consumption of non-food items.

[4]

$$\frac{\partial p_L^*}{\partial T_{\{i,P\}}} = \frac{\bar{\alpha}_x[\bar{\alpha}_x - 1]\bar{p}_\chi^2 \sum_g \sum_i \bar{L}_{\{i,g\}}}{-[\bar{\alpha}\sum_g \sum_i \bar{L}_{\{i,g\}}\bar{p}_\chi - \sum_i T_{\{i,P\}} - \bar{Q} \times \bar{p}_q]^2} > 0 \tag{15}$$

The distinction between food and non-food consumption is important. Cash grants increase recipients' demand for both food and non-food items. For food items, however, the price effect is zero, because the village is a net seller of staple. Furthermore, food items are produced by the village and their consumption is therefore not subject to transaction costs linked to importation.[5] The absence of a price effect and the positive income effect resulting from higher within village wages, therefore, generates exclusively positive externalities on ineligibles' food consumption.

3 Data and Empirical Strategy

3.1 Data

In 1997, the Mexican government started the so called *Progresa* program with the aim of reducing rural poverty and inequality (Schultz, 2004). The program provides monetary grants the poorest households of a village.[6] *Progresa* monetary grants are of substantial size, amounting to about 20 percent of average household monetary income in rural Mexico.

The program was initially not implemented simultaneously in all villages. In 1997, the Mexican government determined all eligible households. Then, a set of villages where the program ought to be implemented first was chosen randomly. Households classified as 'poor' in these villages would receive the first *Progresa* transfer payment in early 1998. The remaining villages would only be incorporated into the program two years later. Households classified as 'poor' in these villages would receive the first *Progresa* transfer only in early 2000. The latter, therefore, serve as a control group for the years 1998 and 1999. In some 320 villages where the program would be implemented first (henceforth referred to as 'treatment villages') and in another 186 villages where the program would start two years later (henceforth referred to as 'control villages') the Mexican government conducted a comprehensive baseline, and three follow-up surveys between October 1998 and November 1999. These surveys are village censuses, i.e. data on all residents of these 506 villages was collected. We thus have a panel of the entire village population, consisting of program-eligible households and program-ineligible households, in each of the 320 treatment and 186 control villages.

3.2 Empirical Strategy

Proposition 2.2 elicits that non-food consumption of a household *not* receiving cash grants (but residing in a village with households that do) does increase only if (1) the household's labor endowment is sufficiently large, or (2) the household's staple endowment is sufficiently low. This implies that, first, the smaller the household size of a *Progresa*-ineligible household residing in a treatment village, the lower *Progresa*'s effect on this household's non-food consumption. Second, the larger the cultivated land of a *Progresa*-ineligible household residing in a treatment village, the lower *Progresa*'s effect on this household's non-food consumption. The regression model I estimate thus takes the form

$$x_i = \alpha_0 + \beta_1[V_i \times N_i] + \beta_2 V_i + \beta_3 N_i + \beta_4' Z_i + \epsilon \quad \text{if } i \in \mathbf{R} \tag{16}$$

where x_i is ineligible household i's monthly expenditure for non-food items. Ideally, instead of expenditure, we would want to have quantities. However, neither non-food quantities nor prices are observed from the *Progresa* data. In one specification, the dummy N_i indicates if the household has below median household size. In another specification N_i will be a dummy indicating if the household has above median cultivated land. V_i is a dummy that indicates whether the household resides in a treatment village, and Z_i is a vector of controls. The regressions includes exclusively ineligible households.

[5] Fafchamps and Hill (2005) show that farmgate selling is the most common selling method of farmers, suggesting no transaction costs linked to exportation.

[6] In order to identify the latter, the Mexican government used a multidimensional poverty index (see Skoufias et al. (2001) for a description of the method).

4 Results

Table 1 reports the results of the regression model (16). The first three columns display the results for non-food expenditure. Column (1) shows that the coefficient on the treatment village dummy is positive (though imprecisely measured) when the interaction effects *treat* × *hhsize-small* are excluded. Including the interaction effects, however, changes this picture. The coefficient on *treat* × *hhsize-small* is negative for all three follow-up surveys. The impact of *Progresa* on non-food expenditure, $\beta_1[treat \times hhsize\text{-}small] + \beta_2 treat$, is smaller for ineligible households with below median household size vis-a-vis households with above median household size. The latter brings empirical evidence for proposition 2.2 that non-food consumption of a household *not* receiving cash grants (but residing in a village with households that do) does increase only if the household's labor endowment is sufficiently large. Local general equilibrium effects, however, may not be the only possible explanation for the observed empirical findings. Angelucci and De Giorgi (2009) find that cash transfer recipient households do share part of the cash transfer with households belonging to their social network (family, friends, etc.), which often includes program-ineligible households. Small ineligible households may have a lower likelihood to be connected to a program-eligible households and, therefore, a lower chance to that eligible households share part of the cash transfer.

Furthermore, and again in line with proposition 2.2, column (3) in Table 1 shows that (except for the third follow-up survey) the coefficient on *treat*×*land-large* is negative. The effect of *Progresa* on non-food consumption expenditure, $\beta_1[treat \times land\text{-}large] + \beta_2 treat$, is smaller for ineligible households with above median cultivated land vis-a-vis households with above median land-holdings.

The model explains these empirical finding by, first, cash grants raising *recipients*' non-food consumption. I empirically test this by comparing expenditure of *Progresa* eligible households in treatment and control villages. Table 4 shows that the majority of the cash transfer is spent on non-food items: In the March 1999 follow-up survey (i.e. about one year after the first cash transfers were given out), for example, the treatment effect on total non-food expenditure is 94.2 peso/month (roughly 10 US Dollar), while the treatment effect on food-expenditure is only 67.9 peso/month (roughly 7 US Dollar). About 50% percent of the increase in expenditure can be explained by higher clothing and shoe expenditure. The cash transfer is further spent on hygiene products (e.g. soap), household utensils (e.g. pens), and toys.

Because more consumption of these non-food items implies higher labor demand for importing the items into the village, and taking into account that the village's labor endowment is limited (the average *Progresa* village size is 44 households), the model predicts the village's wage rate to rise. Empirically, we can infer changes in the wage rate by looking at daily profits made from non-agricultural within-village commercial activities (e.g. petty trade, tailoring, washing and ironing, etc.) of *Progresa*-ineligible households in treatment vis-a-vis control villages. Table 2 reports these treatment effects. The Tobit estimate suggests that daily service profits of ineligible households increase by, on average, 2.8 peso. The OLS estimate is lower than that, yet still statistically significant.[7]

In the model a higher wage, however, has an a priori ambiguous effect on program-ineligibles' non-food consumption. On the one hand, higher wages imply a positive income effect which ceteris paribus increases consumption of non-food items. On the other hand, because importation of these items requires labor, higher wages raise the village price of these non-food items, making their consumption more expensive. Consumption of ineligible households decreases if the price effect outweighs the income effect. Proposition 2.2 suggests that this is the case if the household size (hence income effect from higher wages) is small, or if the household is very rich (and thus consumption hence labor demand is high). Column (2) and (3) in table 1 bring some empirical evidence for this, suggesting that the impact of *Progresa* on non-food expenditure is smaller for ineligible households with below median household size, or for ineligible households with above median cultivated land.

[7] Angelucci and De Giorgi (2009) report no changes in wages. Their definition of wages, however, includes agricultural labor. Their wage measure, therefore, is more likely capture the exogenous outside-village rather than the endogenous inside-village wage rate (because agricultural wage labor is a large share of total hours worked, yet largely consisting of seasonal work on some commercial farm away from the village (Taylor and Dyer, 2009)).

However (as has been pointed out earlier), because the village is a net food seller, the effect on food prices is likely to be zero. Angelucci and De Giorgi (2009) shows that there are indeed no statistically significant differences in food prices between *Progresa* treatment and control villages.[8] The absence of a food price effect, yet the presence of a positive income effect from higher wages, may partly explain the in table 1 (columns four to six) observed increase in food consumption of ineligible households living in treatment villages. Both the treatment dummy and the interaction effects are now positive, bringing empirical evidence for proposition 2.2 that the food consumption externality is positive for all households.[9]

Note that the increase in demand for non-food items is not the only possible explanation for higher village wages. Cash transfers were conditional on regular school attendance. The decrease in market labor supply of children is another factor which could affect village wages.

5 Numerical Example

5.1 Simulation vs. Experimental Benchmark

A numerical exercise with an empirical version of the model of section 2 shall further illustrate how a general equilibrium analysis of experimental data can provide a richer policy analysis. Values for some of the model's parameters can be observed directly from the Progresa randomized control trial (control group) data. Parameters which cannot be observed from the data are calibrated to the Progresa RCT control group.[10] Table 6 shows all the parameter values of the model.

I then add the cash transfer to the income of the Progresa-*eligible* household, and solve the model again. This yields a vector of *simulated* outcomes of the treatment group. The predictions of the model are shown in table 3. Columns (2) and (5) show model predictions for the average program eligible household and ineligible household, respectively. The corresponding from the Progresa RCT observed experimental treatment effects are shown in column (1) and (4).

Both model and experimental benchmark show that *Progresa* raises eligibles' non-food consumption. While the model forecasts an increase of 14.1 percent, the experimental point estimate is 18.2 percent. Because importation of non-food items requires labor, the village's labor demand increases. Higher labor demand leads to an increase in the village's wage rate: The model predicts an increase from 7.39 to 8.27 peso/day. The positive income effect resulting from higher wages affects consumption of *ineligible* households. The model predicts an increase in ineligibles' monthly food consumption of roughly four percent, compared to a from the experimental data observed increase of about two percent. While the model predicts an *increase* in ineligibles' monthly non-food consumption *expenditure* of 3.9 percent, it predicts a *decrease* in *quantity* consumed of seven percent. This compares to a from the experimental data observed increase in non-food expenditure of 4.7 percent (non-food quantities, however, are not observed in the experimental data).

The model is able reproduce the experimental moments quite well, which increases my confidence in the partial equilibrium simulations that will follow.[11]

Columns (3) and (6) in table 3 show the partial equilibrium model predictions for Progresa. In partial equilibrium the wage rate is exogenous. Ineligible households, therefore, are not affected by the program. For eligible households we observe higher program effects on non-food consumption (quantities) in partial equilibrium vis-a-vis general equilibrium (7.7% vs. 2% increase). On the other hand, program effects on food

[8] Furthermore, Cunha et al. (2011) find no food price effects for another more recent cash transfer program in rural Mexico.

[9] Because we don't observe neither prices nor quantities for non-food items, I am not able to test for differences in non-food prices between treatment and control villages. However, because the village price of a non-food item consists of the (exogenous) factory price plus labor costs pertaining to importation, the increase in wages suggested by table 2 does provide some evidence that such price effects may exist.

[10] A detailed description of the empirical version of the model and its calibration is provided in the appendix.

[11] Note that I have not used any data of the Progresa RCT treatment group in the calibration of the model. The fact that the model is nevertheless able to reproduce the moments of the Progresa RCT treatment group sample may be seen as source of model validation (Todd and Wolpin, 2006).

consumption are lower in partial equilibrium compared to general equilibrium (7.7% vs. 14.1% vs. increase).

5.2 Short vs. Long-run Effects

In reality, village wages may not adjust immediately. Suggestive evidence for slow wage adjustment is reported in table 2. Above I described how, in the absence of within-village wage data, one may infer changes in the village wage rate by looking at daily profits made from within-village economic activities, such as buying and reselling, tailoring, washing, ironing, etc. Table 2 reveals that the average treatment effect on Progresa-ineligible households' daily profits from such activities is twice as high in the March 1999 follow-up survey (about 12 months after the launch of Progresa) vis-a-vis the October 1998 follow-up survey (6 months after launch).

A slowly adjusting village wage has implications for the magnitude of treatment effects in each period following the introduction of cash transfers. In the model of section 2, for example, food consumption is a positive function of the village wage (because the positive income effect resulting from higher wages, ceteris paribus, increases demand for food). If the village wage moves slowly to its new equilibrium, this implies that the food consumption treatment effects increase in each period, until the wage has reached its new equilibrium. Because in each period between old and new equilibrium, the cash transfer recipient household experiences an additional positive income effect from a higher village wage (vis-a-vis the previous period).

Table 3 illustrates how the food consumption treatment effects of Progresa may evolve in time. The partial equilibrium model predictions in column (3), where the village wage is fixed, reflect the initial (instantaneous) effect of Progresa on food consumption of cash recipients. The latter is about 7.7 percent. From the Progresa experimental data observed effects in column (1), where the wage is likely to be somewhere in between old and new equilibrium, reflect the medium-term effect. The latter is estimated at 9.7 percent. And the general equilibrium predictions of the model in column (2), where the wage has fully adjusted, reflect the long-run effect. The latter is about 14.1 percent.[12]

The above numerical simulation illustrates how treatment effects may evolve along the transition path from pre-program to post-program steady state. The model, therefore, may provide some structural interpretation for the increase in the magnitude of food consumption treatment effects that other empirical studies document, but not explain.[13]

6 Local General Equilibrium Effects: Further Evidence

In section 3 I described that Progresa eligibility depended on the poverty status of a household. The latter was calculated as a discriminant analysis score, using variables such as household size, household head gender, access to medical services, number of children, education of household head, age of the head of the household, and dwelling characteristics.[14] The lower the score, the higher the level of poverty. A household was considered eligible for Progresa if its poverty score did not exceed a threshold that was set by the Mexican administration. The poverty score was calculated in September 1997, or about 7 months prior to

[12]The reader may then find it puzzling why, for non-food consumption, the long term effect is lower than the medium-term effect. This because I assumed unit elasticity demand functions for non-food and food items. If, more realistically perhaps, I were to assume that the richer the household the higher the share of income spent on non-food items vis-a-vis food items (Engel's law), then the simulated long-term effect would most likely be larger than the medium-term effect.

[13]For example, Angelucci and De Giorgi (2009) report an average treatment effect of 15.84 peso (per month per adult equivalent) in the first Progresa follow-up survey in October 1999, 25.74 Peso in the second follow-up (March 1999), and 30.61 Peso in the third follow-up survey. Note, however, that not all the difference in treatment effects between the three follow-up surveys stems from sluggish wage adjustment. For example, between first and third round of follow-up surveys, the Mexican administration increased the cash transfer size per beneficiary. Furthermore, the eligibility criteria were relaxed, which resulted in more cash transfer recipients per village and, thus, most likely higher wages. I'll address these factors in more detail in the next section.

[14]see Skoufias et al. (2001) and Coady and Parker (2009) for more details.

the disbursement of the first cash transfers. Initially, only 54 percent of households residing in the 506 treatment and control villages were classified as eligible by the Mexican adminstration. Thus, treatment effects obtained in the first follow-up survey (October 1998) reflect a situation where about half of the households in treatment villages received the Progresa cash transfer.

Between the first (October 1998) and second (March 1999) follow-up, however, the Mexican administration adjusted the eligibility threshold. This resulted in half of the households initially classified as *ineligible* suddenly becoming eligible. I'll henceforth refer to the latter as 'newcomers'. In other words, between October 1998 and March 1999, treatment villages experienced a significant increase in the share of cash transfer recipients. From initially 54 percent, the share of cash transfer recipients in treatment villages increased to roughly 80 percent.[15]

My model provides some guidance how this *within-village scale-up* affected the local general equilibrium of the village economy. The model suggests that the within-village scaling-up raises demand for non-food consumption of newcomers. Because importation of non-food items requires labor, the village's labor demand increases. Higher labor demand leads to an increase in the village's wage rate. Above I described how, in the absence of within-village wage data, one may infer changes in the village wage rate by looking at daily profits made from within-village economic activities (e.g. buying and reselling, tailoring, washing, ironing, etc.). Table 2 reveals that the average treatment effect on Progresa-ineligible households' daily profits from such activities is about double as high in the March 1999 follow-up survey (i.e. after within-village scale-up) compared to the October 1998 follow-up survey (i.e. before within-village scale-up).[16]

The model then suggests that the positive income effect resulting from higher wages, in turn, increases *non*-newcomers (i.e. those that were eligible right from the beginning) food consumption demand. This implies that one should observe higher food consumption for non-newcomer households in the March 1999 (i.e. after within-village scale-up) vis-a-vis the October 1998 follow-up survey (i.e. before within-village scale-up). Equivalently, the treatment effect on non-newcomers food consumption should be higher in the March 1999 follow-up survey compared to the October 1998 follow-up. Table 4 shows that this is indeed the case. The average treatment effect on non-newcomer households' monthly food consumption is about 26 Peso in the October 1998 follow-up survey, compared to 68 Peso in the March 1999 follow-up. For non-food consumption, the picture looks similar. The average treatment effect on the monthly value of non-newcomer households' non-food consumption is about 40 Peso in the October 1998 follow-up, compared to 94 Peso in the March 1999 follow-up.

Panel III in table 2 and 4 show that treatment effects for both my village wage proxy as well as consumption are similar in the March 1999 vis-a-vis the last follow-up survey conducted in November 1999. This suggests that the sharp increase of treatment effects between the October 1998 follow-up survey (before within-village scale-up) and March 1999 follow-up (after within-village scale-up) is not due to seasonal fluctuations in demand.

However, not the entire increase in the magnitude of treatment effects between the October 1998 follow-up (before within-village scale-up) and March 1999 follow-up (after within-village scale-up) should be attributed to the within-village scale-up. First, in the previous section I described how slow wage adjustment may generate increasing treatment effects along the transition path from pre-program to post-program steady state. Second, the program administration increased the cash transfer size per beneficiary between October 1998 and March 1999, which in turn is likely to explain part of the increase in treatment effects. Between the March 1999 and November 1999 follow-up, the adminstration again increased the cash transfer size per beneficiary. The latter increase was *larger* than the first increase that took place between October 1998 and March 1999. In the previous paragraph, however, I described that the treatment effects did not increase substantially between March 1999 and November 1999. Because of seasonality, however, one may not di-

[15]In the previous section I included only those program-ineligible households whose eligibility status did not change between baseline and last follow-up survey.

[16]The increase is even higher when considering the Tobit instead of the OLS estimates. The Tobit treatment effect estimate is 0.12 in the October 1998 follow-up survey (i.e. before within-village scale-up) vis-a-vis 2.87 in the March 1999 survey (i.e. after within-village scale-up)

rectly conclude from this that the increase in the cash transfer size per beneficiary to play a minor role in explaining the sharp increase of treatment effects between October 1998 and March 1999.

I can use the numerical model of the previous section to gain more insights of how much of the increase in the magnitude of treatment effects between the October 1998 follow-up and March 1999 follow-up may potentially be attributed to the within-village scale-up. In the previous section, I have simulated the the impacts of cash transfers for the March 1999 data wave (i.e. after within-village scale-up). The model parameters describing the number eligible and ineligible households in the model village, n_P and n_R, were set to the sample averages observed from the March 1999 data wave ($n_P = 35.31$ eligible households and $n_R = 8.82$ ineligible households in the average control village). In the previous section we saw that the resulting simulated treatment effects were a 14.1 percent increase in the value of food and non-food consumption, respectively. In order to shed some light on how much of the increase in the magnitude of treatment effects between the October 1998 follow-up and March 1999 follow-up may be attributed to the within-village scale-up, I replace n_P and n_R with the sample averages observed in the October 1998 follow-up survey ($n_P = 25.94$ and $n_R = 23.61$), while keeping the cash transfer amount constant at the March 1999 level. The resulting simulated treatment effect is a 10.8 percent increase in the value of food and non-food consumption, respectively. Therefore, increasing the number of eligible households from $n_P = 25.94$ to $n_P = 35.31$ increases the treatment effects by 14.1-10.8=3.1 percentage points.

7 Conclusion

In this paper I attempted to explore if analyzing experimental data within a general equilibrium framework can provide a richer policy analysis. I find that such a framework helps to provide a structural interpretation for the magnitudes of the reduced-form experimental treatment effects. By allowing to explore how sensitive the signs and magnitudes of treatment effects are to the local context, how treatment effects would evolve if a pilot program is scaled-up, and how long-run differ from short-run effects, the structural analysis may help addressing concerns of critics regarding the RCT's external validity (generalizability of the signs and magnitudes of treatment effects).

The partial equilibrium outcome can have desirable features vis-a-vis the general equilibrium outcome. From a technical point of view, partial equilibrium (i.e. fix wages and prices) improves the identification of treatment effects by reducing the risk of contamination of the control group through changes in prices and wages. From a social planner's perspective, partial equilibrium avoids negative externalities on households not targeted by the intervention. For example, the simulations I conducted in section 5 suggest that decreasing non-food and leisure consumption leads to a fall in utility of -1.7% for Progresa-ineligible households residing in the same villages as cash transfer recipients.

It is possible to design the experiment in such a way that the partial equilibrium outcome is achieved. The general equilibrium effect stems from transaction costs linked to consumption of non-food items. Non-food items are not produced by the village and thus are subject to transaction costs (labor) pertaining to importation. Cash transfers generate higher demand for non-food items hence labor for importing these items. Higher labor demand raises the village wage. Because the village price consists of the (exogenous) factory price plus (labor) transaction costs for importation, higher wages imply higher village prices. Under these assumptions, the social planner may avoid the wage hence price effect by limiting the increase in labor demand. The social planner can achieve this by supplying non-food items to the village (in addition to giving cash transfers). The labor market equilibrium becomes

$$-x_G + \sum_g \sum_i L_{\{i,g\}} \equiv \sum_g \sum_i x_{\{i,g\}} \tag{17}$$

where x_G denotes the quantity of non-food items that the government supplies to the village. The quantity which induces $\partial p_L / \partial T_{\{i,P\}} = 0$ hence the partial equilibrium outcome is

$$x_G = \sum_{i \in P} \left(\frac{\partial x_{\{i,P\}}}{\partial T_{\{i,P\}}} dT_{\{i,P\}} - \frac{\partial L_{\{i,P\}}}{\partial T_{\{i,P\}}} dT_{\{i,P\}} \right) \tag{18}$$

By avoiding general equilibrium effects, a program design where cash transfers are delivered jointly with (18) could improve the identification of treatment effects (by reducing the risk of contamination of the control group through changes in prices and wages) and help to avoid negative externalities on ineligible households.

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8 Appendix

8.1 Appendix for Numerical Example

A. Full Mathematical Model Statement

Household full income:

$$I_P = \bar{L}_P \times p_L + \bar{Q}_P \bar{p}_q + T_P \quad (19)$$
$$I_R = \bar{L}_R \times p_L + \bar{Q}_R \bar{p}_q \quad (20)$$

Labor supply:

$$L_{\{P\}} = \bar{L}_P - \alpha_l \times I_P / p_L \quad (21)$$
$$L_{\{R\}} = \bar{L}_R - \alpha_l \times I_R / p_L \quad (22)$$
$$(23)$$

Demand non-food item:

$$x_P = \alpha_x \times I_P / (\bar{p}_\chi + m \times p_L) \quad (24)$$
$$x_R = \alpha_x \times I_R / (\bar{p}_\chi + m \times p_L) \quad (25)$$

Demand food-item:

$$q_P = \alpha_q \times I_P / \bar{p}_q \quad (26)$$
$$q_R = \alpha_q \times I_R / \bar{p}_q \quad (27)$$

Village labor market equilibrium:

$$(n_P \times x_P + n_R \times x_R)m = n_P \times L_P + n_R \times L_R \quad (28)$$

B. Calibration

I calibrate the model exploiting data available on the *control group* of the Progresa randomized control trial (March 1999 data wave).

The vector of parameters for which we need values writes:

$$\mathbf{\Omega} = \{\bar{p}_\chi, \bar{p}_q, \bar{L}_i, \bar{Q}_i, \bar{\alpha}_{\{q\}}, \bar{\alpha}_{\{x\}}, \bar{m}, n_i\} \quad i \in \{\mathbf{P}, \mathbf{R}\} \quad (29)$$

where $\bar{p}_\chi$ is the factory price of the imported non-food commodity, $\bar{p}_q$ is the world market price of the staple, and $\bar{L}_i$ denotes the household's endowment with efficient units of labor. $\bar{Q}_i$ is the household's staple endowment. The parameters $\bar{\alpha}_{\{x\}}$ and $\bar{\alpha}_{\{q\}}$ are the Cobb Douglas preferences for non-food and food commodity, respectively.[17] The parameter $\bar{m}$ describes how many units of labor are needed to consume one unit of the non-food commodity such that $p_x = \bar{p}_\chi + \bar{m} \times p_L$. The parameter n_P denotes how many Progresa eligible households exist in the village. The parameter n_R denotes how many Progresa-ineligible households exist in the village.

In Mexico, the staple is corn. 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). I take monthly corn production (in kilogram) of the average Progresa-eligible household in the control group (which amounts to roughly 300kg) as a value for $\bar{Q}_P$. In an analog manner, I take monthly corn production

[17] The preference for leisure is then given by $\bar{\alpha}_{\{l\}} = 1 - \bar{\alpha}_{\{x\}} - \bar{\alpha}_{\{q\}}$.

(in kilogram) of the average Progresa-ineligible household in the control group (which amounts to roughly 450kg) as a value for $\bar{Q}_R$. The number of eligible households in the average control village is 35.3, which serves as value for n_P. In an analog manner, the number of ineligible households in the average control village is 8.8, which serves as value for n_R.

I am left with the following parameters for which values can neither be obtained from administrative records nor from the Progresa RCT data.

$$\mathbf{\Lambda} = \{\bar{p}_\chi, \bar{L}_i, \bar{\alpha}_{\{q\}}, \bar{\alpha}_{\{x\}}, \bar{m}\} \quad i \in \{\mathbf{P}, \mathbf{R}\} \tag{30}$$

In order to obtain values for these parameters I exploit the fact that I do observe from the control group data a couple of in the model endogenous variables: (1) monthly quantity of consumed food (q_P^{RCT} and q_R^{RCT}) of the average eligible and ineligible household, respectively; (2) monthly inside-village labor supply[18] (L_P^{RCT} and L_R^{RCT}) of the average eligible and ineligible household, respectively; (3) monthly non-food expenditure $(p_x \times x_P)^{RCT}$ and $(p_x \times x_R)^{RCT}$ of the average eligible and ineligible household, respectively. Denote this vector

$$\mathbf{Y}_{\mathbb{C}}^{RCT} = \{q_i^{RCT}, (p_x \times x_i)^{RCT}, L_i^{RCT}\} \tag{31}$$

The superscript RCT is used to indicate that these values are obtained directly from the experimental data. Denote $\mathbf{Y}^{sim}(\mathbf{\Lambda})$ the vector of from the model simulated values of these variables. I then calibrate $\mathbf{\Lambda}$ by minimizing the standardized squared distance between $\mathbf{Y}_{\mathbb{C}}^{RCT}$ and $\mathbf{Y}^{sim}(\mathbf{\Lambda})$

$$\min_{\mathbf{\Lambda}} E = \left(\frac{\mathbf{Y}_{\mathbb{C}}^{RCT} - \mathbf{Y}^{sim}(\mathbf{\Lambda}, \mathbf{X}(\mathbf{\Lambda}))}{\mathbf{Y}_{\mathbb{C}}^{RCT}} \right)^2 \quad s.t. \;\; \mathbf{X} = g(\mathbf{\Lambda}) \tag{32}$$

The full calibration writes

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

$$s.t.$$

$$\begin{aligned}
p_x &= \bar{p}_\chi + \bar{m} \times p_L \\
I_i &= \bar{L} \times p_L + \bar{Q}_i \times \bar{p}_q + T_i \\
x_i &= \bar{\alpha}_x \times I_i/p_x \\
(n_P^{RCT} \times x_P^{RCT} + n_R^{RCT} \times x_R^{RCT})\bar{m} &= n_P^{RCT} \times L_P^{RCT} + n_R^{RCT} \times L_R^{RCT} \\
1 &= \alpha_x + \alpha_q + \alpha_l
\end{aligned}$$

where $i \in \{P, R\}$.

C. Model Prediction as Out-of-Sample Forecast

I derive the predictions of the model by conducting out-of-sample forecasts. I add the average Progresa cash transfer to the income of **P**, then solve the model. This yields a vector of *simulated* outcomes of the treatment group. Formally, I compute the simulated treatment effect for some outcome Y_j of household i, $\theta_{\{i,j\}}^{sim}$, as the difference between *simulated* control group, $Y_{\{i,j\}}^{sim}|_{\{\mathbf{\Omega}, T_P=0\}}$, and *simulated* treatment group, $Y_{\{i,j\}}^{sim}|_{\{\mathbf{\Omega}, T_P>0\}}$.

$$\theta_{\{i,j\}}^{sim} = Y_{\{i,j\}}^{sim}|_{\{\mathbf{\Omega}, T_P>0\}} - Y_{\{i,j\}}^{sim}|_{\{\mathbf{\Omega}, T_P=0\}} \tag{33}$$

[18]e.g. buying and reselling, tailoring, washing, ironing, etc.

8.2 Tables and Figures

Table 1: Heterogenous Consumption Effects of Progresa

	Non-Food			Food		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel I: October 1998						
treatment village	12.458	23.259	17.999	17.426	31.482	2.547
	(24.491)	(33.486)	(24.142)	(38.993)	(48.010)	(41.413)
treat×hhsize-small		-24.409			-38.387	
		(28.115)			(45.929)	
treat×land-large			-22.160			77.041
			(60.369)			(87.874)
Controls	YES	YES	YES	YES	YES	YES
Number of Obs	11050	11050	11050	10989	10989	10989
R-squared	0.057	0.055	0.073	0.011	0.010	0.013
Panel II: March 1999						
treatment village	31.942	79.285	56.919	14.011	7.743	2.273
	(40.698)	(56.298)	(40.416)	(27.074)	(35.101)	(29.834)
treat×hhsize-small		-93.840*			9.899	
		(52.705)			(39.311)	
treat×land-large			-225.159*			70.187
			(117.107)			(45.160)
Controls	YES	YES	YES	YES	YES	YES
Number of Obs	4722	4722	4722	4586	4586	4586
R-squared	0.053	0.056	0.067	0.040	0.041	0.051
Panel III: November 1999						
treatment village	-11.725	13.776	-14.557	27.069	10.324	28.116
	(34.623)	(45.743)	(33.855)	(22.336)	(26.326)	(23.846)
treat×hhsize-small		-61.349			38.290	
		(46.381)			(27.875)	
treat×land-large			60.858			-4.907
			(83.044)			(33.495)
Controls	YES	YES	YES	YES	YES	YES
Number of Obs	4722	4722	4722	4586	4586	4586
R-squared	0.053	0.056	0.067	0.040	0.041	0.051

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Displayed are results of regression (16). Standard errors are clustered at the village level. For (1)-(3) the dependent variable is monthly household non-food expenditure of ineligible households (in peso). For (4)-(6) the dependent variable is the household monthly food consumption (i.e. food expenditure plus peso value of consumption of own agricultural production) of ineligible households. Standard errors clustered at the village level. Regressions include program-ineligible households only. The 99th percentile of dependent variable is excluded. Treatment village (treat) is a dummy indicating if ineligible household i lives in a treatment village. $hhsize - small$ is a dummy indicating if household has below median (working-age) household size. $Land - high$ is a dummy indicating if household has above median cultivated land. Regression (2) and (5) controls for cultivated land, and regression (3) and (6) control for household size.

Table 2: Treatment Effect on Progresa-ineligible Households' Daily Service Profits (in Peso)

	OLS	OLS	Tobit	Tobit
Panel I: October 1998				
(before within-village scale-up)				
treatment 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 within-village scale-up)				
treatment 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 within-village scale-up)				
treatment 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		

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at village level. 99th percentile of dependent variable excluded. Standard errors clustered at village level. Displayed coefficients are obtained from the regression $x_i = \alpha_0 + \beta_1 V_i + \beta_2' X_i + \epsilon \ \forall i \in$ ineligible, where V_i is a dummy if ineligible household i resides in treatment village. The dependent variable is household monthly profits (in peso) from buying and reselling, tailoring, cooking, washing, ironing, carpenter services.

Table 3: Consumption Effects of Progresa: Partial vs. General Equilibrium

	Progresa-eligible Households			Progresa-ineligible Households		
	Progresa observed (1)	Progresa predicted(GE) (2)	Progresa predicted(PE) (3)	Progresa observed (4)	Progresa predicted(GE) (5)	Progresa predicted(PE) (6)
I. Food consumption						
control group mean (C)	698.00	660.06	660.06	756.85	731.08	731.08
treatment group mean (T)	811.04	752.97	711.21	757.34	759.44	731.08
$T-C$	67.95	92.91	51.15	14.01	28.35	0.00
(s.e)	(10.01)			(11.16)		
$(T-C)/C$	0.097	0.141	0.077	0.019	0.039	0.00
II. Non-food expenditure [quantity]						
control group mean (C)	516.07[n/a]	534.95 [26.33]	534.95[26.33]	679.00[n/a]	592.51[29.17]	592.51[29.17]
treatment group mean (T)	664.12[n/a]	610.25[26.85]	576.41[28.38]	700.44[n/a]	615.49[27.09]	592.51[29.17]
$T-C$	94.15[n/a]	75.30[0.52]	41.45[2.04]	31.94[n/a]	22.98[-2.08]	0.00
(s.e)	(19.18)			(24.68)		
$(T-C)/C$	0.182 [n/a]	0.141 [0.020]	0.077[0.077]	0.047[n/a]	0.039[-0.071]	0.00

First and second row of columns (1) and (4) in panel I and II show ENCEL survey (March 1999) sample means of monthly HH food consumption (Peso value) and non-food expenditure (Peso value) of program eligible and ineligible households, respectively. The third row of column (1) in panel I and II shows the treatment effect obtained from the treatment effect regression $x_i = \alpha_0 + \beta_1 V_i + \epsilon \ \forall \ i \in$ eligible, where V_i is a dummy if i resides in a treatment village. The third row of column (4) in panel I and II shows the treatment effect obtained from the treatment effect regression $x_i = \alpha_0 + \beta_1 V_i + \epsilon \ \forall \ i \in$ ineligible. The fourth row of columns (1) and (4) in panel I and II shows displayes the ratio of treatment effect (third row) over the control group mean (first row). Columns (2) and (5) in panel I and II show the by the general equilibrium model simulated values (i.e. flexible wage). Columns (3) and (6) in panel I and II show the by the partial equilibrium model simulated values (i.e. fixed wage). Values reported in parenthesis are consumed quantities (quantities are not observed from the Progresa ENCEL surveys).

Table 4: Treatment Effect on Progresa-eligible Households' Monthly Expenditure in Peso

	Non-Food						Food
	(1) hygiene	(2) hh utensils	(3) toys	(4) clothing	(5) shoes	(6) total	(7) total
Panel I: October 1998							
(before within-village scale-up)							
treatment 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 within-village scale-up)							
treatment 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 within-village scale-up)							
treatment 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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at village level. For columns (1)-(7) the dependent variable is monthly household expenditure (in peso) for *item*. Displayed coefficients are obtained from the regression $x_i = \alpha_0 + \beta_1 V_i + \gamma X' + \epsilon \ \forall \ i \in$ eligible, where V_i is a dummy if eligible household i resides in treatment village, and X' is a vector of controls. 99th percentile of the dependent variable is excluded.

Table 5: Treatment Effect on Progresa-ineligible Households' Monthly Expenditure in Peso

	Non-Food						Food
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	hygien;e	hh utensils	toys	clothing	shoes	total	total
Panel I: October 1998							
(before within-village scale-up)							
treatment village	1.951	-0.192	0.000	0.670	-3.895	-14.854	-16.647
	(1.354)	(0.627)	(.)	(4.255)	(3.542)	(15.910)	(12.221)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	4168	4235	4379	4190	4175	4116	4162
R-squared	0.049	0.014	.	0.054	0.077	0.104	0.137
Panel II: March 1999							
(after within-village scale-up)							
treatment village	-2.196	0.588	0.026	-7.151	4.727	2.056	7.633
	(1.661)	(0.864)	(0.051)	(8.366)	(8.296)	(24.676)	(11.168)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	4316	4416	4439	4350	4347	4280	4351
R-squared	0.054	0.008	0.004	0.033	0.055	0.078	0.120
Panel III: November 1999							
(after within-village scale-up)							
treatment village	-0.086	3.034**	0.000	5.202	1.805	4.771	13.886
	(1.464)	(1.258)	(.)	(6.297)	(5.785)	(19.427)	(13.217)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	4430	4464	4651	4488	4406	4377	4342
R-squared	0.105	0.016	.	0.082	0.082	0.147	0.165

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at village level. For columns (1)-(7) the dependent variable is monthly household expenditure (in peso) for *item*. Displayed coefficients are obtained from the regression $x_i = \alpha_0+\beta_1 V_i+\gamma X'+\epsilon \ \forall \ i \in$ ineligible, where V_i is a dummy if ineligible household i resides in treatment village, and X' is a vector of controls. 99th percentile of the dependent variable is excluded.

Table 6: Parameter Values

	average eligible HH		average ineligible HH
Village level parameters			
price of food commodity ($\bar{p}_q$)		1.8	
price of non-food commodity ($\bar{p}_\chi$)		0.1	
labor requirement per unit of x ($\bar{m}$)		2.7	
Household level parameters			
food item endowment ($\bar{Q}_i$)	301.1		481.2
HH efficient units of labor ($\bar{L}_i$)	93.0		63.1
preference food item ($\bar{\alpha}_{\{q\}}$)	0.512		0.512
preference non-food item ($\bar{\alpha}_{\{x\}}$)	0.415		0.415
preference leisure ($\bar{\alpha}_{\{l\}}$)	0.074		0.074